THE KEPLERIAN REVOLUTION:
ASTRONOMY, PHYSICS, AND THE ARGUMENT FOR HELIOCENTRISM

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ABSTRACT

In the period from 1596 to 1609 Kepler succeeded in 'physicalizing' astronomy in a manner that had never before been achieved. He proposed for the first time a truly heliocentric system – a cosmology more innovative in most respects than that of Copernicus. While Kepler's importance in the history of science is frequently acknowledged, his complex and sometimes obscure texts have been given comparatively little attention. This deficiency is reflected in the fact that scholarly opinion on Kepler's natural philosophy has historically been quite diverse and, until fairly recently, even contradictory. With a few notable exceptions, few philosophers of science have concerned themselves with the details of Kepler's arguments.

This work will maintain that the new astronomy for which Kepler argued is predicated on and supported by the concurrent introduction, evident in all of the writings that will be discussed, of a new kind of scientific epistemology. The Keplerian writings, both theoretical and applied, show an innovative and sophisticated awareness of the problems associated with theory appraisal, hypothesis testing, and observational error. Kepler's scientific programme was a bold attempt to overcome not only geocentric astronomy and cosmology, but was also a refutation of scepticism in the mathematical sciences.

The epistemological theory and the corresponding methodological practices that I will identify in Kepler's works place upon astronomical hypotheses precisely the constraints that are required to overcome the kind of scepticism adopted by many of Kepler's contemporaries. In this manner Kepler's natural philosophy can be seen as an extended argument

against the 'compromise of Geminus', that is, the classical distinction between the geometrical study of astronomy and the causal-explanatory study of physics. I suggest that Kepler's success in refuting this distinction is attributable largely to two criteria the satisfaction of which Kepler distinguishes as necessary for the truth of hypotheses, criteria whose practical application can be found in the evolution of Kepler's planetary theory. The first of these Kepler refers to as the principle of "linking of syllogisms", and the second as the principle that all astronomical hypotheses entail unique "peculiar consequences in the physical realm". Kepler describes these principles most explicitly in his *Apologia pro Tychone contra Ursum* of 1600; but I will argue that they underwrite central passages in his earlier *Mysterium cosmographicum* of 1596, and are practically applied in the argumentation of the later *Astronomia nova* of 1609. Accordingly, the thesis is divided into chapters corresponding to these works. With each chapter I place the arguments of Kepler's particular works into the context of his general astronomical endeavour, making particular reference to these principles.

Keywords:

astronomy, history of astronomy, philosophy of science, history of science, Kepler, Copernicus, physical heliocentrism, arguments against scepticism.

Epigraph

The time will come when diligent research over long periods will bring to light things which now lie hidden. A single lifetime, even though entirely devoted to the sky, would not be enough for the investigation of so vast a subject... And so this knowledge will be unfolded only through long successive ages. There will come a time when our descendants will be amazed that we did not know things that are so plain to them... Many discoveries are reserved for ages still to come, when memory of us will have been effaced. Our universe is a sorry little affair unless it has in it something for every age to investigate... Nature does not reveal her mysteries once and for all.

Seneca (ca. 4 B.C.– A.D. 65) - *Natural Questions*, Book 7

Dedicated to my parents,

Heather Dawne Hemsley

and

Kenneth James Bennett

and to my muse, Hypatia.

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Introduction

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Introduction

Few cases of scientific innovation are so interesting, enlightening, and yet widely misunderstood as the developments in planetary theory accomplished by Johannes Kepler (1571-1630) in the final years of the sixteenth century and first years of the seventeenth century. While Kepler's importance in the history of science is frequently acknowledged, his complex and sometimes obscure texts have been given comparatively little attention. This deficiency is reflected in the fact that scholarly opinion on Kepler's natural philosophy has historically been quite diverse and, until fairly recently, even contradictory. With some notable exceptions, remarkably few philosophers of science have concerned themselves with the technical details of Kepler's arguments; and those few that have studied the astronomer's work closely have most often sought out some of the many interesting passages as evidence for their own previously derived convictions, or worse, simply ignored Kepler's intentions through selection and misinterpretation.

In the spirit of the charitable, but critical, apologist the present work will present a defence of the position that there are historically significant and strikingly modern epistemological and methodological principles invoked in a consistent and rational manner throughout Kepler's 'revolutionary' astronomical works. As will be shown in the following chapters, while many of the components of Keplerian astronomy evolved radically over the course of his various studies, the more general objective of his researches was remarkably coherent and constant. Kepler wanted to construct a new science of the heavens – one that would unite the classical sciences of astronomy and physics into a new kind of science. Not only was Kepler clear of purpose, but also abundantly successful in bringing his science into being. Although many defeats were suffered in his campaign to win a new understanding of the cosmos, taken as a whole, the derivation of Kepler's physical astronomy was a philosophical *tour de force* – a victory over ignorance whose gains profoundly changed astronomy and which was crucial to future strategies of scientific inquiry.

This work will maintain that the new astronomy for which Kepler argued is predicated on and supported by the concurrent introduction, evident in all of the writings that will be discussed, of a new kind of scientific epistemology. The Keplerian writings, both theoretical and applied, show an innovative and sophisticated awareness of the problems associated with theory appraisal, hypothesis testing, and observational error. In the context of planetary theory, Kepler exposed and then engaged directly the problems of empirical equivalence and of underdetermination by evidence.

The Keplerian theory of knowledge constitutes an historically unprecedented challenge to the scepticism that was prevalent in astronomy when it was proposed. Indeed, Kepler was able to refute the challenge of the sceptic on many argumentative fronts, a fact that is itself a consequence of the tenets that are found in his position. For, in addition to considerations of a philosophical nature, Kepler's epistemology was profoundly concerned with astronomical practice, with methodological issues, and with the results of their application to the serious problems of late-renaissance astronomy. Kepler not only was able to ask questions in a way that no one had before asked, but he was able to provide answers to those questions that even now are worthy of continued study. In this sense, Kepler was less the last great cosmologist of the classical tradition that includes Ptolemy and Copernicus; more was he the first cosmologist of the modern tradition that, while it has long abandoned the Keplerian world-view, still accepts many of his epistemological insights.

Attention to the *systematic* aspects of Kepler's natural philosophy can greatly inform our reading of his individual works. Moreover, Kepler himself intended that his works be read as the embodiment of a general philosophical project. It was a project that may be favourably compared with that of Descartes in its philosophical scope, and one that has been taken by some authors as demarcating the inception of the great revolution in early-modern science.[1] It was a project that resulted in what was arguably the first dynamical theory of planetary motion.

Yet it may still be plausibly argued that the full philosophical and historical import of the achievements of Kepler has not been sufficiently examined. In particular, there is no thorough account of some of the philosophically unifying aspects of the astronomer's epistemological theory and methodological practice. This work will provide such an account by assessing Kepler's own conceptualization of astronomy through his publications and correspondence, as well as examining, bringing together, and extending previous work in Kepler scholarship. The textual evidence for the position here defended will come predominantly from Kepler's own works, from many of the more didactic and rhetorical passages interspersed through his more mathematical arguments. However, the arguments proposed here would be incomplete without also examining the actual practice of the astronomer on the complicated logical paths he followed to his great discoveries.

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[1] Notably Koyré and Jardine. See Koyré, A., *The Astronomical Revolution*, Ithaca: Cornell University Press, 1973; Jardine, N., *The Birth of History and Philosophy of Science, Kepler's "A Defence of Tycho Against Ursus" with Essays on its Provenance and Significance*, Cambridge: Cambridge University Press, 1984.

Kepler's contribution to science is typically described in terms of the hypotheses that are today called Kepler's laws of planetary motion.[2] As is widely stated, these hypotheses were part of the empirical foundation for the Newtonian achievement of the *Principia*, and more generally of the great conceptual synthesis that natural science has afforded the human intellect over the last few centuries.[3] Perhaps because of his place at the beginning of this synthesis, often referred to as the Newtonian revolution, Kepler is often regarded as "the last great practitioner of the science of ancient astronomy"[4], and the Keplerian achievement as inspired, but ultimately unsuccessful. Even those studies that have concentrated explicitly on the differences of Keplerian astronomy from the science that preceded it almost invariably emphasize his determination of the elliptical orbit and of the areal law. But as shall be emphasized here, these propositions of Keplerian astronomy, albeit important, were two among many that Kepler himself considered indispensable to his more general conclusions.

While the utility of Kepler's work to later science is unquestionable, an unbiased analysis of Kepler's achievement ought to regard it as part of an ongoing process rather than as a means to a later end. Indeed, if we are to credit Kepler in terms of his personal contribution to the understanding of nature, mindful of the context of his time and circumstances, we must

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[2] This is true of a great majority of histories of astronomy. See, for example: Kuhn, T., *The Copernican Revolution*, Cambridge, Mass., Harvard University Press: 1957; Cohen, I. B., *The Birth of a New Phyiscs*, New York: 1960. The characterization of Kepler's laws, so-called (Kepler did not refer to them as laws), has itself had a fascinating history. Newton, for example, refers to the areal law in the first edition of the *Principia* as an hypothesis, but in the second and third editions as a phenomenon. See Wilson, C., "From Kepler's Law's, So-called, to Universal Gravitation: Empirical Factors", *Archive for History of Exact Sciences*, 6, Heidelberg/New York, 1970, pp. 89-170.

[3] The logical status of Kepler's laws in Newton's argument for universal gravitation is subtle and complex. See Wilson, C., *ibid.*

[4] Stephenson, B., *Kepler's Physical Astronomy*, Princeton: Princeton University Press, 1987, p. 1.

appreciate the thoroughness of his radical epistemological and methodological break with ancient astronomy at least as much as his place in astronomy's more modern history. The common emphasis on Kepler's practical contribution to later science has obscured the extraordinary philosophical advance that Kepler made over earlier science.[5]

Some recent work (due mainly to Aiton[6], Wilson[7], Gingerich[8], and Stephenson[9]) has acknowledged that the Keplerian theory of planetary motion was the first to contain many of the recognizably modern elements of a complete physical theory. But scholarship has been far more limited on the subject of the philosophical or epistemological considerations that drove Kepler to question the profoundly entrenched tenets of his contemporaries and then to build a new astronomy in their place. Although Kepler left an extensive and remarkably candid account of the ratiocinations leading to the proclamation of his astronomical theory, some of the most important of Kepler's writings in this regard have only recently been published or translated from their original Latin.[10] Modern, and especially English-

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[5] This point has been acknowledged by Peirce, Hanson, and Russell, for example. Peirce, after initially regarding Kepler's *Astronomia nova* as a vague and confused work, later described it in superlative terms, as representing, "the greatest piece of retroductive reasoning ever performed" ("Kepler", in *Selected Writings*). In *Patterns of Discovery*, Hanson supports and extends Peirce's view of Kepler's accomplishment. Russell remarks in his *History of Western Philosophy* that, "the disovery of the first law, that the planets move in ellipses, required a greater effort of emancipation from tradition than a modern man can easily realize... ".

[6] See, for example, Aiton, E. J., "Kepler's Second Law of Planetary Motion", *Isis* 60, 1969, pp. 75-90.

[7] See, for example, Wilson, C., "Kepler's Ellipse and Area Rule – their derivation from fact and Conjecture", in *Kepler: Four Hundred Years, Vistas in Astronomy* 18, Beer, A., and Beer, P., eds., Oxford: Pergamon Press, 1975, pp.587-91.

[8] See, for example, Gingerich, O., "Kepler and the New Astronomy", *Quarterly Journal of the Royal Astronomical Society* 13, 1972.

[9] Stephenson, B., *op. cit.*

[10] The *Apologia pro Tychone contra Ursum*, in particular, has long been neglected. It was first publisihed in its original Ciceronian Latin in Frisch's *Johannes Kepler opera omnia* of 1868, when it attracted the attention of Cassirier and von Prantl. It has recently been republished in English

language, history and philosophy of science is therefore only now encountering some of the significant ideas of this very crucial figure.

Notwithstanding its historical significance, what has been written on the subjects of the epistemological arguments and the methodological practices upon which Keplerian astronomy was founded has been astonishingly divergent in opinion. Recent literature (primarily due to Westman[11] and Aiton[12]) has identified two contending historiographic traditions in Kepler scholarship.

According to the oldest of these traditions, which has antecedents in the writings of those active in the Paris Academy in the last half of the eighteenth century and the first half of the nineteenth century, and which is epitomized by Delambre's *History of Modern Astronomy* of 1821, it is argued that Kepler was engaged in an essentially *a posteriori* problem akin to what would later be called statistical curve-fitting. While much was made clear with respect to the technical details of Kepler's astronomical models by authors in this tradition, the focus on an overly simplistic interpretation of Kepler as an empiricist who 'found the ellipse in the data'[13], and the analysis, on this basis, of components of Kepler's reasoning, has led to the claim (initially made by Newton[14], and

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translation. The three works upon which I will focus, Kepler's *Mysterium cosmographicum*, *Apologia pro Tychone contra Ursum*, and *Astronomia nova*, were first published in complete form in English translation in the respective years 1981, 1984, and 1992.

11 See for example, Westman, R. S., "Continuities in Kepler Scholarship: The European Kepler Symposia in Historiographical Perspective", in *Vistas in Astronomy* 18, 1975.

12 See for example, Aiton, E. J., "Johannes Kepler in Light of Recent Research", *History of Science* 14, 1976.

13 Wilson has decribed and criticized this general view. See, for example, Wilson, C., "Newton and Some Philsophers on Kepler's Laws", in *Journal of the History of Ideas* 35, 1974, pp. 231-258.

14 See Wilson, C., "Kepler's Ellipse and Area Rule – their derivation from fact and Conjecture", in *Kepler: Four Hundred Years,Vistas in Astronomy* 18, Beer, A., and Beer, P., eds., Oxford: Pergamon Press, 1975, pp.587-91.

later supported by Small[15], Berry[16], Dreyer[17], and still found widely in contemporary literature) that Kepler contributed a description of mathematical regularities rather than a physics of the planets. On this account, most of Kepler's metaphysical reasoning has either been summarily dismissed as "chimerical speculation" (Laplace[18]), or characterized as having obscured Kepler's, and consequently his readers', ability to see the valuable truths that lay buried in a mass of 'worthless speculation' (Berry[19], Sarton[20]).

Authors in the other historiographic tradition (which Westman argues developed mainly in reaction to the deficiencies of the first), such as Whewell[21] and Cassirer[22], have focussed on the non-empirical factors which guided Kepler to his astronomical theory. While Whewell's view of Kepler is perhaps more charitable than most, it has not been influential. Cassirer's account, on the contrary, was very influential, though imbued with an anachronistic, neo-Kantian critical idealism. The more extreme form of this second general tradition, adopted by authors such as Burtt[23], Pauli[24], and to a lesser extent by Koyré[25], insists that Kepler's thought was governed by various

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15 Small, R., *An Account of the Astronomical Discoveries of Kepler*, reprint, Madison: University of Wisconsin Press, 1963; first published London: 1804.

16 Berry, A., *A Short History of Astronomy*, New York: Dover Publications, 1961.

17 Dreyer, J. L. E., *A History of Astronomy from Thales to Kepler*, New York: Dover Publications, 1953; first published Cambridge, Cambridge University Press, 1905.

18 Laplace, P.S., *Exposition du Systeme du Monde*, Paris: 1796

19 Berry, A., *A Short History of Astronomy*, New York, 1961. Berry concludes his remarks on Kepler by stating "if Kepler had burnt three-quarters of what he printed, we should in all probability have formed a higher opinion of his grasp".

20 Sarton, G., "A Review of Kepler's *Die Zusammenklange der Welten*", *Isis* 4, 1922.

21 Whewell, W., *A History of the Inductive Sciences*, London: J. W. Parker, 1857.

22 Cassirer, E., *The Problem of Knowledge*, New Haven: Yale University Press, 1950.

23 Burtt, E. A., *The Metaphysical Foundations of Modern Science*, New York: Doubleday, 1954.

24 Pauli, W., "The Influence of Archetypal Ideas in Kepler", in *Interpretation of Nature and of the Psyche*, New York: 1955.

25 Koyré, A., *The Astronomical Revolution*, *op. cit.*

archetypal, mystical, religious, or other *a priori* principles. Here Kepler is seen primarily as a rationalist, a seeker of archetypal, mathematical, or theological design. The Platonic, Pythagorean, and even Zoroastrian components of Kepler's writings are taken as central and the more empirical components undervalued.

In addition to these two main traditions some other recent authors attempt either to frame Kepler in terms that combine these two approaches, such as Holton[26] and Kozhamthadam[27], or else set Kepler's thought apart from *any* consistent rational interpretation, as with Kuhn[28]. Holton argues that there are essentially three different elements to Kepler's thought, the physical, the metaphysical, and the theological; Kepler's science is seen as arising from the interaction of these three elements. Because of this, Holton views Kepler's reasoning as innovative, but fundamentally inconsistent and suffused with *ad hoc* assumptions. Similarly, Kozhamthadam claims that Kepler can only be understood in the light of a combination of scientific, philosophical, and religious principles. Kozhamthadam's account, however, suffers from a lack of technical detail, and as with Holton's account, there is no general epistemology or methodology regarded as unifying Kepler's thought. Kuhn acknowledges Kepler's role in the unfolding of the Copernican revolution, but also claims that 'revolutionary' science does not advance in any rationally coherent way. In his widely read account of Kepler, *The Watershed*, Koestler[29] also rejects the claim that Kepler's achievement was the

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26 Holton, G., "Johannes Kepler's Universe: Its Physics and Metaphysics", in *Thematic Origins of Scientific Thought*, Cambridge, Mass.: Harvard University Press, 1973.

27 Kozhamthadam, J., *The Discovery of Kepler's Laws*, Notre Dame: University of Notre Dame Press, 1994.

28 Kuhn, T., *The Copernican Revolution*, Cambridge, Mass.: Harvard University Press, 1957.

29 Koestler, A., *The Watershed*, New York: Anchor Books, 1960.

result of any rationally defended philosophical position. Here Kepler is a "sleepwalker" or speculator who more or less 'by chance', 'noticed' the regularities of planetary motion that are described in his works.[30]

The main argument of this dissertation, in its simplest form, is that while some of the more recent positions have some merit, they all fail to see Kepler's *philosophical* achievement for what I will argue it is: a *consistent unification* of both *a priori* and *a posteriori* modes of reasoning. I propose that the epistemological theory and the corresponding methodological practices that I will identify in Kepler's works place upon astronomical hypotheses precisely the constraints that are required to ensure this unification. In this manner Kepler's natural philosophy can be seen as an extended argument against the 'compromise of Geminus', that is, the classical distinction between the geometrical study of astronomy and the causal-explanatory study of physics. I suggest that Kepler's success in refuting this distinction is attributable largely to two criteria that Kepler distinguishes as necessary for the truth of hypotheses, criteria whose practical application can be found in the evolution of Kepler's planetary theory. The first of these principles Kepler refers to as the principle of "linking of syllogisms"[31], and the second as the principle that all astronomical hypotheses entail unique "peculiar

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[30] "Sleepwalker" is Koestler's term. Kuhn uses the words "by chance, noticed" to describe Kepler's recognition that the ellipse would save the appearances. See Kuhn, T., *The Copernican Revolution*, Cambridge, Mass.: Harvard University Press, 1957, p. 212. While some of the components of Kepler's theory may have been fortuitous discoveries, this in no way impugns Kepler's role in integrating them into a coherent astronomical theory. Furthermore, as Kepler's physical astronomy was accepted by virtually no one during his life, it is arguable that Kepler was in a unique position with respect to understanding the importance of these fortuitous components as useful discoveries.

[31] Kepler's actual terminology is "*syllogismorum in demonstrationibus implexum*"; see Kepler's *Apologia*, in Jardine, N., *The Birth of History and Philosophy of Science*, Cambridge: Cambridge University Press, 1984, p. 89.

consequences in the physical realm".[32] Kepler describes these principles most explicitly in his *Apologia pro Tychone contra Ursum* of 1600; but I will argue that they underwrite central passages in his earlier *Mysterium cosmographicum* of 1596, and are practically applied in the argumentation of the later *Astronomia nova* of 1609. Accordingly, the thesis is divided into chapters corresponding to these works. With each chapter I place the arguments of Kepler's particular works into the context of his general astronomical endeavour, making particular reference to these principles.

With the benefit of recent scholarship it has become clear that Kepler was an intensely speculative thinker, a thinker who made great intuitive advances over his predecessors. But he was also a thinker whose ideas were continually pitted against the facts of nature in a long struggle to find the most viable among them. Kepler was not a confused man, given to posterity through accident or as a historical curiosity in the development of a more mature science. It will be shown here that quite the opposite is the case.

Kepler demonstrates an unparalleled understanding of the logic of scientific knowledge and inference. In keeping with the classical sense of 'logic', his studies pertain to syllogistic logic proper, and its use in astronomy, but also to epistemological and metaphysical concepts. Thus Kepler's works can be seen as partly rhetorical works. They are meant to argue for a new way of perceiving the structure of the cosmos, and therefore a new science. There has not really been an account of Kepler's science that presents a unified scientific philosophy, an account that focuses on the continuity of Kepler's endeavours in astronomy, on the subtle combination of theoretical objectives and the results of their application to the celestial appearances.

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[32] Jardine, N., *The Birth of History and Philosophy of Science*, Cambridge: Cambridge University Press, 1984, pp. 141-142.

In the period from 1596 to 1609 Kepler succeeded in 'physicalizing' astronomy in a manner that had never before been achieved. He proposed for the first time a truly heliocentric system – a cosmology more innovative in most respects than that of Copernicus. Indeed, it is arguable that Kepler's science was simultaneously the greatest advance in astronomy since Ptolemy's *Almagest* as well as the greatest advance in the application of physical concepts to the heavens since Aristotle's *De caelo*. It is hardly surprising, therefore, that Kepler regarded Ptolemy and Aristotle as his philosophical mentors. Yet they were also his philosophical adversaries. This is only to say that Kepler attempted to achieve and even surmount the same ends as his predecessors - the provision of a philosophical system that could 'save the appearances' and also causally explain them.

For the first time it is possible to do reasonable historiographic justice to some of the systematic features of Kepler's programme in astronomy. Several recent developments are responsible for this.

The earliest of Kepler's works, the *Mysterium cosmographicum*, has only recently been assessed in the light of a good understanding of the central importance of physical principles in the author's reasoning. The physicalization of astronomy that Kepler proposed entails that astronomical hypotheses depend only on actual physical bodies moving through absolute space. As will be explicated here, this restriction has very serious consequences for a theory of the planets, consequences, however, of which Kepler was uniquely aware and that he strove to accommodate even in the earliest developments of his system. As well as addressing these physical suppositions, Kepler's *Mysterium* confirms an equally strong commitment to logical principles in astronomy. Kepler argues that all of the distinct planetary hypotheses must cohere into a general theory, which although it is about physical entities, is nonetheless mathematically consistent. This position has its source in the *Narratio prima* of Rheticus, and is evident as early as Kepler's disputation at Tübingen, from which the *Mysterium* was in part derived. These early writings contain the ancestors to Kepler's arguments in the *Astronomia nova*, arguments whose conclusions are of unique historical importance - for they represent the first practical unification of classical physics with classical mathematical astronomy. In saying that he had merged these sciences, Kepler was of necessity also saying that he did not agree with some of the principal tenets of classical science.

Another development which allows present-day historians and philosophers of science further confidence in their assessments of Kepler's philosophy, consists in the recent examinations of the more methodological of Kepler's writings, especially the *Apologia pro Tychone contra Ursum*. This work has been taken by some to mark the very birth of history and philosophy of science as a distinctive mode of reasoning on the status of the natural sciences. In its originality and in its anticipation of more modern arguments on the status of science, the *Apologia* is, on this view, at least as significant an achievement as such later works as Bacon's *Novum organum* and Descartes' *Principia philosophiae.* In the *Apologia* Kepler presents his theoretical response to scepticism in astronomy - a refutation of the kind of position commonly adhered to by many of his contemporaries. Kepler was addressing a particular kind of scepticism that relied heavily on 'the argument from empirical equivalence'. So Kepler's scientific programme was a bold attempt to overcome not only geocentric astronomy and cosmology, but was also, in theory and in practice, a refutation of scepticism in the mathematical sciences. Kepler wanted to persuade his contemporaries, and posterity, that he was correct, that he had grasped a great truth - physical heliocentrism. He believed he could share this truth with others through argument: he believed that once the new principles of astronomy as well as the empirical data were carefully examined, reason would lead the reader to the same conclusions as Kepler himself.

It is only recently that history and philosophy of science communities have acknowledged that the account given in the *Astronomia nova* is only superficially historical; it has become clear that Kepler rewrote some parts of the book many times, modifying the argumentative structure in the process. In depicting the process of discovery that is contained in the *Astronomia nova* as chronological, we may consider either that the author was disingenuous or deceitful, or rather that he was mindful of the inevitable arguments of his opponents and the need for change in the rhetorical, explanatory, and epistemological presuppositions of astronomy. The evidence is with the latter, in that Kepler's intentions are clearly proclaimed in the introduction to the summaries of the individual chapters of the *Astronomia nova*.

Recent studies of Kepler's correspondence and manuscripts reveal a more complete description of the philosophical context of Kepler's writing in the period formative to the *Astronomia nova* than had previously been possible. Caspar was the first to acknowledge some of the differences between the manuscript and the printed text of the *Astronomia nova*, but he restricted his study to the aim of publishing it in *Johannes Kepler Gesammelte Werke*, that is in form of final texts.[33] Aiton clarified the logical status of the two 'laws' of planetary motion that appear in the *Astronomia nova*. He determined that the areal law and the elliptical shape of the orbit may be considered as

[33] See *Johannes Kepler Gesammelte Werke*, vol. III, ed., Caspar, M., Munich: C. H. Beck, 1937.

logically independent, but that they are not coherent 'laws' if they are regarded as separate physical principles.[34] Gingerich first identified the convoluted path that Kepler took in performing some of the calculations that are merely summarized in the *Astronomia nova*.[35] Wilson first identified many of the complexities of Kepler's arguments that were imposed by the mathematical and physical problems themselves. He also pointed out flaws in various assumptions made in many other accounts of Kepler in this regard. In addition, Wilson examined the role that the theories of the inner planets had on Kepler's thought in the period between the *Mysterium cosmographicum* and the *Astronomia nova*.[36] Donahue continued in the direction of Gingerich's examinations and emphasized the internal evidence that Kepler rewrote many parts of the *Astronomia nova* for rhetorical reasons before it went to print.[37] Jardine has explored Kepler's arguments against scepticsm in astronomy and their lack of precedent in the natural philosophy of the renaissance.[38] Stephenson was the first to really identify and examine the profound

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[34] See Aiton, E. J., "The Elliptical Orbit and the Area Law", in *Kepler: Four Hundred Years,Vistas in Astronomy* 18, Beer, A., and Beer, P., eds., Oxford: Pergamon Press, 1975, pp. 173-193.

[35] See Gingerich, O., "The Computer versus Kepler", *American Scientist*, 52, 1964, pp. 218-226; and Gingerich, O., "Kepler's Treatment of Redundant Observations, or, the Computer versus Kepler Revisited, in *Internationales Kepler-Symposium, Weil der Stadt*, Krafft, F., Meyer, K., and Sticker, B., eds., Hildesheim: 1973, pp. 307-314.

[36] See Wilson, C., "Kepler's Derivation of the Elliptical Path", *Isis* 59, 1968, pp. 5-25; Wilson, C., "The Inner Planets and the Keplerian Revolution", in *Centaurus* 17, 1973, pp. 205-248; Wilson, C., "Kepler's Ellipse and Areal Rule - Their Derivation from Fact and Conjecture", in *Kepler: Four Hundred Years,Vistas in Astronomy* 18, Beer, A., and Beer, P., eds., Oxford: Pergamon Press, 1975, pp. 587-591.

[37] See Donahue, W. H., "Kepler's Fabricated Figures: Covering up the Mess in the Astronomia nova", *Journal for the History of Astronomy*, 19, 1988, pp. 217-237.

[38] See Jardine, N., *The Birth of History and Philosophy of Science, Kepler's "A Defence of Tycho Against Ursus" with Essays on its Provenance and Significance*, Cambridge: Cambridge University Press, 1984; Jardine, N., "The Forging of Modern Realism: Clavius and Kepler Against the Sceptics", *Studies in History and Philosophy of Science*, 10, 1979, pp. 141-173.

influence of physical principles in Kepler's astronomy and the development of Kepler's physical arguments. He also acknowledged some of the rhetorical intentions of *Astronomia nova*, suggesting that the nature of the changes that Kepler wished to make to astronomy forced him to write his great work with a didactic purpose rather than as a simple chronological account.[39] Voelkel has introduced the context of Kepler's correspondence, and through it provides insight into both Kepler's reasoning and modes of argument, but also into the divergent reasoning of the contemporaries to which the *Astronomia nova* was addressed.[40]

The central aim of this thesis is to reconstruct and assess the success of Kepler's argument for heliocentrism in light of this recent scholarship. The focus will be on the components of Kepler's philosophy that are found to be common to his major works - as such, the systematic aspects of Keplerian epistemology and methodology are explored in detail.

