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KEPLERIAN ASTRONOMY AFTER KEPLER: RESEARCHES AND PROBLEMS

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INTRODUCTION

Until recently, a reader dipping into any number of works in the history of science, or even of astronomy, might be pardoned for failing to realize that nearly eight decades of one of the richest eras in the history of astronomy separate Kepler's *Astronomia nova* of 1609 from Newton's *Principia* of 1687. Many such histories, after presenting Kepler's achievement in the form of three "laws" of planetary motion, say little more about them until they are presented as one of the foundations upon which Newton erected his grand "synthesis".[1] Several decades ago, however, Jean Pelseneer commented on the "troubling mystery" of why Kepler's "laws" were ignored or rejected until Newton began to address the problem of planetary orbital paths, and suggested it as an important subject for research.[2] The lack of apparent response to Kepler by his contemporaries and immediate successors was remarked in the eighteenth century and has been noted frequently ever since. According to a popular history of cosmology, "Not the least achievement of Newton was to spot the Three Laws in Kepler's writings, hidden away as they were like forget-me-nots in a tropical flowerbed".[3] To explain this mystery, historians ascribed to Kepler's readers a distaste for his mysticism, prolixity, stylistic clumsiness, intricate and mistaken calculations, emphasis on physical causality, peculiar insistence on discarding the traditional circles and uniform motion, and his geographical remoteness from the centres of scientific activity. Much of this was speculation, sometimes lightly buttressed by arbitrarily selected illustrations.[4]

The very singling out of Kepler's "laws" from among his multi-faceted encounter with astronomy betrays the presentist character of this perception of an interesting historical problem: the reception of Kepler's astronomical ideas.[5] Although the term 'law' was already being used in the seventeenth century to characterize fundamental principles of natural philosophy, it seems to have been applied to certain of Kepler's rules governing planetary motion only at a later date, beginning in the eighteenth century.[6] The positivist strain in the historiography of science, in which those theories, ideas, or discoveries which became incorporated into the more or less modern views of nature are emphasized in historical investigation, is frequently joined to the assumption that the truth once discovered is eternal and manifest — or ought to be; one may therefore be rightly puzzled by the failure of Kepler's contemporaries to see that Kepler's laws are true.

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Epistemological and historiographical issues are here closely intertwined. The focus on the reception of his laws tends to obscure Kepler's transformation of his discipline and of the new sorts of questions astronomers were being compelled to ask as a result of having received the fruit of his labours.

Three of Kepler's insights nevertheless became the focus for an historical inquiry that has tended to retain its limited character to the present day. The reasons for this are not difficult to find. Certain of Kepler's discoveries concerning the movements of the planets came to be called laws some time after Newton's mathematical demonstration that they were both consequences of certain physico-mathematical assumptions and that as hypotheses they entailed those assumptions. Newton's towering achievement and the special role played in it by Kepler's area rule, ellipse and the harmonic rule (the third law, relating planetary periods and distances from the Sun for all the planets) have overly influenced the direction of research in post-Keplerian astronomy.

In the past three decades the history of these three of Kepler's discoveries has been pursued in some depth, and our knowledge of how his astronomical innovations were received by his contemporaries and successors has been broadened. We have a somewhat better view of who was reading his works and what they thought and said about his ideas. A significant stimulus to this clearer picture has been provided by the large volume of recent research on Copernicus, Kepler and Newton, in part stimulated by anniversary celebrations and the thriving Newton industry. This scholarship has resulted in a new perception of Kepler's laws, research on which has moved backwards in time from Newton on the one hand, and forward from a closer examination of Kepler's process of discovery on the other.[7] It is now apparent that Kepler was more widely read than had been generally believed, and the scope of the Kepler-problem has changed to include an examination of how astronomers reacted to the novelty of Kepler's astronomical procedures and their effectiveness in 'saving the appearances'. The area rule not only violated traditional conceptions of the nature of astronomy, but was also difficult to apply in practice. Although some work has been done on the reception of Kepler's physical mechanism for planetary motions, the emphasis has continued to be on what are now referred to as Kepler's laws.

The development of a broader conception of the nature and meaning of Keplerian astronomy might begin with an inquiry into the uniquely Keplerian component of the turning-point in astronomy initiated by Copernicus. How does Keplerian astronomy differ from and resemble Copernican? Kepler was the leading champion of the Copernican theory until 1610 and, for two decades thereafter, the chief exponent of Copernicanism among technically competent astronomers. Kepler did not merely accept Copernicus's central propositions — a heliostatic *system*, a moving Earth and its physical implications; he modified them so as to render the Copernican system a much more effective vehicle for the astronomical and cosmological principles it embodied. His realism was of a more thoroughgoing kind than Copernicus's, employing the real Sun for calculation and as the cause of planetary motion. His goal of tracing the actual paths over which the planets travelled by postulating a link between speed in orbit and distance from the Sun was ultimately successful owing both to his insistence on seeking the physical causes underlying the motions of the planets and his use of Tycho Brahe's precise data, leading to his eventual abandonment of the ancient axiom of uniform circular motion.

Among Kepler's modifications of the Copernican system, constituting a distinctly Keplerian version of Copernicanism, were the (1) elimination of Copernicus's third motion, a conical rotation of the Earth's axis, (2) use of the true Sun instead of the Mean Sun in the calculation of conjunctions and oppositions, (3) intersection of all orbital planes in the Sun, (4) constant inclination of the orbital planes, replacing Copernicus's variable inclinations, (5) elimination of epicycles, (6) reintroduction of the equant (a point not at the centre of the line of apsides, from which a radius vector generated equal angles in equal times) and (7) provision of a physical role for the Sun, as the cause of non-uniform planetary motion. There has been as yet no general analysis of the role of these and other Keplerian innovations in the acceptance of the Copernican theory.

Kepler's physical theories, although speculative, were intimately involved in the technical features described above. As aspects of Kepler's realism and his effort to end the bimillennial bifurcation in the science of the heavens between astronomy and natural philosophy, they led to the first two of his laws and the transformation of astronomy in the seventeenth century.[8]

KEPLER "IGNORED"

The historians who asserted that Kepler was ignored before Newton were possibly led to this conclusion by concentrating on the writings of the leading natural philosophers of the seventeenth century. Bacon, Hobbes, Pascal, Galileo and Descartes, for example, did in fact ignore what are considered today to be Kepler's major achievements. They were not, however, the mathematical astronomers to whom Kepler's works were directed. Bacon was not even a Copernican, nor was Pascal.[9] Hobbes, on the other hand, praised Kepler for his insistence that the astronomer must be concerned with "celestial physics" as well as saving the phenomena.[10] Beeckman, Mersenne and Roberval were aware of Kepler's early harmonic speculations and his physical ideas on the cause of planetary motion, but had very little or nothing to say about his discoveries concerning planetary motion.[11]

The response to Kepler's international debut as an astronomer with the publication in 1596 of his *Mysterium cosmographicum* was greater than has hitherto been recognized and, parallelling the oft-cited objections to its apriorism and what has been called his "mysticism", were words of praise by some.[12] Mersenne expressed a preference for Kepler's harmonic system over Robert Fludd's, and Jeremiah Horrocks was sympathetic, arguing that final judgement must await further observations and better knowledge of the motions of the planets, an argument made earlier by Michael Mästlin.[13] Among those unwilling to wait had been Tycho Brahe, who rejected the very notion of a role for apriorism in astronomy.[14] Others who felt the same way were Johannes Praetorius, Isaac Beeckman, and Martin

Hortensius. Christopher Heydon, who approved of Kepler's harmonic goals, objected to his particulars.[15] The *Mysterium cosmographicum* was nevertheless the seedbed from which grew the mature concepts of Kepler's later works, and it presented the central themes, not only of harmonics which were to be elaborated in his *Harmonice mundi* of 1618, but also of a celestial physics and of planetary speed as inversely proportional to distance from the Sun. Interest in the *Mysterium cosmographicum* grew after the publication of the *Astronomia nova* in 1609, and a second edition with commentary by Kepler was published at Frankfurt in 1621.[16]

Evidence for familiarity with Kepler's ideas by leading natural philosophers is scanty. Descartes's relationship to Kepler has been subjected to varied interpretations. Leibniz asserted that Descartes used Kepler's results "brilliantly, although, as is his custom, he concealed their author".[17] Alexandre Koyré characterized the influence of Kepler's ideas on Descartes as a "seductive hypothesis" and conceded it as a possibility.[18] That influence is given support by several factors, including the adoption by Descartes of some of Kepler's terminology. Kepler, as did Descartes after him, had used the term *vortex* in describing the medium in which the planets moved, although the Sun in Kepler's scheme was the source or cause of the vortex and thus had a function lacking in the Cartesian system. Descartes, insisting on matter acting on matter as the fundamental cause of action in his plenous, mechanical universe, would have rejected Kepler's quasi-magnetic devices in any case.[19] Descartes, in correspondence about 1648, likewise used the terms 'aphelion' and 'perihelion', which had been coined by Kepler and first appeared in his *Cosmographic mystery* and afterward in the *Astronomia nova*. The expression "natural inertia", another term first employed by Kepler, is also found in Descartes's *Principles of philosophy*.[20]

There are similarities in the two cosmologies, however, that extend beyond the use of similar terms. Descartes's vortices, like Kepler's orbits, were flattened at the sides and the planetary speeds varied, but this was owing to pressure exerted by neighbouring vortices. Descartes even mentioned elliptical or near-elliptical orbits in connection with the Moon, although the Earth in his scheme is in the centre of the ellipse rather than at its focus. He also has the Moon moving with non-uniform motion.[21] Finally, Descartes places the Sun in all the orbital planes, a uniquely Keplerian contribution to astronomy.[22] There is little mention of Kepler's astronomical achievements in the works of Descartes's most influential followers at least until the publication of Newton's *Principia*. The general opinion appears to be that if Descartes had heard of Kepler's first two laws, he made no use of them in the construction of his own theory.[23] Although circumstantial, the weight of evidence would seem to favour the opinion of Leibniz.

The consensus on Huygens appears to be that he did not commit himself to ellipses until he encountered Newton's *Principia* in 1688.[24] He was certainly acquainted with Kepler's ideas long before, since, in a letter to Hevelius on 25 July 1656, he indicated that he thought "Kepler's system" was "more credible" than Copernicus's on the grounds that it had eliminated epicycles and ordered the planet in a simpler kind of motion. In 1661 he was instrumental in having Hevelius publish

Jeremiah Horrocks's *Venus in sole visa*, a decidedly Keplerian work, and, writing to Colbert on 27 August 1682, he clearly described Kepler's principle of non-uniform planetary motion.[25] In a manuscript written in 1686, he accepts the third law, rejects the second, but notes once again that "Kepler has reduced the planetary system to a marvellous simplicity and conceptual facility".[26] Huygens changed his mind about the area rule and ellipses in 1690 after having read the *Principia*.[27]

Galileo's relationship to Kepler has caused even greater puzzlement and generated a literature far greater in volume than that devoted to the rest of Kepler's contemporaries combined. Although he corresponded with Kepler, frequently praised him, and sought his assistance in furthering their common cause, Galileo never directly referred to Kepler's discoveries concerning the shape and nature of the paths of the planets. Koyré called Galileo's ignoring of the work of Kepler "a profoundly troubling fact".[28]

Kepler and Galileo exchanged several letters, but in none was Kepler's theory of planetary motion mentioned, although eight of the surviving ten letters were written after the publication of Kepler's *Astronomia nova*. Galileo's well-known reference to Kepler concerning his theory of the tides, that he "lent his ear and his assent to the Moon's dominion over the waters, and to occult properties, and to such puerilities", very likely came from his reading of the Introduction to the *Astronomia nova*.[29] Moreover, at least two of his correspondents mentioned Kepler's elliptical orbits favourably in letters to Galileo.[30] There can be no doubt of Galileo's familiarity with the main features of Kepler's discoveries on planetary orbits.

The puzzle of Galileo's relation to Kepler has generated a good deal of speculation suggesting that part of the answer lies in the personalities of the two men or Galileo's vanity and his distaste for Kepler's literary style, harmonic speculations, and non-circular orbits. More interesting are the efforts to explain Galileo's rejection of ellipses as stemming from differences in their scientific styles, philosophies and goals.[31] At any event, Galileo and Kepler represent two distinct strategies for the promotion of the Copernican system. The nature of those strategies did not permit a pooling of resources beyond the use of new evidence provided by the telescope.

KEPLER REDIVIVUS

Kepler's reputation alone would have ensured that his ideas received a hearing. By virtue of his title as court astronomer to the Holy Roman Emperor and successor to Tycho Brahe, he was regarded as the leading astronomer in Europe. In 1617, he was invited to succeed Magini in the chair at Bologna, and three years later was urged to settle in England after meeting with Sir Henry Wotton, English ambassador to the Holy Roman Emperor, who wrote to Lord Chancellor Bacon that he had just met Johannes Kepler, "a man famous in the Sciences, as your Lordship knowes ...".[32] Kepler was also a very active publicist in his own cause. He had a wide circle of correspondents to whom he continually made his intentions known with a refreshing candour, and did not hesitate to present his ideas and speculations even when he had not yet fully worked them out. Kepler's notification to his correspondents of the works he had published, was about to publish or intended to publish may be seen as a seventeenth-century form of advertising and a way of keeping the scientific community abreast of his researches.

The reception of Kepler's ideas in the seventeenth century seems to divide naturally into three more or less equal periods: (1) Kepler's lifetime, i.e. to 1630, (2) the 1630s to about 1660, and (3) the 1660s to the publication of Newton's *Principia* in 1687. When Kepler's ideas are limited to his laws and to mention of them in the printed literature, one may accept Russell's conclusion in his pioneering investigation that they "attracted little attention until the publication of the *Rudolphine tables* in 1627".[33] In that work the area rule and elliptical orbits were applied to all the planets as they also had been in the *Harmonice mundi* of 1619 and the *Epitome astronomiae Copernicanae*, published from 1618 to 1621, and not only to Mars as had been the case in the *Astronomia nova*. In Chapter 40 of the last-named work the area rule is provided as a means of approximating to Kepler's supposition that orbital speed is inversely proportional to distance from the Sun, and the ellipse is not given until Chapter 58. For all that, the rules were available to even the casual reader in the front matter of the book. Kepler's most influential work on planetary theory, the *Epitome*, was little read at first, but became the main source of Kepler's influence from 1630 to 1650 and beyond.[34] It is a manual of heliocentric astronomy, detailing his improvements of Copernicus's theory and summarizing his own discoveries in systematic form, avoiding the detours of the process of discovery laid out in the *Astronomia nova*.

For the period up to 1630, Russell identifies only nine men as having expressed familiarity with Kepler's ellipses: Thomas Harriot, William Lower, Federigo Cesi, Giovanni Magini, Christian Severin Longomontanus, Nathanael Carpenter, Peter Crüger and Philip Müller; Ambrosius Rhodius, Willebrord Snel and Jakob Bartsch may have also.[35] To this list may be added, from published and unpublished correspondence and manuscripts, such additional names as David Fabricius, Michael Mästlin, Johann Brengger, Christopher Heydon, Henry Briggs, Albert Curtz, John Bainbridge and Wilhelm Schickard.[36] A number of these were favourably disposed. Russell found no one who stated Kepler's area rule or the harmonic rule during the early period, though granting that there may have been several.[37] Russell's general conclusion is that there was a steady increase of interest in Kepler's laws after 1627 and that by 1666 ellipses and non-uniform motion were well-known to most astronomers. He lists seven who mentioned the third law by 1666 and four who gave the correct formulation of the area rule by that date, while several gave the inverse-distance formulation.[38] It now appears that the *Astronomia nova* was even more widely read, as Russell had surmised, than was apparent from his survey of the printed literature. In addition, some, like Galileo, may have read it, in whole or in part, but failed to comment on it.

In the middle years of the century, the list of those mentioning Kepler's ellipses lengthens considerably, with many by 1666 having adopted ellipses as representing the true planetary orbits. More than two dozen have been identified in the printed literature.[39] Across the sea a New England almanac asserted in 1662 that the planets dance "illiptical Sallyes, Ebbs and flowes" owing to "Magneticall Charmes" issuing from the Sun.[40] Even Tychonians assumed ellipses in the construction of tables and had the planets moving around the Sun and the Sun around the Earth in ellipses.[41]

Some astronomers gave the inverse-distance form of Kepler's relationship between planetary speed and distance from the Sun, or considered it equivalent to the area rule; confusion between the two versions persisted throughout the century.[42] Many who did not mention the area rule, however, must have known of it, for the *Rudolphine tables* were constructed on the basis of ellipse and area rule and an explanation of the area rule is given in that work.[43] In the *Astronomia nova* Kepler had initially proposed the areas ratio as an approximation to his dynamical principle of the inverse proportionality between planetary speed and solar distance, and then concluded that they were equivalent. In the *Epitome*, having recognized that the inverse-distance rule was valid only at the transverse components of the radius vectors, he stated the area law unambiguously.[44] In keeping with the systematic development of the text in the later work, Kepler avoided the intermediate stages involving the circle and the oval, and the reasoning is more direct and geometrically sound. From 1630 to 1650, most writers referred to the *Epitome*, citing particular passages in it.

Kepler's third law was stated clearly and simply in the *Harmonice mundi*, but was not used in the creation of tables. Jeremiah Horrocks referred to it in his *Venus in sole visa*, saying that by repeated calculation he had found it to be absolutely true.[45] During the middle decades of the century there was less interest shown in Kepler's third law than in the others. Before 1666 it was mentioned by only a handful of writers.[46]

Kepler's rules for planetary motion may have found an audience in the 1640s and 1650s through descriptions of them in some widely-read comprehensive works. The later volumes of Pierre Hérigone's textbook of mathematics, *Cursus mathematicus*, reflected the author's conversion to Copernicanism and to Kepler's rules. Some part in making Kepler's laws better known may also have been played by Riccioli's widely-read *Almagestum novum*, which gave a detailed exposition of Kepler's theories, including exact descriptions of the area rule and the harmonic rule.[47]

Scattered references of approval or disapproval may be seen in Kepler's earliest readers, not always accompanied by detailed reasons. An exception was Thomas Harriot and his former pupil William Lower.[48] The latter explained in a letter to his mentor shortly after the publication of the *Astronomia nova*, that he much preferred Kepler's use of the true rather than the Mean Sun. He indicated his approval of Kepler's

> permutation of the medial to the apparent motions, for it is more rational that all the dimensions as of Eccentricities, apogacies, etc. ... should depend rather of the habitude of the sun, than to the imaginary circle of orbis annuus.[49]

With Harriot, he accepted the elliptical orbits with unique eccentricities for each of the planets. Finally, he approved Kepler's placing the Sun in the planes of all the orbits as yielding an improvement in the latitudes.[50] He objected to the approximations and difficult calculations required by elliptical orbits. After Harriot and his circle, one of the earliest English Keplerians was John Bainbridge, chosen as the first Savilian Professor of Astronomy in 1619. Some time between 1627 and 1643 he lectured at Oxford on ellipses and elliptical astronomy. His lecture notes contain worked problems, using Keplerian logarithms and an elliptical lunar theory.[51]

The attachment to perfect circularity in the heavens, however, was not easily broken. A preference for circles on metaphysical grounds was frequently stated in the first half of the century.[52] Nathanael Carpenter, to whom we owe the first mention of Keplerian ellipses in an English printed work, preferred circles to Kepler's ellipses because circles were more natural and in better accord with the perfection of nature. A similar reluctance to abandon circles may be seen in Longomontanus.[53] David Fabricius, friend and correspondent of Kepler, urged Kepler to employ epicycles in generating his elliptical orbit. Some who initially rejected ellipses became later converts, chiefly within the three decades following Kepler's death. Thomas Brush and Jeremiah Shakerley came to accept elliptical orbits, but employed circles to generate them.[54] Samuel Foster, familiar with Kepler's tables also accepted elliptical orbits, but employed circles for his planetary models, asserting that although they "be defective yet it makes no great difference in these small instruments".[55] In addition, the Tychonic system maintained its hold on a number of astronomers into the middle years of the century.[56] Christoph Scheiner was one of the few Jesuits holding to the Tychonic system in the 1620s. The Church, however, pressured the Society of Jesus to publish on the controversy and to uphold the official position. As a result, a number of works appeared in the next twenty years that emphasized the position of the Church, and described Tychonic or semi-Tychonic systems.[57]

For partisans of Kepler a recurrent theme was delight at his doing away with imaginary circles and emphasizing real bodies and actual distances. Some found Kepler's use of the real Sun instead of the Mean Sun appealing. The few who accepted non-uniform motions within elliptical orbits during Kepler's lifetime would certainly have been in that category. One need not have been a Keplerian or even Copernican to use the real Sun in calculating planetary position.[58] By mid-century several astronomers had followed Kepler's path in this regard, including Tycho Brahe, his chief disciple, Longomontanus, and Riccioli.[59] Linked to use of the real Sun was Kepler's placing the Sun in the planes of all the planetary orbits, which, it was recognized even by non-Copernican astronomers, accounted better for the planetary latitudes.[60]

Kepler's bisection of the eccentricity (placing an equant and the Sun on the line of apsides on opposite sides and equidistant from the centre of a circle), a step on the path of his discovery of the elliptical orbit, had an appeal, particularly for those who wished to retain circles. Johannes Phocylides Holwarda, for example, replying to the attacks of Philip van Lansberge and Martin Hortensius, says that he verified the bisection for the Earth, although he found that it did not correspond exactly for the superior planets.[61] In his *Almagestum novum* Riccioli rejected the Keplerian ellipse on various grounds, among them, that the bisection of the eccentricity, which he considered the foundation of the elliptical hypothesis, had not been empirically confirmed. In his *Astronomia reformata* of 1665, however, while still holding to a geostatic system, he indicated his conversion to elliptical orbits as a strong hypothesis, since, using an improved method, he had discovered that it was confirmed by observation of the apogeal and perigeal solar diameters.[62]