The more reflective passages of the *Mysterium* and the first part of the *Apologia* affirm Kepler's strong commitment to the rational investigation not only of astronomical hypotheses themselves but also of the historical and philosophical assumptions that underwrite them. When viewed in its proper rhetorical context the *Astronomia nova* becomes all the more significant as an historical document. We can see that the *Astronomia nova* is a practical example of the application of the Keplerian epistemology and methodology to what were then the central problems of astronomy. Given this context, this

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[39] See Stephenson, B., *Kepler's Physical Astronomy*, Princeton: Princeton University Press, 1987.
[40] See Voelkel, J. R., *Johannes Kepler and the New Astronomy*, New York: Oxford University Press, 1999.

work reassesses the soundness of Kepler's argument, and of his views on scientific method and epistemology.

Keplerian science needs to be interpreted in the context of several theoretical domains - the practical, calculating, domain of the mathematical astronomer who is concerned with 'saving the phenomena', the causal-explanatory domain of the physicist who is concerned with the accounting for the phenomena, and the conjectural or suppositional domain of the metaphysician who is concerned with the substance and structure of the cosmos. Several kinds of reasoning are evident in Kepler's astronomy. Perhaps this feature explains why almost any agenda in philosophy of science can find some support in examples of Keplerian reasoning. But to isolate one of these divergent modalities of argument from the others is necessarily to diminish the most general goal of Kepler's labours, the provision of a new science of the heavens that integrates them.

Chapter 2

Copernican Heliocentrism

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Copernican heliocentrism

In the middle of the sixteenth century, fourteen centuries after the death of Ptolemy (c. A.D. 100-170), his *Almagest* still dominated all of astronomy. With the renaissance of interest in the works of the ancients, the relevance of the Ptolemy's great work had not only not diminished, but was on the increase.[41] But by the end of the sixteenth century and the beginning of the seventeenth the Ptolemaic system was facing serious challenges.[42] The subsequent revolutionary transformation from the geocentric to the heliocentric worldview has been almost universally attributed to the works of Nicholas Copernicus (1473-1543) and the debate over the central claims of the Ptolemaic theory that they supposedly generated in the decades following their publication. This historical position, however, requires qualification. A clear understanding of the historical circumstances surrounding the debate over Ptolemaic astronomy will serve to throw Kepler's astronomy into stark contrast with the astronomy of Copernicus.

Although Copernican astronomy is best known for the radical proposition that the Earth is in motion about the Sun rather than *vice versa*, this was not a claim without precedent, as Copernicus freely acknowledged. In the dedication of his *De revolutionibus orbium coelestium* of 1543 he wrote,

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[41] See van Helden, A., *Measuring the Universe*, Chicago: University of Chicago Press, 1986, pp. 41-53.

[42] For two interesting accounts of this historical period, see Lattis, J., *Between Copernicus and Galileo: Christoph Clavius and the Collapse of Ptolemaic Cosmology*, Chicago: University of Chicago Press, 1994; and Jardine, N., "The Forging of Modern Realism: Kepler and Clavius Against the Sceptics", *Studies in the History and Philosophy of Science*, 10, 1979, pp. 141-173.

> "For a long time, then, I reflected on this confusion in the astronomical traditions concerning the derivation of the motions of the universe's spheres. I began to be annoyed that the movements of the world machine, created for our sake by the best and most systematic Artisan of all, were not understood with greater certainty by the philosophers, who otherwise examine so precisely the most insignificant trifles of this world. For this reason I undertook the task of rereading the works of all the philosophers which I could obtain, to learn whether anyone had ever proposed other motions of the the universe's spheres than those expounded by the teachers of astronomy in the schools. And in fact, I first found in Cicero that Hicetas supposed the Earth to move. Later I also discovered in Plutarch that certain others were of this opinion [whereupon he mentions the Pythagoreans]. Therefore, having obtained the opportunity from these sources, I too began to consider the mobility of the Earth. And even though the idea seemed absurd, nevertheless I knew that others before me had been granted the freedom to imagine any circles whatever for the purpose of explaining the heavenly phenomena"[43]

Copernican heliocentrism emerged from a profound dissatisfaction with the fundamental principles upon which Ptolemy had based his system of the heavens. In particular, the Ptolemaic assumption of the *punctum equans* Copernicus viewed as contrary to the admissible explanations for the true motions of the planets. The *punctum equans*, or equant point, is an eccentric point about which the planet is supposed to subtend a uniform angular speed.

[43] Copernicus, N., *De revolutionibus orbium coelestium*, Nuremberg: 1543, Book V. See Rosen, E., trans., *Nicholas Copernicus, On the Revolutions*, Baltimore: Johns Hopkins University Press, 1992, pp. 4-5.

With this device Ptolemy could account for the apparent variation of the speed of the planets while preserving uniform motion of a kind. But this supposed uniform angular motion about an eccentric point was for Copernicus an inadequate basis for astronomical hypotheses. For motion in accord with the equant deviates from truly uniform circular motion.

Copernicus' first exposition of his heliocentric theory was a small tract called the *Commentariolus*, which was written before 1514 and which was only circulated in manuscript form. In the introduction of the *Commentariolus*, Copernicus raised his main objection to the Ptolemaic theory. He noted that historically the Ptolemaic theory had been adopted in place of the previously accepted theories of both Eudoxes and Calippos because it corresponded better with the observed planetary phenomena. However, Copernicus proposed that there are several problems with the Ptolemaic account irrespective of the fact that it can account for the numerical data. He states,

> "The theories concerning these matters that have been put forth far and wide by Ptolemy and most others, although they correspond numerically [with the apparent motions], also seemed quite doubtful, for these theories were inadequate unless they also envisioned certain equant circles, on account of which it appeared that the planet never moves with uniform velocity, either in its deferent sphere or with respect to its proper centre. Therefore, a theory of this kind seemed neither perfect enough nor sufficiently in accordance with reason."[44]

[44] Quoted from Swerdlow, N., "The Derivation and First Draft of Copernicus's Planetary Theory, A Translation of the Commentariolus with Commentary", in *Proceedings of the American Philosophical Society*, vol. 117, No. 6, 1973, p. 434.

It is clear that Copernicus had no objection to the Ptolemaic theory on the basis of any disagreement with experience. He allowed that Ptolemy's astronomical theory when compared with observations might sometimes be in error from them by as much as several degrees. As did his predecessors, Copernicus simply disregarded these discrepancies. So far as all these astronomers were concerned Ptolemaic astronomy succeeded in describing the appearances of the heavens. Copernicus instead argued that the primary fault of the Ptolemaic system is that it fails to maintain the venerable principle of explaining the planets' movements by means of uniform circular motions alone. This criticism was not new with Copernicus. Precisely the same objection had been proposed by several Arabic astronomers in their earlier attempts to provide an alternative to the Ptolemaic system.[45] Copernicus, however, is unequivocal in asserting that the principle of 'perfect' motion should be maintained in astronomical theories:

> "Therefore, when I noticed these [difficulties], I often pondered whether perhaps a more reasonable model composed of circles could be found from which every apparent irregularity would follow while everything in itself moved uniformly, just as the principle of perfect motion requires. After I had attacked this very exceedingly difficult and nearly insoluble problem, at last it occurred to me how it could be done with fewer and far more suitable devices than had formerly been put forth if some postulates, called axioms, are granted..."[46]

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[45] See Pedersen, O., *Early Physics and Astronomy*, Cambridge: Cambridge University Press, 1993, p. 265.

[46] Quoted from Swerdlow, N., "The Derivation and First Draft of Copernicus's Planetary Theory, A Translation of the Commentariolus with Commentary", in *Proceedings of the American Philosophical Society*, vol. 117, No. 6, 1973, pp. 435-6.

The focus of Copernicus' critical arguments on the *a priori* elements of Ptolemaic astronomy is confirmed in his *De revolutionibus.* It was these critical inquiries that would lead Copernicus to consider the heliocentric hypothesis.

The written record contains little of Copernicus' development of his heliocentric hypothesis itself, and it is therefore difficult to follow in much detail the circumstances of his reasoning towards heliocentrism. The main reason given by Copernicus for the proposition that the Earth circles the Sun is that the motions of the other planets are thereby made much simpler than in a geocentric theory. He states in the *Commentariolus*,

> "The retrograde and direct motion that appears in the planets belongs not to them but to the [motion] of the Earth. Thus, the motion of the Earth itself accounts for a considerable number of apparently irregular motions in the heavens".[47]

Both Aristotle and Ptolemy had proposed in their respective works several arguments for the location of the immovable Earth at the centre of the world system. Copernicus confronts these arguments in Book 1 of *De revolutionibus*, but in a manner that is rather surprisingly brief and unsystematic. From the condensed way in which he considers these issues, it appears that either he failed to anticipate even the most obvious objections to his heliocentric axiom, or what is far more likely, that he wished his readers to consider the proposition as a reasonable conjecture whose facility he would demonstrate,

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[47] Quoted from Swerdlow, N., "The Derivation and First Draft of Copernicus's Planetary Theory, A Translation of the Commentariolus with Commentary", in *Proceedings of the American Philosophical Society*, vol. 117, No. 6, p. 436.

and that he did not wish his writings to become entangled in the broad philosophical issues that he knew his astronomical theory would precipitate.

The anonymous preface to *De revolutionibus* emphasizes the purely stipulative character of its hypotheses.[48] It states that astronomical hypotheses generally should not be assumed to correspond to a physical reality and moreover that this type of assumption falls outside of the purview of the legitimate objectives of astronomy. That Copernicus himself regarded his cosmological system as more than a fictitious stipulation, as physically true, can be seen throughout the text of *De revolutionibus*. This is consistent with his *Commentariolus* where he distinguished his intentions from those of earlier heliocentrists:

> "In case anyone believes that we have asserted the movement of the Earth for no good reason along with the Pythagoreans, he will also receive considerable evidence [for this] in the explanation of the circles."[49]

Copernicus explained that although simply giving an account of the circles in the heavens can do no more than assure the practitioner of the mathematical possibility of the theory, he had in addition given an account of the circular motions that is consistent with the true principles of astronomy, and that he therefore did not merely stipulate his hypotheses as fictitious suppositions for the sole purpose of calculation.

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[48] The preface was shown by Kepler to have been composed by Andreas Osiander, who saw *De revolutionibus* to print.
[49] Quoted from Swerdlow, N., "The Derivation and First Draft of Copernicus's Planetary Theory, A Translation of the Commentariolus with Commentary", in *Proceedings of the American Philosophical Society*, vol. 117, No. 6, p. 439.

With the fundamental assumption of heliocentrism in place, Copernicus elaborated on its consequences for the determination of the theories of the individual planets. While this theoretical embellishment is treated very generally in the *Commentariolus*, it is worked out in great detail in *De revolutionibus*. The author there demonstrates how his heliocentric hypotheses are able to explain, to the same degree of precision as the Ptolemaic hypotheses, all of the prograde and retrograde motions of the planets, as well as the maximum elongations of the inferior planets. Unlike Ptolemy, however, Copernicus could claim to determine all of the necessary parameters of his system from the planetary hypotheses themselves.[50] In the Ptolemaic system the planets are arranged in order of increasing sidereal period. But this convention is open to dispute and is, in addition, complicated by the fact that the Sun, Mercury, and Venus all have identical sidereal periods. It was generally agreed that the Moon must be the lowest body, as it eclipses all the others. But the superlunary Ptolemaic sequence of Mercury-Venus-Sun-Mars-Jupiter-Saturn was accepted on authority alone; Ptolemy provided no compelling evidence for it.[51] Several astronomers disagreed with Ptolemy's ordering. But more often than not the ordering of the planets' spheres was considered an inconsequential issue – for in mathematical astronomy the ordering is simply irrelevant. However, on the Copernican account, the Earth is in motion rather than the Sun, so that estimates for the relative sizes and eccentricities of the heliocentric planetary orbits follow

[50] Copernicus, N., *De revolutionibus orbium coelestium*, Nuremberg: 1543, Book V. See Rosen, E., trans., *Nicholas Copernicus, On the Revolutions*, Baltimore: Johns Hopkins University Press, 1992, pp. 227-304.
[51] See van Helden, A., *Measuring the Universe*, Chicago: University of Chicago Press, 1986, p. 42.

directly from the observed motions.[52] Furthermore, the ordering of the spheres by their sidereal periods does not suffer the complication of the Ptolemaic theory, and is fully commensurate with the geometric determinations. Copernicus states,

> "Having thus assumed the motions which I ascribe to the Earth,... by long and intense study I finally found that if the motions of the other planets are correlated with the orbiting of the Earth, and are computed for the revolution of each planet, not only do their phenomena follow therefrom but also the order and sizes of all the planets and spheres, and heaven itself is so linked together that in no portion of it can anything be shifted without disrupting the remaining parts and the universe as a whole."[53]

This form of argument, based on the congruence of multiple hypotheses, would be central to the later claims of Kepler, whose 'principle of the linking of syllogisms' owes its inspiration to this aspect of Copernicus' position.

Copernicus desired to renovate astronomy. He believed that he had achieved this through the elimination of the Ptolemaic equant and through the reduction of the geometric complexity and the independence of the individual planetary hypotheses that geocentrism entails. While it is true that Copernicus' theory exhibits a systematic coherence that is absent from the Ptolemaic theory, the claim that his theory is simpler than Ptolemy's is much less defensible. Though Copernicus had no equant points in his hypotheses,

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[52] See van Helden, A., *ibid.*, chapter 5, for a brief account of the geometrical inferences that Copernicus employed to determine the dimensions of the orbs.
[53] Copernicus, N., *De revolutionibus orbium coelestium*, Nuremberg: 1543. See Rosen, E., trans., *Nicholas Copernicus, On the Revolutions*, Baltimore: Johns Hopkins University Press, 1992, p. 5.

he did, in a way, maintain the equant through the use of corresponding epicycles, differing from Ptolemy only in the manner in which the eccentricities of the motion are distributed amongst the compounded circular motions.[54] In terms of the mathematical construction of his planetary hypotheses Copernicus was a philosophical conservative. No new measurements were required for Copernicus to make the new deductions of the ratios and eccentricities of the orbs in *De revolutionibus*; he computed these values directly from the Ptolemaic hypotheses. While he did employ his own observations to confirm his hypotheses and to derive estimates of minimal and maximal planetary distances, these were few in number. Notwithstanding their heliocentric physical interpretation, the geometric devices of the Copernican hypotheses were based on those developed much earlier, but cleverly transformed into epicycles so as to avoid the use of non-uniform circular motions, and inverted into heliocentric form so as to allow for the determination of the coherent structure of the world system.

The heliocentric theory of Copernicus is not, strictly speaking, heliocentric, or even heliostatic; the Sun, for Copernicus, is not the centre of the universe. It is instead the centre of the Earth's circular orb that is at the centre of the Copernican system of the world and which is assumed to be absolutely stationary. The centre of this orb, to which the Sun itself is eccentric, is referred to as the 'mean Sun', and this point provides the mathematical origin in Copernicus' theory. The Sun was assumed by Copernicus to execute two long-period motions about the mean Sun – in order to account for the precession of the equinoxes and the change in the apsides of

[54] See Pedersen, O., *Early Physics and Astronomy*, Cambridge: Cambridge University Press, 1993, pp. 275-278.

the Earth's orb.[55] In the first chapter of his *De revolutionibus* Copernicus writes, "Near the Sun is the centre of the universe".[56] All celestial motions are referred to the mean Sun, a point whose projection onto the ecliptic has a constant Easterly rate of change, so that in the period of one solar year, it has moved through its complete circle of 360 degrees. The Sun does not occupy a central position, though it is very close to the centre of the Earth's orb. Hence the Earth, or rather the centre of its orb, provides the privileged frame of reference for the determination of all the motions attributed to the individual planets. There is an exactly corresponding use of the mean Sun in the geocentric theory of Ptolemy and in all the theories of the ancients.[57] In fact, it is clear from *De revolutionibus* that Copernicus consciously employed the Ptolemaic theory of the motion of the Sun in developing his own theory of the motion of the Earth. Ptolemy's account is itself derived from that of Hipparchos (c. 190-120 B.C.).[58]

Even if this deviation from true heliocentricity is disregarded and the assumption that the Sun is at the centre of the universe is allowed, according to Copernicus it is not in any significant way the dynamical centre of the universe. There is, on the accounts given in the *Commentariolus* and *De revolutionibus*, no causal role for the Sun. The planetary orbs that Copernicus and other astronomers of his time supposed to exist could provide a plausible explanation of circular motion without the requirement that a material body be near the centre of the motion or be said to cause the motion. Thus, the

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[55] These motions have respective periods of 3,434 and 53,000 years. If a small enough duration is considered, the Sun may be regarded as stationary. See Pedersen, O., *ibid.*, p. 272.

[56] "*Circa ipsum (solem) esse centrum mundi.*", Copernicus, *De Revolutionibus*, Nuremberg: 1543, trans. Rosen, E., *On the Revolutions*, Johns Hopkins University Press, Baltimore: 1978, p. 20.

[57] See Pedersen, O., *Early Physics and Astronomy*, Cambridge: Cambridg. Univelsity Press, 1993, p. 271-2

[58] Ptolemy, *Almagest*, Book III.

assumption of great planetary orbs did not so much explain the circular motions of the planets but rather defined it as an essential property of them, and thereby rendered all further consideration of causal explanations unnecessary.

None of these qualifications of Copernicus' achievement should be taken to imply that he did not produce an original and significant astronomical system. What Copernicus unquestionably accomplished was to show, in a work of obviously respectable standard, that a heliocentric system of hypotheses could be both mathematically consistent and satisfy the appearances. Astronomers who read *De revolutionibus* with any seriousness were forced to admit that the new system had at least the same empirical merit as the older theories. In the sense that the Copernican hypotheses take intervening observations into account in certain determinations, they are empirically superior.[59]

Aside from the fact that his heliocentric hypotheses could 'save the appearances', Copernicus did what no astronomer had been able to do since the time of Aristotle, that is, give a heliocentric theory legitimacy enough not to be rejected *prima facie*. The Copernican hypotheses collectively undermined the *a posteriori* grounds for rejecting heliocentrism, and consequently those astronomers who construed astronomy in a realist mode were made to consider the physical possibility of a non-geocentric and therefore non-Aristotelian astronomy, if only to dismiss it on grounds of scepticism.

This brief account of Copernican heliocentrism would be incomplete without mentioning the first published work describing the Copernican

[59] Copernicus, N., *De revolutionibus*, Book V, chapters 9, 11, 14, 19, 21, 27. This point is made by Rheticus in his *Narratio prima*.

system – the *Narratio prima*[60] of Joachim Rheticus (1514-1576), published in 1540, three years previous to *De revolutionibus*. Kepler's arguments in the *Mysterium cosmographicum* were undoubtedly inspired by Rheticus' brief work. Not only did Rheticus receive credit in the *Mysterium* for first presenting to the world many of the important virtues of Copernicus' heliocentric theory, but the *Narratio* was republished as an appendix to Kepler's *Mysterium* so that Rheticus' views might receive a wider audience.

A survey of some of the most significant European astronomical texts of the period immediately prior to Kepler's works would include the following: Clavius' *In sphaeram Johannis de Sacro Bosco commentarius*, *Astrolabium*, and *Gnomonices*, and Peurbach's *Novae theoreticae planetarum*. All of these treatises, as well as Copernicus' *De revolutionibus*, share the same basic rhetorical structure – the arguments are presented in the form of a deductive axiomatic system. This form, which is typical of almost all Renaissance astronomical texts, has its antecedent in Ptolemy's *Almagest*, whose form is in turn directly derived from Euclid's *Elements*. However, as its title would suggest, Rheticus' *Narratio prima* is written in a completely different style from these works. Rheticus was not so much concerned with the mathematical details of Copernicus' theory as with rational persuasion that the theory is true. The *Narratio* is written in an argumentative, rhetorical style. It was intended to convince a sceptical reader of the plausibility of the heliocentric system – that it corresponds to a physical truth, and is not merely a mathematical account of the observations.

Rheticus begins his *Narratio* by showing that Copernicus' astronomy can be seen as a natural progression of the work of his predecessors, and

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[60] Rheticus, J., *Narratio prima*, in *Three Copernican Treatises*, 2ND ed., Rosen, E., New York: Dover, 1959.

especially of Ptolemy. In the first third of the work Copernicus is described as the heir of Ptolemy and as the astronomer who, with Ptolemy, has developed the science of astronomy to its proper form. Here Rheticus describes the work of Copernicus as written "in imitation of"[61] Ptolemy, and as with Ptolemy, Copernicus is praised for his prowess as a mathematician, for his use of observation as the basis of his theory, and for the consistency of his methods of exposition and proof. Nowhere in this early part of the *Narratio* is the supposition that the Earth is in motion about the Sun mentioned. Rheticus is in this part of his work concerned solely with the empirical facility of Copernicus' hypotheses. In this regard he suggests that Copernicus has the greater burden of proof than does Ptolemy, since the new system can account for all of the observations of his predecessors as well as those made by Copernicus himself.

But, Rheticus argues, the congruence of geometry and observations is not the only goal of astronomy. He claims that the astronomer should do more than abstract mathematical descriptions from the appearances. In a passage that clearly shows his awareness of the mathematical equivalence of astronomical hypotheses, he states that mathematics by itself will lead the astronomer down an endless path, from which a concern for the true motions that occur in the heavens is the only escape.[62] It is thus the aim of the astronomer, according to Rheticus, to seek out these true motions – and this, he suggests, Copernicus has done most admirably. Having delineated this objective of a complete astronomy, Rheticus abruptly changes his perspective on the astronomical theories of tradition. He states:

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[61] Rheticus, J., *Narratio prima*, in *Three Copernican Treatises*, 2ND ed., Rosen, E., New York: Dover, 1959, p. 131.
[62] Rheticus, J., *Narratio prima*, in Rosen, E., *ibid.*, pp. 163-164.

> "... nevertheless, the observations of all scholars and heaven itself and mathematical reasoning convince us that Ptolemy's hypotheses and those commonly accepted do not suffice to establish the perpetual and consistent connection and harmony of celestial phenomena and to formulate that harmony in tables and rules. It was therefore necessary for my teacher to devise new hypotheses, by the assumption of which he might geometrically and arithmetically deduce with sound logic systems of motion like those which the ancients and Ptolemy ... once perceived ... and which careful observations reveal as existing in the heavens to those today who study the remains of the ancients."[63]

Here the meaning of the term 'hypotheses' is clearly intended to include more than a satisfaction of empirical constraints. Rheticus argues that the saving of appearances, while certainly a necessary component of astronomical hypotheses, is not sufficient. This innovative position, which is nowhere so clearly stated by Copernicus himself, is much elaborated by Kepler in his *Mysterium*. Moreover, while Ptolemy may have regarded astronomical hypotheses in a like manner, he failed to achieve the goal of providing a complete system of such hypotheses.[64]

Rheticus' central claim in the *Narratio* is that the supposition of heliocentrism allows for the parsimonious derivation of many astronomical facts. He says,

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[63] Rheticus, J., *Narratio prima*, in *Three Copernican Treatises*, 2ND ed., Rosen, E., New York: Dover, 1959, p. 132.
[64] See Gross, A., *The Rhetoric of Science*, Cambridge, Mass., Harvard University Press, 1996, chapter 7, pp. 97-110.

> "These phenomena, besides being ascribed to the planets, can be explained, as my teacher shows, by a regular motion of the spherical Earth; that is by having the Sun occupy the centre of the universe, while the Earth revolves instead of the Sun on the eccentric."[65]

Rheticus explains how, from virtually the same set of observations and planetary hypotheses as were employed by Ptolemy, Copernicus arrived at a conclusion that is physically diametrically opposed to the central tenet of Ptolemaic astronomy. He portrays Copernicus as having been reluctantly forced by the economy of the heliocentric axiom to consider it as physically true:

> "Lest any of the motions attributed to the Earth should seem to be supported by insufficient evidence, our wise Maker expressly provided that they should all be observed equally perceptibly in the apparent motions of all the planets; with so few motions was it feasible to satisfy most of the necessary phenomena of nature."[66]

Indeed he proposes that the supposition yields astronomical truths in such abundance that, "it seems impossible that the Earth should occupy the centre".[67] Rheticus also notes that the system of Copernicus, by virtue of its systematic determinations of the planetary hypotheses, is fundamentally different than previous accounts. Because of this important difference, he suggests than any astronomical theory that seeks to displace the Copernican

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[65] Rheticus, J., *Narratio prima*, in *Three Copernican Treatises*, 2ND ed., Rosen, E., New York: Dover, 1959, p. 135.
[66] Rheticus, J., *ibid.*, p. 161.
[67] Rheticus, J., *ibid.*, p. 137.

theory will face the challenge of meeting this systematic criterion. Rheticus says,

> "The assumption of the motion of the Earth on an eccentric [leads to] a sure theory of celestial phenomena, in which no change should be made without at the same time re-establishing the entire system, as would be fitting, once more on proper ground."[68]

Once again, Rheticus has emphasized a feature of heliocentrism that Kepler will later seize upon and develop not only philosophically in his *Mysterium* and *Apologia*, but practically and quantitatively in the *Astronomia nova*.

In one important respect Copernicus failed to meet his own goal in providing a new theory of the heavens. By his own admission, his heliocentric theory was underdetermined by the evidence that he had for it. The problem that Copernicus acknowledged is that combinations of epicycles and eccentrics provide innumerable solutions that will satisfy the appearances. All of these successful combinations are empirically equivalent, and thus any of them is as well supported by the observations as the others. Although it is clear that Copernicus believed that heliocentricity corresponded to physical truth, that the *particular* combination of epicycles and eccentrics that he proposed is real, he could not demonstrate. He makes this clear in *De Revolutionibus*:

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[68] Rheticus, J., *Narratio prima*, in *Three Copernican Treatises*, 2ND ed., Rosen, E., New York: Dover, 1959, p. 140.

> "Since so many arrangements lead to the same result, I would not readily say which one is real, except that the perpetual agreement of the computations and the phenomena compels the belief that it is one of them."[69]

This problem would remain for Kepler to engage a half-century after Copernicus and Rheticus. Its solution would require more than a new astronomy – it would require a reconceptualization of the very science of astronomy itself.

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[69] Copernicus, N., *De revolutionibus*, in *On the Revolutions*, Dobrzycki, J., ed., Rosen E., trans., London: Macmillan, 1978, p. 164.

Chapter 3

Kepler's Response to the Mystery: a New Cosmography

Chapter 3

Kepler's Response to the Mystery: a New Cosmography[70]

This chapter will examine the formative role of the *Mysterium cosmographicum* in Kepler's arguments for his new astronomy. One of the most undervalued components of the Keplerian astronomical writings is the epistemological continuity and coherence evident in his argumentation. These systematic characteristics are evident both in Kepler's comments directly on the subjects of natural philosophy and its history, and in the methodology that is deployed in the applied parts of his works. It will be suggested here that such a unity of epistemological considerations is not only a common factor, but also an essential component of the most important advances Kepler made in revolutionizing astronomy.

The *Mysterium cosmographicum* was the first major publication after Nicholas Copernicus' *De revolutionibus* to argue for heliocentrism.[71] Although Kepler defends the theory of Copernicus, with the *Mysterium* Kepler provided a new kind of theory of the planets. New in kind, because it was the product of the first explicit and thorough attempt to consistently unify the epistemological structures of the hitherto divergent sciences of astronomy and physics.[72] While carefully acknowledging his predecessors in both of these

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[70] Some parts of the present chapter were derived from the author's paper "Kepler's Response to the Mystery: a New Cosmographical Epistemology", published in *Acta historiae rerum naturalium necnon technicarum*, New Series, vol. 2, Prague: 1998, pp. 49-64.
[71] See Gingerich, O., "Kepler and the New Astronomy", in *Quarterly Journal of the Royal Astronomical Society*, vol. 13, 1972, pp. 346-60.
[72] See Koyré, A., *The Astronomical Revolution*, trans., Maddison, R. E. W., Cornell University Press, Ithaca: 1973, Part 2, pp. 117-371.

sciences, Kepler demanded that hypotheses of planetary motion simultaneously meet the epistemological criteria imposed by both the geometrical science of traditional astronomy and the causal-explanatory science of physics.