One of the most important sources of objection to Kepler's ideas was his introduction of physical causes into the domain of astronomy. Johann Brengger confessed himself unable to imagine how physical forces could ever be subject to mathematical treatment.[63] Mästlin wrote to his former pupil in 1616:

> As to what you write concerning the Moon: you treat all its inequalities by physical causes. I do not quite understand this. I think rather that this should be treated by astronomical causes and hypotheses, not physical ones. Certainly the foundations of astronomy clearly require geometrical and arithmetical calculations, not physical conjectures, which greatly confuse rather than instruct the reader.[64]

Such criticism continued for the next few decades. Some, like Peter Crüger, echoed the substance of Mästlin's criticism, that physics was not properly within the province of the astronomer.[65] Ismaël Boulliau, commenting on the system of Giovanni Alfonso Borelli in 1666, asserted that "nothing about Astronomical Hypotheses can be demonstrated from the causes or reasonings of physics".[66] Others, adhering to a strict Copernicanism or to the Tychonic system, raised objections to the magnetic analogy in Kepler's celestial dynamics. Among them about mid-century were Athanasius Kircher, Jacques Grandami, Niccolo Zucchi, and Gaspar Schott.[67] Kircher wrote in 1641 about Kepler that "Concerning the mathematical, no one is better and subtler than he; concerning the physical, no one is worse".[68] In addition to some novel theological anti-Copernican arguments, Jean-Baptiste Morin in 1631 cited the absence of changes in the Earth's motion during solar eclipses and his failure to find "Keplerian fibres" in mines against the Keplerian solar force. He also listed objections from astrology.[69] Others favoured Kepler's programme, while disagreeing with his particular magnetic mechanism. Among them in the early years were Lower, Bainbridge, Holwarda, Horrocks and Shakerley, the last two clearly recognizing the link between Kepler's discoveries on planetary motion and his physical speculations.[70]

The many who had reservations about Kepler's discoveries generally were willing to wait for empirical verification of Kepler's claims. Some were sceptical of the empirical data on which Kepler had based his analysis. Tycho's observations were not to be published until the twentieth century, and, although Kepler had cited the Tychonic data necessary for his calculations, the fund of observations was not yet large enough to compel agreement from all astronomers. Kepler's work contained many computing errors and his use or omission of relevant data was not always clear or apparent in his work.[71] As a result, there were continual calls for Kepler to publish the tables he had been promising so that empirical checks could be made of his theories. As early as 1605, Christopher Heydon, applauding Kepler's intention of publishing his book on the motions of Mars with appropriate tables, also asked about the availability of Tycho's data.[72] One of the most persistent in this regard was Henry Briggs, the first Gresham professor of geometry and afterward the first Savilean professor at Oxford from 1620 to 1630. His professional career coincided with Kepler's and he was acquainted with everyone in his field. His reading of the *Astronomia nova* left him unconvinced that the data there supported an elliptical orbit, and he expressed a desire to see the tables which Kepler in that work had promised to provide at a future date.[73] Some years later he worked out partial tables for the Sun and Mars based on the data in the *Astronomia nova*, presented them to Kepler, and went on to ask Kepler to hurry with the publication of the *Rudolphine tables*.[74] When they appeared he found them unsatisfactory, but his reasons are not mentioned.[75]

The publication of Kepler's tables, however, resulted in bringing his ideas to a wider audience. A marked shift in interest came a few years later through the work of Ismaël Boulliau. From mid-century to the decade of the 'seventies, a principal source for the dissemination of Kepler's ideas, albeit in critical and modified form, was Boulliau's *Astronomia Philolaica*.[76] A convinced Copernican by 1639, Boulliau published a book in that year that presented the theories of the planets in Copernican form.[77] He seems to have become familiar with the *Astronomia nova* some years earlier, however, since, in a letter to Gassendi in 1633, he objected to Kepler's inverse-distance rule as a violation of "the axiom" of uniform circular motion. He indicated his openness, however, to elliptical orbits as long as they were formed in accordance with the "axiom".[78] Between the publication of his first book, the *Philolaus* of 1639 and his second, the *Astronomia Philolaica* of 1645, Boulliau appears to have fully confirmed the superiority of the *Rudolphine tables*, and Chapter XV of his *Astronomia Philolaica* was devoted to demonstrating how the astronomical phenomena were best explained on the basis of elliptical orbits. For Boulliau, however, the path described by a planet was caused by means which were ultimately geometrical, not physical, and bore no relation to the existence of any other celestial body.[79] He rejected Kepler's physical principles, remaining true to the spirit of the ancients and of Copernicus.[80] If a solar force existed, as Kepler wished, argued Boulliau, it should vary in accordance with an inverse-square relationship and not in the simple inverse proportion as Kepler first proposed.[81]

Boulliau also rejected Kepler's area rule as undemonstrated. To account for the apparent inequality, he tried to make use of the properties of the surfaces of various solids of revolution. He settled on a system in which a planet traverses the surface of an oblique cone in an elliptical path with the Sun in one focus of the ellipse and the planet's aphelion near the summit and its perihelion near the base. The axis of the cone passes through the empty focus.[82]

Variations on Boulliau's model, in effect an equant theory, including its later modification, were employed throughout the middle decades of the seventeenth century. Boulliau gained a reputation as the elaborator of Keplerian astronomy, and the tables included in his *Astronomia Philolaica* were in some respects a clear improvement over Kepler's. Among those who leaned on his model were William Shakerley, Vincent Wing, John Newton and Thomas Streete.[83] Russell concludes that after Boulliau, supporters of the elliptical orbit can be divided into "geometers" and "physicists". The former accepted Boulliau's position regarding the geometrical form as explanatory of the planet's motion and usually adopted his form of the modified equant; the latter group sought, as had Kepler, physical causes and generally followed the principle of non-uniform motion and its areal or inverse-distance formulations.[84]

Boulliau was one of the two astronomers of the first post-Keplerian generation who were the chief means for the propagation of Kepler's ideas on planetary motion; the other was Jeremiah Horrocks. Each left some record of his analysis of Kepler's achievement in greater detail than had his predecessors and based his own astronomical work on Kepler's. Unlike Boulliau, however, Horrocks wholeheartedly accepted Kepler's principles, and his frequent compliments reflect the admiration of a talented and youthful disciple who had found a worthy master. His surviving manuscripts are filled with words of the most lavish praise for Kepler, whom he revered above all other astronomers. Horrocks mastered Kepler's work, defended Keplerian astronomy against its detractors and competing systems, and went on to make signal contributions of his own to astronomy.[85] He praised Kepler for his physical speculations, which at last had revealed the true causes of planetary motion and the genuine shape of the orbits.[86] He accepted Kepler's doctrine of elliptical planetary orbits with the Sun situated in the orbital planes and of their constant inclination to the ecliptic. While there is no direct reference to Kepler's area rule, Horrocks accepted the principle on which it is based — the relation between the orbital speed of a planet and its distance from the Sun.[87] At one point he had accepted Kepler's inverse-distance rule, but appears to have been working his way through the *Astronomia nova* at that time.[88] As careful a reader as Horrocks could not but have been aware of Kepler's area rule. Although it is never stated in its given form, he was surely sensitive to its difficulty, as in early 1637 he developed a method of approximation necessary for finding areas of ellipse-segments which was simpler than Kepler's and yielded results which, although not as precise, were observationally indistinguishable from those used by Kepler's method.[89] He also accepted Kepler's harmonic rule, the proportionality between the squares of the planetary periods and the cubes of their mean distances from the Sun, affirming that he had empirically tested it and found exact agreement with his observations.[90]

Horrocks was led to champion Kepler by his intensive use of the *Rudolphine tables*, the accuracy of which he found far superior to that of tables based on the Copernican and Tychonian systems.[91] No amount of tinkering with the parameters, he discovered, could significantly improve the accuracy of any tables but Kepler's. Finding it the most accurate of the tables then in use, he was further led to the conviction that it alone was based on a true system of the universe. A careful observer, he embarked on a program of improving Kepler's tables; he reduced the solar eccentricity and made a number of adjustments to various planetary parameters, thereby attaining improved elements for several orbits. These adjustments allowed him to be the first to predict and observe a transit of Venus in the autumn of 1639.

As from 1637, Horrocks began a series of efforts to improve the theory of the Moon. Following Kepler, he based his lunar theory on the assumption that the Moon's orbit is elliptical and that a number of its inequalities are owing to the effects of solar attraction. He accounted for the evection (an inequality detectable in the quadratures to the Sun) by an oscillation of the apsides and a variable eccentricity. The result was the most significant improvement in lunar theory to that time.[92]

It was not until two decades after Horrocks's death in 1641 that his work became known to the astronomical community. The unfinished manuscript of his description and analysis of the transit of Venus of 1639 was not published until 1662, attached to a work by Hevelius. This prompted the Royal Society to undertake publication of Horrocks's surviving manuscripts, with John Wallis serving as editor.[93] Publication was delayed until 1672, after which his ideas received wider circulation. During the previous decade, however, his papers circulated among members of the Royal Society and others with interests in astronomy, where his ideas on celestial physics, Kepler's harmonic rule of the 3/2 power and his lunar theory exerted some influence.

The adoption of ellipses, with or without the area rule, meant also the eventual abandonment of uniform planetary motion about the centres of circles and the acceptance of the importance of the solar distance–orbital speed relation. In 1678, Robert Hooke, despite reservations about ellipses, noted that "the generality of astronomers [embrace] ... the *Copernican* System, especially as it is refined and rectified by the ingenious *Kepler*".[94]

EMPIRICAL SUCCESSES

The attitude toward accuracy in astronomy had changed considerably from the time of Copernicus to that of Kepler. Improvement of instruments and observational techniques, given the example set by Tycho, had led astronomers to pay increased attention to observational precision. In later years the telescope and its refinements, the pendulum clock, improved telescopic sights and the micrometer made possible an improvement in precision by an order of magnitude by the end of the century. Observatories were built to house large instruments and to undertake systematic programs of observation.[95] Even before the construction of the great national observatories, there developed extensive correspondence networks enabling astronomers to report their observations to one another.[96] The growth of astronomical observation and the acquisition of more precise data quickly led to the improvement of astronomical tables by modification of their parameters.

Kepler's long-awaited tables provided a means of empirically testing the validity of his principles. Their accuracy ensured that elliptical paths and non-uniform motion would remain under consideration, and they played a major role in the acceptance of Kepler's ellipse-cum-unequal motion. Even earlier, in 1615, Magini, modifying the *Prutenic tables*, produced ephemerides using the *Astronomia nova* and data provided in a letter from Kepler. Kepler's own ephemerides appeared in 1617, 1619 and 1630. They were the most accurate up to that time by two orders of magnitude and their reputation spread quickly.[97] Peter Crüger, Professor of Mathematics at Danzig and teacher of Hevelius, was urged by Philip Müller of Leipzig to read Kepler, but, as were many on first acquaintance, he was repelled by Kepler's obscurity and speculations. After reading Book IV of Kepler's *Epitome*, he wrote that he couldn't understand it and commented that, "These theories are based upon uncertain foundations and mere guesswork". After the appearance of the *Rudolphine tables* he changed his mind, pronouncing them, two years after they were issued, as the best extant. He then undertook to study the *Epitome* and the *Astronomia nova* and announced himself convinced by the proofs in the latter. "I am no longer repelled by the elliptical form of the planetary orbits; Kepler's proofs, in his *Commentaria de Marte* have convinced me."[98]

A striking success came four years after the publication of the tables. Astronomers by now had exemplars of the four major distinct planetary models: Ptolemaic, Copernican, Tychonic and Keplerian, the first two having, by the time Kepler's appeared, produced a number of variants, as indeed would the latter two. For much of the seventeenth century astronomers routinely compared and tested them against astronomical observations. The transit of Mercury of 1631 was a triumph for Kepler's tables and succeeded in converting a number of astronomers to elliptical orbits. The most detailed observation was made by Gassendi, whose *Mercurius in sole visus* of 1632, addressed to Wilhelm Schickard, treated the transit as convincing evidence for the superiority of Keplerian astronomy.[99] Responding to Gassendi's little treatise, Schickard compared the excellence of Kepler's prediction to the substantial inaccuracy of the others, and presented an outline of Kepler's major ideas, including elliptical orbits and the inverse-distance rule.[100] Martin Hortensius, while rejecting Kepler's planetary theories and harmonic speculations, and declaring himself a partisan of the tables of Philip van Lansberge, nevertheless agreed that Kepler's *Rudolphine tables* had yielded the best predictions for the Mercury transit, correct within 15′ of arc, while Lansberge's tables produced an error more than five times greater.[101] Likewise, Noël Durret, whose early work was also based on Lansberge, proclaimed himself converted to Kepler by the Mercury transit, citing the observations of Gassendi and others. His ephemerides, beginning with the year 1643, departed from his former reliance on Lansberge and were based instead on Kepler.[102] Gassendi's data for the transit were also important for Boulliau's acceptance of elliptical orbits. It was the orbit of Mercury, and his reading of the *Astronomia nova*, Boulliau contended, that convinced him of the worth of ellipses. "To bring the motion of Mercury under numerical laws was difficult if not impossible for pre-KEPLERIAN astronomers, who used only the circular hypothesis."[103] Other astronomers, among them Jeremiah Shakerley and Vincent Wing, cited the subsequent transits of Mercury in 1651 and 1661 as persuasive of the Keplerian rules.[104]

Over the next few decades, one astronomer after another testified to the superiority of the Keplerian tables over others in predicting planetary positions. In 1638, Samuel Foster, Gresham professor of astronomy, wrote to Horrocks saying that he valued the *Rudolphine tables* above all others. Ralph Cudworth was using them in 1643 to check the accuracy of data in a medieval manuscript. Boulliau, Riccioli and Flamsteed testified to the superior accuracy of the tables for Mars.[105] By the 1650s they seemed to have been in great demand.[106] The solar eclipse of 1666 was reported by a French observer to have been predicted best by the *Rudolphine tables*.[107] A number of commentators noted that Kepler's discovery that the Sun lay in the planes of all the planetary orbits produced a great improvement in the prediction of planetary latitudes compared to all other tables.[108] Kepler's tables came to be praised even by those who, like Joseph Moxon, a Tychonian, rejected the Keplerian theories.[109]

The initial successes of the *Rudolphine tables* encouraged astronomers to engage in a continuing process of correcting Kepler's solar and planetary parameters. Kepler himself was aware of inaccuracies of varying extent for different planets yielded by his tables, and had notified astronomers of the need for revision.[110] Utilization of Gassendi's observation of the transit of Mercury allowed Boulliau to make improvements in Kepler's figures for that planet, and Horrocks likewise made adjustments to the Venus figures after his observation of its transit in 1639. While Horrocks kept to Keplerian principles and changed only the parameters, others, like Boulliau, changed both parameters and fundamental hypotheses.[111] Durret noted in 1639 that Kepler was less successful with Jupiter and Saturn. In his ephemerides for 1652, the almanac-maker Gadbury noted that "Kepler, *the Phoenix of Astronomy, is by late observers, found very much to fail, even in the places of the Slowest-paced Planets*". The following year, Hevelius reported that the *Rudolphine tables* were not as accurate as had been hoped, and a decade later Huygens noted errors for eclipses and conjunctions.[112] The use of the telescopic micrometer in the 1660s led to higher standards of observational precision; any faults in the tables were becoming more readily apparent.

In 1669 Flamsteed, comparing several tables derived from the *Rudolphine*, noted that Streete's tables had erred by 16 minutes of time for the eclipse of 25 October 1668; Boulliau's, even more. As for Saturn, Wing's errors were greater still.[113] A few years later Flamsteed wrote that "I have spent my spare hours of late in correcting Kepler's numbers in the planet Mars, so as they may represent my observations, which I think they will do very accurately ...".[114] He was more precise in the winter of 1679/80. "The Rudolphine numbers are esteemed, and justly, as good as any extant ...." Yet they wanted correction based on Flamsteed's observations of the planets, particularly Saturn and Jupiter. He went on: "In Mars, Kepler's numbers err, but inconsiderably: this planet was his masterpiece; and his great pains bestowed on the limitations of his motions seem to have had suitable success." Flamsteed informed his correspondent that he was working at making figures for other planets and the Moon more accurate.[115]

An important factor in improving the accuracy of Kepler's tables was the reduction of solar parallax. The traditional figure of 3′ adopted from Ptolemy was seen by Kepler as too large and was reduced by him to 1′. In 1625 Gottfried Wendelin, using the method of lunar dichotomies with a telescope concluded that the solar parallax could not be greater than 1′. Ten years later, from assumptions about the ratios of planetary diameters, he asserted that a further reduction to less than 15″ was in order. After Wendelin wrote to Riccioli in 1647, the reduced figure for parallax reached a wider audience through the latter's *Almagestum novum*. A reduction to less than 15″ was also made by Jeremiah Horrocks, using a similar combination of observation and metaphysical assumptions.[116] Reduction of the solar parallax required a reduction in the eccentricity of the Earth's orbit, as did Kepler's bisection of the eccentricity, and a readjustment in the eccentricities and aphelia of the planets. From his reading of Horrocks's manuscript of *Venus in sole visa*, Thomas Streete produced tables that were generally acknowledged as the best of their time.[117]

An additional improvement on Kepler which was not to be adopted for a considerable time concerned the obliquity of the ecliptic. Kepler had doubted that the obliquity varies over time, as did Gassendi, Flamsteed and many others. By comparing ancient eclipses, equinox and solstice records, and from his own solar and lunar observations, Wendelin concluded in 1626 that the ecliptic oscillates 30° about a mean of 24°. Horrocks likewise recognized that the obliquity had decreased during the previous few centuries.[118]

Copies of Kepler's tables and ephemerides constructed from them appeared throughout the century. More numerous were tables and ephemerides based on modifications of the *Rudolphine tables*, chiefly those of Boulliau and Horrocks.[119] Boulliau's tables first became known to astronomers and practitioners through the publication of his *Astronomia Philolaica* in 1645; Horrocks's revision of Kepler's parameters did not become generally known until 1661. Boulliau had assiduously compared the *Rudolphine* and other tables with his own. Examining Gassendi's observation of the 1631 transit of Mercury and his own observations during the early 1630s, Boulliau compared the accuracy of Vlacq, Lansberge, Longomontanus and Kepler.[120] He changed Kepler's parameters for Mercury and Mars, claiming an average accuracy to within 3′ or 4′.[121] Citing a near conjunction of Jupiter and Venus in 1659, he asserted that his *Philolaic tables* were more accurate than the *Rudolphine*.[122] The *Philolaic tables*, or modified versions using Boulliau's calculating procedures, were adopted by a number of astronomers, in whole or in part, among them Shakerley, Riccioli, Streete, Wing and Nicholas Mercator.[123] In the decades of the 1660s and 1680s some had come to rely on Streete and Wing, who had shifted the basis of their tables and ephemerides to the corrections in Kepler's figures made by Horrocks. Flamsteed accounted "Mr. Streete's numbers the exactest of any extant" and Wing's ephemerides "our exactest". [124] Ultimately, the *Philolaic tables* were judged inferior to the *Rudolphine*, owing in part to increasing familiarity with Horrocks's manuscripts after 1660. Horrocks, using Tycho's star catalogue, had frequently checked observed planetary positions against his corrections of Kepler's tables.[125] There remained, however, a number of astronomers of pragmatic and eclectic bent, who used different tables for different planets in the construction of their ephemerides. Mercator, for example, used Tychonic figures, the *Rudolphine tables*, and those of Streete and Boulliau. He used Tycho for the tables of the Sun and Moon, but preferred the *Rudolphine tables* for the planets, though he chose Boulliau and Streete for Mercury "because the Authors of them were help'd by those Observations which Kepler had wished for in vain ...".[126] The result of the improved accuracy of observational data was a pragmatic acceptance by astronomers at mid-century of the fundamentals of Kepler's new astronomy: non-uniform motion in non-circular orbits. They appeared to be the principles that had produced substantially better predictions than had been possible before them.

THEORETICAL AND PRACTICAL DIFFICULTIES OF AREA RULE AND ELLIPSE

As the key to Kepler's discoveries, the relation between planetary speed and distance from the Sun, ellipses and physical conjectures must be seen together. Harmonic considerations and Kepler's elaboration of the realist foundation of Copernicus's claims led him to his first discoveries: the Sun's position in all the planetary orbital planes, the invariability of the inclination of the Martian orbit, and the non-uniformity of planetary motion.[127] In order to solve the problems associated with non-uniform motion, Kepler faced novel difficulties. He was unaware of the concept of instantaneous velocity and of a means of handling the complex problem of non-uniform acceleration; he always thought of finite arcs in finite times. In an effort to find a relationship between the arc traversed and the time, he was led first to what we call his inverse-distance rule, and subsequently to the area rule. The latter is not clearly or explicitly stated in the *Astronomia nova*, and was initially conceived as an approximation to the inverse-distance ratio. His calculations for the *Rudolphine tables*, however, are based on the area law, which is also provided in the *Epitome of Copernican astronomy*. Book V of that work shows Kepler's recognition that the inverse-distance rule applies only to the transverse components of the radius vectors; it was there given in modified form and the area rule stated as the true relation between orbital speed and solar distance. The well-known 8′ discrepancy in Mars eventually led Kepler, after painstaking efforts with different ovoid orbital shapes, to the ellipse as satisfying both observational and physical criteria. Kepler's formulation of the area rule, therefore, did not emerge from an effort to determine the curve corresponding to the varying distances of Mars from the Sun, but from a physical assumption implying non-uniform motion and a fixed proportion between planetary distance and speed in orbit. It was only after Kepler settled on the ellipse that he checked it against distance determinations. He was aware that the calculated figures for planetary distances did not determine the shape of the orbit with precision. He was quite sure, however, that the orbit was flattened at the sides and that the time taken for a planet to traverse a given arc was proportional to the area swept out.

The area rule presented extraordinary difficulties for the practising astronomer, requiring unusual and complex calculations. Kepler's *Rudolphine tables*, however, were introduced by a brief summary of the principles on which they were based and an explanation of his manner of calculating them. The results of the calculations were presented in tabular form for convenience. In explaining the use of his tables, Kepler circumscribed a circle about the ellipse, from which a perpendicular to the major axis of the ellipse passed through the planet. He coined the term 'eccentric anomaly' for the angle formed at the centre of the circle by the line of apsides and a radius from the centre to the point where the perpendicular to the line of apsides passing through the planet meets the circle. From 'eccentric anomaly' can be calculated both 'mean anomaly' (the mean "position" of the planet on the circle representing its sidereal period) and 'true' or 'coequated anomaly', i.e. the angle from aphelion to the planet at the Sun. Kepler's scheme differed from the usage of traditional tables in that mean anomalies appeared as non-integral, rather than integral values. Even worse, the need to compute 'coequated' or 'true anomaly' from 'mean anomaly' gave rise to a mathematical problem. As Kepler correctly surmised, this is not a directly calculable relationship. He was therefore forced to rely on tedious trial and error methods of approximation, and he implored the assistance of geometers in finding a solution to the problem in the most general terms.[128] The problem is to divide the area of a semicircle in a given ratio by a line passing through a fixed point on the diameter. A direct solution to this problem would also resolve the problem of the ellipse, since the crucial relation between an ellipse and a circumscribed circle is known. Thereafter known as 'Kepler's Problem', the challenge was taken up by several mathematicians, especially after the middle of the century when Kepler's works were better known.[129]

The reluctance of astronomers to accept Kepler's second law was based on perceptions of its "ungeometrical" or imprecise character and the tedious methods of approximation required by it. Continual complaints about the area rule were voiced throughout the century; Lower complained to Harriot in 1611 of Kepler's "manie and intollerable atechnies, whence deriue thos manie and uncertaine assayes of calculation".[130] Boulliau, although accepting ellipses, objected to the "ungeometrical" nature of the requirements of Kepler's area rule and also to his approximative method of computing eccentricity and aphelion from observations.[131] Several decades later, John Newton raised similar objections, and Flamsteed opined that Kepler's "method of Calculation be troublesome".[132] Further complicating matters, partisans of the elliptical astronomy were unaware of the incompatibility between the area law and Kepler's dynamical principle, as expressed in the inverse-distance rule. A number thought them, as had Kepler early on, equivalent, failing to note Kepler's corrected view. Among them were Cavalieri, Ward, Borelli, Hooke, Wren, Halley and Leibniz.[133]

For ease of calculation a considerable number of astronomers adopted in various forms what are best called empty-focus equant theories, i.e. geometrical constructions in which equal angles in equal times are generated by a radius-vector from the unoccupied focus of the ellipse to the planet. These constructions were invented by some who already assumed the elliptical form of the orbit. It was obviously much easier to compare and calculate times in a kinematic model using circles and uniform angular motion than to cope with the calculation of mixtilinear areas. Such models, if well constructed as modifications of Boulliau's 'simple elliptical theory', could yield powerful results accurate to within 1′ of arc. Kepler had turned to the equant early in his struggle with Mars.[134] His first version of the equant, denominated by Kepler his 'vicarious theory', placed it three-fifths the distance from the centre of its circle on the line of apsides compared to the Sun's eccentricity on the other side of the centre. The theory when applied to Mars using Tycho's data, yielded accuracies of approximately 1′ of arc in the longitudes, but produced significant errors in the latitudes, and thus in the calculation of planetary distances. He then turned to a bisected eccentricity with Sun and equant equidistant from the centre, which resulted in the famous 8′ of error, leading Kepler to various oviform orbits and eventually to the area rule, initially as an approximation to the inverse-distance relation. He would come to recognize that only an oscillating equant point on the line of apsides would satisfy the speed-distance relation and abandoned both the equant and inverse-distance rule as descriptive of an entire orbit. After Kepler, the earliest effort to adopt the empty focus model seems to have been by Albert Curtz, rector of the Jesuit College at Dillingen, who published his version in his *Novum coeli systema* of 1626. He wrote to Kepler about his idea, and Kepler referred to Curtz's hypothesis in the *Rudolphine tables*, published the following year.[135]

Bailly and Delambre explained the use of the technique by several seventeenth-century astronomers, but only recently has the subject been studied in detail.[136] The most influential of these models were those of Ismaël Boulliau. His *Astronomia Philolaica* of 1645 put forward what came to be known as the 'simple elliptical hypothesis'. It served as a starting point for a variety of empty focus equant theories produced in the second half of the century. This first version of his kinematic model had maximum errors of about 7′ for Mars.[137] In 1654, Seth Ward, professor of geometry at Oxford, published a work which aimed to show that Boulliau's book was lacking in rigour.[138] He pointed out that the methods employed in the *Astronomia Philolaica* imply an equant at the empty focus of the planetary ellipse, a point that had been denied by Boulliau.[139] Thinking the empty focus theory as essentially Ptolemy's bisection of the eccentricity, Ward did not realize that the empty focus or equant theory could never be entirely accurate, and mistakenly assumed that Boulliau's hypothesis and the area rule were equivalent. In 1656 Ward published his *Astronomia geometrica* in which he accepted Kepler's notion of the Sun as the source of planetary motion, but also held to the validity of the equant theory.[140] Ward did not appear to realize that Boulliau's procedure yielded true anomalies (the angle at the Sun from aphelion to the planet) that were in fact less accurate than Kepler's. Ward was less interested in the empirical validity of the theory than in its interest as a geometrical constuction. In response to Ward's criticisms, Boulliau adjusted his original hypothesis in a new publication containing a modified elliptical hypothesis.[141] It argued contra Ward that Ward's method of calculating true from mean anomaly was incorrect. Boulliau's new theory yielded figures with a maximum error for Mars that were better than Kepler's.

Several empty focus equant theories were published, beginning in the 1650s. Vincent Wing's *Astronomia instaurata* of 1656 was influenced by Boulliau's *Astronomia Philolaica* and, with modifications, achieved one of the most accurate equant procedures of the seventeenth century.[142] Blaise Pagan's tables of 1657 used Kepler's aphelia, mean motions and maximum equation of centre, but assumed an equant in the empty focus.[143] John Newton, whose *Astronomia Britannica* of the same year was also based on Boulliau's *Astronomia Philolaica*, asserted that "the planets have one only motion, in one line, and ... those motions are equal, constant and perpetual".[144] He therefore asserted that there must be some centre for the equal motions and devised an hypothesis employing an equant at the empty focus. Isaac Newton developed a number of equant theories beginning in the 1660s, and empty focus theories persisted even after the publication of Newton's *Principia*, Cassini creating one in 1691, Machin as late as 1729, although astronomers had long before begun to cope with the calculations necessary for the use of the area rule.[145] Wherever they stood on Kepler's theories, at some point a significant number accepted Kepler's elimination of the epicycle as a worthy goal. Boulliau — and all the empty focus theorists who followed — either constructed models without epicycles or, if using them, generated non-uniform motion on ovoid paths, frequently using variations of Kepler's 'vicarious hypothesis'.

The inadequacy of the empty focus theories had been recognized by Kepler, who commented that "the equant never says the truth perfectly ...". Moreover, Kepler held, in Ptolemy its proportion varies from planet to planet and it does not reflect "natural causes".[146] Kepler's view was echoed by Wing, who explained that

> there is no such thing in nature as the mean or equall motion, yet that is necessarily supposed to regulate those exorbitances and deviations from equality which their apparent motion are lyable unto.[147]

He went on to say that the published equant theories are generally not as good as Kepler's. Even Boulliau's tables of prosthaphaereses are not as accurate as Kepler's, though Boulliau's figures for the middle motions are better than any other.

A more stringently Keplerian astronomy was propounded by Nicolaus Mercator. In 1664 he forthrightly affirmed the elliptical orbits of the planets with the Sun in one focus, the physical causes of the planetary motions, and Kepler's harmonic law, but constructed an equant theory by utilizing Kepler's 'vicarious hypothesis'.[148] A few years later, in 1670, presenting the area rule correctly, Mercator made clear the justification for his equant theory on pragmatic grounds.

> Nor has anyone hitherto been found who would deny that the areas of Kepler can justify the phenomena. [N]either Kepler himself nor anyone after him could show them by direct calculation ....
>
> [A]lthough Kepler had scruples about retreating from an Hypothesis which he was clearly convinced was *Natural*, why should not others be free to experiment as to whether there is any other way of determining the first inequality of the planets by direct calculation?[149]

Mercator here explained that a simple equant was untenable and that Kepler should have made that clear. Yet in his *Institutionum astronomicarum* of 1676, after again explaining Kepler's area law, he put forward another equant theory, based this time on an elliptical orbit.[150]

The widespread use of equant theories did not necessarily mean that astronomers did not accept the validity of the area rule. By the 1650s, most astronomers seem to have been aware of it, despite whatever confusion they may have had about its equivalence to the inverse-distance rule or its accuracy.[151] Christopher Wren learned of Kepler's area rule by 1658 but in 1677 seems to have believed it equivalent to the inverse distance rule, as did Hooke.[152] Thomas Streete recognized the area rule as true and also Kepler's harmonic rule, but opted for the convenience of the equant model. His *Astronomia Carolina* of 1661, based on Boulliau's 1657 modified version of his *Philolaic astronomy*, but employing improved Horrocksian planetary parameters, yielded a significant improvement on the maximum theoretical error of 1′51″ for Mars in Boulliau's method.[153]

Another problem stemming from the area rule led to efforts to develop a means for computing true anomaly from mean. Closely connected with these efforts were alternative means for determining aphelion, eccentricity, and mean longitude at epoch on the basis of three or more heliocentric longitudes ascertained from observations.[154] Cassini published a paper in 1669 on his method of finding apogees, eccentricities and true anomalies. A few months after its publication it was read at a meeting of the Royal Society.[155] In 1670 Mercator showed Cassini's method to be equivalent to an empty focus theory of the Boulliau-Ward type and questioned its accuracy compared to Kepler's area rule, which he stated accurately.[156] The ellipse and area rule, he held, must stand or fall together. With the touchstone of observational accuracy, and clearly having restudied Kepler's works more carefully, Mercator proved a turning-point in the recognition of the area rule. In his critique of Cassini's method, Mercator indicated how Kepler had once believed in the equant theory, but had come to change his mind; accuracy is not possible without a libration of the equant point. The validity of the area rule has not hitherto been disproved or proven observationally inaccurate. Since true anomaly cannot be found geometrically, a solution to Kepler's Problem, or a way of finding true anomaly directly that is empirically valid, must be sought.[157] In 1676, Edmond Halley devised a method of deriving aphelion and eccentricity of elliptical orbits without using an equant theory. If the orbit and motion of the Earth are known, tangents to the orbits of the inferior planets may be determined from their maximum elongations. For the superior planets, particular positions may be determined by two observations at intervals of their sidereal periods or multiples thereof, keeping them 'stationary' so to speak, while the planet whose orbital elements it is desired to determine moves to three successive stations. With two tangents or two positions known, the eccentricity and aphelion may be determined geometrically.[158] Theoretically valid, but astronomically impractical, it nevertheless represented the state of conviction among astronomers in the decade of the 1670s of the validity of Kepler's elliptical orbits and area rule. In 1680, Flamsteed, employing a skilled computer, used the method of approximation as the best way to compute the difficult lunar positions on the basis of the area rule.[159]

In addition to the problems associated with the area rule, the validity of Kepler's elliptical orbits was questioned up to the publication of Newton's *Principia*, in some cases by those who could not bring themselves to abandon circles, in others, by those who realized with Kepler that his data and calculations were not precise enough to warrant an ellipse over another ovoid. Kepler's choice of the ellipse had been based on the area rule and the closeness of its agreement with the solar distances of Mars and perhaps Mercury. The *Rudolphine tables* were less successful for Venus, and a model for that planet utilizing uniform motion on an eccentric could be made to yield an accuracy of 1′.[160] Those who tended to seek a mechanical account of planetary motion and had accepted the goals of Descartes, Gassendi and Boyle, as did Hobbes, Hooke and at least the early Newton, stressed the inexactitude of Keplerian ellipses.[161] Hobbes wrote that the orbit of the Earth is elliptical or near elliptical.[162] Boulliau called Kepler's planetary ellipses a "happy conjecture" and indicated that the calculations alone were in themselves incapable of determining an elliptical orbit.[163] Making the same point about Kepler's lack of definitive proof of the elliptical orbit, Riccioli in his *Almagestum novum* of 1651 added that he was unwilling to abandon circles.[164] Cassini never accepted elliptical orbits, preferring an ovoid of his own invention, while Wendelin, although accepting in 1652 Kepler's ellipses in general, suggested that the Moon's orbit was ovoid.[165] Robert Hooke expressed the opinion of several in the Royal Society concerning the inexactitude of elliptical orbits. The lunar orbit, influenced in the Keplerian canon by forces emanating from both Earth and Sun, was a prime candidate for questions concerning its ellipticity. In 1665 Hooke inquired about the real orbit of the Moon.[166] From the early 1660s he and others in the Royal Society had been poring over the papers of Jeremiah Horrocks. Perhaps Hooke, seeing there the Horrocksian lunar theory with its oscillating apsides and varying eccentricity, was led to his query about the most difficult orbit of all.[167] In 1666 John Wallis referred to "the line of the *Annual* motion (whether *Circular* or *Elliptical* ...)" and went on to note that the orbit of the Earth is described by the common Earth-Moon centre of gravity.[168] In the same year Hooke read a paper before the Royal Society in which he asserted that the celestial bodies were "moved in circular or elliptical lines".[169] Writing to Newton several years later, Hooke speculated that under an inverse-square law of attraction, a planetary orbit would be a kind of elliptoid, but not precisely elliptical.[170]

While empty focus equant theories, and even eccentric circular orbits with appropriate adjustments, could be made to yield reasonable levels of accuracy, given contemporary standards, by 1685 many astronomers nevertheless accepted the ellipse, even if they realized that it was empirically underdetermined. While ultimate proof of the exactitude of the ellipse was lacking, it came to be recognized that the orbit was a flattened circle or ovoid of some kind and the motion of the planet non-uniform with respect to the centre of its orbit or the Sun. The geometry of the ellipse was understood and its acceptance was due largely, but not entirely, to the empirical successes of Kepler's tables, especially when corrected and their parameters modified by the results of observation. With the use of the telescopic micrometer after the middle years of the century, greater demands would be made on the accuracy of tables; this would serve only to reinforce the reputations of those based on Kepler's.

NON-EMPIRICAL ASPECTS OF THE RECEPTION OF KEPLERIAN ASTRONOMY

Kepler's discovery of the planet laws has always been recognized to have been dependent on the choice of Mars, with its relatively large eccentricity, and on a heightened level of astronomical precision unavailable to Copernicus and irrelevant to the latter's aims and achievement. In the light of Kepler's aims, accuracy would become the touchstone for the validity of his theories as it had not been for Copernicus's. The role of empirical factors in Kepler's discovery of the first two laws of planetary motion has been characterized in various ways. The younger Herschel saw Kepler's laws as arising "entirely from a comparison of observations with each other, with no assistance from theory". Arthur Koestler saw them as emerging from a remarkable act of sleepwalking.[171] What recent scholarship has made apparent, however, is the essential role of physical theory and metaphysics in Kepler's discoveries; empirical considerations alone are insufficient to explain them. Nor did Kepler himself in the *Astronomia nova* or the *Epitome* claim to have derived the laws by strictly empirical means. Omission of the details of Kepler's physical theories in accounts of his planet laws may have resulted from the role of the laws in Newton's *Principia* and subsequent belief in them as the empirical foundation of Newton's edifice.[172] As for the path to discovery traced in the *Astronomia nova* representing a sleepwalking performance, an argument has been made that despite its meanderings, Kepler's argument was intentionally constructed to persuade astronomers of the failures of traditional astronomy and of the superiority of the new, an *astronomia nova*. His tasks were to show how all the old hypotheses were inadequate in the light of Tycho's data and that his hypotheses alone conformed to those data, and to link improved empirical accuracy to a physically real presentation of celestial events.[173]

While it may be true that the ellipse and area rule for Mars could only be determined in conjunction with one another, astronomers after Kepler, from Boulliau on, were able to separate them for philosophical or practical reasons. Kepler's data, although not precise enough to compel total conviction on ellipses, were nevertheless adequate enough to win agreement from many astronomers that orbits were ovoid and planetary motion non-uniform. The success of Kepler's tables alone, however, is not sufficient to explain the gradual conversion of astronomers to Keplerian ellipse and unequal motion. If variants of Kepler's 'vicarious theory' could yield results as accurate as the areal theory, how could the *Rudolphine tables* possibly confirm the elliptical hypothesis? Just as Reinhold, who was not a Copernican, produced the *Prutenic tables*, so Morin, a Tychonian, produced a version of the *Rudolphine tables*. The *Prutenic tables* were also the best of their day, but were not persuasive for Copernicanism. After their publication, as with the *Rudolphine tables*, errors were found which it was by no means certain could be corrected by making simple adjustments rather than by altering fundamental hypotheses. There were those like Durret and Boulliau who claimed to have been converted to ellipses by the accuracy of the *Rudolphine tables*. On the other hand, were those like Hooke, Wallis and Newton, in a period of increased observational precision, who claimed that the ellipse had not been empirically verified. Their objections had been preceded by Boulliau's qualification of his acceptance of elliptical orbits.[174]

If recognition was general that Kepler's ellipses could not be confirmed by observation, why were some persuaded, either to ellipses, or to both ellipses and Keplerian non-uniform motion? Perhaps Kepler's use of the real Sun situated in the planes of all the orbits suggested that the orbits must be 'simple'. This involved reentrant orbits without epicycles in conformity, more or less, with the distance-parameters, that is, that they must be real orbits. In the decades after Kepler, there took place a transition from the axiom of circularity to non-circularity as permissible. Circles in astronomy had always been conceived under a dual rubric, as mere tools having a purely pragmatic function as well as possessing cosmological, metaphysical or physical warrant. The use of the equant in the seventeenth century marks a return to the non-realist attitude in earlier times to the epicycle. Since epicycles can generate ellipses and, except for some Tychonians, were understood to be useful fictions, the Kepler-ellipse cannot be imagined without its realist connotations. When the ellipse or near-ellipse becomes accepted, circles as useful fictions are clearly differentiated from real planetary orbits. Insofar as they had to produce or employ tables, astronomers, some of whom might be characterized as agnostics with respect to theory, could and did employ all sorts of geometrical techniques; insofar as they wished to describe a system of the world, they favoured 'simple' orbits and had to be as accurate as possible in longitudes, latitudes and planetary distances. An example of the former is provided by Henry Briggs, who, although a Copernican, adopted aspects of Kepler's 'vicarious hypothesis', which gave excellent results for the longitudes, but which Kepler had rejected because of its poor values for the latitudes. Writing to Kepler, Briggs rejected "that Physical Hypothesis of yours" as not being in accord with astronomical tradition and the opinions of the moderns, and opted for the "more familiar and more geometrical" hypothesis.[175]

Kepler, for his part, from the beginning of his career, persistently challenged the limitations on physical speculation traditionally placed on the astronomer.[176] In his "Defence of Tycho", written while assistant to the Danish astronomer, Kepler insisted on the necessity of taking into account principles from related disciplines in the construction of geometrical hypotheses in astronomy, and that once one went beyond the effort to predict planetary longitudes alone, the mathematical equivalence of the Ptolemaic, Copernican and Tychonic systems would disappear. He insisted that "the astronomer ought not to be excluded from the community of philosophers who inquire into the nature of things".[177] In this early work he asserted that astronomy should aim to discover the true — and not just the apparent

— motions of the planets. The task of merely saving the phenomena belonged to "the inferior tribunal of geometers".[178] Kepler frequently inveighed against the implications for the real motions of planets of the assumptions of classical planetary hypotheses. False hypotheses could never yield the truth. They would result in "spirals, loops, helices, coils and that whole labyrinth of most intricate curves, a human figment", which could not possibly exist in nature.[179]

Bruce Stephenson has shown that Kepler's thinking was so dominated by physical processes that he analysed the geometrical models of traditional astronomy in terms of their physical plausibility. For example, on the basis of the distance relation, Kepler at a certain stage of his investigation of the Martian orbit showed that the eccentric circle could not be real since calculation of planetary distances from the Sun in different parts of the orbit necessitated an orbit flattened at the quadrants.[180] In a letter to Mästlin on 14 December 1604, Kepler wrote that for him it was inadequate to determine such planetary parameters as equations of centre and distances from the apparent motions alone; they must be obtained from "real causes".[181] Among Kepler's physical theories, the axial rotation of the Sun and its alternate attraction and repulsion of the planets were also extended in the *Epitome* to the rotation of the primaries to account for the revolutions of the satellites. Kepler continually insisted that "astronomy has two ends, to save the appearances and to contemplate the true form of the edifice of the world...".[182] He thereby challenged long-established opinion and practice.