Kepler's objective in the *Mysterium* was nothing less than the development of a theory of the absolute structure of the world-system. Kepler was certainly not the first to attempt to provide a general cosmographic account of the planets, that is, an account that seeks to explain the proportions of the universe as a whole. Both Ptolemy's and Copernicus' respective theories each maintain not only an ordering of the planetary orbs, but contain estimates of the ratios of their dimensions.[73] But in the theory of Ptolemy these estimates are empirically underdetermined because of the independence of each planetary hypothesis, and in the theory of Copernicus, the estimates are strictly *a posteriori* consequences of the heliocentric hypothesis and, because of their reliance on Ptolemaic observations and mathematical constructions, no better corroborated empirically.[74] In the *Mysterium*, however, Kepler's unification of the geometrical hypotheses of astronomy with the dynamical hypotheses of physics produces a theory that is not only predicated on the truth of physical heliocentrism, but attempts to generate the entire structure of the cosmos from this new principle.

As Kepler was well aware, any salient argument to the true nature of the world system must exceed the limits of both astronomy and physics as prescribed by the contemporary interpretations of these disciplines. Such

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[73] Ptolemy considers the subject of the ordering and sizes of the planetary spheres most thoroughly in his *Planetary Hypotheses*, but only very briefly in his *Almagest*. Copernicus' fullest treatment of the subject is found in *De revolutionibus*, Book V.

[74] See Stephenson, B., *The Music of the Heavens*, Princeton: Princeton University Press, 1994, chapter 4, pp. 64-75.

argument must address directly the related problems of evidential underdetermination and empirical equivalence that had led to the prevailing demarcation of astronomy as a purely mathematical discipline, and to the complex and varied interpretations of Copernicus' theory.[75] It was not the empirical success of Copernicus' theory that convinced Kepler of heliocentrism; the tables developed in accordance with the Copernican hypotheses were not much more precise than those based on the Ptolemaic hypotheses, and in some cases they were worse.[76] Though the empirical adequacy of astronomical hypotheses was a necessary condition for Kepler, it was not sufficient. It was the cosmological component of Copernicus' writings that interested Kepler.[77] Indeed, one of the most consistent and outstanding features of Kepler's astronomical works is his enthusiastic support of the cosmological aspects of Copernicus' heliocentric hypothesis. While Kepler would challenge many of the fundamental assumptions of Copernicanism, even in the *Mysterium*, he was from a very early stage in his astronomical researches, and in a manner unlike any of his contemporaries, convinced that the centrality of the Sun is a physical truth. Indeed, his unprecedented synthesis of physics and heliocentric astronomy attests to the fact that he believed Copernicus' theory was not only the most mathematically elegant description of the motions of the planets but that it corresponds to the true nature of the world system.

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[75] See Gingerich, O., "From Copernicus to Kepler: Heliocentrism as a Model and as Reality", *Proceedings of the American Philosophical Society*, vol. 117, 1973, pp. 513-22.

[76] See Kozhamthadam, J., *The Discovery of Kepler's Laws*, University of Notre Dame Press, Notre Dame: 1994, p. 132.

[77] See Gingerich, O., "Kepler as a Copernican", in *Johannes Kepler, Werk und Leistung, Katalog des Oberösterreiches Landesmuseums*, vol. 74, Linz: 1974, pp. 109-14.

In the first chapter of the *Mysterium*, Kepler presents a cogent defence of heliocentrism. It is clear that Kepler considered that Copernicus, while inspired in his transformation of the centre of the world-system from the Earth to the (mean) Sun, had failed to realize the full explanatory benefits that a heliocentric theory might provide. The physical Sun has far less causal relevance in Copernicus' theory than in Kepler's. To argue that the Sun is the centre of the firmament *and* of the planets' motions, and, moreover, to argue that the Sun is the *cause* of the motions, are the paramount tasks of the *Mysterium*.[78]

Greatly expanding on the arguments of Copernicus and Rheticus, Kepler emphasizes both the logical simplification and the causal interconnectedness of the individual planetary hypotheses afforded by heliocentrism.[79] Thus, while Copernicus is able to explain all of the same inequalities of planetary motion that Ptolemy explains, such as stations and retrogradations, Copernicus explains them all with one factor, or common cause, namely the motion of the Earth.

Kepler demonstrates how it is possible that Copernican reality can generate Ptolemaic appearances, that is, how the appearances produced by the several motions attributed to the Earth in a heliocentric theory are saved by various combinations of non-Earthly motions in the geocentric theories of the ancients. Perhaps the clearest demonstration of the basic geometrical equivalence of the two theories is to be found in the two diagrams that accompany Kepler's explanations. Consider diagrams 3.1a and 3.1b, which are simplified versions of the corresponding diagrams in the *Mysterium* - in that

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78 This point is well made by Koyré. See Koyré, A., *The Astronomical Revolution*, (1973), trans., Maddison, R. E. W., Cornell University Press, Ithaca, U. S. A., pp. 127-155.

79 Kepler, J., *Mysterium cosmographicum*, p.13, see Duncan, A. M., *The Secret of the Universe*, Abaris Books, New York: 1981, p. 75.

they consider only one inferior and one superior planet. In diagram 3.1a, which schematically represents the Copernican hypotheses, the Sun is in the centre at S, while Venus, the Earth, and Mars orbit on the circles V, E, and M, respectively. As in the Ptolemaic theory, Venus' motion will appear bound to the Sun, but according to Copernicus this results from the fact that the orbit is actually circumsolar. The first inequality of motion is attributed simply to the motion of the planet on its orbit, while the motion in the second inequality results from the annual circuit of the Earth in its orbit. The maximum elongations of Venus, the directions of v1 and v2 measured relative to the Sun, are seen to be determined simply by the size of the planet's orbit relative to that of the Earth. Mars' orbit is superior to the Earth's so that the planet will not seem to be bound to the Sun as Venus is. But it likewise exhibits an annual motion in the second inequality, which appears as a motion superposed on its longer sidereal circuit. The elongations of Mars in the second inequality, the directions of m1 and m2, are explained by showing that over intervals of the sidereal period of the planet, it is the motion of the Earth that accounts for the second inequality, or annual motion relative to the stars. In diagram 3.1b, which schematically represents the Ptolemaic hypotheses, the Earth is in the centre at E, while the Sun orbits on the circle S. The planet Venus moves on the epicycle circle V which remains aligned with the Sun. It is the ratio of the radius of this circle to that of the larger deferent circle upon which it travels that accounts for the maximum elongations v1 and v2. In the case of Mars the situation is similar except that the elongations are not bound to the Sun. Over intervals of the sidereal period of the planet, the elongations m1 and m2 will likewise be accounted for by the ratio of the epicycle to that of the deferent.

Yet there is a significant difference between these two theories. Saving the single annual motion of the Earth in Copernicus' theory requires eleven motions in Ptolemy's theory. Kepler writes in the *Mysterium*:

> "...the motion of the Earth produces the following advantages, that we do not require three eccentric circles as in the customary hypotheses, namely those of the Sun, Venus, and Mercury... There also disappear, if this motion is assumed, three large epicycles, those of Saturn, Jupiter, and Mars... Further, on account of the Earth's coming nearer to the planets and going further away from them in its circle, the latitudes of the five planets seem to admit of a certain variation; and for Ptolemy to save this oscillation, it was necessary to establish five other motions... All these motions, eleven in number, are banished from the universe by the substitution of the single motion of the Earth..."[80]

The diurnal motion and the precessional motion must likewise be represented as component motions of the heavens in the Ptolemaic theory. Because the motions of the Earth in Copernicus' theory must appear as component motions in the hypotheses of the planets in Ptolemy's theory, there are differences in the number and in the kinds of motions required to account for all of the measured inequalities, and to be explained, by the respective astronomers.

These eleven motions in the Ptolemaic hypotheses are in fact crucial to certain demonstrations that Kepler will later give in the *Astronomia nova*. According to the principle of the linking of syllogisms that is developed in the *Apologia* and then placed into practice in determining the true orbit of Mars,

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[80] Kepler, J., *Mysterium cosmographicum*, p.15, trans. Duncan, A. M., *The Secret of the Universe*, Abaris Books, New York: 1981, pp. 79-81.

and the Earth, Kepler can in principle use each of these motions as independent measures of the orbit of the Earth. Thus it is that in chapter twenty-six of the *Astronomia nova* Kepler claims to have succeeded in falsifying the Ptolemaic hypothesis of the Sun and in providing sound reasons for collapsing the erroneously attributed motions of the epicycles of the planets into a single motion of the Earth. But Kepler could only accomplish this feat after the failure of his first attempt to determine the orbit of Mars had shown him that the simple eccentric orbit for the Earth, that all previous astronomers had found sufficient, was false. This he suspected when writing the *Mysterium*, but could not prove without the precise observational record that Tycho Brahé (1546-1601) had accumulated. The *Mysterium* therefore presents this argument only in a conjectural manner.

In the *Mysterium* Kepler examines the systematic differences between the heliocentric and geocentric theories, illustrating the inconsistencies of the geocentric planetary hypotheses. The five planets exhibit retrograde motion but the Sun and Moon do not, a non-uniformity in appearances which is simplified by reference to a Moon whose annual motion is common with that of the Earth about the Sun. Also, according to Kepler, the geocentrists are unable to account for the relatively large epicycles of the inferior planets (that is, relative to their deferents) as compared to the relatively small epicycles of the superior planets. This division as well is eliminated by a heliocentric determination of the planetary phenomena; the differences in the relative magnitudes of the epicycles becomes an effect of the single magnitude of the Earth's orbit being projected into the hypotheses of the other planets. Moreover, the geocentrists cannot demonstrate the case that the three superior planets, when in opposition to the Sun, are always on the point of their epicycle closest to the Earth. In a heliocentric system, however, it is the motion of the Earth through the point closest to the planet that accounts for this appearance.[81]

As Kepler notes, these arguments in support of Copernicus' theory may be criticized on sceptical grounds:

> "It will perhaps be objected that to some extent it can still be said, and to some extent could once have been said about the old tables and hypotheses, that they satisfy the appearances, yet they are rejected by Copernicus as false; and that by the same logic the reply could be made to Copernicus that although he gives an excellent explanation for what is observed, yet he is wrong in his hypothesis."[82]

To this objection Kepler responds:

> "...of those hypotheses which give a reliable reason for the appearances, and agree with observation, Copernicus denies nothing, but rather adopts and expounds them. For although he seems to have changed a great deal in the customary hypotheses, in fact that is not the case. For it can happen that the same conclusion follows from two suppositions which are different in species, because they are included in the same genus, and the point in question is a consequence of the genus. Thus Ptolemy did not derive the risings and settings of the stars from the proximate intermediate premise of the same logical status, 'Because the Earth is motionless at the midpoint.' Nor did Copernicus derive the same conclusion from the intermediate premise, 'Because the

[81] Koyré, A., *The Astronomical Revolution*, p. 136-7.
[82] Kepler, J., *Mysterium Cosmographicum*, p. 13, trans. Duncan, A. M., *The Secret of the Universe*, Abaris Books, New York: 1981, p. 75.

> Earth revolves at a distance from the midpoint.' For it was sufficient for each of them to say (as both did) that those phenomena follow from the propositions that there is a difference between the motions of the heavens and the Earth, and that there is no sensible difference between the Earth and the midpoint in comparison with the fixed stars."[83]

With respect to the saving of the appearances of the planets relative to the stars, and in the absence of measurable annual stellar parallax, Copernicus' theory is no more demonstrably true than is Ptolemy's. Kepler thus acknowledges the problem of empirical equivalence that most of his contemporaries had commonly taken as the basis for scepticism in astronomy; that is, that it is possible to deduce the same empirical conclusions from different hypotheses. However, Kepler's treatment of this epistemological problem in the *Mysterium* and in his other writings, indicates his conviction that it should not be taken as a premise of sceptical arguments concerning the limits of astronomy, but rather, that it should be surmounted in the context of a 'new astronomy'.

A heliocentric determination of the planetary hypotheses takes the common cause of the first inequalities to be the planet-Sun motion, which is seen to decrease with distance from the stationary Sun, and the common cause of the second inequalities to be the projection of the Earth-Sun motion onto the heliocentric motions. Even Brahé, who does not accept the truth of physical heliocentrism, admits this common cause argument for heliocentric planetary determinations. Kepler states that Brahé:

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[83] Kepler, J., *Mysterium cosmographicum*, p. 14, trans. Duncan, A. M., *The Secret of the Universe*, Abaris Books, New York: 1981, p. 77.

> "...entirely disagreed with Copernicus on the position of the Earth, yet retained from him the point which gives us the reasons for matters not hitherto understood, that is, that the Sun is the centre of the five planets. For the proposition that the Sun is motionless at the centre is a more restricted intermediate premise for the derivations of retrogressions, and the general proposition that the Sun is in the centre of the five planets is sufficient."[84]

Hence, even if the issue of the absolute motion or immobility of the Earth is ignored, the motions of the other planets are more coherently ordered by reference to the Sun than to the Earth. Both the theories of Copernicus and of Brahé, although they are contraries with respect to the motion of the Earth, share the same general geometric proposition concerning the centre of the planets' orbs, and are therefore empirically equivalent with respect to it. But, by virtue of this same fact, either of these theories is to be chosen over the theory of Ptolemy.

According to this position, Ptolemy does not demonstrate the motions of the planets from false hypotheses, but commits the fallacy of considering that which happens because of the *genus*, that is, the relative angular positions of the Sun, planets, and stars, happens because of the *species*, that is, the geocentric hypotheses. Kepler admits that a similar criticism will apply to the hypotheses of Copernicus, and hence that geometrical demonstrations alone are *insufficient* to decide between the geocentric and heliocentric hypotheses. Kepler is here justifying a radical departure from traditional geometrical astronomy to an astronomy that includes physics and cosmography. This

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[84] Kepler, J., *Mysterium cosmographicum*, p. 14, trans. Duncan, A. M., *The Secret of the Universe*, Abaris Books, New York: 1981, p. 77.

position will similarly be found in Kepler's *Apologia pro Tychone contra Ursum*, in more general terms, and again, in the *Astronomia nova*, as underwriting his reasoning to the new astronomy.

The difference between two empirically equivalent theories, Kepler argues, is to be found in the physical implications of their respective hypotheses. In this respect, the Copernican hypotheses of planetary motion may be seen to be far superior to the geocentric theory of Ptolemy, and the geo-heliocentric theory of Brahé. For the Copernican hypotheses exhibit a consistency in the ordering of motions and distances that is not possible in any geocentric theory. Only this ordering is commensurate with the various true motions that would be caused, as Kepler suggests in the *Mysterium*, by an emanation from the Sun.

In addition to the geometrical advantages of Copernicus' theory over Ptolemy's, Kepler proposes that, "reasons are supplied for a great many other matters for which Ptolemy for all his many motions could give no account".[85] Kepler states,

> "The old hypotheses simply do not account at all for a number of outstanding features. For instance, they do not give the reasons for the number, extent, and the time of the retrogressions, and why they agree precisely, as they do, with the positions and mean motions of the Sun. On all these points, as a magnificent order is shown by Copernicus, the cause must necessarily be found in it."[86]

[85] Kepler, J., *Mysterium cosmographicum*, p. 16, trans. Duncan, A. M., *The Secret of the Universe*, Abaris Books, New York: 1981, p. 81.
[86] Kepler, J., *Mysterium cosmographicum*, pp. 13-14, trans. Duncan, A. M., *The Secret of the Universe*, Abaris Books, New York: 1981, pp. 75-77.

For Kepler the primary virtues of the Copernican theory are the systematic coherence of its individual planetary hypotheses and the commensurability of these systematically related hypotheses with physical causes:

> "...for the things at which from others we learn to wonder, only Copernicus magnificently gives the explanation, and removes the cause of wonder, which is not knowing the causes."[87]

Kepler extends beyond Copernicus the scope of physical heliocentrism when he submits in the final chapters of the *Mysterium* that the heliocentric speeds of the planets' motions are in an inverse relation to their distances from the physical body of the Sun, both for each planetary orbit with respect to the others and for each planet *within* its respective orbit.[88] Qualitatively, a planet moves more slowly the further from the Sun it is situated. Kepler and Copernicus were not the first to attempt to relate the motions of the planets to their distances from the centre of the world-system. Aristotle supposed that the more distant planets must necessarily have longer periods because they have a longer path to travel.[89] Ptolemy concurred with this position.[90] But, as Kepler notes, no geocentric planetary theory can consistently accommodate this principle. In geocentric theories, the saving of appearances requires that the motions that account for the second inequalities of the planets must all be ascribed identical periods. Hence, only the first inequality follows the principle; the periods of the planets in the second inequalities cannot increase

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87 Kepler, J., *Mysterium cosmographicum*, p. 13, trans. Duncan, A. M., *The Secret of the Universe*, Abaris Books, New York: 1981, p. 75; see also Kozhamthadam, J., *The Discovery of Kepler's Laws*, Notre Dame: University of Notre Dame Press, 1994, p.127.

88 Kepler, J., *Mysterium cosmographicum*, chapters 15, 20, 22.

89 Aristotle, *De coelo*, Lib.II, chap.10.

90 Ptolemy, *Almagest*, IX, 7.

with distance from the centre. Copernicus' theory, on the contrary, is fully consistent with the principle of increasing periods being in relation to increasing distance from the centre of the world-system. This follows from the fact that in the Copernican theory the second inequalities are not true motions of the planets, but rather are appearances caused by the motion of the Earth, which itself is consistent with the principle. But, aside from its reference of the planetary motions to the mean Sun rather than the physical Sun, Kepler considered Copernicus' version of this principle incomplete; Copernicus did not apply it to each planetary body at varying positions in its path. The dynamical explanation of planetary motion that Kepler is able to develop upon the basis of physical heliocentrism is historically without precedent. The distance-delay principle, that the time interval or delay required for the planet to traverse some constant angle in heliocentric longitude is proportional to its distance from the Sun, which is predicated on the truth of physical heliocentrism, is introduced in the *Mysterium* and developed extensively and quantitatively in the *Astronomia nova*.[91]

In chapter twenty-two of the *Mysterium*, Kepler explored the motion of the planets in the various parts of their individual planetary orbits. All of the planets move in orbits that are eccentric to the Sun. Kepler reasoned that the speed of each planet in various parts of its circuit should be determined by the same cause that moves the planets generally. As this cause is seen to diminish with increasing distance from the Sun, so too should each of the planets move more slowly near their aphelia and faster near their perihelia. For Kepler the variation of the speed of the planets in their individual orbits was therefore actual and not merely due to perspective. Kepler's acceptance of the actual

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[91] See Kepler, J., *Mysterium cosmographicum*, chap. 22, and *Astronomia nova*, ch. 32-40.

variation in the speed of each planet was precisely contrary in intent to the Copernican hypotheses, whose complex combinations of regular circular motions were devices intended to absolve astronomy of imperfect, changeable motions. This feature of Kepler's view of planetary hypotheses was also crucial to his eventual arrival at the conception of the non-circular orbit, for initially he accounted for the apparent non-uniformity of the planets' motions by the changing speed of the planet *in a circular orbit.* Only when this type of hypothesis was found to not satisfy all of the constraints that Kepler required, was he forced to abandon the ancient assumption. The details of Kepler's reasoning are to be found in chapters nineteen and twenty of the *Astronomia nova.* There he explains how he came to realize that no hypothesis for the orbit of Mars that combines the assumption of a circular orbit with the assumption of an equant point could be true.[92]

Although the idea that the speed of a planet actually varies was out of keeping with the Copernican criteria for sound astronomical hypotheses, it allowed Kepler to establish not only a systematic causal link among all the planets' motions but also to explain the variations in speed of each individual planetary motion *on the basis of the same cause.* This unified account of the systematic variations of all the planetary motions is a consequence of a physical principle, not a mathematical assumption. The importance of this advance to Kepler's later conclusions can hardly be overstated. It would determine the direction of Kepler's astronomical researches from the *Mysterium* onward, and would be central in underwriting the success of the *Astronomia nova.*

Consider diagram 3.2, which represents an hypothesis of an eccentric circular orbit with an equant point. The Sun is at S. Assume a concentric

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[92] See Chapter 5 of the present work.

circle about the Sun, the circle of longitudes Ls. The centre of the true eccentric orbit of the planet is at C. The orbit is therefore denoted by Lc. The planet is assumed to move at a uniform speed on Lc. Hence, its angular speed around the Sun when compared to a uniform motion on Ls, will be slower near the aphelion A and faster near the perihelion P. Furthermore, the subtractions and augmentations of the angular speed will be proportional to the eccentricity, the distance SC. But, Kepler's hypothesis is that the planet does not move uniformly on the circle Lc, but is actually slower in its motion near A and faster near P. This actual variation in speed is again proportional to the excess or defect in the distance of the planet from the Sun. Thus, while the planet moves on the circle Lc, its angular speed about the Sun is such that the planet appears to move uniformly on a circle, Le, whose eccentricity is twice that of the true orbit, that is the distance SE. On this account, half of the variation of the angular speed about the Sun is due to the perspective effect of the distance from the Sun of the various parts of the orbit, while half is due to an actual change in the orbit speed which is proportional to the variation in distance.

Ptolemy had supposed that the motions (in the first inequalities) of the superior planets were of this form: that they moved in an eccentric circle, but with a speed determined as though they moved in a circle of twice the eccentricity. This was his principle of 'the bisection of the eccentricity'. But the ancient astronomer had offered no explanation of why such a curious motion should take place. Indeed, such motion would be quite difficult to explain on the basis of a physical principle under the assumption of geocentrism. In such a theory it must be composed with the annual motion (or motion in the second inequality). Copernicus, who essentially reconfigured the hypotheses of Ptolemy, did not perceive the physical principle that could explain this kind of motion. In addition, the simple nature of the motion was obscured in the heliocentric theory of *De revolutionibus* by the author's insistence on the use of compounded uniform circular motions in the form of epicycles.

Kepler knew that the argument is only exact near the apsides of the orbit, or in orbits of very small eccentricity. Although the argument outlined in the *Mysterium* is rather qualitative, the very same proposal is detailed in a far more rigorous manner in chapter thirty-two of the *Astronomia nova* where it is applied to the motion of the Earth. No astronomer before Kepler perceived the requirement of anything more than a simple eccentric hypothesis to account for the motion of the Earth, or equivalently, the motion of the Sun. But the heliocentric planetary hypotheses of Copernicus were enough to suggest the possibility of a coherent physical explanation to Kepler, an explanation that not only accounted for the motion of each planet in its individual orbit, but that was linked by a physical principle to the motions of all of the planets - including the Earth.

Thus Kepler proposes in the *Mysterium* that geometric heliocentrism has determinate physical corollaries. The introduction of the absolute motion of the Earth, while inconsistent with Aristotelian physical principles, immensely simplifies the true motions of the other planets. Since the second inequality is regarded as being caused by the Earth's motion, the divisions between retrograde and non-retrograde planets and those with relatively large epicycles and relatively small ones are eliminated. While the heliocentric hypotheses may be empirically equivalent to geocentric hypotheses, they are physically more coherent.

Kepler articulates the philosophical implications of this multiply grounded logic of heliocentrism. He rejects the interpretation of *De* *revolutionibus*, suggested by Osiander in the anonymously added preface to that work, by confronting the charge, also later propounded by Ursus, that Copernicus' hypotheses may be false and yet produce such calculations as will save the appearances. Here Kepler is addressing directly the problem of empirical equivalence, the fact that geometric celestial appearances may be consistent with more than one physical interpretation. Arguing against those who cite examples of the deduction of true conclusions from false premises, Kepler states:

> "The conclusion from false premises is accidental, and the nature of the fallacy betrays itself, as soon as it is applied to another related topic - unless you gratuitously allow the exponent of that argument to adopt an infinite number of other false propositions, and ever arguing forwards and backwards to reach consistency. That is not the case with someone who places the Sun at the centre. For if you tell him to derive from the hypothesis, once it has been stated, any of the phenomena which are actually observed in the heavens, to argue backwards, to argue forwards, to infer from one motion to another, and to perform anything whatever that the true state of affairs permits, he will have no difficulty with any point, if it is authentic, and even from the most intricate twistings of the argument he will return with complete consistency to the same assumptions."[93]

Hence, Kepler's response to the problem of empirical equivalence in astronomy is to extend the scope of permissible demonstrations, so that

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[93] Kepler, J., *Mysterium cosmographicum*, p. 13, trans. Duncan, A. M., *The Secret of the Universe*, p. 75.

theories of the same *genus* are no longer equivalent in *species*, while maintaining the demand for the consistency of the conclusions of such demonstrations with themselves. With this type of argument Kepler seeks to establish the relevance to astronomy of coordinative relations that make geometrical hypotheses physically determinate. Although two hypotheses may merit the same degree of confidence with regard to the saving of appearances, the introduction of physical distinctions will render the hypotheses different; different in ways, which although not directly comparable to empirical evidence, are relevant to an astronomy that includes physics and cosmography. This type of argument is seen in more general and explicit terms in the *Apologia pro Tychone contra Ursum*, where it is identified as the principle of "linking of syllogisms", and again in the *Astronomia nova*, where it is applied.

Kepler calls himself a follower of Copernicus, and this is accurate insofar as he adopts the Copernican tenets of the immobility of the Sun and the multiple motions of the Earth. But the central epistemological claims of the *Mysterium cosmographicum* are far more historically divergent than the term 'Copernican' would indicate.[94] Kepler vastly expands the scope of the arguments in support of heliocentrism. For he realizes that a consistent unification of the principles of physics and astronomy, and the subsequent constraining of hypotheses to these combined principles, is the means of distinguishing the true form of the world from those that are true in respect to appearances only. The epistemological criteria involved in the "linking of syllogisms" and the demand for "physical correlates to geometrical hypotheses" ensure that this unification is manifest in the heliocentric theory

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[94] Koyré, A., *The Astronomical Revolution*, trans., Maddison, R. E. W., Ithaca: Cornell University Press, 1973, pp. 127-155

that Kepler proposes.[95] These criteria of linked syllogistic logic and of physically determinate hypotheses not only allow Kepler to demonstrate the logical advantages of heliocentrism over geocentrism in comparative geometric simplicity, but also allow him to show that the assumption of physical heliocentrism can support a causal theory of the world-system in a manner unlike any alternative theory. Furthermore, these epistemological criteria underwrite the remarkable methodological achievement of the *Astronomia nova*, where the criteria are applied to the determination of the true motion of the planet Mars.

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[95] See Jardine, N., *The Birth of the History and Philosophy of Science*, p. 140 and pp. 141-2.

Chapter 4

The *Apologia pro Tychone contra Ursum* and its Place in Kepler's Natural Philosophy

Chapter 4

Kepler's *Apologia pro Tychone contra Ursum* and its Place in his Natural Philosophy

It has been conjectured by Jardine that, with the writing of the *Apologia*, Kepler may justifiably, though guardedly, be considered the first philosopher of science. This chapter will examine in detail the suggestion by Jardine that we may find in Kepler's *Apologia* the birth of a "distinct mode of reflection on the status of natural science".[96] The objective is to show what the distinct mode of thought is. It may be described as the mutually supportive inference that results from two principles, which Kepler discusses explicitly in the *Apologia*, combined: what Kepler called the principle of "linking of syllogisms", combined with the requirement of unique physical correlates to geometrical hypotheses. This distinct mode of thought has the effect, most evident in the argumentative structure of the *Astronomia nova*, of forcing Kepler to adopt *physically meaningful* coordinates in describing planetary motion and to develop astronomical hypotheses on the basis of such coordinates.

In this light, we can view Kepler's *Apologia* and his subsequent 'war on Mars' as a tour-de-force against the 'compromise of Geminus'. Named for the noted Stoic philosopher of the first century B.C. who wrote, among other works, a great exposition on the classification and principles of the mathematical sciences, this 'compromise' represented a widespread agreement during the Renaissance on the division of the subject matter of the disciplines

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[96] Jardine, N., *The Birth of History and Philosophy of Science, Kepler's A Defence of Tycho Against Ursus with Essays on its Provenance and Significance*, Cambridge: CambridgeUniversity Press, 1984, p. 2.

of physics and astronomy. Astronomy, largely developed and taught by mathematicians, was to be devoid of the consideration of the causes of the celestial motions, while the attempt to generate causal explanations for the phenomena was the purview of qualitative natural philosophy.[97] This stance on the classification of the sciences embraced the sceptical opinion that an astronomical system of the world was a mathematical convenience, a predictive tool rather than a representation of a physical reality. As Geminus wrote:

> "In general it is not the astronomer's business to see what is by nature immovable, and of what kind the movable things are, but framing hypotheses as to some things being in motion, and others being fixed, he considers which hypotheses are in conformity with the phenomena of the heavens."[98]

Astronomers are not to be concerned with physical questions but solely with 'saving the phenomena'.

Aristotle's works, principally *De caelo* and his works on 'first philosophy', were written prior to this 'compromise', and the cosmological parts of these texts formed the basis for much of natural philosophy until the time of Kepler and Galileo. These influential works ingeniously combined mathematical reasoning with the study of causes, or 'physics'. But when Hipparchos, around 150 B.C., was able to show that the Earth cannot be at the exact centre of the Sun's uniform motion, as had been stipulated in

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[97] See Drake, S., "Hipparchus-Geminus-Galileo", *Studies in the History and Philosophy of Science*, 20 (1989), pp. 47-56.
[98] Mason, S. F., *A History of the Sciences: Main Currents of Scientific Thought*, London: Routledge & Kegan Paul Ltd., 1953, p. 64.

Aristotelian cosmology, a dichotomy was created between the astronomer, who could frame hypotheses in accord with the appearances, and the philosopher, who could pursue causal explanation, or 'metaphysics'.

At the end of the sixteenth century and the beginning of the seventeenth, the 'compromise of Geminus' was so well entrenched that even Kepler's teachers and closest colleagues believed his physical approach to astronomy to be fundamentally misguided. Maestlin wrote to Kepler,

> "When you write about the moon, you say that you would like to adduce physical causes for all its inequalities. I simply do not understand this. On the contrary, I believe that physical causes can be dismissed altogether, and that it is fitting to explain astronomical phenomena only through astronomical methods, by means of astronomical, not physical causes and hypotheses. For calculations demand astronomical bases from geometry and arithmetic, which, so to speak, represent their wings, rather than physical hypotheses, which would more likely confuse than instruct the reader."[99]

This letter is all the more remarkable to the modern reader given that it was written in 1616 - long after the writing of the *Apologia* and the publication of the *Astronomia nova.*

In the sense that Kepler's objective in the *Apologia* is to justify a unification of astronomy with physics, the work represents a return to the Aristotelian view of astronomy as a true science, that is, a discipline involving

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[99] Quoted from Kozhamthadam, J., *The Discovery of Kepler's Laws*, Notre Dame: Notre Dame University Press, 1994, p. 103.

the attempt to understand natural phenomena on the basis of causes that are not immediately evident to the senses. But the eventual product of this unification in the writings of Kepler, the physical astronomy developed in the *Astronomia nova*, would collapse the Aristotelian cosmological distinction between the changeable sublunary realm and the immutable superlunary realm. For in Kepler's physically heliocentric theory, the Moon, Mars and the other planets are places – as material, and therefore as knowable, as the Earth. By linking the mathematical discipline of astronomy with the causal-explanatory discipline of physics, Kepler not only connected terrestrial 'science' with celestial 'science', but two standards of epistemology and method that had hitherto been distinct.

The central claim of this chapter is that Kepler's achievements in practical astronomy ought rightly to be seen in relation to the *theoria* deployed in the *Apologia*. There the reader will see that the arguments of the *Mysterium cosmographicum* have been generalized and codified. With the *Apologia*, Kepler is no longer merely arguing for and explaining the Copernican system, or even expanding upon Copernicus' heliocentric hypothesis, but he is arguing against scepticism in astronomy.

That Copernicus had proceeded on the basis of combinations of uniform circular motions and deduced the details of the configurations of his planetary system in accordance with this assumption meant that it was incomplete because it was underdetermined. Many combinations of epicycles could be said to account for the appearances, yet there could be only one combination that determined the actual path of the planet. Paradoxically, it was Kepler's insistence on the ancient principle of circular motion that ultimately led to its rejection in the *Astronomia nova*. But this feat was only made possible because

Kepler's realist conception of the circular orbit was far different from that of his predecessors. It was the stringent epistemological and methodological constraints that are developed in the *Apologia* that allowed him to make Brahe's extensive and precise observational data into convincing evidence for the new planetary theory of the *Astronomia Nova.*

Kepler never entirely doubted that the kind of theory that would result from the constraints that he imposed was possible; that the world would not adhere to such strict and interconnected conditions was something he could not accept. Indeed, he thought that such unity was necessary; and because of this deep conviction, Kepler was the first person to construct an epistemology that attempts directly to overcome the problems of empirical equivalence and underdetermination by the evidence. Kepler's awareness of these problems led to his development of methods for avoiding their consequences.

Written in the final months of the 1600 and the first of 1601, the *Apologia* remained unpublished until 1858.[100] The *Apologia* was Kepler's first major assignment under the auspices of Brahé. The ostensible purpose for which the *Apologia* was composed was the adjudication of a dispute between Brahé and the Imperial Mathematician, Baer (Latinized as Ursus) over priority. Both Brahé and Ursus published works in 1588, with the respective titles *De mundi aetherei recentioribus phaenomenis*[101] and *Fundamentum*

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[100] On the dating of the composition of the work, see Jardine, N., *The Birth of History and Philosophy of Science, Kepler's A Defence of Tycho Against Ursus with Essays on its Provenance and Significance*, Cambridge: Cambridge University Press, 1984, p. 1 and p. 72. The *Apologia pro Tychone contra Ursum* was first published in: Frisch, C., *Joannis Kepleri astronomi opera omnia*, i, I, (1858-71), Frankfurt and Erlangen, pp. 236-276.

[101] *De Mundi Aetheri recentioribus phaenomenis*, (1588), Uraniborg, republished in Dreyer, J. L. E., *Tychonis Brahe Dani opera omni*, Copenhagen: 1913-29.

astronomicum[102], which proposed nearly identical geo-heliocentric planetary theories as alternatives to the theories of Ptolemy and Copernicus. In 1596, with the publication of a selection of his correspondence, Brahé accused Ursus of plagiarism.[103] Ursus responded in 1597 with the publication of *De Hypothesibus astronomicis tractatus*[104], which contains both an account of the history of astronomical hypotheses and an account of their proper usage. Kepler began composition of the *Apologia*, which he referred to as his 'Tract on Hypotheses', as a condition of his tenure with Brahé. With Brahé's death, Kepler set the work aside and never brought it to completion.

Ursus' recounting of the history of astronomy in his *Tractatus*, aside from its being an attempt to provide an historical basis which might discredit Brahe's claim to priority, is an attempt to justify a sceptical instrumentalism regarding hypotheses. Thus Ursus appeals to hypotheses as nothing more than modes of calculation which merely save the phenomena and which, ultimately, upon investigation, are demonstrated to be false. Ursus' rejection of the truth of hypotheses, while perhaps polemical, was typical of the philosophical position of a large majority of astronomical practitioners of the period. Even those few astronomers who considered themselves Copernicans did not, for the most part, deviate from the traditional scepticism concerning hypotheses.[105]

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[102] Ursus, N., *Fundamentum astronomicum*, Strasbourg: 1588.
[103] Brahe, T., *Epistolarium astronomicarum liber primus*, Uraniborg: 1596. republished in Dreyer, J. L. E., *Tychonis Brahe Dani opera omni*, Copenhagen: 1913-29.
[104] Ursus, N., *De hypothesibus astronomicis tractatus*, Prague: 1597.
[105] Jarrel, R., "The Contemporaries of Tycho Brahe", in *Planetary Astronomy from the Renaissance to the Rise of Astrophysics*, *The General History of Astronomy*, 2A, Taton. R., and Wilson, C., eds., Cambridge: Cambridge University Press, 1989, pp. 22-32.

Kepler had sent a letter full of praise to Ursus in 1596 in order to seek approval of his recently published Copernican treatise, the *Mysterium cosmographicum*. Kepler became involved in the priority dispute when, without his permission, Ursus published Kepler's letter with the *Tractatus*. In 1599, when he began his labours on the orbit of Mars with Brahé, Kepler simultaneously began composition of what he called the *Tractatus hypothesibus*, which later became known as the *Apologia*.

The first English-language translation of the *Apologia* was published in 1984 by Jardine in a book entitled *The Birth of the History and Philosophy of Science*. Jardine conjectures that, if any single work can be seen as demarcating the inception of philosophy and history of science as "a distinctive mode of reflection on the status of natural science", it is Kepler's *Apologia*.[106] While his commentary well situates the *Apologia* in the context of sixteenth- and seventeenth-century scientific thought, Jardine admits to having given little attention to the place of the work within Kepler's other writings.[107] This chapter presents a brief account of the relation of the *Apologia* to the earlier *Mysterium cosmographicum* and to the later *Astronomia nova*.

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[106] Jardine writes, "The *Apologia* is a work of great originality not only in its theses, but also in its concerns. Kepler focusses throughout the work on theoretical progress in the history of astronomy and the means whereby such progress may be achieved. Concern with theoretical progress is prevalent at every level, historical and philosophical, academic and popular, of modern reflection on the natural sciences. Yet in this respect the *Apologia* appears to be without substantial precedent in the sixteenth century writing on the epistemology and history of the mathematical arts. Claims about the origins of genres and disciplines inevitably oversimplify complex processes and are vulnerable to the discovery of earlier documents. Nevertheless I conjecture that if any one work can be taken to mark the birth of history and philosophy of science as a distinctive mode of reflection on the status of natural science it is Kepler's *Apologia*". See Jardine, N., *The Birth of History and Philosophy of Science, Kepler's A Defence of Tycho Against Ursus with Essays on its Provenance and Significance*, Cambridge: Cambridge University Press, 1984, pp. 1-2.

[107] Jardine, N., *ibid.*, p. 5.

Contrary to the *prima facie* implication of its title, the *Apologia* does not consist merely of a sustained defence of Brahé against Ursus on the matter of priority.[108] Although the matter of literary sources is discussed in great detail, Kepler's objective throughout the *Apologia* is the refutation of Ursus' general denial in the *Tractatus* of the ability of astronomers to truly "portray the form of the world".[109] Hence, what the *Apologia* does contain is many generally applicable arguments against the kind of sceptical instrumentalism that Ursus defends in his *Tractatus*.

It is evident from the preface of the work that Kepler never intended to address directly the issue of plagiarism. It appears likely, according to Jardine, that in the course of his historical researches Kepler came to regard Brahe's charge of plagiarism as indefensible.[110] Certainly Kepler considered the priority dispute to be trivial and unwarranted, since much of the contentious content of both men's works consists in the relation of the ideas of collaborators or other authors.[111]

The text of the *Apologia* is divided into four chapters, the last of which was never completed. The final three of these four chapters constitute a scholarly refutation of Ursus' history of astronomical hypotheses as it is presented in the *Tractatus*. With these final chapters Kepler presents his own comprehensive account of astronomical hypotheses and their respective sources, from Zoroaster and Pythagoras to Brahé and Ursus. A remarkable

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[108] Jardine, N., *The Birth of History and Philosophy of Science, Kepler's A Defence of Tycho Against Ursus with Essays on its Provenance and Significance*, Cambridge: Cambridge University Press, 1984, p. 72.
[109] Jardine, N., *ibid.*, pp. 72-74, quote, *ibid.*, p. 1.
[110] Jardine, N., *ibid.*, pp. 72.
[111] Tycho, in his published correspondence, the *Epistolarium*, accuses Ursus of plagiarism from both himself and his collaborators Cristoph Rothmann and Paul Wittich. Ursus, in the *Tractatus*, accuses Tycho of plagiarism from Apollonius of Perga and Copernicus.

work of historical scholarship, Kepler's historical analysis demonstrates that the evidence provided by Ursus in the *Tractatus* is replete with misrepresentations and incongruencies. Although Ursus is not judged as being guilty of plagiarism from Brahé, he is rebuked for his misconstrual of history. Based on three previous assessments of Ursus' *Tractatus* in Kepler's correspondence, it is probable that he intended to complete the *Apologia* with a discussion of Ursus' interpretation of Copernicus' *De Revolutionibus* and a summary or peroration of previous arguments.[112]

The first chapter of the *Apologia* provides Kepler's response to Ursus' arguments in the *Tractatus* that astronomical hypotheses are to be considered generally as falsities. A discussion of Ursus' more specific historical claims is deferred to the later chapters in order that his generic arguments be given prior consideration. Ursus claims that all astronomical hypotheses contain unreasonable assumptions and that true hypotheses cannot, in fact, be established. He states in the *Tractatus*,

> "An hypothesis or fictitious supposition is a portrayal contrived out of certain imaginary circles of an imaginary form of the world system, designed to keep track of the celestial motions, and thought up, adopted and introduced for the purpose of keeping track of and saving the motions of the heavenly bodies and forming a method for calculating them... These contrived hypotheses are nothing but certain fabrications which we imagine and use to portray the world-system. So it is not in the least necessary, nor is it necessarily required of the creators of

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[112] Jardine, N., *The Birth of History and Philosophy of Science, Kepler's A Defence of Tycho Against Ursus with Essays on its Provenance and Significance*, Cambridge: Cambridge University Press, 1984, pp. 72-73.

these hypotheses, that those hypotheses correspond altogether, that is to say in all respects or in all ways, to the world-system itself..."[113]

Kepler's rejection in the *Apologia* of Ursus' sceptical arguments is of interest for its informed and profound engagement of the important epistemological theses that concern the astronomy of his time. Kepler's response to scepticism is predicated on constraining the epistemological requirements sufficient to warrant the truth of hypotheses.

In the *Apologia*, Kepler argues that the truth of astronomical hypotheses is, in addition to empirical adequacy, dependent on both their logical and physical inter-coherence. This qualification of the truth of hypotheses is exacted in the *Mysterium* and *Astronomia nova* through physical principles such as Kepler's 'distance-delay law', and logical principles such as what Kepler calls in the *Apologia* the principle of "the linking of syllogisms". Hence, while Kepler was able to place increasingly stringent empirical demands on his hypotheses with the most precise observational records of his day, he simultaneously sought an increase in the evidential unification afforded by the same hypotheses.

The rudiments of the arguments that are developed explicitly in the *Apologia* are proposed in the earlier *Mysterium cosmographicum* and specified by application in the later *Astronomia nova*, but the *Apologia* gives them a complete and concise formal expression. In providing his own account of

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[113] Ursus, N., *Tractatus*, sig. Biv, v., quoted from Jardine, N., *The Birth of History and Philosophy of Science, Kepler's A Defence of Tycho Against Ursus with Essays on its Provenance and Significance*, Cambridge: Cambridge University Press, 1984, p. 41.

astronomical hypotheses and a critique of Ursus' account, Kepler defends not Brahé, but astronomical hypotheses themselves against Ursus' scepticism.

Kepler begins his argument with an etymologically based definition of the term 'hypothesis'.[114] In contrast to Ursus' interpretation of the term, Kepler, arguing from the classical (Greek) use of the term, states,

> "We call 'a hypothesis' generically whatever is set out as certain and demonstrated for the purpose of any demonstration whatsoever. Thus in every syllogism the hypotheses are what we otherwise call 'propositions' or 'premisses'. But in a longer demonstration, which includes subordinate syllogisms, the premisses of the initial syllogism are called 'hypotheses'. Thus in astronomy suppose we demonstrate with the help of numbers and figures some fact about a star we have previously observed, from things we have seen when carefully examining the heavens. Then in the demonstration we have set up, the observation constitutes a hypothesis upon which that demonstration chiefly rests."[115]

The greater part of Kepler's argument in the first chapter of the *Apologia* consists in an elaboration of this definition of hypotheses with regard to their proper argumentative function in the establishment of claims about

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[114] On the classical use of the term hypothesis, see Taub, L., *Ptolemy's Universe*, Chicago: Open Court, 1993, p. 41.

[115] Kepler. J., *Apologia*, 266r, Jardine, N., *The Birth of History and Philosophy of Science, Kepler's A Defence of Tycho Against Ursus with Essays on its Provenance and Significance*, Cambridge: Cambridge University Press, 1984, pp. 88, 138-139.

astronomical phenomena. Kepler explicitly rejects the position of Ursus on the definition of hypotheses. Ursus claims in the *Tractatus*,

> "It is the distinguishing characteristic of hypotheses to inquire into, hunt for and elicit the truth sought from feigned or false suppositions. And so it is permitted and granted to astronomers, as a thing required in astronomy, that they should fabricate hypotheses, whether true or false and feigned, of such a kind as may yield the phenomena and appearances of the celestial motions and correctly produce a method for calculating them..."[116]

But, according to Kepler's definition of hypotheses, only those hypotheses that are considered to be true are of any use in astronomy. To argue from admittedly false hypotheses, he proposes, is futile. Kepler is not claiming that there are no hypotheses that will be evaluated as false in virtue of their failure to save appearances, only that hypotheses are not generally assumed to be false. He states,

> "None of those whom we honour as authors of hypotheses would wish to run the risk of errors in his conclusions, it follows that none of them would knowingly admit amongst his hypotheses anything liable to error. Indeed they worry not so much about the outcome and conclusions of demonstrations, but often more about the hypotheses they have adopted: thus almost all notable authors to date assess them on

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[116] Ursus, N., *Tractatus*, sig. Biv, v., quoted from Jardine, N., *The Birth of History and Philosophy of Science, Kepler's A Defence of Tycho Against Ursus with Essays on its Provenance and Significance*, Cambridge: Cambridge University Press, 1984, p. 42.

both geometrical and physical grounds and want them to be confirmed in all respects."[117]

In contrast to Ursus' claim that astronomy is concerned with the attempt to elicit truth from "false or feigned suppositions", Kepler proposes that astronomy is concerned with the attempt to elicit true conclusions from true suppositions. Hence, on Kepler's account, the conclusions of arguments from supposedly true hypotheses may be false, but if the truth of the hypotheses is assumed, then the false conclusions must be due to the inappropriateness of the arguments proceeding from the hypotheses and not the hypotheses themselves. Kepler's concern here is the qualification of valid modes of syllogistic reasoning in regard to astronomical hypotheses. For only in cases where hypotheses are considered true can the appropriateness of the arguments following from them be assessed - we cannot learn that a syllogism is sound if its premisses are admittedly false.

Kepler makes the distinction between the classical meaning of the term hypotheses, that is, premisses, and the contemporary (to Kepler) meaning of the term, that is, syllogistic demonstrations in general. The former of these two types of hypotheses are the simple constituents of syllogisms, while the latter are constituted of complexes of syllogisms.

"When we speak in the plural of 'astronomical hypotheses', we do so in the manner of present-day learned discourses. We thereby designate a certain totality of the views of some notable practitioner, from which

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[117] Kepler, J., *Apologia*, 267r, Jardine, N., *The Birth of History and Philosophy of Science, Kepler's A Defence of Tycho Against Ursus with Essays on its Provenance and Significance*, Cambridge: Cambridge University Press, 1984, pp. 89, 140.

totality he demonstrates the entire basis of the heavenly motions. All the premisses, both physical and geometrical, that are adopted in the entire work undertaken by that astronomer are included in that totality. They are included if the practitioner for his convenience has borrowed them from elsewhere. They are likewise included if he has already demonstrated them from observations, and now, in the reverse manner, requires that what he has demonstrated should be conceded to him by the learner as hypotheses: from which hypotheses he promises to demonstrate with syllogistic necessity both those observed positions of the stars (which had in the first place been used by him as hypotheses) and also, he hopes, those which are about to appear in the future."[118]

Kepler's argument, based on this distinction, is that the truth of the simpler, premissed hypotheses is a necessary condition for the truth of the more complex hypotheses. He continues,

"We hold whatever there is in our conclusions to have been established as true. Besides, for the truth to be legitimately inferred the premisses, that is, the hypotheses, must be true. For only when both hypotheses [that is, major and minor premisses] are true in all respects and have been made to yield the conclusion by the rule of the syllogism shall we achieve our end - to reveal the truth."[119]

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118 Kepler, J., *Apologia*, 266r-266v, Jardine, N., *The Birth of History and Philosophy of Science, Kepler's A Defence of Tycho Against Ursus with Essays on its Provenance and Significance*, Cambridge: Cambridge University Press, 1984, pp. 88-9, 139.

119 Kepler, J., *Apologia*, 266v, Jardine, N., *ibid.*, pp. 89, 139.

Following this argument, Kepler proposes that Ursus' account in the *Tractatus*, besides its historiographic inaccuracies, is based on an equivocal definition of hypotheses.[120] Kepler states of Ursus' definition,

> "In these words he openly denies that there exists an hypothesis which is not false. He confirms this opinion when he says that hypotheses are nothing but certain 'fabrications'; and further he says that they would not be hypotheses if they were true; and again, that 'it is the mark of hypotheses to yield what is true from what is false'. So on this view the Earth will neither be moved nor stand still. For Ursus will acknowledge both as hypotheses."[121]

Kepler's subsequent claim is that some hypotheses are to be considered as true, and, moreover, that these are the hypotheses with which astronomy is concerned. At the least, Kepler supposes, observations, considered as premissed hypotheses, must be true.

> "The fact is that observation of the celestial motions guides astronomers to the formation of [astronomical] hypotheses in the right way, and not the other way around."[122]

While Kepler concedes that it is logically possible to generate true conclusions from false hypotheses, he argues that this is not a probable occurrence, and

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[120] Kepler, J., *Apologia*, 269r, Jardine, N., *The Birth of History and Philosophy of Science, Kepler's A Defence of Tycho Against Ursus with Essays on its Provenance and Significance*, Cambridge: Cambridge University Press, 1984, pp. 92, 144.
[121] Kepler, J., *Apologia*, 269r, Jardine, N., *ibid.*, pp. 92, 144.
[122] Kepler, J., *Apologia*, 269r, Jardine, N., *ibid.*, pp. 92, 144.

that astronomy should certainly not be dependent on such an occurrence for its truth in general. Kepler refers in this discussion to his earlier work, the *Mysterium* of 1596:

> "If an error has crept into one or another of the premissed hypotheses, even though a truth may be occasionally obtained, nonetheless, as I have already said in the first chapter of my *Mysterium cosmographicum*, this happens only by chance and not always, but only when the error in one proposition meets another proposition, whether true or false, appropriate for eliciting the truth."[123]

Kepler provides a specific historical example of this meeting of errors. In *De revolutionibus*, Copernicus' corroborates his account of the latitudes of the Moon with the observation of a lunar occultation of the star Aldebaran that he previously inferred would occur.[124] Although Copernicus' hypothesis of the lunar latitudes was in fact in error by assigning too small a latitude, it so happened that his estimate for the latitude of the star was too small by the same amount.[125] Such agreement is fortuitous, and these mutually cancelling errors were later perceived by means of other inferences. Kepler continues,

> "...false hypotheses, which together yield the truth once by chance, do not in the course of a demonstration in which they have been combined with many others retain this habit of yielding the truth, but betray

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[123] Kepler, J., *Apologia*, 267r, Jardine, N., *The Birth of History and Philosophy of Science, Kepler's A Defence of Tycho Against Ursus with Essays on its Provenance and Significance*, Cambridge: Cambridge University Press, 1984, pp. 89, 139-40.
[124] Copernicus, N., *De revolutionibus*, Book IV, chapter 27.
[125] Kepler, J., *Apologia*, 267r, Jardine, N., *ibid.*, pp. 89, 140.

> themselves. Thus in the end it happens that because of the linking of syllogisms in demonstrations, given one mistake an infinite number will follow."[126]

On this account, the conclusion of a valid syllogistic demonstration involving the concatenation of many independent syllogisms is more probably true than a conclusion that has not been subjected to the same constraints. This argument from "the linking of syllogisms" constitutes a focal epistemological thesis in Kepler's argument against sceptical instrumentalism. A sustained application of this principle informs many of the innovative developments in astronomy for which Kepler is responsible.

Another important principle which Kepler presents in the *Apologia*, and which has already been alluded to, is the distinction between the geometrical and physical aspects of hypotheses. He employs the distinction in his arguments to refute Ursus' contention that some contrary hypotheses, such as the Brahéan and the Copernican hypotheses, may be kinematically equivalent, so that there is no more truth ascribable to one than the other. In restricting the domain of astronomical hypotheses to kinematics, that is, the saving of appearances alone, Ursus disallows any criterion for distinguishing truth from falsity in the case of kinematic equivalence. Kepler, however, rejects Ursus' basic claim, that is, that kinematic equivalence implies equivalence *simpliciter*. Kepler states that there is no hypothesis whether simple or complex,

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[126] Kepler, J., *Apologia*, 267r, Jardine, N., *The Birth of History and Philosophy of Science, Kepler's A Defence of Tycho Against Ursus with Essays on its Provenance and Significance*, Cambridge: Cambridge University Press, 1984, p. 89, 140.

> "...which will not turn out to have a conclusion peculiar to it and separate and different from all the others. Even if the conclusions of the hypotheses coincide in the geometrical realm, each hypothesis will have its own peculiar corollary in the physical realm."[127]

Copernicus, for example, not only wished to give a mathematical account of the motions of the planets, but, among other things, give a necessary cause for the fact that the superior planets are always closest to the Earth at opposition, a fact that in a geocentric theory must simply be assumed. In the geo-heliocentric theory developed by Brahé, the planets revolve about the (mean) Sun while the Sun itself orbits the immobile Earth. Geometrically, this theory is thus virtually identical to the Copernican system, but transformed into geocentric hypotheses. Yet these systems must be regarded as entirely contradictory if an appeal to physical principles is made in explaining them. Thus, argues Kepler, even when Brahé's theory exactly agrees with Copernicus in placing the planet against the stars, there is an important difference in intention between the two theories. Brahé does not admit many things in his hypotheses that Copernicus does, the most important of these being the mobility of the Earth and the immense distance of the stars that follows from allowing that the Earth orbits the central Sun. Kepler also mentions the treatise of Antonio Magini (1555-1617), the *Novae coelestium orbium theoricae congruentes cum observationibus N. Copernicus*, of 1589. This work is an explicit attempt to render the individual planetary hypotheses

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[127] Kepler, J., *Apologia*, 268r, Jardine, N., *The Birth of History and Philosophy of Science, Kepler's A Defence of Tycho Against Ursus with Essays on its Provenance and Significance*, Cambridge: Cambridge University Press, 1984, p. 89, 141-2.

of Copernicus' theory in a geocentric form and show that they nonetheless remain very nearly in complete agreement with the Prutenic Tables that are based on *De revolutionibus*. But it does not follow, if one includes the intentions of these astronomers in formulating their theories, that the astronomers have demonstrated the same result.

Taking this distinction between the purely positional and the physical aspects of astronomical hypotheses further, Kepler examines the contradiction inherent in the geocentric versus heliocentric interpretations of the annual motion. Since the proposition that the Sun orbits the Earth accounts for the same appearances as the proposition that the Earth orbits the Sun, one could conclude that, since one of these propositions must be false, what is true may follow from what is false. In a response to this argument very much in keeping with the arguments of the first chapter of the *Mysterium*, Kepler proposes that the appearances considered,

> "...happen neither because of the motion of the heavens, nor because of the motion of the Earth, insofar as it is a motion of the heaven or of the Earth. Rather they happen insofar as there occurs a degree of separation between the Earth and heaven along a path which is regularly curved with respect to the path of the Sun, by whichever of the two bodies that separation is brought about. So the above-mentioned things are demonstrated from two hypotheses insofar as they fall under a single genus, not insofar as they differ. Since therefore, they are one for the purpose of the demonstration, they certainly are not contradictory propositions. And even though a physical contradiction

> inheres in them, that is still entirely irrelevant to the demonstration."[128]

Similarly, the well-known equivalence between a concentric circle with an epicycle and an eccentric circle is a result of the fact that both of these hypotheses assume the same generic middle term (that is the measured motion that is being accounted for) and whatever else may be ascribed to these hypotheses, it will be irrelevant for the purpose of demonstrating that motion. Kepler says of these example propositions,

> "Neither the former nor the latter deserves the title 'astronomical hypothesis', but rather what both have in common, namely, that it is assumed and posited that there is a definite and measured part of the circle that the planet traverses, which lies in one half of the circle of the zodiac. This, I say finally, is a proper hypothesis from which the length of time spent in each half of the circle may be demonstrated."[129]

It is therefore incorrect, according to Kepler's analysis, to claim that such examples of equivalence show that truth may be derived from falsity. In these examples, the sense in which one of the 'contradictory' hypotheses must be false has nothing to do with the demonstrative purpose for which the hypotheses were contrived.

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[128] Kepler, J., *Apologia*, 268r, Jardine, N., *The Birth of History and Philosophy of Science, Kepler's A Defence of Tycho Against Ursus with Essays on its Provenance and Significance*, Cambridge: Cambridge University Press, 1984,p. 142. See also Kepler, J., *Mysterium Cosmographicum*, pp.13-15, in Duncan, A. M., *The Secret of the Universe*, Abaris Books, New York: 1981, pp. 75-77.

[129] Kepler, J., *Apologia*, 268r, Jardine, N., *ibid.*, p. 143.

An analogous set of arguments to these can be found in the *Astronomia nova*, chapter twenty-one, where Kepler, in view of the fact that his *hypothesis vicaria* of chapter sixteen has been shown to fail, discusses the ability of 'false hypotheses' to produce true conclusions. There he shows how an hypothesis which assumes an eccentric circular orbit and an equant point can be determined to be false in saving the distances of a planet form the Sun, and yet simultaneously be configured so as to be true in giving correct longitudes.