Two explanatory modes for celestial phenomena had been established in Antiquity. One was that of mathematical astronomy, which was unconcerned with finding physical explanations for celestial phenomena. In the broad sense of the term, early mathematical models for prediction of planetary positions may be seen as calculators, as practical and useful computer programs. The culmination of this approach was realized in Ptolemy's *Almagest*.[183] The other, in the tradition of Aristotle, sought to explain through the application of physical principles the nature of celestial events and their causes. Most astronomers from Hipparchus to Kepler tried to avoid whenever possible exceeding the limited question of planetary position. They took, however, three axioms from philosophy: the Earth at the centre and immobile, circular motion, and uniform motion. At the same time, from observations, astronomers gave several empirical foundations to the philosophers: a partial ordering of the planets, periodic or synodic times, changes in the relative distances of each planet in the course of its orbit, etc. Geometrical models incompatible with metaphysical or physical assumptions, such as the equant, were generally seen as pragmatic expedients of a useful but temporary nature, or expedients to which we are reduced because of the limitations on our knowledge.[184]

In the sixteenth century the division of labour with respect to the study of celestial phenomena which had originated in Antiquity was still alive and stronger than ever. It was reinforced by uncertainty about the truth of physical speculation as against the certainty attached to mathematical demonstration.[185] As been effectively shown by Donahue, the dichotomy began to break down only in the early seventeenth century and the two disciplinary areas became intermingled, after growing familiarity with Copernican realism, both explicit and implicit, and efforts to determine the parallaxes of the nova and comet of the 1570s.[186] It is one thing to hold that astronomy can tell us something about philosophy — as Kepler pointed out, it always had — and another to say that natural philosophy can tell us anything about astronomy beyond the position and stability of the Earth and the use of circles and uniform motion. Most late sixteenth-century astronomers took the position that the mathematical hypotheses used by astronomers were fictions and "fundamentally incompetent in the realm of physics".[187] Donahue's thesis is that, owing to the dissolution of the celestial spheres, this position was being rejected by the end of the century, and, by 1630, those who still held to the existence of solid planetary spheres were in a minority.[188] The natural philosopher would have to choose, it appears, between Kepler and, at a somewhat later date, Descartes for a physical theory of planetary motion.[189]

Whatever some philosophers might have felt about the relationship between astronomy and physics, most astronomers, including Copernicans, during Kepler's lifetime and for some time after, rejected the notion that astronomers ought to concern themselves with the physics of the heavens. Many of Kepler's contemporaries and immediate successors objected to his raising philosophical or physical issues in astronomy, or to his particular choices, staunchly maintaining that the purpose of astronomy was to save the appearances rather than to deal with causes.[190] Their objections were based either on the alleged impossibility of mathematizing physical entities or on the theological principle of the imperfect nature of our knowledge contrasted with the perfect knowledge possessed by the Creator. A notable exception in the first half of the century to those questioning the validity of physical speculation in astronomical theorizing was Jeremiah Horrocks, who saw in Kepler the only astronomer who had transcended the use of only geometrical methods as appropriate in astronomy.[191] Kepler himself recognized that his was a lonely voice within the community of astronomers.[192] He was keenly aware of the inexactitude inherent in the postulation of physical forces. He noted that the elements of an orbit may vary over time and that the "motions of the Sun, Moon, and *primum mobile* are not precisely equable, but receive small intentions and remissions *extraordinem*".[193]

It was possible to accept Kepler's geometrical relationships without accepting his physics at all. A number of astronomers did so. Yet accepting ellipses implied the acceptance of the importance of distance-relations and therefore of true paths and helped give the death-blow to traditional "fictionalism". Kepler thus provided a new constraint on planetary hypotheses: planetary–solar distances could no longer be ignored or treated in general or relative terms. The acceptance of Kepler's non-uniform planetary motion was always recognized as expressing a real, not apparent nonuniformity, whether given in the form of area rule, inverse-distance rule or equant.

The empirical successes of the *Rudolphine tables* may have called attention to the significance of Kepler's achievement, but may also have stimulated an interest in its theoretical foundations and cosmological implications. After the established successes of Kepler's tables, astronomers began to think about their data in a new manner related to the Sun; Kepler's discoveries gave primacy to the Sun, which focused the orbital planes and served as the planetary mover.[194] Acceptance of a solar role in planetary motion increased after Galileo's discovery of sunspots, which seemed to confirm the Keplerian speculation. With growing belief in the fluidity of the heavens early in the century, angelic movers began to be dropped for (1) God's commands directly or through the substantial form, or (2) planet souls. A group influenced by Gilbert's *De magnete*, which included Kepler, introduced physical or quasi-physical mechanisms of planetary motions.[195] Kepler's model of planetary motion was widely known and provided a "possibilist" or hypothetical framework for attacking the problem of the cause of planetary motion on the lines of the analogy of gravitation with magnetism. From Kepler to Newton, Kepler's programme for a new natural philosophy of the heavens increasingly gained the attention of astronomers.

Beginning with the publication of the *Mysterium cosmographicum* in 1596, Kepler put forward the idea of the Sun as the cause of planetary motion and elaborated it further in the *Astronomia nova*. In Book IV of his *Epitome*, using an analogy with the lever and balance, he developed his hypothesis of an immaterial motive virtue emanating from the rotating Sun as mover of the planets, and the alternating attractive and repulsive functions of the Sun and the orientation of the planets to it to account for the elliptical shape of the orbits. To this was added the rotation of the primaries as the cause of the revolutions of their satellites.[196] Kepler further speculated that the solar virtue's effect on a planet was determined by its volume as well as its distance, and that the planetary volumes increased directly in proportion to distance. In the *Epitome* and *Rudolphine tables* he cited telescopic observations he thought supported this supposition.[197]

Knowledge of the Keplerian speculations on the dynamics of planetary motions was well developed during the middle decades of the seventeenth century. Beeckman, Morin, Gassendi, Roberval, Mersenne, Hobbes, Horrocks, Nicolaus Caussin, Thomas White, Otto von Guericke, Hevelius and Mercator all show familiarity with elements of the Keplerian account of the cause of planetary motion.[198] Growing knowledge of Kepler's works reinforced Gilbert's old doctrine concerning the magnetic nature of gravity. In 1654 Walter Charleton declared himself in agreement with Kepler and Gassendi that there is a "certain Magnetick Attraction of the Earth".[199] Robert Hooke, discussing gravity before the Royal Society in 1666, noted that

> GILBERT began to imagine it a magnetical attractive power, inherent in the parts of the terrestrial globe; the noble VERULAM also, in part, embraced this opinion; and KEPLER (not without good reason) makes it a property inherent in all celestial bodies, sun, stars, planets.[200]

Kepler's physical ideas were also reflected in the popular literature. John Wilkins's frequently reprinted popular work on the Copernican theory and the new science brought Kepler's ideas on celestial dynamics to a wide audience. John Milton drew on Wilkins's work when he referred in *Paradise lost* to the Sun's "attractive virtue" as a possible cause of planetary motion. Almanacs in the middle years of the century frequently included references to Kepler's solar magnetic attractions.[201]

Objections to solar magnetism, however, were voiced by a number of Kepler's readers. William Lower had written to Thomas Harriot in 1611 that, despite his acceptance of Kepler's elliptical hypothesis, he did not "phansie those magnetical natures". Thomas Hobbes rejected Kepler's "magnetic virtue" and "friendly" and "unfriendly" sides of the planets on the grounds that since "nothing can give motion but a body moved and contiguous", Kepler's explanation smacked of the magical. J. B. Morin claimed to have investigated the existence of "magnetic fibres" empirically and rejected them in 1631. Huygens likewise rejected the role in planetary motion Kepler had assigned to the Sun.[202] The rise to importance in the middle years of the century of the mechanical philosophy, rejecting any concept involving attraction, called in question the magnetic features of the Keplerian explanation for planetary motion. Objections were raised to any possible magnetic effect from the Sun since heat is destructive of magnetism.[203] In pursuit of answers to the problem that Kepler had so vigorously enunciated, the key Aristotelian component in Keplerian dynamics, that everything moved requires a mover, and Kepler's "solar virtues" and "magnetic fibres" were successively pared away. In their place, non-Aristotelian elements were developed, modified and built upon; the essential element was the unity between celestial and terrestrial attractive forces and the role of mass — or in seventeenth-century parlance, "quantity of matter" — in attraction.[204]

Two aspects of Kepler's celestial dynamics have sometimes been confused. For Kepler the ability to predict position alone was inadequate as explanation, for that might have been achieved by the use of 'fictional' mathematical models. His concept of causality required a link between predicted position and actual path, which in turn required an explanation proper to natural philosophy, i.e. as caused by innate or external sources of motion. The astronomers who accepted ellipse-cum-non-uniform motion accepted only one part of Kepler's realist programme, the part that dealt with real orbits and the concomitant elimination of eccentrics and epicycles. Accepting Kepler's ellipse and unequal motion can still be limited to kinematics, but nonetheless represent a realist position. This may be seen in the failure by post-Keplerians to distinguish between Kepler's inverse-distance rule and his area rule. Distances must be calculated for verification in either case. But the point is that they both make real the apparent non-uniform motion on the assumption of a solar role in moving the planets. One could reject Kepler's physical causes and still be a realist with respect to planetary path and non-uniform motion. Possibly the partial rejection of Aristotelian principles of motion had made it easier. Halley's paper in the *Philosophical transactions* of 1676, which attempted to determine elements of an orbit without any physical assumptions, by assuming elliptical orbits, was itself based on an underlying physical assumption. Kepler's equant represented a radical break in conception from Ptolemy's, which had been conceived as merely a useful and necessary mathematical device. The followers of Kepler also thought of it as a device, but one reflecting the reality of unequal motion. No one thought of the ellipse and unequal motion as mere calculating devices. They were the means of uniting the two ancient and distinct traditions, cosmological and astronomical, a description and physical explanation of actual planetary motions as well as the best means of 'saving the appearances'.[205]

Those who saw problems in Kepler's physical cause of quasi-magnetic forces adopted mechanical analogies from whirlpools, the pendulum, projectiles and falling bodies. They included Horrocks, Beeckman, Campanella, Thomas White and Descartes.[206] After the appearance of the Cartesian system of the world, some linked Keplerian and Cartesian conceptions and presented systems in which both were features.[207] In the early 1660s Isaac Newton, citing Descartes's *Principles of philosophy*, inquired "Whither ⊙ move ye vortex by his beames".[208] Descartes, however, neither states nor implies that the Sun functions as the cause of the vortex or of the planetary motions. Years later, Newton opened his treatise the *System of the world* with a brief account of the opinion of the ancients regarding the cause of planetary motion. He then went on to note that "the later philosophers pretend to account for it either by the action of certain vortices, as *Kepler* and *Descartes*; or by some other principle of impulse or attraction ...".[209] There seems to have been an identification of Kepler and Descartes to the extent of merging their conceptions and ignoring the differences in the light of the important similarities.

A turning-point had come with the work of Borelli, who added a new dimension to the discussion.[210] Familiar with the work of Kepler, Galileo and Descartes, Borelli exhibits influences from all three.[211] Borelli avoided the word *attraction* and its implications, using instead a tendency of the celestial bodies to approach the Sun, or the satellites their primaries, analogous to the tendency of heavy bodies to fall or iron to move toward a magnet. This tendency is counterbalanced by a tendency to recede from the centre of revolution, as does a stone whirled in a sling. This illustrates the use of all sorts of mechanical analogies such as the use of the lever, sling and pendulum in the wake the breakdown of the Aristotelian distinction between the heavens and the Earth. Borelli was proposing a dynamic equilibrium in which a planet, if undisturbed, tends to return to a mean state, as with the pendulum. Since a planet moves faster when in proximity to the Sun, stronger centrifugal forces are generated; the centrifugal force varies inversely as the distance, but the tendency toward the centre remains constant. The two forces alternately predominate and this accounts for the elliptical orbit. The novelty of Borelli's contribution lies in his adaptation of the Cartesian conception that celestial motions generate centrifugal forces, a concept not found in Copernicus, Kepler or Galileo.[212]

Borelli differs from Descartes in his tendency toward the centre, and from Kepler in that the tendency is unmodified by distance, nor was it identified with gravity. He further differs from Kepler in substituting the material light rays from a rotating Sun for Kepler's immaterial motive virtue as the means of planetary propulsion, a concept Kepler had rejected because of the failure of the planets to cease or slow their motions when occulted. The motive rays of both Kepler and Borelli are based on the principle of the lever or balance, the centre of rotation of which is the

Sun or primary planet. The longer the lever-ray, the weaker its action; more exactly, it is inversely proportional to distance. The basic difference in the celestial mechanics of Kepler and Borelli is that the latter accepted the principle of inertia (or conservation of velocity). Both explained the motions of the planets, however, on the basis of the solar rotation, the revolutions of the satellites by the rotation of their primaries. But in Borelli, the Sun also affects the motions of the satellites and accounts for "supplementary" anomalies in their motions.[213] Since Borelli understood motion to persist once it had begun without the need for continued application of an impulse, one might wonder why he required solar rays as pushing forces at all. Westfall has suggested that in the new approaches to dynamic phenomena in the aftermath of the destruction of Aristotelian physics, there was a confusion between impact and the persistence of motion in the Galilean or Cartesian sense.[214] In any case, Kepler, still tied to an Aristotelian concept of motion, required both attraction and propulsion. The need for propulsion would eventually be eliminated with adoption of the principle of inertia.

Borelli's work and the availability of Horrocks's manuscripts in the 1660s marked a shift in the Royal Society away from the Gilbertian-Keplerian tradition of a role for magnetism in the explanation of planetary motion and toward the use of analogies from mechanics. Christopher Wren and Robert Hooke were among the first to resolve the dynamics of planetary motion into central attractive and tangential inertial forces, Hooke generalizing the concept of attraction to a greater extent than had been the case until then.[215] A new stage in thinking about celestial physics had been reached, and in the 1670s attention began to be focused on the desirability of quantifying celestial forces, "endeavours" or "tendencies".[216]

Kepler had been proud of his achievement in eliminating the fictitious mathematical devices from astronomy, which, if they were to describe the actual motions of the planets, would yield, in the words of his successors, a farrago of helices, figments, superfluities, feigned suppositions, fictitious circles and whimsies.[217] This point was made by almost every one of those who accepted two of the fundamental points of Kepler's transformation of astronomy, the elliptical orbits and non-uniform motion. The most common term employed was "figment", derived from the Latin *fingere*, to shape, mould an image, figure or model of something; in another connotation, something invented, fraudulent or arbitrary, i.e. not real, a product of art, not nature. Epicycles, with the loops and helices they traced, were always known to be 'figments' in this sense. After Kepler, they are often cited as figments in contrast to the true path found by Kepler.[218] Vincent Wing provides a representative passage. Citing Kepler's *Epitome*, *Mysterium cosmographicum* and *Astronomia nova*, he praises Kepler for having

> freed himself of those enormious Engines and figments of the Peripateticks ... and thereupon he became the most absolute Instaurator of Astronomy that the World afforded, as evinced from those *Ephemerides* and Calculations streaming from thence ....[219]

Aiding the acceptance of the Keplerian ellipse may have been its (pre-Newtonian) reentrant nature, conforming to the argument of Copernicus in favour of circles. The criterion of simplicity, expressed in the oft-cited maxim that God does nothing in vain, was doubtless a factor. It was cited by Hooke in 1662 as

> the reason why the Copernican [theory] has obtained with all the modern and best Astronomers against all the other, as being the most Simple, and the least incumber'd of any; especially as it is improved by the incomparable Kepler.[220]

NEWTON AND AFTER

Much has been written about the manner in which Newton became acquainted with Kepler's ideas. He seems not to have owned or read any of the astronomer's works, nor does Kepler's name appear in any of his early manuscripts.[221] Beginning in the 1660s he learned of Kepler's ideas and their modifications by Boulliau, Ward and Horrocks by reading Vincent Wing's *Harmonicon coeleste* (1651) and Thomas Streete's *Astronomia Carolina* (1661). After the publication of Nicolaus Mercator's *Institutionum astronomicarum* (1676), Newton appears to have consulted it regularly. He became acquainted with Kepler's harmonic rule by 1664 or early 1665, when it was clearly stated in one of his notebooks.[222] From it, two decades later, he claimed to have deduced an inverse-square law of attraction.[223] In 1669 he entertained some doubts of its exactitude, as had Wing in his *Astronomia Britannica* of 1669.[224] Some time before 1684, however, Newton accepted Kepler's third law as having been empirically confirmed by Kepler and Boulliau.[225] He learned of the ellipse and area law before 1676, since some time before that he had developed a procedure for calculating tables based on areas within ellipses.[226] Newton's notes on the endpapers of Wing's *Astronomia Britannica*, while questioning the ellipticity, show his awareness of Kepler's empty-focus theory, of the position of the Sun in the planes of the planetary orbits and of their oval shapes.[227]

Despite Propositions I–XI in Book I of the *Principia* dealing with all three of Kepler's laws, nowhere in Book I is Kepler's name mentioned in any of the three editions published in Newton's lifetime. In Book III, however, the third law is mentioned in connection with Kepler, where it is also characterized as applying to the satellites. Newton called the Keplerian version of heliocentrism the "Copernican hypothesis" in the *Principia*, although in drafts and correspondence he referred in various ways to the hypothesis linking Kepler and Copernicus.[228] In one manuscript he even struck out the name Kepler, which he had originally written, and wrote in "some men".[229]

In the first edition of the *Principia*, Newton refers to Kepler's second and third laws as "hypotheses", perhaps because he had shown that the principle of universal gravitation had demonstrated that they must be modified. In the second and third editions of 1713 and 1726, Newton calls all three laws "Phaenomena", which Halley had done earlier in his review of the *Principia* for the *Philosophical transactions*.[230] In the 1687 edition of the *Principia*, Newton referred to the second law as "Propositio est Astronomis notissima" (Hypothesis VIII) and in later years referred to the law of ellipses as a "proposition" and the second law as a "notion".[231] After the *Principia*, none of Kepler's laws was seen by Newton as having been empirically established by Kepler. Since he had been in doubt about the elliptical orbit, he was compelled to doubt the second law, since the shape of the orbit must be known to compute the areas. He took as his starting point in the *Principia* Kepler's second law as an approximation to "empirical truth".[232] In an oft-quoted passage in a letter to Halley on 20 June 1686, Newton wrote that "Kepler knew ye Orb to be not circular but oval & guest it to be Elliptical" and "guest right".[233]

Newton's assertion in the *De motu* of 1684 that he had proved in the Scholium that "the major planets revolve in ellipses having a focus in the centre of the sun [and] describe areas proportional to the times, entirely [or exactly] as Kepler supposed" is not quite true, since it assumes a one-body system and mass-points and works only as a mathematical construction.[234] In the manuscript of that work, in a scholium to Theorem 3, he concludes, after deriving Kepler's first and second laws, that they were "exactly as Kepler supposed". An undated manuscript entitled "Phaenomena" Newton wrote between 1687 and 1713, says the planets move in "Ovals about the Sun placed in the inferior node of the Oval" and cites the area rule. "Kepler by an elaborate discourse has proved this in the planet Mars & Astronomers find that it holds true in all the primary Planets."[235]

For Newton, Kepler's proof of the area law, however, was faulty; he had worked out the oviform orbit for Mars and only "guessed" it to be an ellipse and applicable to the other planets. Newton felt he was therefore justified in claiming credit for the first two laws and had been bold and imaginative in showing their physical meaning and conditions of mathematical generality. This is what was novel in 1687.[236] In a letter to Richard Towneley, 4 November 1686, Newton credited Kepler with the area rule and the third law, and insisted that Kepler had not explained either of them.[237] Newton felt that only he had explained them by demonstrating how they were demonstrable for all the planets on the assumptions of a principle of inertia and a centripetal inverse-square law.[238] The theory of universal gravitation was to follow shortly thereafter.[239] I. B. Cohen suggests that Newton was "trying as hard as he could to mark off the true character of his own discovery of the inverse-square law from 'guesses' made by Hooke, or — for that matter — by many others". Despite all this, Newton recognized that his own work had been made possible by Kepler's.[240]

Newton was careful to distinguish his own principle of inertia from Kepler's. In the *Epitome* Kepler's use of the term had both Aristotelian and anti-Aristotelian connotations. In the *Astronomia nova* he had asserted that the planets have "an inherent tendency for rest or an absence of motion".[241] In the *Epitome*, citing the absence of planetary spheres, and having dropped his earlier idea of planetary souls, Kepler asserted that planets as material bodies are incapable of moving except under the application of an external force.[242] Kepler remains an Aristotelian in his retention of the principle that everything moved must have a mover. Yet he had certainly left the Peripatetic camp through his conviction that the same principles apply in the heavens and on the Earth and in the absence of a distinction between natural and violent motion. In his posthumous *Somnium* Kepler reiterated his point and added the density of the planets as a factor in their resistance to being moved.[243]

In his attempt to cope with the implications for an Aristotelian cosmos of a moving Earth, Kepler put forward ideas about matter and the ether which were novel and a starting point for later conceptions, eventually becoming part of Newton's physical ideas. Kepler used the word *moles*, usually translated as 'bulk', in an early manuscript; he later described it as inertial resistance to motion.[244] He further defined *moles* in a manner which became standard in the seventeenth century, as proportional "to the bulk of the body and the density of its matter", what Newton would call "quantity of matter", the product of density and volume. With respect to the medium in which the planets moved, Kepler held the ether to be a million times rarer than air and therefore offering very little and possibly no resistance to planetary motion. The resistance (*vis inertiae*) of the planets themselves is much greater.[245] With the creation of his celestial mechanics, Newton would be compelled to address the resistance of the ether and its rarified nature. In addition, examining Kepler's laws as he had derived them in relation to Cartesian vortices in Book II of the *Principia*, Newton found them incompatible and thus presented a problem for Cartesians which they were forced to address in succeeding years.[246]

Newton's eighteenth-century *epigones*, Keill, Halley, Gregory, MacLaurin, Pemberton *et al.*, seem to have rejected Newton's characterization of Kepler's laws as "non-empirical" and held them to be the empirical foundation of Newton's derivation of the inverse-square law.[247] Since, through the influence of Newton, an empirical foundation came to be thought necessary for induction of natural laws, historians before the twentieth century came to think of Kepler ellipses as having been empirically validated by Kepler and that they should therefore have been accepted by Kepler's contemporaries. In the post-*Principia* period, it is to Colin MacLaurin that we owe the first full recognition of the true achievement of Kepler and its relation to Newton's masterpiece.