Kepler concludes his argument in the *Apologia* by proposing that, when properly considered, there are no contradictory astronomical hypotheses, as they may only be contradictory in senses that are not opposable through geometrical demonstration. He proposes that,

> "...each consequence in astronomy is derived from only one middle term and presupposes a hypothesis which is of single form even if it is differentiated insofar as it is considered apart from that demonstration. And conversely, every hypothesis whatsoever, if we examine it minutely, yields some consequence which is entirely its own and is not shared with any other hypothesis. Nor can it happen in astronomy that what was originally founded on a false hypothesis should be true in every respect. So this is the distinctive nature of hypotheses (if we are to depict the form of legitimate hypotheses), to be true in every respect. And it is not right for an astronomer knowingly to assume false or ingeniously contrived hypotheses in order to demonstrate from them the celestial motions."[130]

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[130] Kepler, J., *Apologia*, 268r, Jardine, N., *The Birth of History and Philosophy of Science, Kepler's A Defence of Tycho Against Ursus with Essays on its*

The reader of the *Astronomia nova* will find Kepler taking his own astronomical hypotheses to task on just the philosophical principles that are delineated here in the *Apologia*. Kepler tests his hypothesis of the latitudes of Mars and then his *hypothesis vicaria* until they respectively are either confirmed beyond reasonable doubt or else have been 'betrayed' by the false assumptions upon which they are shown to be founded.

Kepler allows that astronomers often adopt a manner of communicating their theories that lends itself to the dismissal of their own intentions. He states,

> "...practitioners are not always in the habit of taking account of that diversity in physical matters, and they themselves often confine their own thinking within the bounds of geometry or astronomy and tackle the question of the equipollence of hypotheses within one particular science, ignoring the diverse outcomes which dissolve and destroy the vaunted equipollence when one takes account of related sciences."[131]

It is precisely such considerations of "diversity in physical matters", judiciously combined with geometric demonstration, which inaugurate Kepler's new astronomy.

Provenance and Significance, Cambridge: Cambridge University Press, 1984, p. 143.

[131] Kepler, J., *Apologia*, 268r, Jardine, N., *ibid.*, p. 142.

Chapter 5

The New Astronomy: The Birth of Astrophysics

Chapter 5

The New Astronomy: The Birth of Astrophysics

This chapter will examine the arguments of the *Astronomia nova*. It will show that many of Kepler's most important victories in his 'war on Mars' can be identified as procedures guided by the epistemology and methodology developed in his earlier works. It will argue that Kepler's philosophy of science, as developed in the *Mysterium cosmographicum* and the *Apologia*, is extended and placed into practice in the *Astronomia nova*, and most especially in the earliest chapters of the work. More specifically, a detailed account will be given of how the two epistemological principles that have been characterized in the earlier Keplerian texts are applied in the reasoning that concludes with Kepler's new planetary hypotheses. These two principles, the requirement of 'syllogism linkage' and the requirement that 'all hypotheses entail particular consequences in the physical realm', provided Kepler with a rational means of adjudicating between the contrary (if interpreted in a physical sense) but empirically equivalent planetary theories of his time. Rather than succumbing to the scepticism into which most contemporary astronomers were driven by the problem of planetary motion, Kepler endeavoured to find a sound epistemological basis upon which to demonstrate the truth of one system, a physically heliocentric one, and the falsity of all others.

The *Astronomia nova* may justifiably be considered one of the most important works in the history of science. Within its seventy brief chapters are contained a truly new kind of natural philosophy. Four centuries after

Kepler's speculations and investigations, we are fortunate to know which components of his philosophy were sound, both in terms of the conception he had of planetary motion and its causes, and in terms of the way he regarded the various sciences, their respective subjects, and their relations to one another. But this retrospective knowledge has tended to obscure our view of Keplerian philosophy.

Kepler is often said to have shown that the planets orbit the way they do, and Newton why. The most common versions of this distinction maintain that Kepler was able to discover some of the regularities of planetary motion, and that these regularities would only later be explained in terms of Newtonian universal gravity. The soundness of this dichotomy, however, is premised upon our privileged position in history, as it might equally well be applied to Newtonian physics itself in comparison to Einsteinian physics. When examining Kepler's science, one must be mindful of the fact that any successful theory that followed Kepler's had to account for the regularities that he had demonstrated. Kepler's theory could be rejected on the basis of the physical assumptions that underwrite it, some of which did not comply with widely accepted Aristotelian ideas, but it could not be denied that the Keplerian hypotheses give a good mathematical account of the appearances. In effect, the theory of the *Astronomia nova* did to the theories of Ptolemy, Copernicus and Brahé what Einstein's did to Newton's in a much later time: it invoked a new conceptualization of the regularities accounted for by the older theory and delivered an account, on the same conceptual basis, of regularities previously unperceived.

But our obvious and yet rarely examined assumption that the sciences of physics and astronomy are to be directly relevant to one another at all is itself an important and revolutionary claim defended within Kepler's philosophy.

Kepler was arguably the first to recognize that the problem of the observational equivalence of astronomical hypotheses, if it were to be surmounted, would require the unification of astronomy with physics. In the *Astronomia nova* Kepler placed both of the ancient scientific traditions of astronomy and physics together to found a new kind of theory of the motions of the planets - a theory that provided causal reasons for the facts of positional astronomy. In doing this he was attempting to provide sound causal explanations for astronomical regularities that, previous to the *Astronomia nova*, were not even recognized as explicable in causal terms. Kepler's astrophysical theory was, with a few exceptions, largely dismissed by his contemporaries, and was certainly overtaken by the science of the *Principia*. Horrocks and Borelli were practically the only thinkers who could be called followers of Kepler, in that they alone took seriously and further developed the Keplerian system of causes as well as the kinematics of his heliocentric hypotheses. Even after Kepler's predictive tables based on the new heliocentric hypotheses, the *Tabulae rudolphinae* of 1627, were clearly demonstrated to be far superior in precision compared to any other tables, very little attention was paid to the physical principles upon which they were founded. Most of practical astronomy from the period between Kepler and Newton was devoted to reformulating Kepler's astronomy into traditional forms.[132] Kepler's 'laws' of planetary motion, as they came to be called, were only legitimized to the majority of astronomers when they were subsumed by the concept of universal gravitation in the *Principia*. Even then, many

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132 See Wilson, C., "Predictive Astronomy in the Century after Kepler", in Taton, R., and Wilson, C., eds., *Planetary Astronomy from the Renaissance to the Rise of Astrophysics*, *The General History of Astronomy*, 2A, Cambridge: Cambridge University Press, 1989, pp. 161-206. See also Russell, J. L., "Kepler's Law of Planetary Motion: 1609-1666", *The British Journal for the History of Science*, 2, 1964, pp. 1-24.

astronomers were unwilling to engage the difficult *Astronomia nova* with the studiousness that Kepler hoped his readers would have. This does not mean, however, that Kepler did not recognize the need for a new kind of theory, and that he did not in the *Astronomia nova* attempt to provide it – even if the result of this attempt was deemed a failure by later standards.

In retrospect, it is easy to see the medieval and ancient artifacts that remain in Kepler's astrophysical theory. His concept of inertia contaminated his celestial physics with the Aristotelian concept of natural place.[133] Kepler was unable to free his physics from the conception of inertia as the tendency of matter to come to rest in the absence of an acting force. This led Kepler to describe the solar force acting upon the planets in terms of a continuous tangential action, and this would ultimately lead to the rejection of his physical theory. In one important respect however, Kepler would have been pleased to see the eclipse of his theory by the dynamical account of Newtonian universal gravitation. For the fall of his theory on the basis of an erroneous physical axiom represents the fulfilment of Kepler's most general philosophical goal, the physicalization of astronomy.

In the *Astronomia nova* Kepler broke from nearly two and a half millennia of traditional assumptions and arguments in two fundamental ways. First, he was the first to perceive, and to show empirically, that the ancient assumption of circular motions, if adopted in a physically meaningful manner, is insufficient to account for planetary motions unless numerous additional and *ad hoc* hypotheses are allowed in order that theory may be adapted to appearances. Through the combination of his determination to create a

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[133] Kepler's fullest treatment of the concept of inertia is found in his *Epitome astronomiae Copernicana*, published in three parts betwenn 1618 and 1621. For a summary of Kepler's concept of inertia, see Stephenson, *Kepler's Physical Astronomy*, Princeton: Princeton University Press, 1987, pp. 141-145.

physically heliocentric theory of Mars together with the precision of the observational data collected by Brahé, Kepler was forced to the undeniable conclusion that the actual path of the planet, irrespective of whether it be given by a deferent/epicycle or eccentric/equant combination, could not be circular. This conclusion was deeply disturbing to Kepler who, perhaps as much as anyone else of his time, accepted the perfection of the heavens and the embodiment of this perfection in the principle of circular motion. But Kepler took the principle of circular motion to mean that the path of the planet, as determined by the physical forces acting on it, is truly a circle. No astronomer had made this assumption so scrupulously before Kepler. In geocentric theories such as those of Ptolemy or Brahé, and even in the heliocentric theory of Copernicus, the spatial path of the planet relative to the central body is a complicated spiral or diverged otherwise from a true circle. The assumption that the Earth is immobile requires that its motions be attributed to each of the planets - individually as Ptolemy does in his hypotheses, or collectively as Brahé does in making all the planets travel with the Sun in its revolution about the Earth. In either case, the planets' paths must contain great loops or curves that approach the Earth when the planet, if it is superior, is in opposition, or if it is inferior, when it is at inferior conjunction. Furthermore, these complex paths are not even closed figures so that the planet does not ever return to its previous path.[134] Copernicus, in setting forth his equant-free hypotheses, had endeavoured to eliminate from astronomy the Ptolemaic supposition that planetary motion is only uniform in a fictitious circle, the equant circle. But if taken to represent the actual paths

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[134] Kepler, J., *Astronomia nova*, chapter 1. See Donahue, W., *Johannes Kepler, New Astronomy*, Cambridge: Cambridge University Press, 1992, pp. 118-120. See also Small, R., *An Account of the Astronomical Discoveries of Kepler*, London: 1804, reprint, Madison: University of Wisconsin Press,1963, pp. 109-111.

of the planets, Copernicus' hypotheses are actually far more complicated than are those of Ptolemy. Though the motions in each of the Copernican epicycles is uniform and circular, their combined effect is to make the actual orbit of the planet deviate from a circle, extending everywhere else in its path beyond the radius at the apsides. Kepler abandoned Copernicus' principle of uniform circular motion and reintroduced the Ptolemaic equant – but in the context of physically heliocentric hypotheses. Thus, for the first time in the history of astronomy, the planets were ascribed <u>truly</u> circular orbits.[135] In fact, it was Kepler's assumption in the early chapters of the *Astronomia nova* that the true path of Mars in space is a perfect circle, and his subsequent attempts to determine this circular orbit, that led directly to the destruction of the ancient principle.

Kepler's second major break with traditional astronomy consists in his innovation as the first practitioner of what we now refer to as celestial mechanics. He developed a dynamical astronomy in place of the formal systems of his predecessors; he explained the motions in the heavens by causal hypotheses rather than mathematical rules. It is not without reason that the *Astronomia nova* bears the subtitle '*Physica Coelestis*', for it represents the results of a search for a single and universal physical principle by which to explain the motions of all the planets. While engaged in the researches that would lead to the *Astronomia nova* Kepler wrote of the relation of his new astronomy and celestial physics,

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[135] This point is made by Koyré. See Koyré, A., *The Astronomical Revolution*, Maddison, R. E. W., trans., Ithaca: Cornell University Press, 1973, p. 169.

> "I believe that both sciences are so closely interlinked that one cannot attain completion without the other."[136]

At around the same time he similarly wrote,

> "I am much occupied with investigation of the physical causes. My aim in this is to show that the celestial machine is to be likened not to a divine organism but rather to a clockwork... insofar as nearly all the manifold movements are carried out by means of a single, quite simple magnetic force, as in the case of a clockwork all the motions [are caused] by a simple weight. Moreover, I show how this conception is to be presented through calculation and geometry"[137]

These two fundamental breaks with previous astronomy, which are the ultimate culmination of the two principles that have here been shown to be articulated in his earlier works, are in the context of Kepler's philosophy, inseparable. Although it is possible to regard these innovations as distinct in a logical sense, historically Kepler was driven to his physical conclusions by a precise quantitative analysis of the successes and deficiencies of the principle of circular motion, and it was his particular genius in physics that allowed him to place astronomy on a new empirical foundation which did not require that the nature of the planetary bodies be fundamentally different from terrestrial bodies.[138]

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136 Letter to Cristian Severin Longomontanus, quoted from Holton, G., "Johannes Kepler's Universe: Its Physics and Metaphysics", *American Journal of Physics*, 24, 1956, p.342.
137 Letter to Herwart von Hohenburg, quoted form Holton, G., *ibid.*, p.342 – Caspar p.142
138 See Caspar, M., *Kepler*, New York: Dover Publications, 1993, p.141.

The philosophical importance of Keplerian heliocentrism has, even in our time, been largely unrecognized. The arguments employed by Kepler to establish the basis of his planetary astronomy have been too long regarded either as the inevitable consequence of Copernicanism or as necessary or implicit requirements for the rejection of geocentrism. Koyré, for example, while cognizant of the importance of Kepler's transformation of astronomical theory into a physical science, holds to the former of these positions, arguing that the simplification of astronomy that results from employing the apparent position of the Sun was implicit in the Copernican system. Stephenson maintains the latter position, describing the conclusions of the early chapters of the *Astronomia nova* as "preliminaries" to Kepler's important discoveries. In this view Stephenson follows Caspar, whose notes in the 1937 *Johannes Kepler Gesammelte Werke* edition of the *Astronomia nova* refer to the first part of the work as a 'prelude to the main plot'.[139]

Yet if we are to accept that the truly heliocentric theory of Kepler was of fundamental importance to the history of astronomy, then we may well consider his reasoning to this theory to be equally important to the history of natural philosophy. If, as Jardine maintains, Kepler's *Apologia* embodies "the birth of history and philosophy of science as a distinctive mode of reflection on the status of natural science"[140], Kepler's focus in the *Astronomia nova* on theoretical progress in astronomy and the means whereby such progress may be achieved should be considered as part of the development of the general philosophical position that was proposed in his earlier works. Indeed, although the *Astronomia nova* is a work fraught with the technical minutiae

[139] Caspar, M., ed., *Johannes Kepler Gesemmelte Werke*, vol. 3, *Astronomia nova*, Munich: C. H. Beck, 1937, p. 439.
[140] Jardine, N., *The Birth of History and Philosophy of Science, Kepler's A Defence of Tycho Against Ursus with Essays on its Provenance and Significance*, Cambridge: CambridgeUniversity Press, 1984, p. 2.

of empirical astronomy, concern with theoretical progress is prevalent throughout the text. In this context, the first chapters of Kepler's great work embody the announcement of a new philosophy of the heavens, a new kind of knowledge concerning the patterns that can be discerned in the motions of the planets. While the historical importance of Kepler's eventual successes in bringing this philosophical programme in astronomy to a practical and demonstrably true form is not in question, these successes were, in a sense, merely the detailed consequences of a remarkably acute vision of the potential of astronomical theories to truly 'portray the form of the world'. To put this point another way, if Kepler had concluded his researches with the demonstrations of the first two parts of the *Astronomia nova* – well before his arrival at the areal law and the elliptical form of the orbit - he would nonetheless have provided compelling evidence for the physical significance of the Sun in planetary motions and seriously impugned the methodological principles upon which the planetary hypotheses of his predecessors had been constructed.

Kepler's approach to astronomy as both a physical and mathematical science represents a remarkable evolution in the consideration of observations, hypotheses, and theories and the relations that are said to obtain between them. Epistemologically, the *Astronomia nova* is an explicit and remarkably innovative attempt to render reality from appearances. Throughout his work, Kepler is not only engaged in the practices of astronomical hypothesis generation and appraisal, but also in the development of a new science of astronomical hypotheses generally.

From the earliest pages Kepler argues that 'saving the phenomena' is a necessary, but not a sufficient condition for the physical astronomy that he proposes. But if Kepler had included only the geometrical (that is, purely positional) results of his researches in his published works, he would still have succeeded in revolutionizing astronomy. Indeed, history relates that almost all of Kepler's contemporary readers, while they rejected the physical reasoning so necessary for Kepler himself, were forced to concur with the geometrical advantages of the Keplerian astronomical hypotheses. After the *Astronomia nova* the employment of the mean Sun in astronomy would soon become an anachronism – but not, at least immediately, for all of the robust reasons for which Kepler had argued.

When Kepler began the studies whose results would appear in the *Astronomia nova*, current astronomical theory was already insufficiently precise within even the limited (for Kepler) domain of positional astronomy. In particular, Brahé's record of numerous and precisely measured observations showed that even the most recently computed planetary tables contained serious errors. However, the very fact of these grave errors gave Kepler the means to argue that Copernicus was fundamentally correct in asserting the heliocentricity of the world-system – while simultaneously denying the role of the mean Sun that Copernicus, Brahé, and others had adopted.

More generally and as is evident from his earlier works, Kepler was acutely aware of the equivalence of the theories of the ancients and his contemporaries. As regards the limited domain of geometrical determinations, the early chapters of the *Astronomia nova* make it abundantly clear that it does not matter whether the Sun orbits the Earth or the Earth orbits the Sun.[141] Kepler states in the introduction,

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[141] See also Kepler's remarks in the introduction to the *Astronomia nova*, 'on the schools of thought in astronomy'. See Donahue, W., *Johannes Kepler, New Astronomy*, Cambridge: Cambridge University Press, 1992, pp. 47-48.

> "My aim in the present work is chiefly to reform astronomical theory (especially of the motion of Mars) in all three forms of hypotheses[142], so that our computations from the tables correspond to the celestial phenomena. Hitherto, it has not been possible to do this with sufficient certainty. In fact, in August of 1608, Mars was a little less than four degrees beyond the position given by calculation from the *Tabulae Prutenicae* [of Erasmus Reinhold (1511-1553)]. In August of 1593 this error was a little less than five degrees, while in my new calculation the error is entirely suppressed."[143]

As indicated by this passage, many chapters of the *Astronomia nova* contain astronomical derivations in three or even four forms: Ptolemaic, Tychonian, Copernican, and those of Kepler himself. While all of these manners of calculation may be considered approximately equivalent with respect to positional astronomy, with respect to the actual natures of the stars, Sun, planets, and their respective motions, they diverge widely, and are even contrary to one another. Ultimately, it was the problem of the actual motions of the celestial bodies that concerned Kepler:

> "...although I place this goal [of providing an empirically accurate account of the motions of the planets] first and pursue it cheerfully, I also make an excursion into Aristotle's *Metaphysics*, or rather I inquire into celestial physics and the natural cause of motions. The eventual result of this consideration is the formulation of very clear arguments

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[142] Kepler means to reform astronomy through consideration of the hypotheses of Ptolemy, Brahé, and Copernicus.
[143] Kepler, J., *Astronomia nova*, trans., Donahue, W., *Johannes Kepler, New Astronomy*, Cambridge: Cambridge University Press, 1992, p. 48.

> showing that only Copernicus' opinion concerning the world (with a few small changes) is true, that the two others [the opinions of Ptolemy and of Brahé] are false..."[144]

Kepler continues, by referring to the 'linkage of syllogisms' and physical hypotheses that he had discussed in the *Apologia.*

> "...all things are so interconnected, involved, and intertwined with one another that after trying many different approaches to the reform of astronomical calculations, some well-trodden by the ancients and others constructed in emulation of them and by their example, none other could succeed than the one founded upon the motions' physical causes themselves, which I establish in this work."[145]

Although, *prima facie*, it may seem that the *Astronomia nova* is a loosely organized treatise on several diverse and vaguely related subjects, close examination reveals these subjects to be intimately conjoined to one another; they are, in Kepler's terms, "interconnected, involved, and intertwined". Kepler had to convince the astronomers that the merely probable truths of physics have a place in astronomical hypotheses, and he also had to convince the physicists that a causal dynamics similar to that of terrestrial bodies should be applied to the motions of the planets through space. This meant that Kepler had to write with a broad didactic purpose in mind. He had to present his case in both the axiomatic deductive form of the traditional treatises of mathematical astronomy, and the more oratorical form of metaphysical

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[144] Kepler, J., *Astronomia nova*, trans., Donahue, W., *Johannes Kepler, New Astronomy*, Cambridge: Cambridge University Press, 1992, p. 48.
[145] Kepler, J., *ibid.*, p. 48.

studies. Individual chapters of the *Astronomia nova* that may seem tangential to one of the declared purposes of the work are nonetheless central to Kepler's wider objective. These unusual characteristics of the rhetorical style of Kepler's great work are not mere idiosyncrasies, but are an integral part of the project that engaged his efforts from his time at Graz onward.[146] As he asserts in the introduction of the *Astronomia nova*:

> "... since I have mingled celestial physics with astronomy in this work, no one should be surprised at a certain amount of conjecture. This is the nature of physics, of medicine, and of all the sciences which make use of other axioms besides the most certain evidence of the eyes".[147]

Kepler remarks that the purpose of the work is therefore two-fold. The first is to provide a method of calculation such that the measured appearances of the sky are accurately retrodicted and predicted by theory. Obtaining this mathematical correspondence between theory and appearances, at least, is the common purpose of all astronomy - though admittedly very different (interpretative) means are employed by various astronomers to obtain the correspondence itself. There can be no doubt that on this account Kepler's theory succeeds admirably. But Kepler clearly shows that this 'saving of the appearances' is not sufficient for a physical astronomy.

The second purpose of the *Astronomia nova* is to explicate the "very clear arguments showing that only Copernicus' opinion concerning the world (with a few small changes) is true."[148] Kepler's modesty regarding the "few

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[146] Holton, G., "Johannes Kepler's Universe: Its Physics and Metaphysics", *American Journal of Physics*, 24, 1956, p. 341.
[147] Kepler, J., *Astronomia nova*, trans., Donahue, W., *Johannes Kepler, New Astronomy*, Cambridge: Cambridge University Press, 1992, p. 47.
[148] Kepler, J., *Astronomia nova*, trans., Donahue, W., *ibid*, p. 48.

small changes" that he makes to Copernicus' theory should not belittle the profound philosophical differences that separate the two astronomers and their respective theories. Kepler was the first to argue that the world-system is heliocentric, in the modern sense of the term; he was the first to propose a cosmology in which the physical body of the Sun is the cause of the planets' motions. In defending a theory of physical heliocentrism Kepler changed the very natures of mathematical astronomy and of the natural philosophy of the heavens.

Accordingly, we may identify a third, implicit, purpose behind the composition of the *Astronomia nova*. This purpose concerns the synthesis of the first two. The *Astronomia nova* not only provides a new geometrical theory which corresponds to the appearances, and gives a detailed account of the natural causes whereby actual physical motions may produce such appearances, but in addition, proposes a unification of these two sciences into one. Reading through the complex of arguments that are presented in the work, one finds that where geometrical demonstration is deficient, causal reasoning provides support, but where causal reasoning is not supported by geometrical demonstration from observations, it is rejected. The interaction between these two formerly bifurcated modes of natural philosophy is justified by frequent appeals to the rationality of the reader. The reader finds arguments that invoke new considerations of the complex relations between observations, hypotheses, and theoretical principles. In this regard, the entire *Astronomia nova* may be seen as a practical extension of the philosophy of the *Apologia pro Tychone contra Ursum*.

Kepler's argument in the *Astronomia nova* for a *truly* heliocentric theory, as opposed to a theory of the mean Sun, as was supposed by all previous astronomers, is predicated on the introduction of physical reasoning into astronomy. By introducing physically meaningful coordinates, Kepler was able to describe the apparent motions of each of the planets in terms of physically meaningful components. In essence, the observed positions of the planets were shown to be describable as the rather simple effects of a physical emanation the strength of which is determined by nothing else than the distance of the planet from the Sun. Furthermore, he was able to show that the orbit of the Earth and those of all the other planets are of identical form.

In the first Chapter of the *Astronomia nova,* Kepler describes the observed motions that form the basis of any astronomical theory:

> "Before the distinction between the first motion and the second motions was established, people noted (in contemplating the Sun, Moon and stars) that their diurnal paths were visually very nearly equivalent to circles. These were, however, seen to be entwined one upon another... part of them north and part of them south of the greatest circle [i.e. the celestial equator]. They saw also that the stars have different speeds in this diurnal and apparent motion. The fixed stars are fastest of all, since those that are in conjunction with any of the planets on the preceding day come to their setting first. The Sun is slower, and so on the following day its setting follows that of the fixed stars with which on the previous day it was conjoined. Slower than this still, slower than all the stars, is the Moon, since after setting with the Sun today, it lags by an appreciable interval tomorrow when the Sun sets."[149]

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[149] Kepler, J., *Astronomia nova*, trans., Donahue, W., *Johannes Kepler, New Astronomy*, Cambridge: Cambridge University Press, 1992, p. 115.

It is clear that Kepler considers these actual appearances to require no theoretical assumptions - all astronomers will agree on the observations. But it is insufficient for astronomy to simply record the passing of the celestial objects. Astronomy requires causal reasoning - for without it we have no reason to suppose that our records may tell us anything about the future state of the sky. He continues:

> "This first adumbration of astronomy explains no causes, but consists solely of the experience of our eyes, extremely slowly acquired. It cannot be explained in figures or numbers, nor can it be extrapolated into the future, since it is always different from itself, to the extent that no spiral is equal to any other in elapsed time and none carries over into the next with a curvature of the same quantity."[150]

If, however, an astronomer investigates the distinct motions of the Sun, Moon, and planets *relative* to the stars, the observed motion is then resolved into two component motions, one being the swift motion of the entire sky to the West, the other being the much slower motions of the Sun. Moon, and planets generally Eastward. Kepler states:

> "...it was very helpful for astronomers to understand that two simple motions, the first one and the second one, the common and the proper, are mixed together, and that from this confusion there necessarily follows that continuous sequence of conglomerated motions. Thus when

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[150] Kepler, J., *Astronomia nova*, trans., Donahue, W., *Johannes Kepler, New Astronomy*, Cambridge: Cambridge University Press, 1992, p. 116.

> that common and extrinsically derived diurnal revolving effect is removed, the fixed stars are suddenly no longer the swiftest and the Moon the slowest, but quite the opposite, the latter being the swiftest in itself while the former are very clearly very slow or immobile."[151]

The issue of whether or not the swift diurnal motion of the sky to the west is in fact an "extrinsically derived effect" becomes relevant once the division of the single observed motion into two superposed component motions is made. According to Brahé's theory, for example, wherein the Earth does not rotate, the entire heavens turn about the celestial poles, whereas according to Copernicus', who allows the rotation of the Earth, the stars are fixed. Indeed, this simple example of empirical equivalence provides the reader with an introduction to an issue that recurs throughout the work.

Having distinguished the diurnal revolution of the sky from the remaining motions, Kepler remarks that again in the case of the remaining motions we are able to distinguish two further components:

> "This [elimination of the diurnal motion] turned out to be enormously helpful in astronomy in grasping the simplicity of the motions. Instead of unending spirals, there remained little but the solitary circles and a single common motion, either of all the planets and the whole world as well in a direction opposite to the proper motions, or (following Aristarchus, making the world stand still) of the Earth's globe around the axis [of the world] in the same direction as the proper motions."[152]

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[151] Kepler, J., *Astronomia nova*, trans., Donahue, W., *Johannes Kepler, New Astronomy*, Cambridge: Cambridge University Press, 1992, p. 117.
[152] Kepler, J., *Astronomia nova*, trans., Donahue, W., ibid., pp. 117-118.

Thus each of the planets' motions may be analysed or distinguished into motions on "solitary circles" particular to each planet, and a common motion. The common motion, annual in periodicity, may either be supposed to be produced by the motion of the Sun and planets about the Earth, as in the theory of Brahé, or by the motion of the Earth itself, as in the theories of Aristarchus and Copernicus. This division of motions is made by Ptolemy as well, but he does not suppose that the component motions correspond to a superposed proper motion, either of the Earth or the Sun and planets, but rather to individual motions of the each of the planets themselves.

These component motions, or inequalities, are termed the first and the second. Hence, planetary positions as measured from the Earth, and the motions composed of these positions over time, are said to be composed of first and second inequalities. Each measure of angular motion is made up of 1) that part which is associated with the sidereal period of the planet and attributable to the motion of the planet about the Sun in a heliocentric theory - the first inequality, and 2) that part which is associated with the annual periodicity and attributable to the motion of our viewpoint on the Earth in a heliocentric theory - the second inequality. In his *Astronomia nova* Kepler effectively redefines these component measures on the basis of heliocentric planar orbits for each of the planets and the Earth. Previous to this redefinition the first inequality and second inequalities were not completely independent. This was the result of two factors combined: 1) the use of the mean Sun, which is a point removed by several solar diameters from the true Sun, and 2) the use of an incorrect eccentricity for the orbit of the Earth. The consequence of these factors is that all the planets' motions contained components that were, in effect, determined by the position of the Earth (in the theory of Copernicus) or the position of the Sun (in non-heliocentric theories such as those of Ptolemy and Brahé). With the *Astronomia nova* is given the first theory of Mars' motion that depends solely on the planet's relation to the Sun. Once the size of the orbit, its eccentricity, the direction of its line of apsides, and the planet's longitude within the orbit are given, knowledge of the inclination of the orbit and the longitudes of the nodes allows the first inequality to be uniquely determined in both longitude and latitude. To further determine the second inequality the position of the Earth must be computed and then the projection of the first inequality onto that position – that is, the position of the planet as it appears from the Earth – can thereby be determined. It is the fact that the two inequalities are combined in any set of earthbound observations of the planet that makes the solution of the actual orbit of the planet a daunting task. For both the true heliocentric longitudes and heliocentric latitudes of the planet will, when observed, be augmented or diminished in accordance with the proximity or remoteness of the Earth. Thus the geocentric (that is, observed) longitudes and latitudes will bear a complex relation to the heliocentric longitudes and latitudes. Furthermore, since the orbit of the Earth is not concentric to the Sun, this effect of perspective will vary depending upon which part of the ecliptic the Earth happens to occupy. Indeed, before Kepler could precisely determine the orbit of Mars he had to be sure that his hypothesis for the orbit of the Earth was true within the bounds that the observations of Brahé could justify.