> To the admirable *Kepler* we owe the discovery of the true figure of the orbits, and the proportions of the motions of the solar system: but the philosophical improvement of these phenomena was reserved for Sir *Isaac Newton*.[248]

MacLaurin seems to have familiarized himself with the body of Kepler's work; he described Kepler's use of the five solids and had read all the major works. He gently criticized Kepler's speculations based on the application of mathematical analogies. He never used the word 'laws' or any similar characterization. He cites Kepler approvingly on the concept of attraction and also on his physical astronomy, and praises Kepler for his conviction concerning future astronomical progress.[249] With Newton a new phase in the fate of Kepler's laws had been reached. At the moment of their triumph as descriptions of motions of the planets on new fundamental physical principles, they were shown to be imprecise as implied by the law of gravitation.

SUMMARY AND CONCLUSION

It is now clear that Kepler's ideas were more widely known than had been thought for two hundred years prior to the middle of our own century. The reception of his laws has been explored in detail, the fate of his ideas concerning the position of the Sun in the orbital planes and his harmonics less so. The role of the *Rudolphine tables* in gaining conviction for elliptical orbits has been well established, and the methods of approximation required by the second law are recognized as having been responsible for resistance to it. In the course of time, after the dissolution of the celestial spheres, increasing challenges to Aristotelian principles, the rise of two new independent cosmological traditions, magnetic and mechanical, and the growth of the idea that astronomy must deal with physics as well as mathematics, resistance to Kepler's ideas would lessen. The creation after mid-century of numerous models employing circles and equants was based on acceptance of fundamental Keplerian principles. Increasing demands for greater precision would begin to displace equants by the time of the composition of Newton's *Principia*. As Newton learned of Kepler's laws, he shared the position of a number of his contemporaries that ellipse and area rule had not been empirically verified, although ellipsoidal orbits and non-uniform motion were beyond question. He took the position that they were hypotheses which were only confirmed by his proof from the assumptions of the principles of universal gravitation and inertia.

Having traced the fate of his laws, historians have begun to examine how Kepler transformed astronomical inquiry, how his goals for astronomy were accepted, rejected or modified. Questions still remain, although some have been partially addressed. Why was the equant so abhorrent to the two generations following Copernicus and quite acceptable to the two generations following Kepler? In Kepler's lifetime, the Copernican theory was held by a minority of astronomers. To what extent did Kepler's elaboration of Copernican astronomy persuade astronomers to heliocentrism? During his lifetime Kepler was the chief champion of Copernican astronomy. Yet his influence on the adoption of Copernicus, by eliminating its remaining Ptolemaic remnants, has not been explored. The reception of Keplerian astronomy was intimately related to the progress of the Copernican theory. Astronomers became Copernicans for non-empirical reasons, but many became interested in Keplerian astronomy through recognition of its predictive superiority. The demand for "real orbits" and an end to "figments" led some to support Kepler and a physically grounded astronomy.

In a tradition that still persists, it is held that Kepler's achievements, and in particular what we now designate as his three laws of motion, had to wait upon Newton's genius for their recognition.[250] While it is true that their significance was enhanced and given new meaning through the creation of Newtonian celestial mechanics, it would be a mistake to ignore them in the context of the history of astronomy and cosmology. Two issues are involved: the new appreciation of Kepler's laws in a Newtonian context, and their role in the transformation of astronomy before the *Principia*.[251]

The question of whether Kepler's role was revolutionary has been raised by I. B. Cohen, who concludes that "there was no Keplerian revolution before 1687", since "he did not succeed in converting the greater part of his contemporaries and immediate successors to either his elliptical planetary astronomy or his celestial physics".[252] That this misses the mark has been shown above. His revolutionary role lay in his successful attempt to solve the problem of uniting astronomy and natural philosophy which had been sought for two thousand years. Edmond Halley understood the radical nature of Kepler's innovations and their relation to Newton's when he contrasted the approach of the ancients with that taken by Kepler.

> ...the Hypothesis of Eccentricks, and Epicycles [was] introduced by the Ancients only to represent the Motions, and that but coursely too; with the Opinion of *Ptolomee* himself thereon, that the natural Motions were otherwise performed, ought not to be valued against that elegant Theory of the planetary Motions, first invented by the acute Diligence of *Kepler*, and now lately demonstrated by that excellent Geometer Mr. *Newton* ....[253]

The astronomical revolution of the sixteenth and seventeenth centuries consisted not only in Copernicus's replacement of a geocentric by a heliocentric conception of the universe and in Kepler's substitution of ellipses for circles, but also in a new attitude toward the goals of astronomical inquiry.

> The whole tendency of the scientific revolution was to rebel against this view of the astronomer as a mathematician, a deviser of models to save the phenomena, and to see astronomy as a science comprehending the totality of knowledge concerning the heavens and the relations of the Earth to the celestial regions.[254]

Even if this is an overstatement, as I believe it to be, who did more than Kepler to initiate and promote this tendency? Norwood Hanson likewise goes too far in saying that the *De revolutionibus* could have been written immediately after Ptolemy, that "the line between Ptolemy and Copernicus is unbroken", but he is surely correct in his remark that "The line between Copernicus and Newton is discontinuous, welded only by the mighty innovations of Kepler".[255] Kepler transformed his discipline and, as Koyré pointed out, if there had been no Kepler, there would have been no Newton.[256]

Keplerian astronomy can be understood only in the context of the successive breaches in the Aristotelian fabric during the sixteenth and seventeenth centuries that led to his innovations and prepared the ground for their acceptance. It is necessary to distinguish Kepler's rules of non-uniform and elliptical motion from the technical methods appropriate and useful for the most precise determination of planetary position. Kepler, unlike his predecessors and contemporaries, thought like a physicist while doing astronomy. Astronomers, when they thought of the physics of the heavens at all, tended to do so in separate mental compartments. Is the use of circular uniform motion an axiom of cosmology or of procedure? Before Kepler it was always both. After Kepler, it remains a procedural device *of* *choice*, but no longer an axiom, of either cosmology or procedure.

Scholars have not entirely freed themselves from the study of Keplerian astronomy as a precursor of Newtonian celestial mechanics. The focus upon Newton's monumental accomplishment so as to make Kepler's achievement appear as prolegomenon obscures the nature of that achievement in its own terms. Not surprisingly, Newton's own treatment of Kepler and Galileo contributed to the failure of historians to deal with Kepler in non-Whiggish terms. The focus on Kepler's laws tends to mask his transformation of the discipline, of the new sorts of questions with which the astronomer needed to be concerned. The fixation on Kepler's role in Newton's celestial dynamics resulted in a failure to recognize that seventeenth-century astronomy has to be seen in important respects as an effort to come to terms with the Keplerian revolution.[257] In the period from Kepler to Newton, astronomers sided enthusiasticallly with Kepler, partially with him, or not at all. What has been missed in most recent research is that those objecting to Kepler's first two laws nevertheless accepted the revolutionary cosmological principles underlying them: non-uniform and non-circular motion, even when given in its pure form, or in the variously modified forms of the empty focus equant.

For the period under review, discussions of who was a 'Keplerian' has reflected some confusion. Unless the nature of an astronomer's Keplerianism is specified, there is little point in the characterization, so that to characterize Foster as a Keplerian, or Boulliau as a non-Keplerian is not very helpful.[258] Most astronomers in the seventeenth century were problem solvers, and what they were most interested in was better solutions to traditional problems and most particularly the central problem from Antiquity: the accurate prediction of planetary positions. This involved a host of traditional subsidiary problems, which were independent of cosmology. Scientists possess theoretical constructions and procedures for solving problems within those theoretical structures as well as some which are independent of them. Copernicus and Tycho Brahe made possible a new research programme based on planetary distances; Kepler was the first to seize the opportunity thus presented and to recognize the importance of its implications for a new celestial physics. The battle for Keplerian astronomy was waged not so much between 'realists' and 'fictionalists' as between those primarily interested in the construction of tables and ephemerides and those interested in cosmological or physical issues as well as in prediction and recognized the importance of their union.

In dealing with problems of reception, it is useful to distinguish the nature of the 'scientific community', a complex task with respect to the seventeenth century.[259] The nature and evolution of the astronomical community in the seventeenth century as compared to the sixteenth is a matter yet to be explored. Who and where were the 'receivers' of Keplerian astronomy? We may suppose several layers: (1) professionals, competent in theory, mathematics and observation and who published, theoretical innovators, improvers of parameters whose work had to be taken note of by others similarly engaged; (2) professors and competent amateurs — including authors of textbooks and almanacs, who were also familiar with astronomical techniques; (3) natural philosophers knowledgeable about theory, but without training or interest in detailed astronomical techniques. It would be interesting to know the distribution at different times among these groups of those with an interest in cosmological theory and those interested only in prediction of planetary position; in other words, the extent in detail of the growing conviction of the importance of the Keplerian programme for a union of astronomy and physics. It would be interesting to know more about what predisposed some and not others. Why are early seventeenth-century astronomers not impressed, as Kepler was, with the question of why individual planets move more slowly in those parts of their orbits more distant from the Sun?

Another question bearing on reception concerns the transmission of text. More could be learned about how Kepler's ideas were disseminated. Prosopographical studies might provide some useful answers. We know too little about how Keplerian astronomy was treated in the universities.[260] Throughout the seventeenth century, there existed a Keplerian "tradition" at Bologna. The expression is Russell's, but it does not make clear in what ways Keplerian astronomy was attractive to the Bolognese.[261] An investigation into the role of national styles in the reception of scientific ideas could also serve to clarify some problems. For example, were Germans more likely to be exposed to Keplerian astronomy than those living elsewhere?[262] Was Kepler more favourably received in England than on the Continent? If so, why?

In Kepler's day astronomers were still tied to the separation of disciplines. What role was played by the dissolution of the celestial spheres and the evidence of a rotating Sun in paving the way for astronomers to accept a union of astronomy and physics? Westman has pointed out that academic structures within the university served to sustain the boundary between mathematics and natural philosophy.[263] But Westman also points out that the converse is not true: why certain individuals adopted a new theory cannot be explained on grounds of a new social role or changing standards for the practitioner.[264] After Kepler, a new social role would follow not only the adoption of a new theory, but also a redefinition of the discipline.

The concentration on the reception of Kepler's laws has tended to obscure the fact that, whether astronomers accepted the first two laws or not, they accepted his research programme: the determination of the cause of planetary motion as a valid aim of astronomical inquiry and the determination of actual planetary paths and velocities. The former involved a conscious addition to the tasks of the astronomer; the latter the discarding of the ancient axioms of circles and uniform motion as constituents of cosmological explanation. This is the essence of the Keplerian achievement.

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1. See, for example, René Taton, "Sur la diffusion du Copernicanisme et les progrès de l'astronomie aux xvii[me] et xviii[me] siècles" in Suzanne Delorme *et al.* (eds), *Avant, avec, après Copernic: La représentation de l'univers et ses conséquences épistémologiques* (Paris, 1975), 295–308. An example in a popular work is Timothy Ferris, *Coming of age in the Milky Way* (New York, 1988), chaps. 4, 5.

2. Jean Pelseneer, "Gilbert, Bacon, Galilée, Képler, Harvey et Descartes: Leurs relations", *Isis*, xxvi (1932), 171–208, p. 201. Pelseneer mistakenly asserted that Kepler's works were even ignored by the Church (p. 201, n. 73). Kepler's *Epitome of Copernican astronomy*, however, was placed on the *Index* in 1619, but not because of the planet laws *per se* (Max Caspar, *Kepler*, transl. by C. Doris Hellman (New York, 1959), 298).

3. Arthur Koestler, *The sleepwalkers: A history of man's changing vision of the universe* (New York, 1959), 396. Newton did not learn of Kepler's achievements by reading Kepler, nor were they hidden away, as will be shown below. See also Alexandre Koyré, *La révolution astronomique: Copernic, Kepler, Borelli* (Paris, 1961), 363. For the earlier period, see Jean Etienne Montucla, *Histoire des mathématiques* (Paris, 1758), ii, 254; Jean-Sylvain Bailly, *Histoire de l'astronomie moderne* (Paris, 1799), ii, 210–11; Adam Smith, "The history of astronomy" (written about mid-century), in W. P. D. Wightman *et al.* (eds), *Essays on philosophical subjects ...* (Oxford, 1980), 33–105, pp. 87–88; Jean-Baptiste J. Delambre, *Histoire de l'astronomie moderne* (Paris, 1826), i, 360; *idem*, *Histoire de l'astronomie au dix-huitième siècle* (Paris, 1827), 61.

4. Representative examples include Norbert Herz, *Geschichte der Bahnbestimmung von Planeten und Kometen* (Leipzig, 1887–94), ii, 218; Pelseneer, *op. cit.* (ref. 2), 205; Ernst Zinner, *Entstehung und Ausbreitung der Coppernicanischen Lehre* (Erlangen, 1943), 331; Giorgio de Santillana, *The crime of Galileo* (Chicago, 1955), 170, n. 11; Koyré, *op. cit.* (ref. 3), 363–4, 377, n. 5; William D. Stahlman, Foreword to Robert Small, *An account of the astronomical discoveries of Kepler* (reprint of 1804 edn, Madison, 1963), p. ix; A. Rupert Hall, *The revolution in science 1500–1700* (London, 1983), 143; I. Bernard Cohen, *Revolution in science* (Cambridge, Mass., 1985), 132.

5. See, for example, the statement from the author of the well-known pejorative term for this historiographical vice — Whig history — that Kepler "has to his credit a collection of discoveries and conclusions ... from which we can pick out three that have a permanent importance in the history of astronomy" (Herbert Butterfield, *Origins of modern science, 1300–1800* (rev. edn, New York, 1962), 75–76).

6. Leibniz may be the only figure in the seventeenth century who associated Kepler in general terms with the discovery of the laws of the heavens. Leibniz, in his *Tentamen* or "Essay on the causes of the motions of the heavenly bodies", after reading a review of the *Principia* in the *Acta eruditorum* of 1689, praised Kepler extravagantly as having been the first "to publish the laws of the heavens". He there also cited all three of Kepler's "laws" accurately and based his own celestial dynamics on Kepler. See I. Bernard Cohen, "Newton and Keplerian inertia: An echo of Newton's controversy with Leibniz" in Allen G. Debus (ed.), *Science, medicine and society in the Renaissance: Essays to honor Walter Pagel* (London 1972), 192–211, p. 205, and Eric J. Aiton, *The vortex theory of planetary motion* (New York, 1972), 127. Kepler himself did not employ the term 'law' for the discoveries we now call by that name. Curtis A. Wilson characterized them most appropriately in the title of his important article, "From Kepler's laws, so-called, to universal gravitation: Empirical factors", *Archive for history of exact sciences*, vi (1970), 89–170. See also Nicholas Jardine, *The birth of history and philosophy of science* (Cambridge, 1983), 240. (In deference to established usage, quotation marks will be omitted from further references to the three discoveries known as Kepler's laws.)

7. For material published before 1975, see Eric J. Aiton, "Johannes Kepler in the light of recent research", *History of science*, xiv (1976), 77–100. Recent efforts include *idem*, "Kepler's path to the construction of his first oval orbit for Mars", *Annals of science*, xxxix (1978), 173–90; I. Bernard Cohen, *The Newtonian revolution* (Cambridge, 1980); J. Bruce

Brackenridge, "Kepler, elliptical orbits and celestial circularity: A study in the persistence of metaphysical commitment", *Annals of science*, xxxix (1982), 117–43; Bruce Stephenson, *Kepler's physical astronomy* (Berlin and New York, 1987); Yasukatsu Maeyama, "Kepler's hypothesis vicaria", *Archive for history of exact sciences*, xli (1990), 53–92; A. E. L. Davis, several articles forming the entire issue of *Centaurus*, xxxv (1992), 97–191; William H. Donahue, "Kepler's first thoughts on oval orbits", *Journal for the history of astronomy*, xxiv (1993), 71–100; *idem*, "Kepler's invention of the second planetary law", *The British journal for the history of science*, xxvii (1994), 89–102; Peter Barker and Bernard R. Goldstein, "Distance and velocity in Kepler's astronomy", *Annals of science*, li (1994), 59–73.

8. This is denied in the account by A. E. L. Davis, "Kepler's resolution of individual planetary motion", *Centaurus*, xxxv (1992), 97–102. But see James R. Voelkel, "The development and reception of Kepler's physical astronomy 1593–1609", unpubl. Ph.D. diss., Indiana University, 1994.

9. See the comments of James Spedding and Robert L. Ellis (eds), *The works of Francis Bacon, Baron of Verulam, Viscount St. Albans, and Lord High Chancellor of England* (London, 1887–1901), iii, 511, 723–6. For Bacon's scepticism regarding the Copernican system, see his *Novum organum*, Book ii, Art. xxxvi. Pascal refused to commit himself to Copernicanism, possibly out of religious scruples (Léon Brunschvicq and Pierre Boutroux (eds), *Oeuvres de Blaise Pascal*, première serie (Paris, 1908), ii, 100).

10. William Molesworth (ed.), *The English works of Thomas Hobbes of Malmesbury* (London, 1839–45), i, p. viii; vii, 101.

11. Cornelis de Waard (ed.), *Journal de Isaac Beeckman tenu de 1604 à 1634* (La Haye, 1939–53), iii, 65–66; Reijer J. Hooykaas, "Isaac Beeckman", in Charles C. Gillispie (ed.), *Dictionary of scientific biography* (New York, 1970–80; hereafter *DSB*), i, 566–8, p. 568a; Robert Lenoble, *Mersenne, ou la naissance du mécanisme* (Paris, 1943), 12; Léon Auger, *Un savant méconnu: Giles Personne de Roberval (1602–1675)* (Paris, 1962), 106–7. William Donahue (*The dissolution of the celestial spheres 1595–1650* (New York, 1981), 291) notes that Mersenne's reference to Kepler's physical mechanism was taken from Hobbes rather than from Kepler.

12. Johannes Kepler, *Prodromus dissertationem cosmographicarum continens mysterium cosmographicum* (Tübingen, 1596). A useful reprint with English translation is *Mysterium cosmographicum: The secret of the universe*, transl. by A. M. Duncan with an introduction by Eric J. Aiton (New York, 1981). The reception of the work merits further detailed study. An important beginning has been made by James R. Voelkel, *op. cit.* (ref. 8). See also Christine J. Schofield, *Tychonic and semi-Tychonic world-systems* (New York, 1981), 234–5; Donahue, *Dissolution* (ref. 11), 177.

13. Jeremiah Horrocks, *Opera posthuma* (in some copies titled *Opuscula astronomica*), ed. by John Wallis (London, 1672, 1673, 1678), 10.

14. Tycho to Mästlin, 21 April 1598; Tycho to Kepler, 9 December 1599, in Johannes Kepler, *Gesammelte Werke*, ed. by Walther von Dyck *et al.* (Munich, 1937– ; hereafter cited as Kepler, *GW*), xiii, 204–5; xiv, 94.

15. Caspar, *Kepler* (ref. 2), 68; Donahue, *Dissolution* (ref. 11), 164; Curtis A. Wilson, "The inner planets and the Keplerian revolution", *Centaurus*, xvii (1972), 205–48, p. 243; Christopher Heydon, *An astrological discourse* (London, 1650), 82–85, 96. Kepler, aware of these objections, omitted his third law from the *Rudolphine tables* in calculating planetary distances (Curtis A. Wilson, "Horrocks, harmonies and the exactitude of Kepler's third law" in Erna Hilfstein *et al.* (eds), *Science and history: Studies in honor of Edward Rosen* (*Studia Copernicana*, xvi; Wrocław, 1978), 235–58, pp. 238, 240).

16. Kepler (*Mysterium cosmographicum* (ref. 12), 41) writes that booksellers, friends and natural philosophers had been pressing for a reissue.

17. Quoted from the "Tentamen de motuum coelestium causis", *Acta eruditorum*, Feb. 1689, by I. Bernard Cohen, "Newton and Keplerian inertia" (ref. 6), 205. See also Domenico Bertoloni Meli, "Public claims, private worries: Newton's *Principia* and Leibniz's theory of planetary motion", *Studies in history and philosophy of science*, xx (1991), 415–49, p. 424, where it is

pointed out that there were "three main areas in which Kepler was important for Leibniz, namely astronomy and the laws of planetary motion, the order, regularity and essentially harmony of nature; and the role of theology in many aspects of his work ...", and that he was considered an ally in Leibniz's battles with Newton.