The importance of this successful disentangling of the planetary inequalities consists in the fact that the planetary motions could henceforth be computed on the basis of, and be explained by, in the case of the first inequalities, the solar emanation that was supposed by Kepler to cause them, or, in the case of the second inequalities, the purely perspectival changes in the observed position introduced by the motion of the Earth. Copernicus had been correct in setting the Earth in motion around the Sun, but he had not deduced the true motions of the Earth, and of the other planets, from correct principles – those that would truly determine the first and second inequalities. Herein is an important feature separating Kepler's version of heliocentrism from Copernicus'; it is the physically correct distribution of the first and second inequalities of motion amongst the individual planetary motions that is the main purpose of Kepler's project in the *Astronomia nova*. Various combinations of supposed actual motions could produce the same appearances on Earth. But only one of these combinations of motions can be true - unless scepticism in astronomy be adopted. Kepler's conviction that the physically true inequalities of the planets' motions could be discovered was stronger than his adherence to any of the formal principles of traditional astronomy. By constraining astronomy to physical explanation he was forced to discard many of the principles that his predecessors had considered the legitimate foundation of the science. But he wanted to be, at the end of his researches, in possession of the truth, and not merely a falsehood which was ingeniously contrived to give the appearance of truth in a limited domain.

Moreover, (and this is a point barely mentioned even in the most recent works in Keplerian scholarship,) Kepler developed the first consistent single hypothesis of both planetary latitudes and longitudes. Although the hypotheses of previous astronomers could be used to predict both of these quantities, albeit with less precision than Kepler's hypothesis, distinct hypotheses were used, one for each parameter.[153] In this regard, Kepler's

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[153] See Small, R., *An Account of the Astronomical Discoveries of Kepler*, London: 1804, reprint, Madison: University of Wisconsin Press,1963, pp.101-3 on Copernicus' account of planetary latitudes, pp. 132-3 on Brahé' account.

introduction of the planar orbit at constant inclination to the ecliptic may be considered a highly significant step in the immense simplification of the geometry of planetary theory which Kepler proposed, and one that confirmed directly Kepler's physical convictions. It is no exaggeration to say that Kepler was the first to conceive of the planetary 'orbit' in the modern sense of the term – as a closed planar trajectory through space.

In the first part of the *Astronomia nova*, Kepler shows how the first and second inequalities are defined in the theories of Ptolemy, Copernicus, and Brahé. In the heliocentric theory of Copernicus the motion in the second inequality is produced by the trigonometric projection of the motion of the Earth onto the proper motion of the planet in the first inequality. In the geocentric theory of Brahé motion in the second inequality is produced by the motion of the Sun, upon and in addition to which all the planets' proper motions in the first inequality are executed. In the geocentric theory of Ptolemy, motion in both inequalities is produced by an individual eccentric and deferent combination for each of the planets.

Ptolemy, Copernicus, and Brahé, and all astronomers previous to Kepler, supposed that the first inequality is measured in relation to the mean Sun. The mean Sun is, in a heliocentric theory, the centre of the circular motion of the Earth about the Sun, or in a geocentric theory, the centre of the circular motion of the Sun about the Earth. All astronomers previous to Kepler supposed that the Sun's or Earth's motion was explained by a single uniform motion on an eccentric circle. The centre of such a circular motion is therefore also a *punctum equans*, a point in relation to which uniform motion is measured. Kepler considered rejecting this assumption of the coincidence of the centre of the eccentric with the equant point for the Earth in his

Mysterium cosmographicum. In the *Astronomia nova* he went further – he was sanctioned by the failure of his *hypothesis vicaria* to give a complete account for the first inequality of Mars to establish a new theory of the Earth (or equivalently, of the Sun).

Kepler argued in the *Mysterium* that, for physical reasons, the first inequality of the planets should be measured in relation to the true position of the Sun. He recounts that Brahé had argued that all of the appearances involving the first inequality could be accounted for in his own theory, which uses the mean Sun.[154] Kepler's response was to suggest that this should not prevent him from devising his own account of the observations of the planetary motions using the Sun's apparent motion, and that it would be in the second inequality that it would be seen which was more nearly correct. Kepler states, "What I answered then is what is set out to be proved in the first part of the book."[155]

The part to which Kepler refers consists of the first six chapters of the *Astronomia nova*. These six chapters together demonstrate that the referral of the first inequality to the physical Sun rather than the mean Sun introduces very small changes to the computed motions in first inequality. Thus, the account given of the motion of the planets in second inequality, when combined with the account of motion in the first inequality, indeed becomes the decisive criterion for choosing amongst the theories in question. But independently of this further empirical confirmation, Kepler claims "my method [of reference to the physical Sun] is in agreement with physical causes, and their old one [of reference to the mean Sun] is in disagreement."[156]

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154 Kepler, J., *Astronomia nova*, trans., Donahue, W., *Johannes Kepler, New Astronomy*, Cambridge: Cambridge University Press, 1992, p. 49
155 Kepler, J., *Astronomia nova*, trans., Donahue, W., *ibid*., p. 80
156 Kepler, J., *Astronomia nova*, trans., Donahue, W., *ibid*., p. 49

The first part of the *Astronomia nova* thus concerns the empirical permissibility of introduction of the basic form of Kepler's new hypotheses of physical heliocentrism. In fact, as Kepler will demonstrate in subsequent chapters, his new hypotheses are not only commensurate with empirical tests employing Brahé's observations, but more importantly they bring a surprisingly simple and universal means of collecting and assembling the appearances into physically plausible phenomena. The historical importance of this early component of Kepler's new astronomy was acknowledged by Small, who claims:

> "...excepting only the system of Copernicus, it was an improvement more important, and of greater consequence, to simplify science, than any which had been introduced in all the preceding ages; and his successful and decisive establishment of its truth and propriety, may be justly ranked among his greatest discoveries, and equally deserves our attention with those which have been more generally celebrated."[157]

Yet even the improvement made by Copernicus in placing the centre of the planets' orbits at the mean Sun, can reasonably be regarded as part of Copernicus' conservative goal of returning astronomy to the form it had assumed with Hipparchos – as a means to the end of providing a formally correct astronomy. Although the heliocentric hypotheses of *De revolutionibus* reduced the number of circles needed to account for the motions of the planets in their second inequalities, the elimination of the equant principle simultaneously necessitated the introduction of epicycles

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[157] Small, R., *An Account of the Astronomical Discoveries of Kepler*, London: 1804, reprint, Madison: University of Wisconsin Press,1963, p. 154.

into each planetary hypothesis, so that the question of whether the Copernican system in fact simplified astronomy is not obviously granted in its favour. The Keplerian introduction of the planar orbit about the true Sun, however, divested the science of astronomy not only of the underdetermined hypotheses of compounded uniform circular motions in the account of the longitudes of the planets, but also rendered unnecessary the cumbrous hypotheses of latitude that the ancients had been forced to devise. Hence, the simplification of astronomy of which Small writes can only be clearly regarded as a simplification after Kepler's radical modifications to the Copernican heliocentric theory are recognized.

The referral of the first inequality motions of the planets to the actual body of the Sun has several important consequences. Referring the positions of the planets to the physical Sun implies that both the planetary lines of apsides (the line joining the places of the planet's greatest and least distance from the Sun) and places of opposition (where the first inequality is identical to the second) must also be referred to the physical Sun.[158]

At times of the opposition of a planet, the first inequality is, by definition, equal to the second. Hence, observations made at such time, referred to as acronychal observations, are important because they allow a direct measure of the first inequality. Indeed, within any theory, if a given hypothesis could not predict when and where the second equality of a planet would become negligible, then there could be little hope of further advancement in the determination of the planet's general motions. Such acronychal observations were therefore employed by nearly all astronomers in calibrating their theories to the appearances. It is Kepler's contention in

[158] See Kozhamthadam, J., *The Discovery of Kepler's Laws*, Notre Dame: University of Notre Dame Press, 1994, p. 113.

the first six chapters of the *Astronomia nova* that in using observations made at oppositions to the mean Sun rather than to the physical Sun, all previous astronomers had failed to completely eliminate the true second inequality from their computations of the first inequality based on oppositions. Thus, he maintains, these astronomers attributed motions to the planets that are in actual fact motions of the Earth or Sun.

Whatever their other respective merits, the simplest hypotheses of the first and second inequalities in all these theories are false in the sense that they proceed by assuming that the second inequality is measured with respect to the mean Sun, and therefore that the first inequality is measured by the differences of the apparent motions of the planet from the mean Sun. If hypotheses are supposed to be true, then the actual motions (as opposed to the observed motions) of the planets are determined differently depending on which measure is adopted.[159]

According to Kepler's analysis, if the centre of the Earth's (or the Sun's) supposed motion is used to determine the division of the motions of the planets into first and second inequality, this will necessarily introduce an error into the eccentricity of the eccentric circle (and therefore the distances of the planet from the solar body), but more importantly it introduces errors into the eccentricity of the point of constant angular motion, the equant point. As Kepler was well aware, this eccentricity is far more significant in determining the orbit (and more precisely determined from observations) than the centre of the eccentric circle.[160]

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[159] See Small, R., *An Account of the Astronomical Discoveries of Kepler*, London: 1804, reprint, Madison: University of Wisconsin Press,1963, p. 151 for an explanation of this claim.

[160] See Wilson, C., "Kepler's Derivation of the Elliptical Path", *Isis* 59, 1968, p. 8.

The introduction of the practice of using true oppositions instead of mean oppositions – of employing acronychal observations reckoned from the body of the Sun rather than the calculated place of the mean Sun - entails a change in the position of the line of apsides, and hence the positions of the centres of the angular equations. Kepler wanted to confirm that the point of slowest motion was indeed the point of greatest distance from the actual body of the Sun, not the physically empty mean Sun. Only a truly heliocentric motion would be consistent with Kepler's introduction of physical causes. He maintained that there could be no other plausible reason for the planets to move more slowly than that the immaterial emanation from the Sun attenuates with distance, and that on those points of the planetary circle most distant from the Sun the emanation is weakest.

The practice of using mean oppositions, Kepler argued, is objectionable on physical grounds and can be seen to introduce many errors into the theories of all astronomers who had accepted it. Kepler considered that one might ascribe some or other 'innate affection' or 'propensity', or an intelligence or power by which the planet might regulate its own motion, but it would seem implausible that the planets would in such a circumstance be regulated by reference to an imaginary point, distinguished by no apparent signs or other 'accidental' properties, placed at no greater than two or three solar semi-diameters from the body of the Sun, a body of great magnitude. This arrangement is made even more improbable, Kepler suggests, when one considers that the difference of position between the true Sun and the mean Sun is not constant, but variable over long durations, as Copernicus assumed.

The *Astronomia nova* may be regarded as an extended application of the theory of hypotheses as proposed in the *Mysterium* and the *Apologia*. Kepler provides numerous and varied arguments based on his principle of "the linking of syllogisms" in both the refutation and the confirmation of astronomical hypotheses, and also considers the physical implications of these various hypotheses.

An example of the principle of syllogism linkage may be had from considering the eleven supposed motions in Ptolemy's hypotheses that Copernicus "banished from universe" by referring the second inequalities of the planets to the Earth's motion about the Sun. On Kepler's account of syllogism linkage, since each of these appearances is caused by the motion of the Earth, each can be regarded as an independent measure of the orbit of the Earth. Similarly, if the orbit of the Earth is assumed, the appearances of the other planets provide measures of their own true motions. These inferences are fully consistent with the demand stated in the *Mysterium* that the astronomer must be able to derive,

> "any of the phenomena which are actually observed in the heavens, to argue backwards, to argue forwards, to infer from one motion to another, and to perform anything whatever that the true state of affairs permits".[161]

In principle, each of the eleven apparent motions which Ptolemy had mistakenly imparted to the planets can be used as a measure of the true motions which are inferred from them and which, reflexively, are said to cause these same apparent motions.

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[161] Kepler, J., *Mysterium cosmographicum*, p. 13, trans. Duncan, A. M., *Mysterium Cosmographicum (The Secret of the Universe)*, New York: Abaris Books, 1981, p. 75.

The *Astronomia nova* is replete with examples of linked demonstrations of this kind. In chapter twelve, for example, Kepler employs a new method to calculate the position of the line of nodes of the orbit of Mars, that is, the line formed by the intersection of Mars' orbit with the plane of the orbit of the Earth about the Sun.[162] To compute the longitude of the nodes astronomers previous to Kepler had adopted an erroneous method using latitudes of the planet in opposition. Because these astronomers treated the Earth as the reference point in their hypotheses, they supposed the geocentric projection of the motion of the planet to be the true orbit and from this assumption derived the longitude of the node. But because, on the assumption that the motions are referred to the mean Sun, the latitudes of Mars in opposition will be different from one opposition to the next, the place of the node as determined by this method will also vary. Kepler's method was to observe the planet when it is in the plane of the ecliptic, and therefore possesses no latitude, and to employ several estimates of Mars' longitude to triangulate the node. Consider diagram 5.1. When Mars, M, is observed to have no latitude, it must be at one of its nodes, either N or N'. With a computed position for the Earth, the longitude of the node, the direction of the line NSN', may be found by triangulation on the observation. Since in this configuration both the Earth and Mars are in the plane of the ecliptic, the erroneous reduction of the longitude of the planet is altogether avoided. In this new method, Kepler effectively makes the various positions of the Earth when Mars is seen on the ecliptic each generate an independent estimate of the longitude of the node, which in fact agree with subsequent estimates. Kepler was able to confirm the placement of the nodes by examining the latitudes of Mars at intervals of its

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[162] Kepler, J., *Astronomia nova*, trans. Donahue, W., *New Astronomy*, Cambridge: Cambridge University Press, 1992, pp. 216-20.

sidereal period, when it should be found at the node again, and again possess no latitude. Kepler's arguments for physical heliocentrism are supported by the fact that the line of nodes, thus determined, passes through the body of the Sun.

An even more obvious case of reasoning by the linking of syllogisms is found in chapter thirteen, where three independent methods are used to calculate the inclination of Mars' orbit with respect to the Earth's orbit.[163] Kepler shows that when the inclination is referred to the true Sun, it assumes a constant value. Thus the orbit of Mars was determined to be planar. With this discovery, Kepler showed that a variety of hypothetical devices required in the astronomical theories of his predecessors simply do not need to be supposed in a truly heliocentric theory.

In generating empirical support for heliocentrism, Kepler applies the epistemological criteria discussed in the *Mysterium* and the *Apologia*. In each of the methods that are deployed to determine the inclination, the estimate is calculated from quite different sets of configurations of the Sun, Earth, and Mars. On the basis of these collected demonstrations, Kepler could claim that the proposition that the inclination of the orbit of Mars with respect to the Sun is constant would have almost certainly been divulged as false if this were in fact the case. On the contrary, he found that all three of the methods he used generate the same value for the estimate of the orbital inclination to a high degree of precision, and thus that the inclination of the orbit is not affected by either the position of the Earth or of Mars with respect to the Sun.

[163] Kepler, J., *Astronomia nova*, trans. Donahue, W., *New Astronomy*, Cambridge: Cambridge University Press, 1992, pp. 221-31.

The first method for determining the inclination of the orbit is as follows. It requires that Mars is at either of its limits (the points in the orbit at 90 degrees from the nodes and therefore of greatest latitude from the ecliptic) and that the Earth and the Sun are the same distance from Mars. There are four such configurations, two for each limit. The determination of the inclination by this method also requires values for: the longitudes of the nodes of the orbit of Mars, the ratio of the sizes of the orbit of Earth and the orbit of Mars, and the eccentricity of the orbit of Mars.

Consider diagram 5.2. The Sun is at S. The Earth is at E. Mars is in the limit of its orbit at M. M' is the projection of the position of Mars onto the plane of the ecliptic. The longitudes of the nodes of the orbit, N and N', have been determined. The longitudes of the limits are therefore at N + 90 degrees and N' + 90 degrees. For each of the two limits, there are two points, E and E', in the Earth's orbit such that the distance EM equals the distance SM. With E at such a point, the triangle SME is isosceles. If the size and eccentricity of the orbit of Mars is assumed, the heliocentric distance of Mars, SM, is determined. Assume point D, bisecting ES. Therefore ED and DS are equal to half of ES. Since EDM is a right triangle and EM and ED are known, the angles MED and EMD may be determined. EMS will be twice the value of EMD. Since the angle EMS is known, and since the longitude of M is known, the angle SEM, that is, the longitude of Mars as seen from the Earth, can be determined.

So, when Mars in one of the limits and the Earth is at one of the two places in its orbit such that the observed elongation of Mars from the Sun is equal to the appropriate value, the observed latitude of Mars will precisely equal the inclination of the orbit. The nodes of the orbit of Mars are at 47 degrees and 227 degrees. Therefore the limits of the orbit are at 137 and 317 degrees. From Brahé's data it was determined that the distance SM in terms of

SE = 1000 parts, is 1666.6 parts at first limit, and 1375 parts at the opposite limit. So, for the first limit, since SM = 1666.6 and ED 500 parts, it follows that the angle MED is equal to 72 degrees 32 minutes. For the second limit, SM = 1375 parts and it follows that the angle MED for this opposite limit is equal to 68 degrees 40 minutes. Therefore, when Mars is in one of the limits and the Earth is at one of the two places in its orbit such that the observed elongation of Mars from the Sun is equal to plus or minus 72 degrees 32 minutes from the first limit, or plus or minus 68 degrees 40 minutes from the second, the observed latitude of Mars will precisely equal the inclination of the orbit.

Kepler gives nine instances of this procedure. Six of these are very near a configuration where the triangle SME is isosceles. These give the inclination as approximately 1 degree 50 minutes. The three remaining cases are not so near the correct configuration, but the computed values of the inclination, when adjusted for the errors in displacement from the ideal configuration, are consistent with the estimate of 1 degree 50 minutes.

This general method is applicable to all planets with the exception of Mercury because all the planets but Mercury may be situated at an equal distance from the Earth and the Sun.

The second method for determining the inclination of the orbit requires that the Earth is in the line of nodes of the orbit of Mars and Mars is at quadrature to the Sun (that is, 90 degrees from the Sun). There are four such configurations, two for each of two points where the Earth may pass through the plane of the orbit of Mars. The determination of the inclination by this method also requires values for the longitudes of the nodes of the orbit of Mars.

Consider diagram 5.3. The Sun is at S. The Earth is at E. Mars is at M. M' is the projection of the position of Mars onto the plane of the ecliptic. The longitudes of the nodes of the orbit, N and N', have been determined. Assuming the Earth to be on the line of nodes of the orbit of Mars, the heliocentric longitudes of Mars at quadrature to the Sun are therefore: N + 90 degrees and N' + 90 degrees.

When this configuration occurs, the observed latitude of Mars will precisely equal the inclination of the orbit. The nodes of the orbit of Mars are at 47 degrees and 227 degrees. Therefore, the heliocentric longitudes of Mars at quadrature to the Sun are 137 and 317 degrees.

Kepler gives four instances of this procedure. Three of these have Mars very near quadrature at 137 degrees. Of these three, two are near the configuration where the Earth is at 227 degrees, and one is near the configuration where the Earth is at 47 degrees. These three cases give the inclination as approximately 1 degree 50 minutes. The remaining case has Mars in quadrature near 317 degrees. The computed value of the inclination, when adjusted for the errors in displacement, is not inconsistent with the estimate of 1 degree 50 minutes.

Kepler considers an extension of the second method that requires only that the Earth be in the line of nodes of the orbit of Mars. Mars may be anywhere on its orbit. This configuration occurs twice annually, though for instances where the elongation of Mars from the Sun is small the planet will be impossible to observe. This method requires only values for the longitudes of the nodes of the orbit of Mars.

Consider diagram 5.4. The Sun is at S. The Earth is at E. Mars is at M. M' is the projection of the position of Mars onto the plane of the ecliptic. D is the place on the orbit of Mars with the same heliocentric longitude as the observed longitude of Mars – so that EM and SD are parallel. D' is the projection of the position of D onto the plane of the ecliptic. The longitudes of the nodes of the orbit of Mars, N and N', have been determined. Assuming the Earth to be on the line of nodes of the orbit of Mars, the observed latitude of the planet will be equal to the heliocentric latitude at D. Since EM is parallel to SD, MEM' is equal to DSD'. The longitude of the planet from the nearest node, angle NSM, is known. The angle DSD' will be equal to the inclination of the orbit of Mars only when the planet is in quadrature (as in the previous method), or NEM is 90 degrees. But since the angle of inclination is small, the ratio of SIN(NSM) / SIN(90) is approximately equal to SIN(DSD') / SIN(inclination). From this relation the inclination can be determined.

Kepler gives only one case of this generalized second method. The Earth was at 226 degrees and Mars was observed at 141 degrees. Thus the latitude of Mars at the heliocentric longitude of 141 degrees will be the same. Once this latitude is corrected for its displacement from the limit, the inclination is again found to be 1 degree 50 minutes.

The third method for determining the inclination of the orbit requires that Mars is at opposition to the Sun. Furthermore, as with the first method, it requires values for: the longitudes of the nodes, the ratio of the sizes of the orbit of Earth and the orbit of Mars, and the eccentricity of the orbit of Mars.

Consider the diagram 5.5. The Sun is at S. The Earth is at E. Mars is in opposition to the Sun at M. M' is the projection of the position of Mars onto the plane of the ecliptic. The longitudes of the nodes of the orbit, N and N', have been determined.

The ratio of SE to SM can be determined from the location of the nodes of the orbit of Mars, the eccentricity of the orbit of Mars, and the ratio of the sizes of the orbits. If the size and eccentricity of the orbit of Mars is known, the heliocentric distance of Mars, SM, is determined. The ratio SE / SM will, by the law of sines, be equal to SIN(SME) / SIN(SEM). But the angle SEM is the supplement of the observed declination of Mars, that is, the supplement of angle MEM'. So the angle SME can be determined. The heliocentric latitude of Mars, the angle ESM is the observed declination angle MEM' minus angle SME. The angle ESM will be equal to the inclination of the orbit of Mars only when the planet is in the limits or N'SM is 90 degrees. The longitude of the planet from the nearest node, angle N'SM, is known. Since the angle of inclination is small, the ratio of SIN(N'SM) / SIN(90) is approximately equal to SIN(ESM) / SIN(inclination). So the inclination of the orbit may be determined.

Kepler gives only one example of this demonstration, which places the value of the inclination at 1 degree 53 minutes. Kepler proposes that the error lies in the assumed ratio of the orbits – a ratio that is corrected in part IV of the *Astronomia nova*.

As a consequence of the variability of the planetary latitudes, Ptolemy ascribed an oscillation to the epicycle of each planet. Ptolemy was led to this conclusion by his failure to perceive, probably due to the relatively small number of observations with which he developed his system, that the entire variability in latitude could be accounted for by the supposition that the planetary epicycle is always parallel to the ecliptic.[164] Since, instead, he supposed that the epicycles all intersect at the Earth, he had to accommodate

[164] See Small, R., *An Account of the Astronomical Discoveries of Kepler*, London: 1804, reprint, Madison: University of Wisconsin Press,1963, p. 174-5.

the appearances by a cyclical variation in their inclinations. Copernicus ascribed the oscillatory motion that Ptolemy had characterized not to the epicycle but to the orbit of the planet itself. Although Copernicus was able to explicate the increases and decreases in the variation of the latitudes as due to the approach and recession of the Earth from the planet, when he found that he could not account for the observations in latitude to the same degree of precision as Ptolemy, he was forced by the presumed immobility of the ecliptic to transfer the motions to the planets' orbits.[165] This situation made the Copernican account of the latitude exceedingly difficult to understand, since each of the planets varied in latitude not according to their positions in their respective orbits, but according to the place of the Earth in the ecliptic, which itself does not oscillate in latitude at all. Having eliminated the annual epicycles in his hypotheses of longitude by allowing that the Earth orbited the Sun, Copernicus' hypotheses of latitude nonetheless retained all the complications of previous astronomy. Brahé, too had been forced to account for the variability, and did so with his improbable notion of the infraction of the orbit.[166] It was Kepler who finally disentangled all of these supposed motions, and eliminated all of the manifold hypotheses used to explain them. He states:

> "The convolutions of Ptolemy's hypotheses forced him to accumulate many monstrosities in the theory of latitudes... Copernicus, ignorant of his own riches, ever took it upon himself to express Ptolemy, not the nature of things, to which, nonetheless, he of all men came closest. (In this regard, see Rheticus' *Narratio prima.*) For although he rejoiced to

[165] See *De revolutionibus*, Book VI, ch. 1.
[166] See Small, R., *An Account of the Astronomical Discoveries of Kepler*, London: 1804, reprint, Madison: University of Wisconsin Press,1963, p. 174

find that when the Earth approaches a planet, the latitudes are in general greater, he still did not dare to reject the remaining Ptolemaic increase in the latitudes... Instead, in the style of Ptolemy, he fabricated librations of the planes of the eccentrics... I always opposed this gratuitous connection of diverse orbs as a cause of motion... But lest anyone deem me untrustworthy on this very account, claiming that I would treat the observations with prejudice, let him now witness that I have most solidly demonstrated the absence of librations in the inclination of the eccentric."[167]

Kepler laments the astronomer who has,

"wasted so many ingenious meditations trying to express, by means of spirals and corollae and helices and volutes and a vast labyrinth of the most intricate curves, a human figment which the nature of things clearly disowns".[168]

At the time of the writing of the *Astronomia nova* Kepler had not only shown the constancy of the orbital inclination for Mars, but had demonstrated the same conclusion for the cases of Venus and Mercury as well. That the planets' orbits should lie in a plane is consistent with the physical cause that Kepler supposed to emanate from the Sun. Since the planets not only travel about the Sun but also are moved by it there is no plausible physical cause that requires that the orbital planes should librate. Moreover, whatever moves Mars in its orbit should not be constrained by the position of the Earth or the centre of its

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[167] Kepler, J., *Astronomia nova*, trans. Donahue, W., *New Astronomy*, Cambridge: Cambridge University Press, 1992, pp. 232-233.
[168] Kepler, J., *Astronomia nova*, trans. Donahue, W., *ibid.*, p. 234.

orbit. That this is true Kepler demonstrated. The Earth orbits within a plane, the ecliptic, so that with the proof that the other planets' orbits have invariable inclinations, the structure of the motions is shown to be general.

In chapter sixteen of the *Astronomia nova* Kepler attempts to develop the first consistent single hypothesis of both the planetary latitudes and longitudes of Mars. He uses an iterative procedure to fit a divided eccentricity theory to longitudes of Mars observed when the planet is in opposition. Consider diagram 5.6. The Sun is at S and Mars is at M. The centre of Mars' eccentric orbit is at C. The equant point is at E, so that the aphelion and point of slowest heliocentric speed is at A, while the perihelion and point of swiftest speed is at P. Kepler does not assume, as Ptolemy had, that the eccentricity of the orbit is half of the eccentricity of the equant point. Rather he determines the ratio of the eccentricities from the data. Where the radius of Mars' eccentric orbit 100,00 parts, Kepler finds that the eccentricity of the eccentric, SC, is 11,332 parts and the eccentricity of the equant, CE, is an additional 7,232 parts. An hypothesis of bisected eccentricity would assume that each eccentricity is equal to half the total eccentricity, or 18,564 / 2 = 9,282. With this non-bisected hypothesis, Kepler found that he could account for the first inequality of Mars, that is, the heliocentric longitudes of Mars in opposition, with a precision of about two minutes of arc.

This hypothesis was a remarkable achievement in itself. Yet in chapters nineteen and twenty of the *Astronomia nova*, two distinct methods are used to demonstrate that this *hypothesis vicaria* of chapter sixteen is false. Triangulations on the observed latitudes of Mars in opposition, *and* triangulations on the observed longitudes of Mars when the planet is not in opposition, are employed to force the *hypothesis vicaria* of chapter sixteen to "betray" itself.

The criterion that both the longitudes and latitudes must be given by a single hypothesis was argued for in the *Apologia*. Kepler's demand for a single hypothesis is explicit in many places in the *Astronomia nova*, but is nowhere so clear as in his rejection of the *hypothesis vicaria* in chapter nineteen. The *hypothesis vicaria* was rejected because Kepler determined that it gives incorrect Sun-Mars distances – as Kepler determined through examinations of its predictions for latitudes in opposition. The observed latitudes of the planet in opposition are clearly shown to be inconsistent with the ratio of eccentricities determined in chapter sixteen – and indicate that the eccentricity should be approximately bisected. As Kepler demonstrates, the *hypothesis vicaria* cannot be modified to give the correct Sun-Mars distances by adjusting the ratio of the eccentricities, without simultaneously destroying the accuracy with which it delivers the heliocentric longitudes - a discrepancy of eight minutes of arc between the observational data and the retrodictions of the hypothesis will necessarily follow. These are the famous eight minutes of arc, which Kepler could not neglect in view of the precision of Brahé's observations, and which "led the way to the reformation of all astronomy".[169]

In chapter twenty, distance triangulations on the observed longitudes of Mars when the planet is not in opposition are similarly found to be inconsistent with the ratio of the eccentricities of the *hypothesis vicaria*.

Chapter sixteen, in combination with chapters nineteen and twenty, effectively show that the eccentricity of the orbit of Mars has one value when

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[169] Kepler, J., *Astronomia nova*, trans. Donahue, W., *New Astronomy*, Cambridge: Cambridge University Press, 1992, p. 286.

it is determined from acronychal observations, about 11,332 parts as in the *hypothesis vicaria*, but a different value, about 9,282 parts, when it is triangulated from other observations. This conundrum shows Kepler that either of the two assumptions upon which the *hypothesis vicaria* was founded, or possibly both, must be false. Either there is no *punctum equans* or equant point for the motion of Mars in its orbit, or the orbit of Mars is not a circle. In fact both assumptions are false.

In chapter twenty-one the epistemology of the *Astronomia nova* is made explicit in a very general discussion of philosophical arguments that are concerned with the truth of astronomical hypotheses. Here, as in the *Mysterium* and the *Apologia*, Kepler addresses the criticism that false hypotheses may yet yield true conclusions. He proposes that truth be identified not just with the accuracy of hypotheses in terms of their ability to save particular appearances, but also with the consistency of those hypotheses amongst themselves. An astronomical hypothesis which, within the accuracy of the observations, accounts for both heliocentric longitude and latitude, is, by such a criterion, more probably true than two separate hypotheses, one for each angular measure. Thus, if an hypothesis which saves heliocentric longitudes over time is found to be inconsistent in also saving latitudes over time or vice versa, it is thereby more probably false than an hypothesis that is consistent in saving both measures. This *kind* of argument forms part of the basis of Kepler's reasoning in rejecting the *hypothesis vicaria* of chapter sixteen of the *Astronomia nova*.[170] For, although this hypothesis gives

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[170] Kepler, J., *Astronomia nova*, trans. Donahue, W., *New Astronomy*, Cambridge: Cambridge University Press, 1992, pp. 281-93, Koyré, A., *The Astronomical Revolution*, p. 179, Small, R., *An Account of the Astronomical Discoveries of Kepler*, London: 1804, reprint, University of Wisconsin Press,

empirically adequate values for heliocentric longitudes, the heliocentric distances (and therefore the eccentricity of the orbit, and therefore the heliocentric latitudes) calculated from the hypothesis are in error relative to those which can be triangulated from observation, and by more than can be attributed to uncertainties in the observations themselves. Conversely, if the division of the eccentricities is assumed so that it is consistent with the triangulated distances, the longitudes will be in error. Such reasoning is directly commensurable with Kepler's discussions in the *Apologia* regarding the use of "the linking of syllogisms" to impugn hypotheses so that they "betray themselves".

If the distances of the planet in its orbit are a criterion by which astronomical hypotheses are claimed to be true, then the *hypothesis vicaria* is false, and yet it produces the heliocentric longitudes to within the uncertainties in the observations. Kepler denies in the *Astronomia nova* that this is an instance of a false hypothesis yielding a true conclusion.[171] Two distinctions are proposed. First, the heliocentric longitudes produced by the *hypothesis vicaria* are not necessarily true; they are only known to be true to within the uncertainties in the observations. Second, the *hypothesis vicaria*, with the eccentricities of the eccentric circle and equant point assumed to correspond to a ratio of 11332 / 7232, is true only as regards its applicability to heliocentric longitudes over time, and not, in fact, as regards distances over time. While Kepler employs the *hypothesis vicaria* to calculate values of heliocentric longitudes throughout the *Astronomia nova*, his objective is an

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Madison: 1963, pp. 189-96, Stephenson, B., *Kepler's Physical Astronomy*, Princeton: Princeton University Press, 1987, pp. 45-7.

[171] Stephenson, B., *Kepler's Physical Astronomy*, Princeton: Princeton University Press, 1987, p. 47.

hypothesis whose truth is not constrained to only heliocentric longitudes, but which also recovers latitudes and distances as well.

It is clear from chapter twenty-one of the *Astronomia nova* that Kepler considers the truth of astronomical hypotheses to obtain in both the empirical accuracy of the hypotheses with respect to their individual observable measures as well as the consistency of such hypotheses with respect to the physical space of angular position and radial displacement. For Kepler both of these components of the truth of astronomical hypotheses are essential. Physical considerations such as those adduced in the *Mysterium* and the *Apologia* figure prominently in the reasoning employed by Kepler in the *Astronomia nova.* In particular, the consideration of the observable planetary longitudes and latitudes and their relations, through various astronomical hypotheses, to physical distances may be regarded as an application of the physical corollaries to geometric hypotheses that Kepler distinguishes in these earlier writings.

Distance determinations allowed Kepler to conclude that in the case of the Earth, the centre of the orbit cannot be also an equant point, and in the case of Mars, that the centre of the orbit could not be where it had been placed in the *hypothesis vicaria.* In both cases the centres of the orbits were shown to be approximately halfway between the Sun and the equant point, that is, that the total eccentricity is approximately bisected by the centre of the orbit. With these conclusions, as approximate as they were, Kepler *assumed* that the bisection of the eccentricity is exact. The assumption of bisected eccentricity permitted him to reintroduce the physical cause for the planets' orbital motions that he had conjectured in the *Mysterium cosmographicum.* With the assumption of bisection, the planets' speeds in their aphelia and perihelia are made to be inversely proportional to the distance of the planet from the Sun. This accords with the 'distance-delay' principle of chapter twenty-two of the *Mysterium.* Kepler's aim in reintroducing this principle in the *Astronomia nova* was to show that not only are the planets' motions consonant with it, but that the motion produced by the assumption of an equant point at the same eccentricity as the orbit *could be explained in physical terms.* For at the equant point itself there is no physical body, and therefore no source for a cause of planetary motions.

Having shown that the geometry of the planets' motions could be consistently described in truly heliocentric terms, Kepler gave physical or causal reasons for the component motions that he identified as true motions and not merely the effects of perspective - in the case of the first inequalities of the planets, a motive force produced by and emanating from the body of the Sun. The existence of such an emanation he had conjectured in the *Mysterium cosmographicum*, but it is in the early chapters of the *Astronomia nova* that Kepler presents compelling evidence, in terms of consistent physical hypotheses for the orbits of Mars and the Earth, for its truth.

When Kepler initially argued, in the *Mysterium*, that the motions of the planets should be referred to the body of the Sun, he could do no more than show that such referral was not inconsistent with the observations. In the *Mysterium* he referred the eccentricities of the planets to the body of the Sun, and the Copernican observations were saved by this hypothesis to roughly the same degree of precision as referring the eccentricities to the mean Sun. But once in possession of the observations of Brahé, Kepler was able to confirm his supposition more strictly. He wrote to Herwart of his studies of Mars in 1600:

> "Since [in the *Mysterium*] I had referred the eccentricities of all the planets to the body of the Sun, I greatly feared that Tycho, like Copernicus, would refer it to the mid-point of the Sun's [or equivalently the Earth's] orbit. But Mars constantly rejected all points other than the centre of the Sun".[172]

Kepler proposed again in chapter six of the *Astronomia nova* that the eccentric of Mars should be referred to the body of the Sun. In fact, however, decisive empirical evidence for the physical centrality of the body of the Sun in Mars' orbit, that is, including determinations of the heliocentric distances and the eccentric equations or the heliocentric angular positions, was not presented until chapters fifty-one and fifty-two of the *Astronomia nova.* Until these later chapters the planetary distances were not explicitly examined with reference to the question of the centrality of the Sun. Before these chapters, Kepler uses the computed planetary distances almost exclusively to test various hypotheses for the correct production of eccentric equations of the planet. Determinations of distances and determinations of eccentric equations are used, in the *Astronomia nova*, as complementary methods, each designed to leverage the empirical advantages of the other, a fact that many accounts of Kepler's derivation of the final elliptical orbit have failed to emphasize.[173] This methodological strategy clearly owes much to the epistemology of the *Apologia*.

As Kepler was well aware, distance determinations of a planet are sensitive to errors in the measured angles used to compute them. Moreover, these determinations are necessarily dependent on a sufficiently precise

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[172] Quoted from Kozhamthadam, J., *The Discovery of Kepler's Laws*, Notre Dame: University of Notre Dame Press, 1994, p. 147.
[173] Wilson, C., "Kepler's Derivation of the Elliptical Path", Isis 59, 1968, pp. 5-25.

hypothesis of the orbit of the Earth. Because of these combined uncertainties, Kepler could not simply derive the orbit of Mars by triangulating the planet's position at various places in its orbit. In practice, the task is much subtler than most accounts of the *Astronomia nova* have allowed. But given the quality of Brahé's data, distance determinations were sufficiently precise to show Kepler that several of the hypotheses that he considered could not be true. Determinations of Mars' distances were precise enough to prove that the eccentricity of the orbit of Mars in the *hypothesis vicaria* of chapter sixteen was false, notwithstanding the fact that this hypothesis produced very precise heliocentric longitudes, and distance triangulations on the Earth were enough to show, in subsequent chapters, that it could not move uniformly on an eccentric orbit.[174]

Kepler's ultimate objective in determining the orbit of Mars was to find a physically plausible hypothesis from which could be derived both correct equations and correct distances. But planetary distances must be inferred from equations, plus assumptions about the form or shape of the orbit. Therefore, Kepler needed to be confident that both the heliocentric longitudes and latitudes of the planet had been precisely accounted for by his hypotheses in order to claim to have established the true heliocentric distances. Only after this was done could he proceed with determinations of the orbit based on the distances themselves.

These later distance determinations, based as they were on a single orbital hypothesis that could precisely generate both the latitudes and longitudes of the planet, were then justifiably deployed by Kepler as evidential support for the physical centrality of the Sun. In 1604, after much of the work on the *Astronomia nova* had been accomplished, Kepler wrote to Fabricius,

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[174] *Astronomia nova*, chapters 22 to 31.

> "From the line of apogee of the Sun nothing remains in excess in the eccentric of Mars if you refer it to the Sun itself. But if you refer it to the point or position of the mean Sun, something does indeed remain in excess. It is not large as far as the locations are concerned, but it is greater for distances. And I have included this among the reasons why I refer this theory to the true centre of the Sun."[175]

Here Kepler is claiming that when the heliocentric orbit of Mars is abstracted from the observations the use of the true Sun rather than the mean Sun will leave no reference to the Earth's orbit in that of Mars. Thus, while a Martian hypothesis based on the mean Sun may save the inequalities with only small errors, the distances are not given symmetrically, but instead depend on the apsides of the orbit of the Earth.

In chapter fifty-one Kepler establishes symmetrical placements of corresponding Sun-Mars distances on either half of the orbit, when the orbit is referred to the body of the Sun. In chapter fifty-two he provides a general geometrical argument to show that the symmetry of the distances described in the previous chapter entails that the eccentric of the planet must be referred to the body of the Sun, and not to the mean Sun. He concludes chapter fifty-two stating:

> "So the faith that was pledged in chapter six and in many other places in this work, I have redeemed from all tincture of self-justification, and I have shown that the eccentric of Mars cannot be referred to anything

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[175] Quoted from Kozhamthadam, J., *The Discovery of Kepler's Laws*, Notre Dame: University of Notre Dame Press, 1994, p. 147.

but the Sun itself; and that, in addition, it is not only reason that stands with me, but also the observations themselves, in my releasing the observations of Mars from the Sun's mean motion and measuring them out by the apparent motion of the Sun."[176]

It is clear that while Kepler was intent on showing that the precise observations of Brahé would support his truly heliocentric theory, he was convinced of its truth by 'reason' before sufficient empirical evidence could decide the case. Even in the *Mysterium* Kepler had encountered empirical difficulties that suggested difficult unsolved problems for a truly heliocentric theory. In discussing the variation in orbital speeds that he proposed was related to the changing heliocentric distances of the planets, he states:

"...in the cases of Venus and Mercury this slowness and quickness fits in, not with the planet's distance from the Sun, but only with the Earth's motion. And if anyone elaborates this question with a law of their motion different from that of the superior planets, what explanation will he eventually put forward for the annual motion of the Earth? For it did not need an equant either in Ptolemy's theory or in Copernicus'"[177]

Significantly, when Kepler added author's notes to the *Mysterium* for its second printing in 1621, he remarked on this passage in light of the

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[176] Kepler, J., *Astronomia nova*, trans. Donahue, W., *New Astronomy*, Cambridge: Cambridge University Press, 1992, 528.
[177] Kepler, J., *Mysterium cosmographicum*, p. 85, trans. Duncan, A. M., *Mysterium Cosmographicum (The Secret of the Universe)*, New York: Abaris Books, 1981, p. 219.

innovations of the *Astronomia nova*. These included the introduction of an equant into the hypothesis of the orbit of the Earth, of which Kepler wrote,

> "...in my *Commentaries on Mars* I have made this one of the chief features of the book, and I have laid it like a cornerstone at the foundation. Indeed, I deservedly called the key to astronomy the fact, which I have demonstrated clearly from the actual motions of Mars, that the annual motion either of the Sun or of the Earth is controlled by a different centre from the equant, and the eccentricity of its orbit is only half the eccentricity believed by the authorities"[178]

Kepler is here referring to the demonstrations of chapters twenty-two through thirty-one of the *Astronomia Nova*. In these chapters convincing evidence is given for the non-uniformity of the motion of the Earth on its orbit. This, to Kepler's great satisfaction, not only makes the Earth's motion the same in kind as that of all the other planets, but because of this universality, provides strong confirmation of the physical cause for the planets' motions that Kepler had supposed to emanate from the Sun.

Kepler concluded his addenda to chapter twenty-two of the *Mysterium* with a general description of the provenance of the book:

> "You see, then, assiduous reader, that in this book there were scattered seeds of each and every one of the things which since that time in this

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[178] Kepler, J., *Mysterium cosmographicum*, p. 85, trans. Duncan, A. M., *Mysterium Cosmographicum (The Secret of the Universe)*, New York: Abaris Books, 1981, p. 219.

new and, to the masses, absurd astronomy I have established and demonstrated from the thoroughly exact observations of Brahé..."[179]

Such seeds of reason, when they reached their flourishing in the *Astronomia nova* effectively declared the downfall of ancient astronomy. They collectively showed that the kinetic structure of the world system is surprisingly simple in form and, furthermore, that such simplicity of form is commensurate with the causal efficacy of a physically central Sun.[180]

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[179] Kepler, J., *Mysterium cosmographicum*, p. 85-86, trans. Duncan, A. M., *ibid.*, p. 219-220.

[180] A precise and thorough explication of the mechanics of the Keplerian causal account may be found in: Davis, A. E. L., "Kepler's Physical Framework for Planetary Motion", *Centaurus*, 35, 1992, pp. 97-191.

Chapter 6

Conclusion

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The claim is often made that the most salient feature distinguishing the astronomy of the renaissance from modern astronomical science is the introduction of the telescope.[181] It certainly cannot be denied that the new instrument increased by orders of magnitude the precision with which data might be gathered from the heavens. All of Kepler's important discoveries, however, were pre-telescopic. They nonetheless involved a revolution in the empirical corroboration of hypotheses, but this revolution was founded on the numerous and accurate observations of Brahé. While the high precision of Brahé's observational record was certainly necessary for Kepler's eventual success in the formation of his physical astronomy, Kepler had to reinterpret all of this data as a consequence of the supposition that the true Sun should provide the point of reference for all the planets' motions. With this supposition he redefined the very meanings of many astronomical measures involving the planets. The transformation to the true Sun necessitated small but significant changes in the measures of the first and second inequalities, the times and positions at which oppositions were supposed to obtain, and the various mathematical reductions of the planets' positions in their orbits to eccentric equations.

Kepler's was not the kind of philosophical conversion that followed from the overwhelming visual evidence contrary to the Aristotelian world-view that Galileo presented in his *Siderius nuncius*, but rather a theoretical

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[181] See, for example, van Helden, *Measuring the Universe*, Chicago: University of Chicago Press, 1986, chapter 1, pp. 1-4.

conversion that saw the evidence and arguments of previous science in a radically new way. It might be argued that many of the changes that Kepler made were in some sense an inevitable consequence of Copernicus' *De revolutionibus*. But this belittles the fact that almost no one but Kepler understood the profound philosophical implications of his changes to Copernican heliocentrism. In addition, it would appear that until at least Kepler's time the cosmological implications of the Copernican system were not commonly considered nearly so important as the mathematical facility of heliocentric hypotheses. In the decades following Copernicus' works, his mathematical conclusions were introduced into the curricula of many European universities, principally through the *Tabulae Prutenicae* of Reinhold.[182] Influential astronomers such as Philipp Melanchthon (1497-1560) and Caspar Peucer (1525-1602) undertook significant studies of the equant-free hypotheses of *De revolutionibus*. But Copernicus' cosmological theory received almost no attention, or was simply rejected as absurd.[183] This widespread view of the Copernican works was also advanced by Magini, who, although he gave credit to their author as a great astronomer, could only concur with everything claimed therein with the proviso that the hypothesis of the motion of the Earth was to be ignored.[184] Some astronomers, most notably Brahé and Johannes Praetorius (1537-1616), sought to exploit the

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[182] See Gingerich, O., "The Role of Erasmus Reinhold and the Prutenic Tables in the Dissemination of Copernican Theory", *Studia Copernicana*, VI, *Colloquia Copernicana*, II, 1973, pp. 43-62.
[183] See Westman, R., "The Melanchthon Circle, Rheticus, and the Wittenberg Interpretation of the Copernican Theory", *Isis*, 66, 1975, pp. 165-193.
[184] See Rosen, E., *Three Imperial Mathematicians: Kepler Trapped between Tycho Brahe and Ursus*, New York, Abaris Books, 1986, pp. 98-100.

advantages of referring all of the planets' first inequalities to the (mean) Sun, but refused to admit the mobility of the Earth in their respective theories.[185]

In short, there was no real 'crisis' in astronomy when Kepler wrote the works that have been here discussed. Copernican astronomy, at least in its mathematical form, was without great upheaval, integrated into the predominately Ptolemaic corpus of astronomical science. Although by 1600 there were several challenges to the authority of Ptolemy in the form of other geocentric or geo-heliocentric systems of hypotheses, in the minds of an overwhelming majority of astronomers who did not maintain a purely sceptical stance, the Earth was still solidly fixed at the centre of the cosmos. Thus, Kepler's perceptive insight into the cosmological innovations that might be founded on the advances of Copernican heliocentrism was singular and of astonishing genius.

Several influential philosophers have, I suggest, misplaced the more revolutionary aspects of heliocentrism with Copernicus. Kuhn, for example, while acknowledging that the effect of *De revolutionibus* was not immediate or universal, states in *The Copernican Revolution* that, "every man who used the *Prutenic Tables* was at least acquiescing in an implicit Copernicanism".[186] Here Kuhn implies that astronomers who employed Reinhold's work were thereby committed to the plausibility of the Copernican cosmology. But, aside from the fact that Reinhold himself described Copernicus most often as an eminent 'calculator', the use of the *Tabulae Prutenicae* in no way obliged the astronomer to seriously consider those features of Copernicus' theory that could be said to make it paradigmatically different from Ptolemy's theory. In

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[185] See Schofield, C., *Tychonic and Semi-Tychonic World Systems*, New York, Arno Books, 1981.

[186] Kuhn, T., *The Copernican Revolution*, Cambridge, Mass.: Harvard University Press, 1957, p. 188.

The Structure of Scientific Revolutions Kuhn claims that, "Copernicus' innovation was not simply to move the Earth. Rather, it was a whole new way of regarding the problems of physics and astronomy, one that necessarily changed the meaning of both 'Earth' and 'motion'".[187] Without wishing to detract from Copernicus' historical influence or from the fact that he was the first astronomer to give heliocentrism a serious mathematical treatment, Kuhn's claim seems indefensible in referring only to Copernicus.[188] Copernican heliocentrism undoubtedly was the ultimate source for the innovations that Kuhn mentions, but these innovations must surely be considered the work of Kepler, among others, as well as Copernicus.

In *Against Method* Feyerabend claims that acceptance of the Copernican system in the time of Galileo was not supported by evidence and arguments, but "*by irrational means* such as propaganda, emotion, *ad hoc* hypotheses, and appeal to prejudices of all kinds", and that belief in the truth of heliocentrism in this time was tantamount to "blind faith".[189] It has been widely recognized that, in their initial form, the Copernican heliocentric hypotheses were not clearly better as predictive astronomical devices than the Ptolemaic hypotheses. Yet even before the publication of *De revolutionibus*, Rheticus had offered in his conversion narrative cogent non-empirical arguments for the new system. In view of Kepler's *Mysterium* and especially the *Astronomia nova*, it becomes difficult to admit Feyerabend's assertions without qualification. While these works of Kepler's may not have had the large

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[187] Kuhn, T., *The Structure of Scientific Revolutions*, 2ND ed., Chicago: Chicago University Press, 1970, pp. 149-150.
[188] Westman has made these criticisms of Kuhn's account. See Westman, R., "The Wittenberg Interpretation of the Copernican Theory", in *The Nature of Scientific Discovery*, Washington: Smithsonian Institution Press, 1975, pp. 393-429.

audience that Galileo addressed, they at the very least show that a rational and non-*ad hoc* defence of heliocentrism was then possible, even if it was not widely acknowledged. This claim does not negate that there are many non-empirical factors to be considered when examining the history of astronomy. Indeed, Kepler's works provide fine examples of the relevance in theory change of rhetorical style, appeals to the historical record, arguments from simplicity, and a realist interpretation of hypotheses. But it is possible to admit these factors without simultaneously voiding the evidential components of the same works.

Among others, Kuhn and Feyerabend have emphasized the discontinuities between renaissance astronomy and that of the 'post-revolutionary' modern era. However, one of the most striking features of Keplerian science is Kepler's indebtedness to previous theoreticians. As the *Apologia* in particular makes obvious, Kepler was an accomplished historian of astronomy as well as a practitioner. He was clearly inspired by the extraction of heliocentric motions from the appearances in *De revolutionibus*, excited by the physical possibilities of the Ptolemaic equant, and he clearly acknowledged the rhetorical burden of the *Narratio prima*.

Kepler was keenly aware of the arbitrary and conventional elements of ancient and renaissance astronomy. It was this awareness that allowed him to see that the eccentricities of the Earth and other planets were confabulated in previous theories – that changes in the planets' positions that Ptolemy and even Brahé and Copernicus had attributed to the planets themselves could be consistently explained by positing the correct physical motion of the Earth within a physically heliocentric cosmos. Without these insights into the

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[189] Feyerabend, P., *Against Method*, reprint: London, Verso, 1978, pp. 153-154, emphasis original.

fundamental possibilities that physical heliocentrism held for science, the telescope would still have done what it did for Galileo and others who found the evidence for their claims in the actual appearances of the heavens, but it would have been of little relevance to astrophysics, a science that seeks the reasons for the appearances.

Kepler saw through the veil of appearances perhaps more clearly than any single individual before or since. It is not without reason that Immanuel Kant referred to Kepler as "the most acute thinker ever born".[190]

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[190] See Wasianski, E., *Immanuel Kant in seinen letzen Lebensjahren*, Königsberg: 1804, p. 97. Quoted from Baumgardt, C., *Johannes Kepler, Life and Letters*, New York: Philosophical Library Inc., 1951, p. 17.