18. Koyré, *La révolution astronomique* (ref. 3), 457, n. 4. He thought it unlikely, however.
19. For Kepler's use of the term 'vortex', see, for example, the Introduction and chapter summaries in the *Astronomia nova*, Kepler, *GW* (ref. 14), iii, 34, 44.
20. For Descartes's use of the Keplerian terms in correspondence, see Aiton, *Vortex theory* (ref. 6), 43; Descartes, *Principles of philosophy*, transl. by Valentine R. Miller and Reese P. Miller (Dordrecht, 1983), Part iii, Art. 36. In the first edition of the *Mysterium cosmographicum* only the word *aphelion* (in Greek) appears (Duncan transl. (ref. 12), 161, 163, 183). The second edition (1621), annotated by Kepler, uses both terms, noting that he invented them. Both terms also appear in a marginal note on p. 32 of the *Astronomia nova*: "*Aphelium et perihelium quid?*", *GW* (ref. 14), iii, 93; in the chapter summaries of chaps. 28 and 50, *ibid.*, 42, 46, 49; and in Kepler's Index of Terms, in the front matter of the work. Kepler gave a new connotation to the Latin term *inertia*, applying it to the tendency of planets to remain at rest unless put into motion by a mover. The term appears in his *Epitome*, and the second edition of the *Mysterium cosmographicum*, *GW*, vii, 94, 296, 330; *Cosmographic mystery* (ref. 12), 171. See also Cohen, "Newton and Keplerian inertia" (ref. 6), 209, n. 11, and Edward Rosen, "Kepler's harmonics and his concept of inertia", *American journal of physics*, xxxiv (1966), 610–13.
21. *Principles* (ref. 20), iii, Art. 153; Kepler, *GW*, iii, chapter summary for chap. 37. Descartes may, however, have encountered the idea in Galileo or in another author. See, for example, Galileo, *Dialogue concerning the two chief world systems — Ptolemaic and Copernican*, transl. by Stillman Drake (Berkeley and Los Angeles, 1962), 453.
22. *Principles* (ref. 20), Part iii, Art. 35. Descartes likewise may have acquired this from secondary sources. The opinion of Miller and Miller (*Principles of philosophy* (ref. 20), 99, n. 31) that "Descartes seems acquainted only with Kepler's work in optics" would therefore seem to require further analysis.
23. Pelseneer, *op. cit.* (ref. 2), 181–2; J. E. McGuire and Martin Tamny, *Certain philosophicall questions: Newton's Trinity notebook* (Cambridge, 1983), 169; Aiton, *Vortex theory* (ref. 6), 43, 62, n. 60, 72; Daniel Garber, *Descartes' metaphysical physics* (Chicago, 1992), 349, n. 32; William R. Shea, *Magic of numbers and motion: The scientific career of René Descartes* (Canton, Mass., 1991), 285.
24. See, for example, Pierre Costabel, "Réception de la cosmologie nouvelle à la fin du xvii[e] siècle", *Avant, avec, après Copernic* (ref. 1), 261–6, p. 262. On Huygens and Kepler, see also Ernst Apelt, *Die Reformation der Sternkunde* (Jena, 1852), 245 and Derek T. Whiteside, "Newton's early thoughts on planetary motion: A fresh look", *The British journal for the history of science*, ii (1964), 117–37, p. 121, n. 16.
25. Christiaan Huygens, *Oeuvres complètes*, ed. by Société Hollandaise des Sciences (The Hague, 1888–1950), i, 463–4; iii, 438; viii, 376. Costabel's comment (ref. 24, *loc. cit.*) that Huygens's notes of 1682 made no allusion to Kepler's laws is misleading.
26. Huygens, *Oeuvres* (ref. 25), xxi, 124–32, 349–50.
27. *Ibid.*, xxi, 272 ff; Costabel, *op. cit.* (ref. 24), 263.
28. Alexandre Koyré, "Attitude esthétique et pensée scientifique", *Critique*, ix (1955), 835–47, p. 840. He was seconded by Giorgio de Santillana, who called it "one of the strangest mysteries of the history of natural philosophy", *op. cit.* (ref. 4), 36, n. 8, 169. Expressions of similar sentiment could doubtless be traced back to the eighteenth century.
29. Kepler, *GW*, iii, 26; Galileo, *Dialogue* (ref. 21), 462; Stillman Drake, "Galileo's theory of the tides", *Galileo studies: Personality, tradition, and revolution* (Ann Arbor, 1970), 200–13, pp. 209, 213, n. 24 (original version in *Physis*, iii (1961), 185–94); *idem*, "Galileo and the concept of inertia", *ibid.*, 240–56, p. 254 (originally in "An unpublished letter of Galileo to Peiresc", *Isis*, liii (1952), 201–11).

30. Erwin Panofsky, *Galileo as a critic of the arts* (The Hague, 1951), 23; Koyré, "Attitude esthétique" (ref. 28), 840.
31. I am preparing a detailed analysis of their relationship.
32. For the Bologna chair, see Kepler to Roffenius, 17 Apr. 1617, Kepler, *GW*, xvii, 222–4; for Wotton's letter, *ibid.*, xviii, 42. Kepler had shortly before been visited by John Donne, who had referred anonymously to Kepler in his early works (Wilbur Applebaum, "Donne's meeting with Kepler: A previously unknown episode", *Philological quarterly*, l (1971), 132–4).
33. John L. Russell, "Kepler's laws of planetary motion: 1609–1666", *The British journal for the history of science*, ii (1964), 1–24, p. 20.
34. Russell ascribes the less than enthusiastic initial reception of the *Epitome* "to the influence of Tycho Brahe, and the learned world was not much disposed to listen to" the defence of Copernicanism (*ibid.*, 7). "No other work is mentioned so frequently or, for the most part, with so much respect where planetary theory is concerned" (p. 20). The First Part, containing Books i–iii, was printed in 1617 (Caspar, *Kepler* (ref. 2), 293). The entire work was reprinted at Frankfurt in 1635. The least read of Kepler's works was the *Harmonice mundi* (Russell, *op. cit.* (ref. 33), 6). According to Kepler, the *Astronomia nova* had been issued in few copies and at a steep price; the exact number is unknown (Max Caspar, *Bibliographia Kepleriana: Ein Führer durch das gedruckte Schrifttum von Johannes Kepler* (Munich, 1936), 55).
35. Russell, *op. cit.* (ref. 33), 6–9. Russell notes that in 1615 Magini "used Kepler's laws in calculating ephemerides for Mars", but besides the acknowledgement, he gave no details.
36. Wilbur Applebaum, "Kepler in England: The reception of Keplerian astronomy in England, 1599–1687", unpubl. Ph.D. diss., State University of New York at Buffalo, 1969, chap. ii; Adam J. Apt, "The reception of Kepler's astronomy in England: 1596–1650", unpubl. D.Phil. diss., Oxford University, 1983; Wilbur Applebaum, "Wilhelm Schickard", *DSB* (ref. 11), xii, 162–3, p. 163. Schickard's little treatise on the transit of Mercury of 1631, published the following year, mentions the ellipse and the inverse-distance rule, but he had been a friend and correspondent of Kepler's and had known about the ellipses during Kepler's lifetime.
37. One was Christopher Heydon, as shown in his correspondence with Henry Briggs in 1610. Bodleian Library: MS Ashmole 242, ff. 168b–170b.
38. The second law was mentioned by Pierre Hérigone (1642), Riccioli (1651) and John Wallis (1659), with Noël Durret presenting a geometrical construction equivalent to it. Russell, *op. cit.* (ref. 33), 20–21.
39. *Ibid.*, 1, 10–19; Wilson, "From Kepler's laws" (ref. 6), 106–27. During the decade and a half from 1630 to 1645 most French astronomers accepted the idea of elliptical orbits; most English by 1655.
40. The almanac was written by Nathaniel Chauncy, son of Harvard's President (Samuel E. Morison, *Harvard College in the seventeenth century* (Cambridge, Mass., 1936), 217). Several of Vincent Wing's astronomical textbooks published in the 1650s and 1660s were owned by Harvard students (*ibid.*, 216). Donald K. Yeomans's claim ("The origins of North American astronomy — seventeenth century", *Isis*, lxviii (1977), 414–25, p. 417, n. 16), therefore, that in 1665 "Johannes Kepler's ideas were not well known in colonial America" requires some modification. He notes that in 1674, however, an almanac states that "astronomers are of the opinion (received from Kepler) that planets move in ellipses not circles".
41. Angus Armitage, *John Kepler* (London, 1955), 179–80; Russell, *op. cit.* (ref. 33), 10; Schofield, *op. cit.* (ref. 12), 189.
42. For Christopher Wren, for example, see A. Rupert Hall, "Wren's problem", *Notes and records of the Royal Society of London*, xx (1963), 140–4, p. 141. For Robert Hooke, see Hooke to Newton, 17 Jan. 1679/80, *The correspondence of Isaac Newton*, ed. by Herbert W. Turnbull *et al.* (Cambridge, 1959–77), ii, 309.
43. Kepler, *GW*, x, Precepts, chap. xx. Russell notes that "It is possible ... that the exact form was in fact known to many who never actually stated it" (*op. cit.* (ref. 33), 5). Jeremiah Horrocks used his own method of approximation to it for his lunar theory: Curtis A. Wilson, "Predictive

astronomy in the century after Kepler", in Michael Hoskin (ed.), *The general history of astronomy* (Cambridge, 1989–), ii, ed. by René Taton and Curtis Wilson, *Planetary astronomy from the Renaissance to the rise of astrophysics*, Part A: *Tycho Brahe to Newton*, 161–206, p. 198a.

44. *GW*, vii, 376ff.

45. The manuscript was written in 1640, but not published until 1662: "Venus in sole visa, seu tractatus astronomicus" in Johann Hevelius, *Mercurius in sole visus* (Gdansk, 1662), 111–45. See J. Horrocks, *The transit of Venus across the Sun*, transl. by Arundell B. Whatton (London, 1859), 204.

46. Russell, *op. cit.* (ref. 33), 1, 14. Russell names Horrocks, Holwarda, Hérigone, Riccioli, and Streete.

47. *Ibid.*, 11–12, 15; Armitage, *op. cit.* (ref. 41), 181; Pierre Hérigone, *Cursus mathematicus* (Paris, 1634–42; reprinted 1644); Giambattista Riccioli, *Almagestum novum* (Bologna, 1651; 2nd edn, Frankfurt, 1653).

48. Harriot seems usually to have had quick access to Kepler's publications. John J. Roche, "Thomas Harriot's astronomy", unpubl. Ph.D. diss., Oxford, 1977, 37, n. 1; *idem*, "Harriot, Galileo, and Jupiter's satellites", *Archives internationales d'histoire des sciences*, xxxii (1982), 9–51, p. 19.

49. Henry Stevens, *Thomas Hariot: The mathematician, the philosopher, and the scholar* (London, 1900), 122.

50. *Ibid.*, 123–4.

51. Apt, *op. cit.* (ref. 36), 193.

52. Copernicus had insisted on circles, and Kepler himself did not easily leave the circle when initial evidence from his effort to find a mathematical relationship governing planetary speed and distance from the Sun presented itself (Donahue, "Kepler's first thoughts" (ref. 7), 71, 75). Even after the ellipse, the circle retained its importance in Kepler's philosophical and theological outlook. See Brackenridge, *op. cit.* (ref. 7), 117.

53. Nathanael Carpenter, *Philosophia libera* (2nd edn, Oxford, 1622). On Longomontanus, see Russell, *op. cit.* (ref. 33), 7.

54. Fabricius to Kepler, 20 Jan. 1607, Kepler, *GW*, xv, 377. See also Tycho Brahe's letter to Kepler of 9 Dec. 1599, some years before Kepler's discovery of the elliptical orbit (*ibid.*, xiv, 94). On Brush and Shakerley, see Apt, *op. cit.* (ref. 36), 76.

55. Samuel Foster, *Miscellanies: or mathematical lucubrations* (London, 1659), 25.

56. The grip of the geoheliocentric theory in Tycho's native land was so strong that no one used the *Rudolphine tables* there until Römer moved to Copenhagen from Paris in 1681. Longomontanus's creation of tables based on a theory that was a compromise betweeen Tycho and Copernicus, and his having been a professor at Copenhagen for several decades, exerted a powerful influence. Kristian P. Moesgaard, "How Copernicanism took root in Denmark and Norway", in Jerzy Dobrzycki (ed.), *The reception of Copernicus' heliocentric theory* (Dordrecht and Boston, 1972), 116–51, pp. 126–34, 141.

57. Several of their authors may have been dissembling, Scheiner and Riccioli among them. Additional Jesuits taking a nominally Tychonic position were Inchofer, Biancani, Kircher, Polaccus, Beati, Tacquet and de Chasles (Schofield, *op. cit.* (ref. 12), 281–9).

58. Christine Schofield shows that Tycho Brahe made use of it (*ibid.*, 64). Wilson conjectures that this may have been through Kepler's influence ("From Kepler's laws" (ref. 6), 93).

59. Schofield, *op. cit.* (ref. 12), 64.

60. See, for example, William Lower in Stevens, *op. cit.* (ref. 49), 122; Jeremy Shakerley, *Anatomy of urania practica* (London, 1649), 15–16; Wilson, "From Kepler's laws" (ref. 6), 94.

61. Wilson, "Predictive astronomy" (ref. 43), 166b.

62. Wilson, "Kepler's laws" (ref. 6), 105. The problem earlier had arisen from the use of very small apertures, which, with the resulting diffraction, yielded diameters that were too large. Riccioli

now used Kepler's method, requiring a larger aperture whose diameter is subtracted from the diameter of the solar image. After many observations between 1661 and 1665, his figures yielded an eccentricity of 0.0169, almost one-half the eccentricity of the equant using Ptolemaic procedures. Similar confirmations were made by Grimaldi, Cassini and Flamsteed. Wilson, "Predictive astronomy" (ref. 43), 161b, 167, 185.

63. Letter of 30 Oct. 1607, Kepler, *GW*, xvi, 71. On resistance to Kepler's physical ideas, see Fritz Krafft, "The new celestial physics of Johannes Kepler", in Sabetai Unguru (ed.), *Physics, cosmology and astronomy, 1300–1700* (Dordrecht, 1991), 185–227.

64. Letter of 21 Dec. 1616, Kepler, *GW*, xvii, 187.

65. Crüger to Philipp Müller, 1 Jul. 1622, Kepler, *GW*, xviii, 92.

66. Robert A. Hatch, *The collection Boulliau (BN, FF. 13019–13059): An inventory* (Philadelphia, 1982), p. xxix.

67. Fritz Krafft, "Sphaera activitatis — orbis virtutis. Das Entstehen der Vorstellung von Zentralkraften", *Sudhoffs Archiv*, liv (1970), 113–40, pp. 134–5; Martha R. Baldwin, "Magnetism and the anti-Copernican polemic", *Journal for the history of astronomy*, xvi (1985), 155–74, pp. 159–60, 168–9.

68. Quoted in Kircher's Latin in Fritz Krafft, "Keplers Beitrag zur Himmelsphysik", in Fritz Krafft *et al.* (eds), *Internationales Kepler-symposium Weil der Stadt 1971* (Hildesheim, 1973), 55–139, p. 134.

69. Michael Hoskin and Christine Jones, "Problems in late Renaissance astronomy", in *La soleil à la Renaissance: Sciences et mythes* (Brussels, 1965), 21–31, p. 26.

70. For Lower, see Stevens, *Hariot* (ref. 49), 121; for Bainbridge, see Apt, *op. cit.* (ref. 36), 195; for Holwarda, see Wilson, "Predictive astronomy" (ref. 43), 166b; for Horrocks, Wilbur Applebaum, "Between Kepler and Newton: The celestial dynamics of Jeremiah Horrocks", *Actes du xiii^me^ Congrès International d'Histoire des Sciences 1971* (Moscow, 1974), iv, 292–9; Shakerley, *op. cit.* (ref. 60), 15–16. Kepler believed that the rate of the Earth's diurnal rotation and annual revolution fluctuated, which was denied by Horrocks and Holwarda.

71. Otto Neugebauer, "Notes on Kepler", in Arthur Beer and Peter Beer (eds), *Kepler: Four hundred years* (*Vistas in astronomy*, xviii (1975)), 781–5, pp. 781–2. See also William H. Donahue, "Kepler's fabricated figures: Covering up the mess in the *New Astronomy*", *Journal for the history of astronomy*, xix (1988), 217–37. Donahue's conclusions are challenged in Volker Bialas, "Keplers komplizierter Weg zur Wahrheit: Von neuen Schwierigkeiten die 'Astronomia nova' zu lesen", *Berichte zur Wissenschaftsgeschichte*, xiii (1990), 167–76.

72. Letter of 4 Feb. 1605, Kepler, *GW*, xv, 149.

73. Briggs to Archbishop Ussher, August 1610, Richard Parr, *The life of the most reverend father in God, James Usher* (London, 1686), 12.

74. Briggs to Kepler, 20 Feb. 1625, Kepler, *GW*, xviii, 225–5, 229.

75. Henry Bourchier to Archbishop Usher, 26 Mar. 1629, Parr, *op. cit.* (ref. 73), 404.

76. *Ismailis Bullialdi astronomia Philolaica* (Paris, 1645).

77. Ismaël Boulliau, *Philolai sive dissertationis de vero systemate mvndi, libri iv* (Amsterdam, 1639).

78. Hatch, *Collection Boulliau* (ref. 66), pp. xxviii–xxix.

79. Boulliau reaffirmed his position in a third work, *Astronomia philolaica fundamenta clarìus explicata* (Paris, 1657), 5.

80. Aiton characterizes Boulliau's approach as Platonic as does Russell; Wilson as Aristotelian. Both characterizations are just, as they refer in the one case to the role of geometry, in the other, to the separation of disciplines. Aiton, *Vortex theory* (ref. 6), 91; Russell, *op. cit.* (ref. 33), 16; Wilson, "From Kepler's laws" (ref. 6), 109, n. 74.

81. *Astronomia Philolaica* (ref. 76), 3–7, 21–24.

82. On Boulliau's system, see Hatch, *op. cit.* (ref. 66), Introduction; Wilson, "Predictive astronomy" (ref. 43), 172–3; Carl B. Boyer, "Ismael Boulliau", *DSB* (ref. 11), ii, 348–9, p. 349; *idem*,

"Notes on the epicycle and the ellipse from Copernicus to Lahire", *Isis*, xxxviii (1947), 55–56; Delambre, *Histoire* (ref. 3), ii, 146–50; John L. E. Dreyer, *History of astronomy from Thales to Kepler*, rev. by William H. Stahl (2nd edn, New York, 1953), 420. Delambre mistakenly says Boulliau gave no reason for his rejection of Kepler's second law (*op. cit.* (ref. 3), ii, 147).

83. Russell, *op. cit.* (ref. 33), 19; Wilson, "Predictive astronomy" (ref. 43), 176.
84. Russell, *op. cit.* (ref. 33), 18. See also Aiton, *Vortex theory* (ref. 6), 91. This seems an oversimplification, as some employed Boulliau's modification of the equant, but were "physicists" as well.
85. "Horrocks was a genius of the same stamp as Kepler. He appeared to have the same imagination and ... he joined to it the same perseverance in calculation" (Delambre, *Histoire* (ref. 3), ii, 499). See Wilbur Applebaum, "Jeremiah Horrocks", *DSB* (ref. 11), vi, 514–16.
86. Horrocks, *Opera posthuma* (ref. 13), 8, 181–2.
87. Cambridge University Library: Royal Greenwich Observatory MSS, Flamsteed papers, lxviii, Horrocks, "Philosophicall exercises", 1. The manuscript appears to have been begun about mid-1637.
88. *Ibid.*, 23.
89. Letter of 24 Apr. 1637, Horrocks, *Opera posthuma* (ref. 13), 276.
90. *Ibid.*, 35, 60. Horrocks, *Transit of Venus* (ref. 45), 204. Koyré is therefore mistaken in asserting (*Révolution astronomique* (ref. 3), 458, n. 8) that Horrocks defended Kepler only in generalities and failed to mention the second and third laws.
91. He was introduced to Kepler's tables by his friend and correspondent William Crabtree, who lived near Manchester (Wilbur Applebaum, "William Crabtree", *DSB*, iii, 547–8). For Horrocks's conviction of the superiority of Kepler's tables, see his letter of 3 Jun. 1637, *Opera posthuma* (ref. 13), 287.
92. Curtis Wilson, "On the origin of Horrocks's lunar theory", *Journal for the history of astronomy*, xviii (1987), 77–94.
93. A few astronomers had learned of it earlier. See Wilbur Applebaum and Robert A. Hatch, "Boulliau, Mercator and Horrocks's *Venus in sole visa*: Three unpublished letters", *Journal for the history of astronomy*, xiv (1983), 166–79.
94. Robert Hooke, "Cometa or, remarks about comets", in *The Cutler lectures of Robert Hooke*, ed. by Robert T. Gunther (*Early science in Oxford*, viii; Oxford, 1931), 217–71, p. 252.
95. Olaf Pedersen, "Some early European observatories", in Arthur Beer and Peter Beer (eds), *The origins, achievement and influence of the Royal Observatory, Greenwich: 1675–1975* (*Vistas in astronomy*, xx (1976)), 17–28, pp. 24–25; Wilson, "Predictive astronomy" (ref. 43), 168a. Jeremiah Horrocks paid considerable attention to the accuracy of his angle-measuring devices, observing conditions, sources of observational error, and the need to compensate for atmospheric refraction and ocular parallax (*Opera posthuma* (ref. 13), *passim*).
96. Among the the most important was the correspondence maintained by Boulliau with astronomers in Germany, Poland, Italy and England, as well as in France. Boulliau and Hevelius wrote to one another over several decades during the middle of the century (Hatch, *Collection Boulliau* (ref. 66), pp. xvii, xxxii, n. 44; xlix).
97. Volker Bialas, "Ephemerides in the early 17th century", *Vistas in astronomy*, xxii (1978), 21–26, p. 25, n. 6.
98. Russell, *op. cit.* (ref. 33), 7–8.
99. It was the only "published and usable observation" (Albert Van Helden, "The importance of the transit of Mercury of 1631", *Journal for the history of astronomy*, vii (1976), 1–10, p. 3). See also Bernard Rochot, "Pierre Gassendi", *DSB*, v, 284–90, p. 285a.
100. Russell, "Kepler's laws" (ref. 33), 10–11. Wilbur Applebaum, "Wilhelm Schickard", *DSB*, xii, 162–3.
101. Wilson, "Predictive astronomy" (ref. 43), 165a. See also *idem*, "Inner planets" (ref. 15), 242–4.