Diagram 3.1a - The Copernican System of Hypotheses

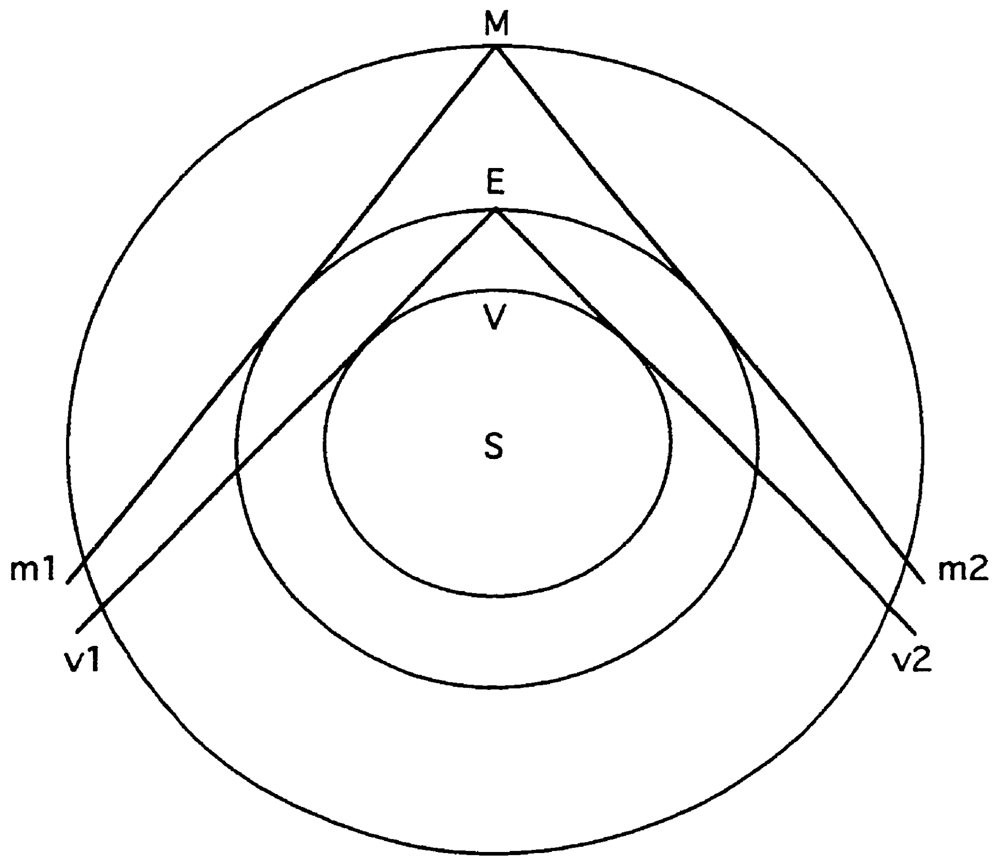


Diagram 3.1b - The Ptolemaic System of Hypotheses

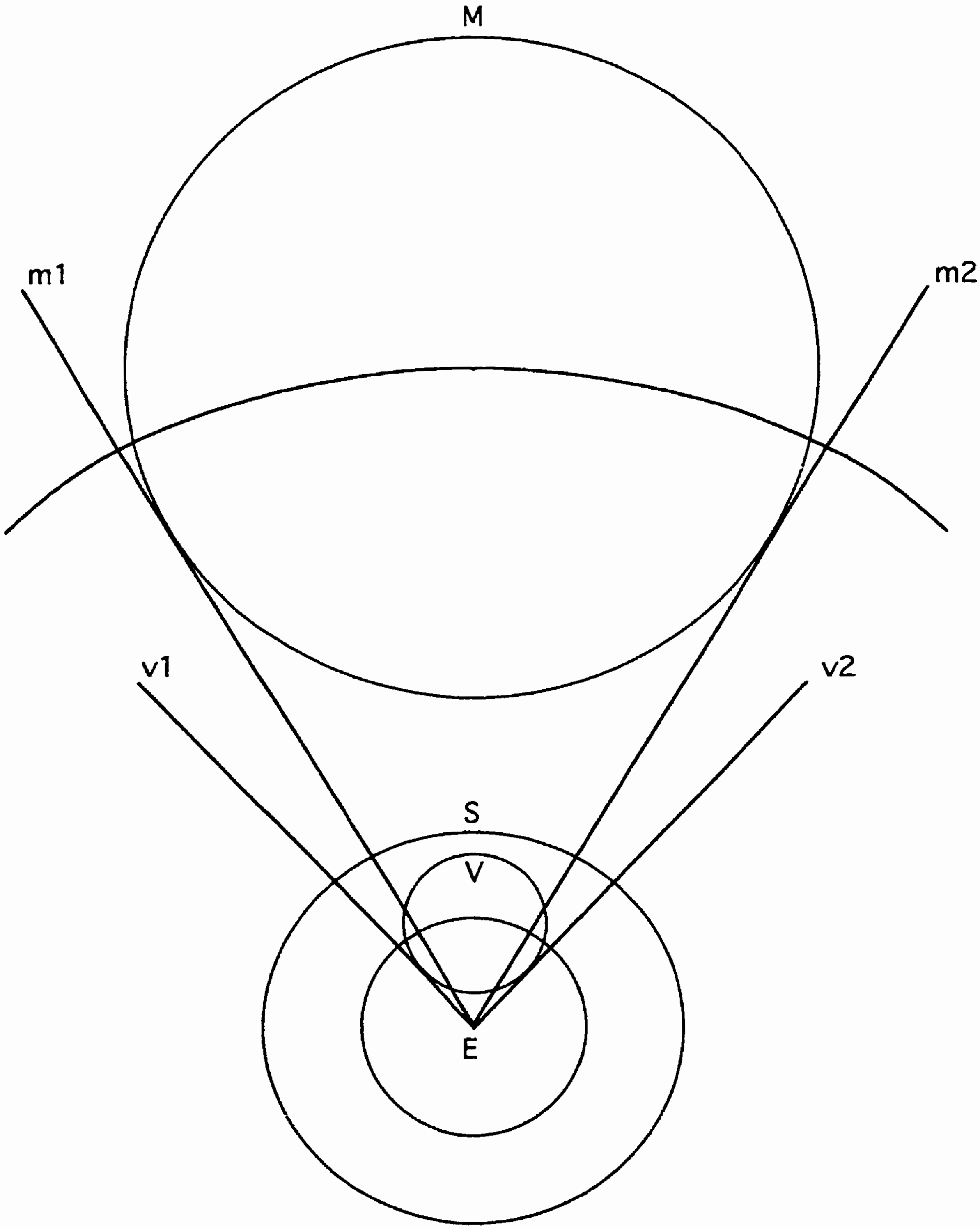


Diagram 3.2 - Hypothesis of Eccentric Orbit with Equant

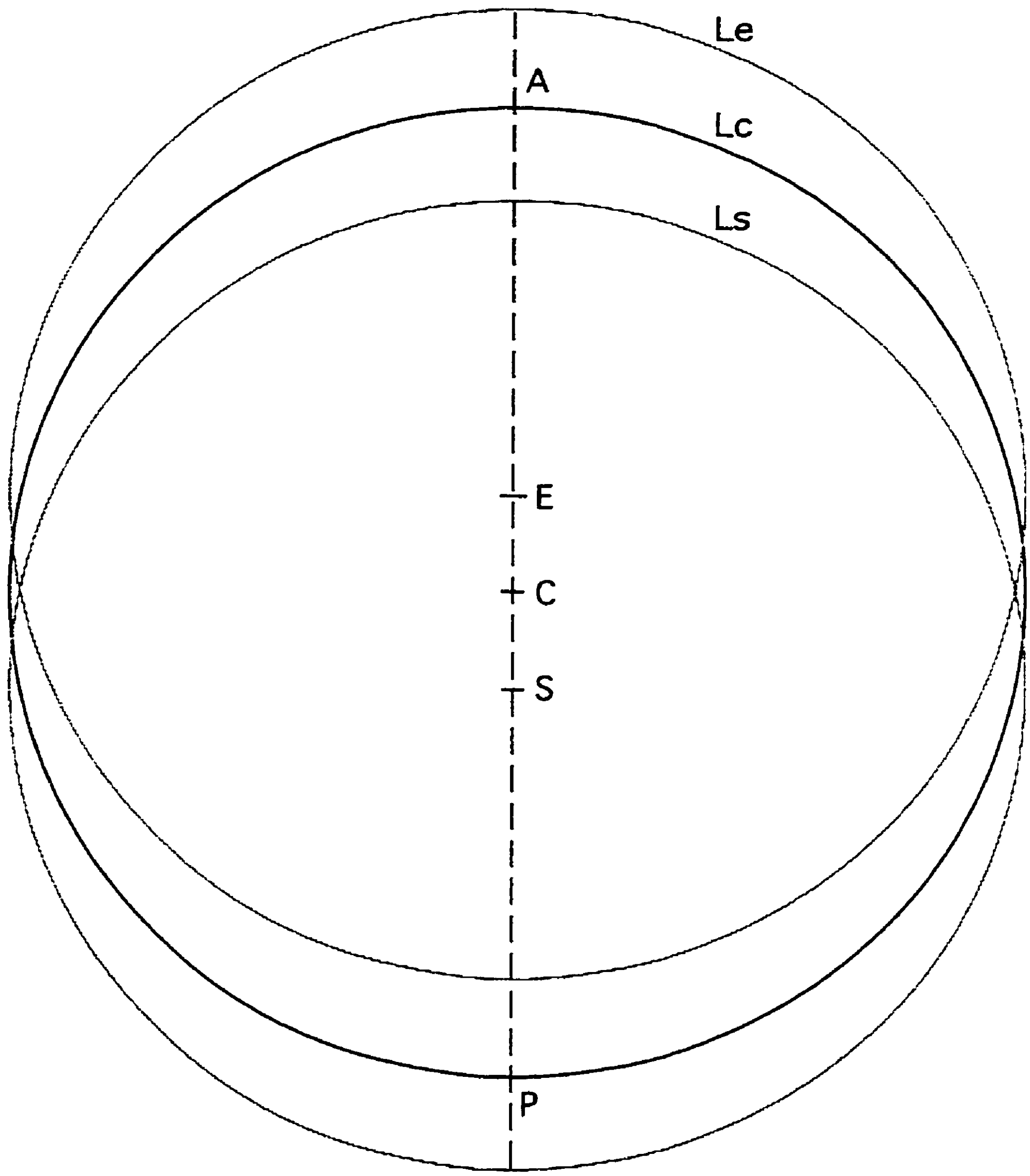


Diagram 5.1 - Inclined Planar Orbits

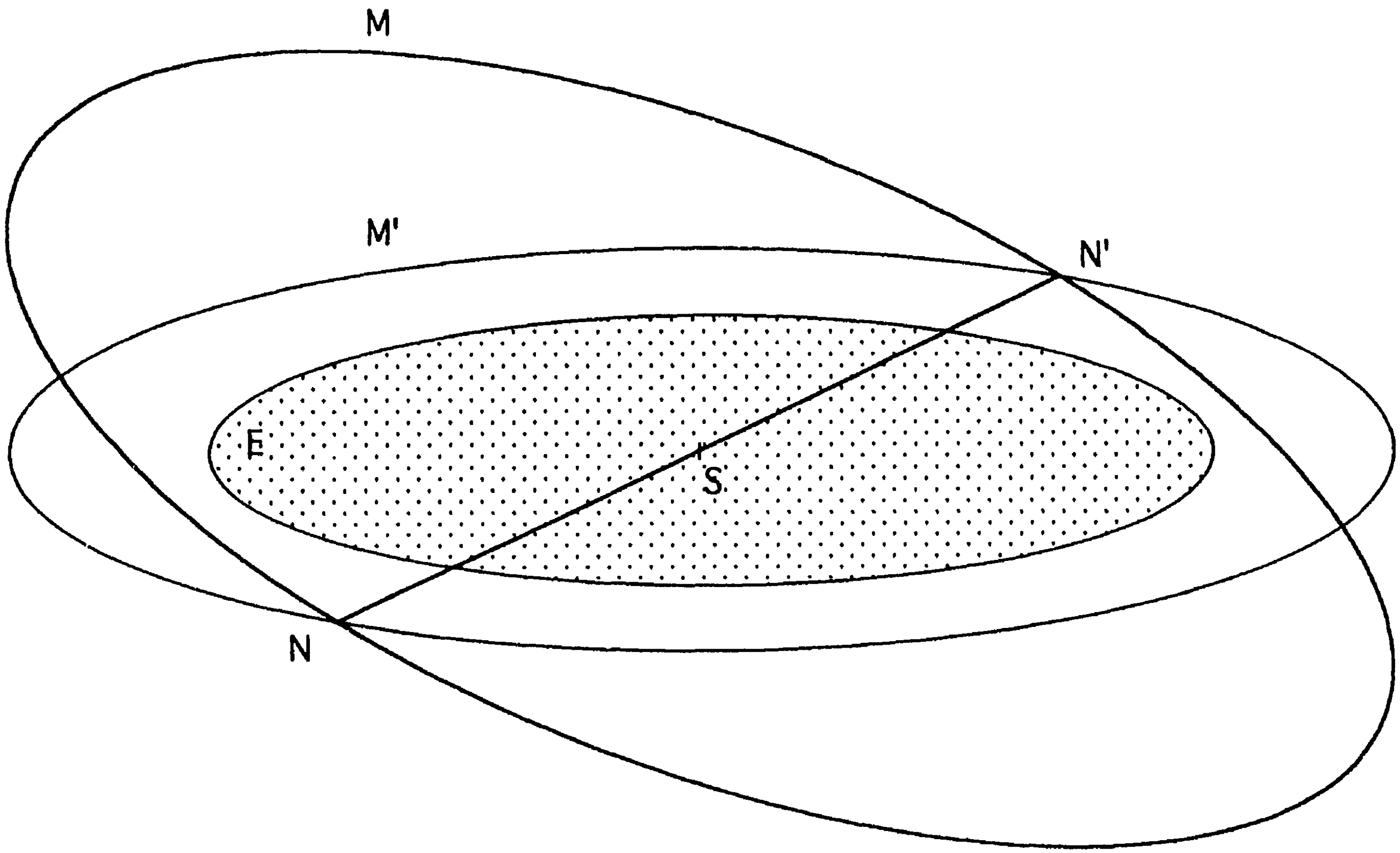


Diagram 5.2 - First Method for Determination of Orbital Inclination

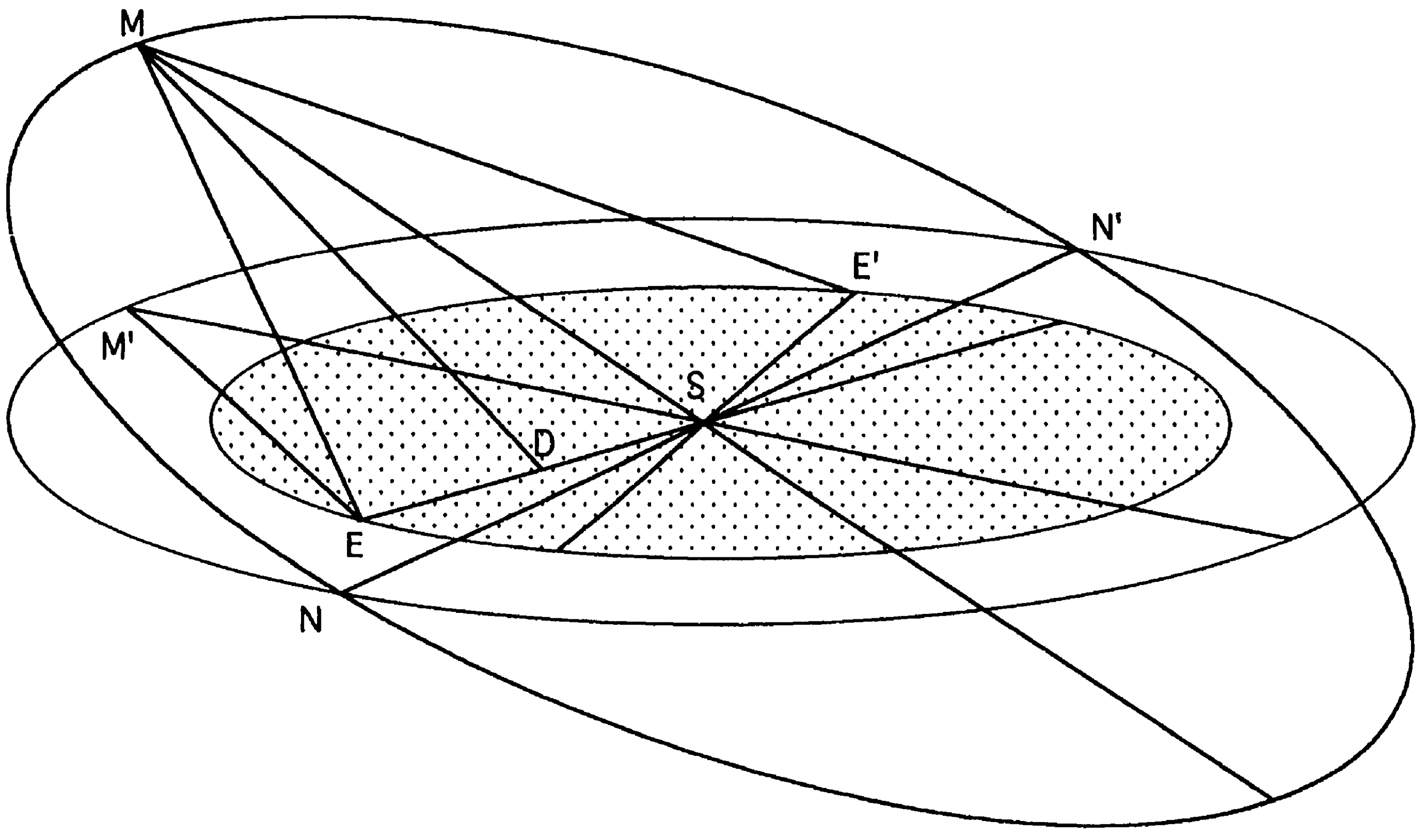


Diagram 5.3 - Second Method for Determination of Orbital Inclination

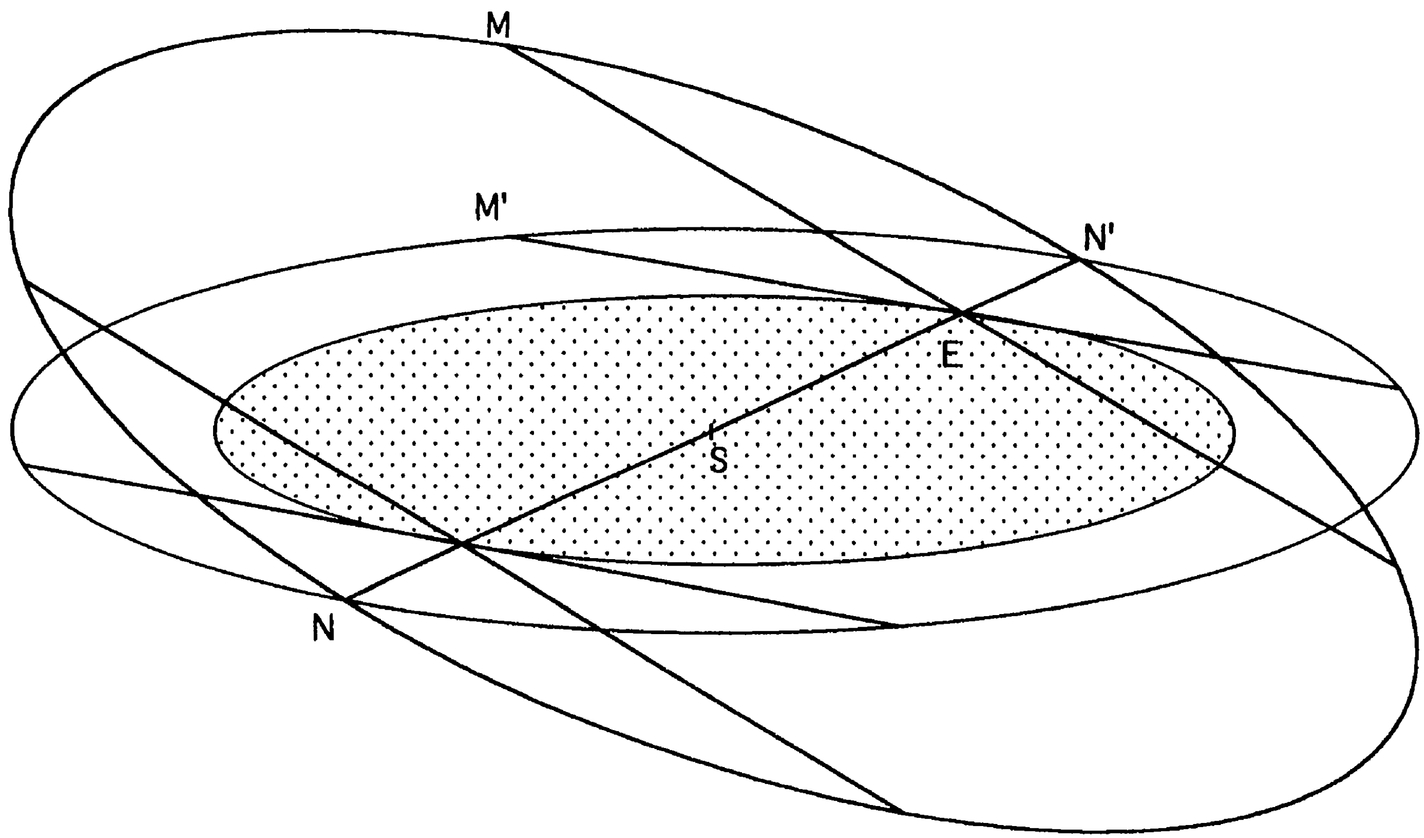


Diagram 5.4 - Second Method for Determination of Orbital Inclination, Generalized

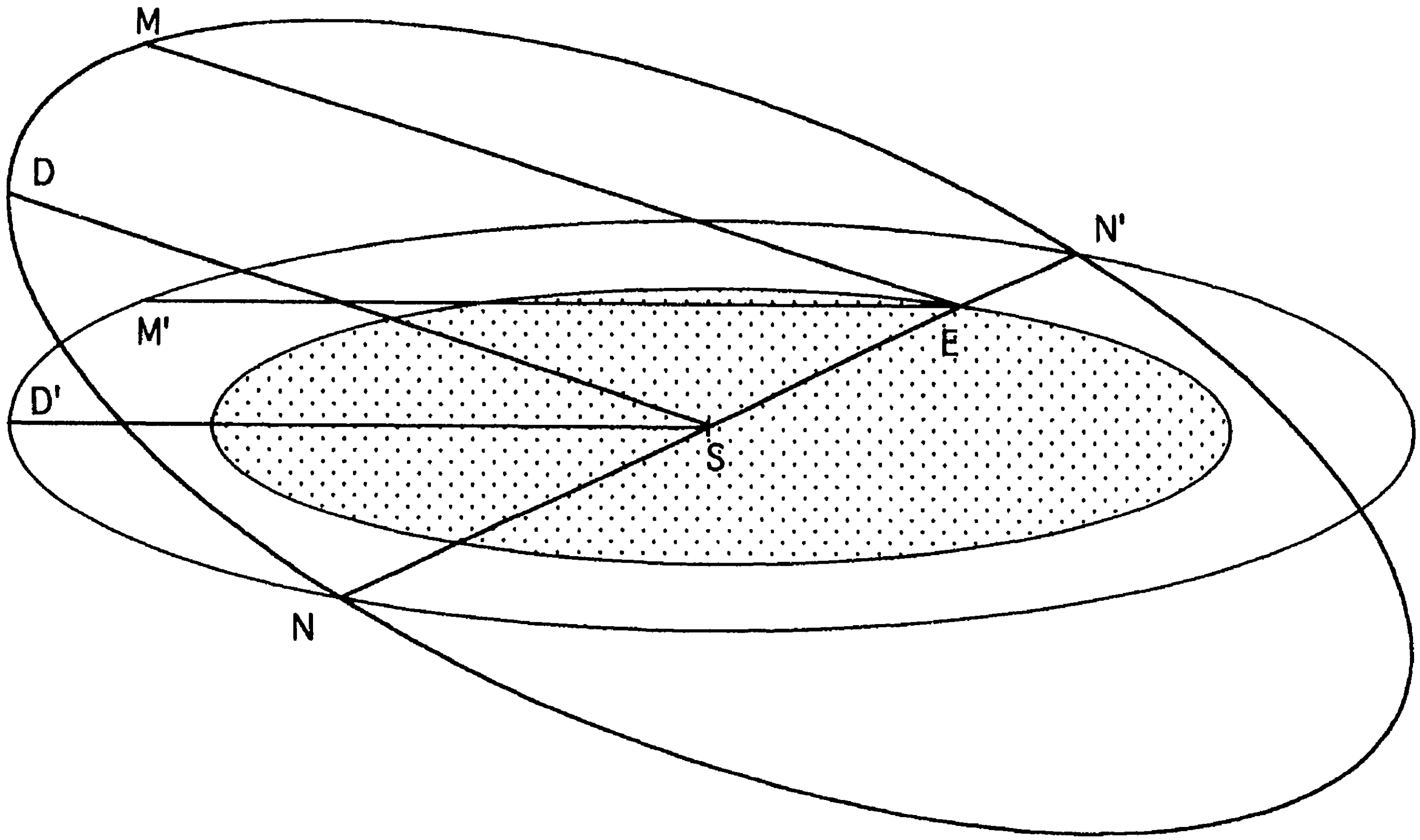


Diagram 5.5 - Third Method for Determination of Orbital Inclination

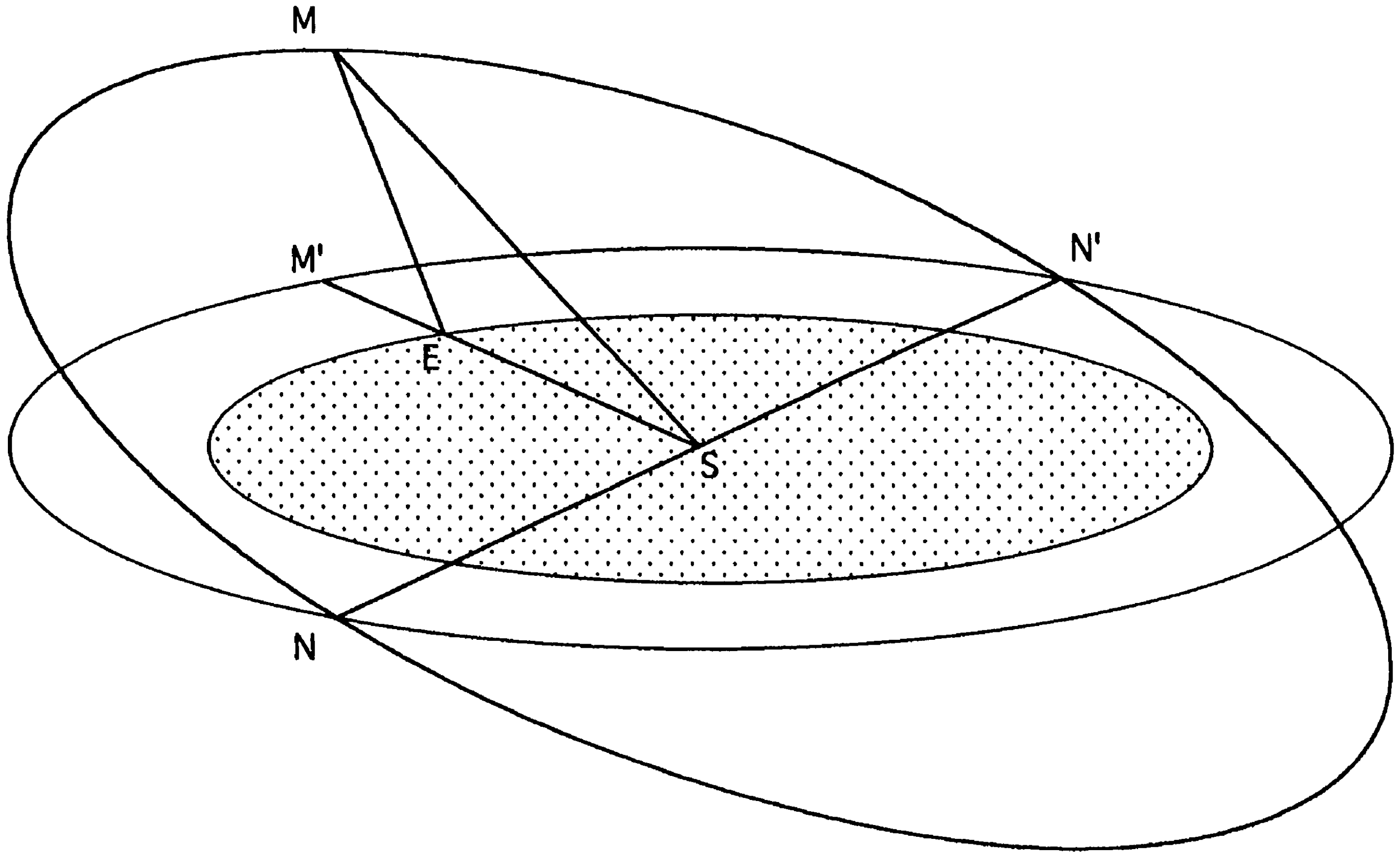
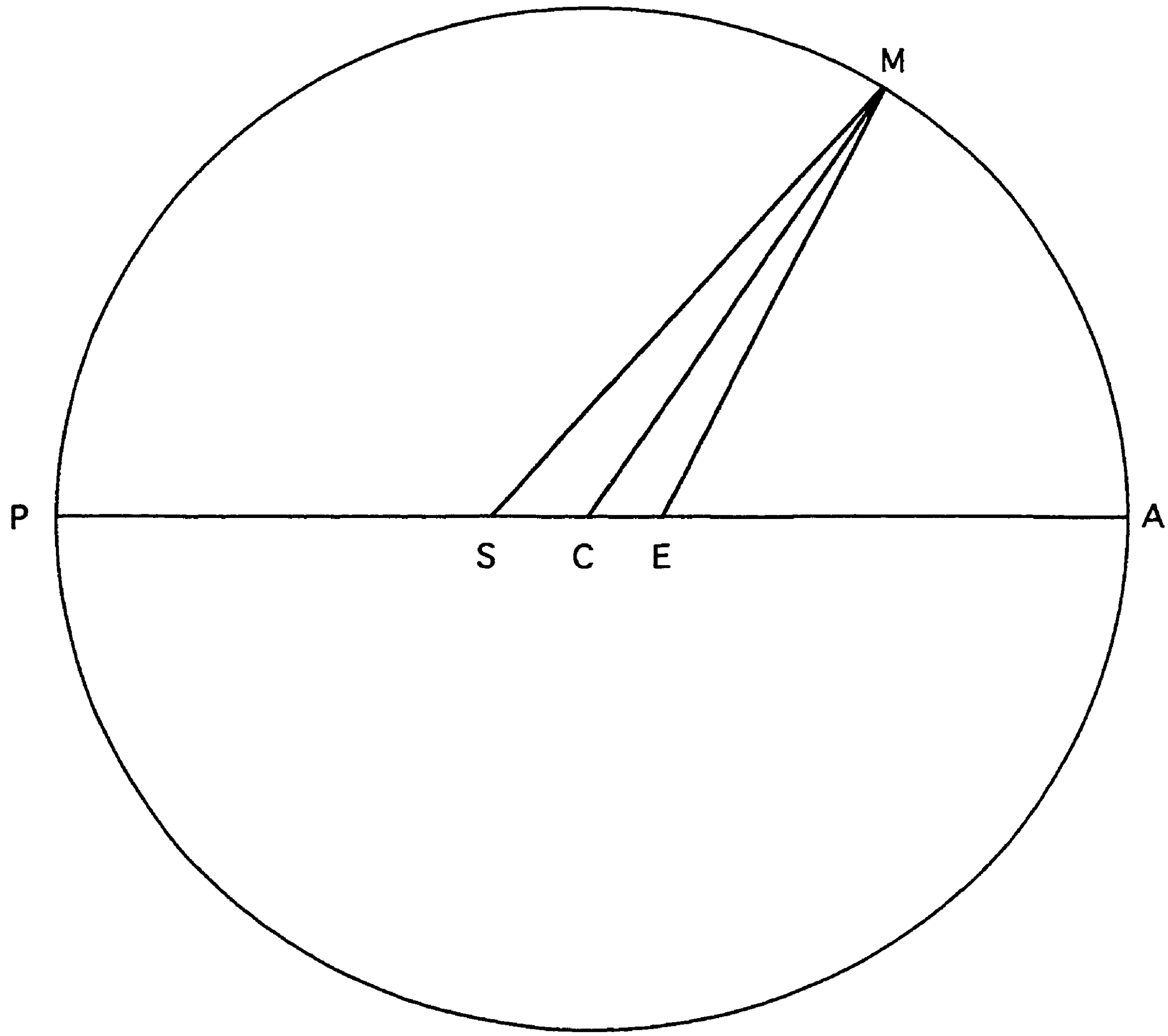


Diagram 5.6 - The *Hypothesis Vicaria*



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