All other tables were off by several degrees of longitude (*idem*, "From Kepler's laws" (ref. 6), 100).

102. Noël Durret, *Nouvelle théorie des planètes* (Paris, 1635). His accompanying *Supplementi tabularum Richelianarum pars prima* were for the most part translations of Lansberge (Owen Gingerich, "Kepler's place in astronomy", in Beer and Beer (eds), *Kepler* (ref. 71), 261–78, p. 272, n. 8).
103. Hatch, *Collection Boulliau* (ref. 66), p. xxxvi, n. 62; Wilson, "Inner planets" (ref. 15), 243. The quotation is from the *Astronomia Philolaica*, 355 as transl. by Wilson, "From Kepler's laws" (ref. 6), 100.
104. Apt, *op. cit.* (ref. 36), 86–87.
105. Horrocks, *Opera posthuma* (ref. 13), 306; Mordechai Feingold, *The mathematician's apprenticeship* (Cambridge, Mass., 1984), 156–7; Wilson, "From Kepler's laws" (ref. 6), 101.
106. John Digby wrote from Paris in 1656 that he could not obtain a copy of Kepler's ephemerides; six months later he was successful in borrowing a set, which he thought might be the only one in Paris (Historical Manuscripts Commission, *Eighth report* (London, 1881), part i, vol. vii (append.), 219b). Digby may have meant the ephemerides of Andreas Argoli or Lorenz Eichstadt, which were based on the *Rudolphine tables* (Wilson, "Predictive astronomy" (ref. 43), 187–9). The last year for which Kepler calculated ephemerides was 1636; two versions of Kepler's tables were published in 1650, one in 1657 and an English version in 1676 (Caspar, *Bibliographia Kepleriana* (ref. 34)).
107. [Henry Oldenburg], "Observations made in several places ...", *Philosophical transactions of the Royal Society*, ii (1667), 295–7. "Kepler's planetary positions were generally about thirty times better than any of his predecessors' ..." (Owen Gingerich, "Ptolemy, Copernicus, Kepler" in Mortimer J. Adler and J. Van Doren (eds), *The great ideas today* (Chicago, 1983), 137–80, p. 179).
108. Small, *op. cit.* (ref. 4), 297; Dreyer, *op. cit.* (ref. 82), 393.
109. Joseph Moxon, *A tutor to astronomy and geography* (London, 1659), 268.
110. Armitage, *Kepler* (ref. 41), 166.
111. See Yasukatsu Maeyama, "On the order of accuracy of Kepler's solar theory", in Beer and Beer (eds), *Kepler* (ref. 71), 769–80, p. 780.
112. On Durret, see Wilson, "Predictive astronomy" (ref. 43), 165b. Examining 26 calculations for Saturn from the *Rudolphine tables* and comparing them with a modern ephemeris, Wilson finds an average error of 3′21″ with some amounting to 12′ or 13′ ("From Kepler's laws" (ref. 6), 102, n. 40). James Gadbury, *Ephemerides of the celestial motions* (London, 1652), sig. b5b; he reiterated his complaint in his ephemerides published in 1672. See also Delambre, *Histoire moderne* (ref. 3), ii, 456; Albert Van Helden, "Huygens and the astronomers", in Henk J. M. Bos *et al.* (eds), *Studies on Christiaan Huygens* (Lisse, 1980), 147–65, p. 165, n. 76.
113. Flamsteed to the Royal Society, 24 Nov. 1669, Stephen P. Rigaud and Stephen J. Rigaud (eds), *Correspondence of scientific men of the seventeenth century* (Oxford, 1841), ii, 89.
114. Letter to Collins, 5 May 1673, *ibid.*, ii, 163.
115. Flamsteed to Seth Ward, 31 Jan. 1679/80, in Francis Baily, *An account of the rev'd. John Flamsteed* ... (London, 1835), 121–2.
116. Wilson, "Predictive astronomy" (ref. 43), 171b. Using observations of parallaxes of Mars in opposition and of Venus near inferior conjunction and conjectures about the relative sizes of the planets in sequence from the Sun as well as the actual size of the Sun compared to the planets, Horrocks claimed that a reduction in solar parallax gives better results for lunar and solar eclipses and yields better elements for the planets, particularly Venus (*ibid.*, 167b–169a).
117. Thomas Streete, *Astronomia Carolina* (London, 1661), 12. See also Wilbur Applebaum, "Thomas Streete", *DSB*, xiii, 96. Huygens and Picard at the Académie were also familiar with the *Venus in sole visa* and accepted the necessity of a reduction in the solar parallax. In the 1670s Cassini and Flamsteed had, through determinations of the Martian parallax, reduced

the solar parallax to less than 10″. Flamsteed even thought it likely that it could be as small as 7″. Flamsteed to Collins, 20 Feb. 1672/73, Rigaud, *Correspondence* (ref. 113), ii, 160; Wilson, "Predictive astronomy" (ref. 43), 176a, 189a; Eric Forbes, "Early researches of John Flamsteed", *Journal for the history of astronomy*, vii (1976), 124–38, p. 129; Maeyama, "Kepler's hypothesis vicaria" (ref. 7), 87.

118. Wilson, "Predictive astronomy" (ref. 43), 171a.

119. Among the published tables based on the *Rudolphine* were those of Noël Durret (Paris, 1639), Vincent Renieri (Florence, 1639), Maria Cunitia (Oels, 1650), J. B. Morin (Paris, 1650; London, 1675, 1676), T. Streete (London, 1661), H. Coley (London, 1675), N. Mercator (London, 1676) (Owen Gingerich, "Kepler", *DSB*, vi, 289–312, p. 308a). For English almanacs and ephemerides based on Kepler, see Applebaum, "Kepler in England" (ref. 36), 130–1.

120. Hatch, *Collection Boulliau* (ref. 66), p. xxvii.

121. Wilson, "Kepler's laws" (ref. 6), 100, citing *Astronomia Philolaica*, 354–92. Boulliau adopted Kepler's fixed inclinations of planetary orbits as simplifying the problem of the latitudes (*ibid*, 110).

122. Boulliau to Huygens, in Huygens, *Oeuvres* (ref. 25), ii, 492. At the end of the *Philolaic tables*, Boulliau, possibly having had second thoughts about the accuracy of his own figures for Mars compared to Kepler's, inserted a table from Kepler behind his own. Modifying Kepler's eccentricity for Mars in 1657, Boulliau obtained a slight improvement, making his tables slightly better than Kepler's. Hatch, *Collection Boulliau* (ref. 66), p. xlvii.

123. *Ibid.*, note 141; Boulliau's calculating procedures were used by Streete and Wing, and Mercator adopted Boulliau's before developing his own hypothesis. Shakerley's *Tabulae Britannicae* were essentially the *Philolaic tables* calculated for London and the Julian calendar, as were the tables of John Newton's *Astronomia Britannica* (Wilson, "Predictive astronomy" (ref. 43), 176b).

124. Flamsteed to the Royal Society, 24 Nov. 1669, in Rigaud, *op. cit.* (ref. 113), ii, 88. See also John Newton, *Cometographia, or a view of the celestial and terrestrial globes* (London, 1679).

125. Testing those corrected positions against Tuckerman's ephemeris indicates their accuracy to "within 2′ — and frequently to within less than 1′ — of arc" (Wilson, "Predictive astronomy" (ref. 43), 168b).

126. Mercator's *Institutionum astronomicarum* of 1676 was Englished and included in William Leybourn's *Cursus mathematicus* ... (London, 1690). The passage is cited from the latter work, p. 803.

127. The subject of the non-uniformity of planetary motion before Kepler could benefit from close examination and clarification. It is not always clear whether a particular astronomer is violating Aristotelian precepts about uniform motion, or referring to apparent motion, or to a description of a geometrical model employing epicycles or equants which was not meant to reflect physical reality. At any event, Kepler's eventual insistence on non-uniform motion in 'simple' orbits was unique.

128. The problem was first posed in the *Astronomia nova* (*GW*, iii, 381).

129. The general form of 'Kepler's Problem' was presented by Christopher Wren in a broadside printed in 1659 and is reproduced in Hall, "Wren's problem" (ref. 42), 142–3. Other mathematicians offering solutions, either geometrical or analytical, were Boulliau, Seth Ward, John Wallis, James Gregory, Newton, John Keill, John Machin and Euler. See Russell, "Kepler's laws" (ref. 33), 3; John Ward, *Lives of the professors of Gresham College* (London, 1740), 97; Herbert W. Turnbull (ed.), *James Gregory tercentenary volume* (London, 1939), 220, n. 4; Newton, *Correspondence* (ref. 42), i, 149, n. 4.

130. Stevens, *op. cit.* (ref. 49), 123. 'Atechnies' is a term used by Kepler (in Greek) in chap. xlviii of the *Astronomia nova* (*GW*, iii, 310) to characterize the complex methods he had employed in an early version of the area rule. He was not enamoured of them either.

131. Wilson, "Predictive astronomy" (ref. 43), 174b–175a. Wilson also notes that the demand by Boulliau and others for a direct method of deriving true anomaly from mean anomaly "seems

associated with a kind of neo-classic purism; astronomy is taken to be both a mathematical art and an esoteric science of quasi-divine things, and the astronomer becomes a supreme *artifex*, following strict rules that are imposed both by the nature of the art and by the supposedly sublime nature of the celestial objects" ("From Kepler's laws" (ref. 6), 115).

132. John Newton, *Astronomia Britannica* (London, 1657), sig. $A_2$6. For Flamsteed, see Edward Sherburne, *The sphere of Marcus Manilius* (London, 1675), 84–85.

133. Koyré, *Révolution astronomique* (ref. 3), 130, 444 n. 103, 495; Whiteside, "Newton's early thoughts" (ref. 24), 124. For Hooke, see Hooke to Newton, 6 Jan. 1680, *Correspondence* (ref. 42), ii, 309. Thoren avers that the area rule was "both known and appreciated by most of the astronomers of the period" and concludes that in the 1660s and 1670s there existed a "reluctant" belief in the area rule, but that its difficulty of application led to its neglect in practice, but also to its omission from the works of the period (Victor Thoren, "Kepler's second law in England", *The British journal for the history of science*, vii (1974), 243–56, pp. 243–4, 255). This appears to ignore the persistent confusion between the area and inverse-distance rules until the early 1670s.

134. Ptolemy had not applied the equant to the solar orbit. The strategy, tactics and reasoning employed by Kepler in his struggles with the Martian orbit have been elaborated in great detail in the several works cited in ref. 7 above.

135. Kepler, *GW*, x, 172. Bailly has Curtz as the originator of the empty focus theory; Delambre has Boulliau in that role (Bailly, *op. cit.* (ref. 3), ii, 144, 211–12; Delambre, *Astronomie moderne* (ref. 3), ii, 161). See Sidney B. Gaythorpe, "Horrocks's treatment of evection and the equation of the centre, with a note on the elliptic hypothesis of Albert Curtz ...", *Monthly notices of the Royal Astronomical Society*, lxxxv (1925), 858–65, pp. 861–2. The first to suggest the use of circles to generate the ellipse or to employ the empty focus model, however, appears to have been David Fabricius in a letter to Kepler, 20 Jan. 1607, to which Kepler replied on 1 Aug. of that year (Kepler, *GW*, xv, 376–86; xvi, 14–30). See also Herz, *op. cit.* (ref. 4), ii, 219–20.

136. In addition to those mentioned above, the most prominent examples during the century were Cavalieri in 1632, Boulliau in 1645 and 1657, Seth Ward in 1654 and 1656, Pagan and John Newton in 1657, Streete in 1661, Mercator in 1664, 1670 and 1676, Wing and Cassini 1669, Isaac Newton in 1670 and 1679, Halley in 1676 and Huygens in 1681. Cavalieri and Horrocks independently found means of handling 'Kepler's Problem' by a method of approximation. Bailly, *op. cit.* (ref. 3), ii, 209–14; Delambre, *Astronomie moderne* (ref. 3), ii, *passim*; Whiteside, "Newton's early thoughts" (ref. 24), 122, n. 18; *idem*, "Before the *Principia*: The maturing of Newton's thoughts on dynamical astronomy", *Journal for the history of astronomy*, i (1970), 5–19, p. 9; Wilson, "From Kepler's laws" (ref. 6), 117–33; *idem*, "Predictive astronomy" (ref. 43), 169b–70b; Yasukatsu Maeyama, *Hypothesen zur planetentheorie des 17. jahrhunderts* (Frankfurt am Main, 1971).

137. Wilson, "Predictive astronomy" (ref. 43), 174b.

138. Seth Ward, *In Ismailis Bullialdi astronomiae Philolaica fundamenta inquisitio brevis* (Oxford, 1653). That its publication actually took place in 1654 was pointed out by Robert A. Hatch, *Collection Boulliau* (ref. 66), p. xlvi, n. 132.

139. Ward, *Inquisitio brevis*, 3; [Boulliau], *Astronomia Philolaica* (ref. 76), 286. See Wilson, "From Kepler's laws" (ref. 6), 117–18.

140. Wilson, *ibid.*, 121.

141. *Ismailis Bullialdi astronomiae Philolaicae* (ref. 79).

142. Wilson, "From Kepler's laws" (ref. 6), 140. With a proper eccentricity, using a librating equant point along the major axis of the ellipse, an accuracy of 20″ is possible (Wilson, "Predictive astronomy" (ref. 43), 178a).

143. Blaise Pagan, *La théorie des planètes* ... (Paris, 1657). See Wilson, "From Kepler's laws" (ref. 6), 122–3.

144. John Newton, *op. cit.* (ref. 132), 66. The statement is confused, as the motion of the radius vector at the empty focus is "equal", i.e. uniform, but the motion of the planet is not.

145. Whiteside, "Before the *Principia*" (ref. 136), 9. A similar division between theory and practice

seems to have occurred among seventeenth-century mathematicians regarding the use of indivisibles. Quite a few sought pragmatic solutions for quadratic equations despite what they knew as violations of mathematical rigour. Douglas Jesseph, "Philosophical theory and mathematical practice in the seventeenth century", *Studies in the history and philosophy of science*, xx (1989), 215–44.

146. Kepler, *GW*, vii, 380.
147. Vincent Wing, *An ephemerides of the celestial motions for xiii years* (London, 1658), 140. John Collins wrote to James Gregory the following year in words that almost repeat those used by Wing earlier (*Gregory tercentenary volume* (ref. 129), 202).
148. Mercator set his equant on the line of apsides somewhat closer to the Sun than the empty focus. It yielded a maximum error for Mars of less than 2′ (*Hypothesis astronomia nova* (London, 1664), Sig. 3a). See also Derek T. Whiteside, "Mercator", *DSB*, ix, 310–12, p. 310. Maeyama points out that Mercator could have obtained better results utilizing circles and the vicarious hypothesis had he used his own more precise data rather than Tycho's, since he had an improved figure for solar parallax through his familiarity with Horrocks's modification of it ("Kepler's hypothesis vicaria" (ref. 7), 89–90).
149. [Mercator], "Some considerations of Mr. Nic. Mercator ...", *Philosophical transactions of the Royal Society*, v (1670), 1168–75, p. 1174.
150. *Nicolai Mercatoris ... institutionum astronomicorum libri duo* (London, 1676), 162–73. Kepler of course had made it clear that the equant was inadequate.
151. Victor Thoren asserts that "virtually all the English text-writers — the very people who adopted, adapted, and disseminated the empty-focus equant theories — held decidedly relaxed views on the subject of astronomical exactitude" (*op. cit.* (ref. 133), 244. This seems to go too far, since such theories were also employed by the best astronomers for whom both convenience as well as mathematical equivalence or near-equivalence in saving the appearances served as it had for two thousand years.
152. Hall, "Wren's problem" (ref. 42), 141; J. A. Bennett, "Hooke and Wren and the system of the world: Some points toward an historical account", *The British journal for the history of science*, viii (1975), 32–61, pp. 35, 37.
153. Streete, *op. cit.* (ref. 117), 342; Curtis Wilson, "Predictive astronomy" (ref. 43), 179.
154. Attempts were made by Cavalieri, Horrocks, Boulliau, Wing, Ward, Mercator, Cassini, Flamsteed and Halley (Wilson, *ibid.*; see also Maeyama, *Hypothesen zur Planetentheorie* (ref. 136) and Owen Gingerich and Barbara Welther, appendix to Thoren, *op. cit.* (ref. 133), 257–8).
155. Thomas Birch (ed.), *The history of the Royal Society* ... (London, 1756–57), ii, 417. Oldenburg read Cassini's paper from the *Journal des Savants* for 2 Sept. 1669 to the Society.
156. *Philosophical transactions of the Royal Society*, v (1670), 1169–75.
157. *Ibid.*, 1174–5. Thoren's assertion (*op. cit.* (ref. 133), 255) that there must have been extra-empirical grounds for acceptance of the second law since its empirical aspects "had already been essentially duplicated by the refined equant theories" fails to recognize Mercator's contribution. Nor, despite Alexandre Koyré (*Newtonian studies* (London, 1965), 130), was Newton the first to recognize that inverse-distance and area rules were not equivalent. Brian S. Baigrie mistakenly claims that the "area rule is absent in the scientific literature prior to Newton", and that the ellipse was treated by astronomers as a "mere computational device" ("The justification of Kepler's ellipse", *Studies in the history and philosophy of science*, xxi (1990), 633–64, pp. 652–3).
158. Possibly three tangents or positions (see Angus Armitage, *Edmond Halley* (London and Edinburgh, 1966), 15–16; Wilson, "From Kepler's laws" (ref. 6), 158). Halley's method is described in "Methodus directa et geometrica ...", *Philosophical transactions*, xi (1676), 683–6. His English draft title was "A direct geometrical process to find the aphelion, eccentricities, and proportions of the orbs of the primary planets, without the supposition, hitherto employed, of the equality of motion at the other focus of the ellipsis", in Rigaud, *op. cit.* (ref. 113), ii, 237. The complete draft is on pp. 237–41.
159. See Thoren, "Kepler's second law" (ref. 133), 254 and the appendix by Gingerich and Welther,

pp. 257–8. Here "best" must be understood in the context of Boulliau's and Mercator's equant theories, which were accurate to within one minute of arc.

160. Wilson, "Predictive astronomy" (ref. 43), 161b; *idem*, "Kepler's derivation of the elliptical path", *Isis*, lix (1968), 5–25, p. 21; *idem*, "From Kepler's laws" (ref. 6), 101.
161. Curtis A. Wilson, "Newton and some philosophers on Kepler's 'laws'", *Journal of the history of ideas*, xxxv (1974), 231–58, p. 257.
162. Hobbes, *On body*, in his *English works* (ref. 10), i, 435.
163. [Boulliau], *Astronomia Philolaica* (ref. 76), 25.
164. Wilson, "From Kepler's laws" (ref. 6), 104.
165. Whiteside, "Newton's early thoughts" (ref. 24), 121, n. 16; I. Bernard Cohen, *Newtonian revolution* (ref. 7), 225; Russell, "Kepler's laws" (ref. 33), 14.
166. Robert Hooke, *Micrographia* (London, 1665), 238.
167. See Wilbur Applebaum, "Horrocks", *DSB*, 514–16, p. 516.
168. John Wallis, "... Hypothesis *about the* flux and reflux of the sea", *Philosophical transactions*, i (1666), 263–89, pp. 272, 280–1. Wallis surely knew that if the orbit was not elliptical, it was certainly not circular.
169. Thomas Birch, in Robert T. Gunther (ed.), *The life and work of Robert Hooke* (*Early science in Oxford*, vi; Oxford, 1930), 265. A possible source for the statements by Hooke and Wallis on whether orbits are circular or elliptical may have been Boulliau's assertion in the *Astronomia Philolaica*, 25, that the eccentricities of Earth and Venus were too small to detect a difference between a circle and an ellipse.
170. Newton, *Correspondence* (ref. 42), ii, 305.
171. John F. W. Herschel, *A preliminary discourse on the study of natural philosophy* (London, 1830), 178; Koestler, *op. cit.* (ref. 3), 328.
172. David Gregory, early in the eighteenth century, was unusual in recognizing the importance of physical theory in Kepler's discoveries (Aiton, "Kepler in recent research" (ref. 7), 78).
173. Stephenson, *Kepler's physical astronomy* (ref. 7), 2–3, 22.
174. See ref. 163 above. For Hooke, Wallis and Newton, see below.
175. Apt, *op. cit.* (ref. 36), 183–5. Briggs's model appeared to reject even the unequal motion of the vicarious theory. Christopher Heydon was aware of the importance of realism with respect to unequal motion for Kepler's "hypothesis wc he cals genuine" in contrast to the fictive model of Briggs (Heydon to Briggs, *c.* 1610, Bodleian Library: MS Ashmole 242, f. 168b).
176. All of Kepler's major works make this clear, as reflected in the title-pages of the *Astronomia nova*, and of Book iv of the *Epitome*, which Kepler calls a supplement to Aristotle's *De caelo*. For pre-Keplerian efforts in the wake of the dissolution of the celestial spheres, see Mary S. Kelly, "Celestial motors: 1543–1632", unpubl. Ph.D. diss., University of Oklahoma, 1964. The importance of Kepler's physical theories in the construction of his rules for planetary motion has been investigated in concrete detail. See Norwood R. Hanson, *Patterns of discovery: An inquiry into the conceptual foundations of science* (Cambridge, 1958), 73–85; Wilson, "Kepler's derivation" (ref. 160); and Stephenson, *op. cit.* (ref. 7).
177. Jardine, *op. cit.* (ref. 6), 144.
178. *Ibid.*, 154, 156.
179. Kepler, *Astronomia nova*, *GW*, iii, 142. See also Jardine, *op. cit.* (ref. 6), 143 for Kepler on the nature of hypotheses.
180. Stephenson, *op. cit.* (ref. 7), 116. Hanson remarks that "his discovery of Mars' orbit is physical thinking at its best" (*op. cit.* (ref. 176), 72–73).
181. *GW*, xv, 72.
182. Kepler, *Epitome*, *GW*, vii, 257.
183. This is not to say that physical precepts are not embodied in the *Almagest*, nor that Ptolemy was unconcerned about them.
184. See Aristotle on the difficulty of gaining knowledge of the heavens, *De partibus animalium*,

644b 25; on the supremacy of fact over theory, *De generatione animalium*, 760b. To say as Drake does that Ptolemy in the *Almagest* "explicitly excluded physics and metaphysics from its purview as a treatise on mathematical astronomy" (*Almagest*, I, 1, preface), ignores the fact that some of the mathematical hypotheses of Ptolemy's models are based on physical and metaphysical assumptions (Stillman Drake, "Galileo's steps to full Copernicanism and back", *Studies in the history and philosophy of science*, xviii (1987), 93–105, p. 95).

185. Robert S. Westman, "Three responses to the Copernican theory" in Robert S. Westman (ed.), *The Copernican achievement* (Berkeley, 1975), 285–345, p. 303; *idem*, "The astronomer's role in the sixteenth century: A preliminary study", *History of science*, xxii (1980), 105–47, p. 107; *idem*, "Magical reform and astronomical reform: The Yates thesis reconsidered", in J. E. McGuire and Robert S. Westman (eds), *Hermeticism and the scientific revolution* (Los Angeles, 1977), 3–91, pp. 68–69.

186. Donahue, *Dissolution* (ref. 11), 66–69.

187. *Ibid.*, 71. This does not seem to be true at least of Clavius, who seems to have held that epicycles and eccentric circles exist in nature, on the grounds that true conclusions could not come from false premises (William A. Wallace, discussion comments in Owen Gingerich (ed.), *The nature of scientific discovery* (Washington, D.C., 1975), 382–7, p. 383). Ironically, Kepler, arguing from the same principle, would conclude that epicycles and eccentric circles do not exist in the natural realm.

188. "It can therefore be said that by the beginning of the seventeenth century there was no longer any clear-cut distinction between traditional and non-traditional theories [about the composition of the heavens], nor between astronomy and physics" (Donahue, *Dissolution* (ref. 11), 71).

189. The problem is complex and has by no means been adequately addressed. Even Donahue hedges (*ibid.*, preface and 219). Field points out that Tycho rejected solid celestial spheres by 1588, but did not cite the comet observations of 1577 as the reason. Moreover, Mästlin still accepted them, while Kepler didn't. See Judith V. Field, "Kepler's rejection of solid celestial spheres", *Vistas in astronomy*, xxiii (1978), 207–11. See also the criticism of John Heilbron, that Donahue fails to distinguish between the opinions of the natural philosophers and the astronomers on the relation of physics to mathematical astronomy and has ignored the opinion of medieval speculative natural philosophers, where he would have found many of the ideas he attributes as novel to the minor thinkers of the later sixteenth century ("Commentary: Duhem and Donahue" in Westman (ed.), *The Copernican achievement* (ref. 185), 276–84, pp. 277–8).

190. Among them were Tycho, Mästlin, Fabricius, Longomontanus, Boulliau and Pagan (John L. Russell, "Kepler and scientific method", in Beer and Beer (eds), *Kepler* (ref. 71), 733–45, pp. 741–2). To Russell's list may be added Brengger, Briggs and Riccioli among others (for Brengger, see Kepler, *GW*, xvi, 71; for Briggs, *GW*, xviii, 225). Rejecting both Kepler's physical causes and Boulliau's geometrical necessities, Riccioli held that God's ultimate means were unknown and the planetary motions are governed by intelligences, following a divine harmony and Divine Providence (Wilson, "From Kepler's laws" (ref. 6), 103–4).

191. Horrocks, *Opera posthuma* (ref. 13), 179.

192. Kepler, *Epitome*, Book iv, pref., *GW*. vii, 249; Kepler to V. Bianchi, 17 Feb. 1619, *ibid.*, xvii, 321–8.

193. Wilson, "Predictive astronomy" (ref. 43), 49–50, citing the preface to the *Rudolphine tables*. See also Kepler to Bernegger, 25 Jun. 1625, *GW*, xviii, 237.

194. Wilson, "Inner planets" (ref. 15), 244.

195. Donahue, *Dissolution* (ref. 11), 191–2.

196. The idea was adopted by several, including Wallis ("Hypothesis *about the* flux and reflux of the sea" (ref. 168), 270).

197. Discussions of this speculation are found in Stephenson, *op. cit.* (ref. 7), 143–4; Gingerich, "Kepler" (ref. 119), 303b; *idem*, "Ptolemy, Copernicus, Galileo", in Adler and Van Doren (eds), *The great ideas today* (ref. 107), 137–80, p. 170. At first Kepler believed that the surface areas of the planets were proportional to their distances from the Sun. In the *Epitome*, however, citing

telescopic observations, but not without a dash of speculation concerning archetypes, he concluded that volumes were proportional to distance (*GW*, iii, 281–2). A number of astronomers, including Horrocks, Streete, Wendelin, Remus Quietanus and "possibly Huygens" were attracted to these ideas. For Huygens, see Albert Van Helden, *Measuring the universe: Cosmic dimensions from Aristarchus to Halley* (Chicago, 1985), 122–4; *idem*, "Halley and the dimensions of the solar system", in N. J. W. Thrower (ed.), *Standing on the shoulders of giants: A longer view of Newton and Halley* (Berkeley and Los Angeles, 1990), 143–56.

198. Donahue, *Dissolution* (ref. 11), 162–3, 251, 272–4, 291, 293–4; Schofield (ref. 12), 189, 243; Horrocks, *Transit of Venus* (ref. 45), 181; Mercator, *Institutionum astronomicarum* (ref. 150), 145. Boulliau, while rejecting Kepler's physical speculations on the cause of planetary motion, accepted his conjecture concerning an annual variation in the Earth's rate of diurnal rotation, as did Wing and Streete (Wilson, "Predictive astronomy" (ref. 43), 196–7).

199. Walter Charleton, *Physiologia Epicuro-Gassendo-Charletoniana* (London, 1654), 277.

200. Birch, in Gunther (ed.), *op. cit.* (ref. 169), 256. Hooke continued to use the analogy of magnetism for gravity as late as 1678 (Gunther (ed.), *op. cit.* (ref. 94), 228–9.

201. Applebaum, "Kepler in England" (ref. 36), 110–14.

202. See Stevens, *op. cit.* (ref. 49), 121; Hobbes, *op. cit.* (ref. 10), i, 434 and vii, 102; Schofield, *op. cit.* (ref. 12), 243; Koyré, *Newtonian studies* (ref. 157), 117.

203. Hevelius moved from a Keplerian to a Cartesian mechanism (Donahue, *Dissolution* (ref. 11), 293–4). For the rejection of magnetism on empirical grounds, see Newton, *Correspondence* (ref. 42), ii, 341–2.

204. Kepler's role in the history of astronomy extends beyond his planet laws and for too long has been restricted to them. For a brief discussion of Kepler's physical ideas, see Hall, *Revolution in science* (ref. 4), 144–5. It misses the mark to say that because his Aristotelian "dynamics was already outmoded, Kepler's physical explanations could exert little influence" (Aiton, *Vortex theory* (ref. 6), 2).

205. We see this in Borelli, who insists on ellipses as the true planetary path (Koyré, *Révolution astronomique* (ref. 3), 468). The Jesuit defenders of Tycho, however, insisting on the preservation of circular motion, argued that the planets actually move in "spirals" (Schofield, *op. cit.* (ref. 12), 227–30).

206. Applebaum, "Between Kepler and Newton" (ref. 70); Donahue, *Dissolution* (ref. 11), 192. For White, see John L. Russell, "The Copernican system system in Great Britain", in Jerzy Dobrzycki (ed.), *The reception of Copernicus' heliocentric theory* (Dordrecht and Boston, 1972), 189–239, p. 223.

207. Among them were Roberval, Holwarda, Hobbes, Streete, Wing and the early Newton. See Aiton, *Vortex theory* (ref. 6), 90–91; Donahue, *Dissolution* (ref. 11), 249–50; Wilson, "Kepler's laws" (ref. 6), 125; Applebaum, "Kepler in England" (ref. 36), 158–60; J. A. Bennett, "Cosmology and the magnetic philosophy, 1640–1680", *Journal for the history of astronomy*, xii (1981), 165–77, p. 175, where a magnetic cosmological tradition in England is ascribed to the influence of Gilbert.

208. Descartes, *Principles*, Part iii, sec. xxx; A. Rupert Hall, "Sir Isaac Newton's notebook, 1661–65", *Cambridge historical journal*, ix (1948), 239–50, p. 244. Wilson surmises that Newton's query derives from his reading of Wing's *Astronomia Britannica* of 1669 ("From Kepler's laws" (ref. 6), 142).

209. *Sir Isaac Newton's mathematical principles of natural philosophy and his system of the world*, transl. by Andrew Motte, rev. and ed. by Florian Cajori (Berkeley, 1960), 550.

210. The chief discussions of Borelli worth noting are Angus Armitage, "Borell's hypothesis and the rise of celestial mechanics", *Annals of science*, vi (1950), 268–82; Koyré, *Révolution astronomique* (ref. 3), part iii; Richard S. Westfall, *Force in Newton's physics: The science of dynamics in the seventeenth century* (London, 1971), 213–30.

211. The Keplerian influence may be seen in the very title of his work on celestial dynamics, which includes the expression "ex causis physicae deductae", corresponding to the Greek *aitiologetos*

in the full title of Kepler's *Astronomia nova* (G. A. Borelli, *Theoricae Mediciorum planetarum ex causis physicis deductae* (Florence, 1666)). See Koyré, *Révolution astronomique* (ref. 3), 510, n. 2.

212. Koyré, *ibid.*, 462.
213. *Ibid.*, 466. For Kepler this occurs only for the Earth's satellite.
214. "Impact was a dynamic action, and to deal with it he grasped blindly at available dynamic concepts whatever their import for his concept of motion" (Westfall, *Force in Newton's physics* (ref. 210), 216).
215. Stephen Pumfrey, "Magnetical philosophy and astronomy", in Taton and Wilson (eds), *op. cit.* (ref. 43), 45–53, p. 53a; Bennett, "Magnetic philosophy" (ref. 207), 172; Richard S. Westfall, "Hooke and the law of universal gravitation: A reappraisal of a reappraisal", *The British journal for the history of science*, iii (1967), 245–61, pp. 249–50; *idem*, *Force in Newton's physics* (ref. 210), 268–72. In the 1660s Hooke and Newton, among others, had not yet abandoned Cartesian vortices while considering central attractive forces.
216. See, for example, Hooke's review of the fate of Kepler's hypothesis and its variants, concluding that "they are fain to be most thrown aside when they come to calculation" (Richard Waller (ed.), *The posthumous works of Robert Hooke S.R.S.* (London, 1705), 179). See also Mercator, *Hypothesis astronomia nova* (ref. 148), sig. B2r.
217. For Kepler, see, for example, *Rudolphine tables*, *GW*, x, 42–43.
218. For a partial list see Schofield, *op. cit.* (ref. 12), 226–7.
219. Vincent Wing, *Ephemerides ... for ... 1659 ... 1671* (London, 1657), sig. Ar. Similar language may be found in New England almanacs shortly afterwards (Morison, *op. cit.* (ref. 40), 11).
220. Hooke, *Posthumous works* (ref. 216), 167. See also F. F. Centore, "The philosophy of heliocentrism in pre-Newtonian English science", *Organon*, x (1974), 75–85. The same point was made by Huygens (see text and ref. 25 above).
221. Cohen, "Newton and Kepler's inertia" (ref. 6), 201. The works through which Newton learned of Kepler's ideas are discussed in Whiteside, "Newton's early thoughts" (ref. 24), 131, n. 48; *idem*, "Sources and strengths of Newton's early mathematical thought", *Texas quarterly*, x (1967), 69–85, pp. 72–75; J. E. McGuire and Martin Tamny, "Newton's mathematical apprenticeship: Notes of 1664/5", *Isis*, lxxvi (1985), 349–65, p. 352; *idem*, *Certain philosophical questions* (ref. 23), 300; Richard S. Westfall, *Never at rest: A biography of Isaac Newton* (Cambridge, 1980), 94; I. B. Cohen, *Newtonian revolution* (ref. 7), 345, n. 12; *idem*, "Newtonian astronomy: The steps toward universal gravitation", *Vistas in astronomy*, xx (1976), 85–98, pp. 89–96.
222. John W. Herivel, *The background to Newton's* Principia*: A study of Newton's dynamical researches in the years 1664–84* (Oxford, 1965), 121; Hall, however, dates Newton's notes as having been made in 1661 or 1662 (A. Rupert Hall, *Isaac Newton: Adventurer in thought* (Oxford, 1992), 62). See Whiteside, "Newton's early thoughts" (ref. 24), 124, where Whiteside points out that Newton's citation of the Kepler's third law is from Streete's *Astronomia Carolina*. Streete learned of it from the papers of Horrocks, who encountered it in Kepler's *Harmonice mundi*.
223. Newton to Halley, 14 Jul. 1686, *Correspondence* (ref. 42), ii, 445, where Newton writes: "for ye duplicate proportion I can affirm yt I gathered it from Kepler's theorem about 20 yeares ago". He repeated the assertion in the draft of a letter penned in 1718 (Whiteside, "Newton's early thoughts" (ref. 24), 117).
224. Newton expressed his uncertainty in the endpapers of his copy of Wing's book (Wilson, "Newton and philosophers" (ref. 161), 238; Cohen, *Newtonian revolution* (ref. 7), 345, n. 12).
225. Newton, "System of the world" in *Newton's mathematical principles* (ref. 209), 549–626, p. 559; Wilson, "Kepler's laws" (ref. 33), 91. After discovering the law of gravitation, Newton could no longer assume the exactitude of the third law. Flamsteed was no help one way or another in 1684–85 when queried by Newton on the matter with respect to the satellites of Jupiter (Wilson, "Horrocks, harmonies" (ref. 15), 258).

226. Wilson, "Kepler's laws" (ref. 6), 90, n. 4. I. B. Cohen says Newton learned of the second law in 1678 after seeing it in Mercator's *Institutionum astronomicarum* (*Introduction to Newton's Principia* (Cambridge, Mass.,1971), 52, n. 17; *idem*, *Newtonian revolution* (ref. 7), 250–1, where Cohen adds that Newton never thought of it in astronomical terms until 1679). See also Derek T. Whiteside, "Newton and Kepler", *Nature*, ccxlviii (1974), 634, where Cohen's dating is questioned. John Herivel also questions Cohen's dating in the latter's *Introduction to the Principia* (review of I. B. Cohen and Alexandre Koyré (eds), *Philosophia naturalis principia mathematica, Nature*, ccxlvii (1974), 163–4, p. 164b). Newton could well have encountered the second law in 1670 from seeing Mercator's article in the *Philosophical transactions* of that year (Curtis Wilson, private communication).
227. Wilson, "Inner planets" (ref. 15), 244.
228. Elliptical orbits and the area rule, however, are attributed to Kepler in the tract *De motu*. Koyré speculates that it may have been due to an aversion to Kepler's "continuous mixture of 'metaphysical hypotheses' with 'natural philosophy'" (Koyré, *Newtonian studies* (ref. 157), 101–2, n. 2; see also Cohen, *Introduction* (ref. 225), 130–1; *idem*, "Newton and Keplerian inertia" (ref. 6), 199, n. 2; *idem*, *Revolution in science* (ref. 4), 496; Aiton, *Vortex theory* (ref. 6), 101). Aiton also points out that Newton also did so in a manuscript *c.* 1700, as shown in Newton, *Correspondence* (ref. 42), iv, 1.
229. Cohen, *Introduction to Principia* (ref. 226), 31.
230. Edmond Halley, *Philosophical transactions of the Royal Society*, xvi (1687), 292. Perhaps Newton was making a distinction between hypothesis in its traditional mathematical sense as an initial assumption, not necessarily true, and his recognition that they represented approximations to real orbits.
231. I. B. Cohen, *Introduction to Principia* (ref. 226), 295–6; *idem*, "Newton's theory vs. Kepler's theory and Galileo's theory: An example of a difference between a philosophical and a historical analysis of science" in Yehuda Elkana (ed.), *The interaction between science and philosophy* (Atlantic Highlands, N.J., 1974), 299–338, p. 312.
232. Wilson, "Newton and some philosophers" (ref. 161), 233–5; also *idem*, "Kepler's laws" (ref. 6), 89.
233. Newton, *Correspondence* (ref. 42), ii, 436.
234. A. Rupert Hall and Marie B. Hall, *Unpublished scientific papers of Isaac Newton* (Cambridge, 1962), 277; Herivel, *op. cit.* (ref. 222), 282; Derek T. Whiteside (ed.), *The mathematical papers of Isaac Newton* (Cambridge, 1967–81), vi, 49. In the *Principia* Newton credited Kepler only for the third law, but omitted mention of his name in connection with the second and third laws (Cohen, "Newton's theory vs Kepler's theory" (ref. 231), 313).
235. Hall and Hall, *op. cit.* (ref. 234), 378. Curtis Wilson has pointed out to me that Newton's use of "prove" (*probare*) meant "test". For Newton the word *demonstratio* stood for mathematical proof.
236. Cohen, *Newtonian revolution* (ref. 7), 229.
237. Cohen, *Introduction to Principia* (ref. 226), 136.
238. Cohen, "Newton's theory" (ref. 231), 311–12. Kepler, however, was aware that the orbital shape and the area rule were underdetermined and particularly in the case of the Moon was he aware of attractive forces from both Sun and Earth as acting to modify the ideal orbit. Cohen's statement that Kepler "really investigated the orbit only in the neighborhood of the apsides" is a puzzling one. Newton became convinced of universal gravitation only in 1684 after satisfying himself about the extent of the perturbations and that only gravitation was at work. Only at the end of that year did Flamsteed confirm to him that Jupiter's satellites obeyed Kepler's third rule. Newton then concluded that they obeyed the second law and that Jupiter and Saturn perturbed one another's orbits. He had yet to solve the problem of cometary orbits, how spheres attracted one another and if the inverse-square rule holds at their surfaces. See Wilson, "From Kepler's laws" (ref. 6), 164–7.
239. Cohen, *Newtonian revolution* (ref. 7), 43.
240. Cohen, "Newton's theory" (ref. 231), 313, 300.

241. *GW*, iii, 244.
242. *GW*, vii, 296–7.
243. Kepler, *Somnium seu opus posthumum de astronomia lunaris ...* (Sagan and Frankfurt, 1634), *GW*, xi (2), 317–67. The expression *inertia materiae* appears in *GW*, vii, 94. It also there refers to the *Astronomia nova* and Book iv of the *Epitome*.
244. *Moles* was first used in his note 90 to his translation of Plutarch's *The face in the Moon*. See note 5 to the 2nd edition, *Cosmographic mystery* (ref. 12), 171. See also Edward Rosen (ed. and transl.), *Kepler's* Somnium*: The dream, or posthumous work on lunar astronomy* (Madison, 1967), 69, n. 142, and Kepler's Introduction to the *Astronomia nova*, *GW*, iii, 25.
245. The figure is arrived at by examining the refractive indices of air compared to water (30′ to 48°), assuming a like ratio of ether to air and cubing the number (*Epitome*, *GW*, vii, 261).
246. Aiton, *Vortex theory* (ref. 6), 260.
247. Newton, however, as shown by his arguments in Book III of the *Principia*, "depended on none of them as precise empirical laws" (Wilson, "Newton and philosophers" (ref. 161), 234; *idem*, "From Kepler's laws" (ref. 6), 89).
248. Colin MacLaurin, *An account of Sir Isaac Newton's discoveries* (London, 1748), 47.
249. *Ibid.*, 47–54.
250. See, for example, Koestler, *op. cit.* (ref. 3), 396; Neugebauer, "Notes on Kepler" (ref. 71), 384–5; Cohen, *Revolution in science* (ref. 4), 127, 229.
251. Brian Baigrie sees this as a difference in approach between historians of science who see Kepler's laws as important in their own right and those who see them as gaining in significance only after the *Principia*. He uses this as a case study in the "transformation of scientific problems", this being the sub-title of his article "Kepler's laws before and after the *Principia*", *Studies in history and philosophy of science*, xvii (1987), 177–208, p. 179. See also Stephenson, *op. cit.* (ref. 7), 202.
252. Cohen, *Revolution in science* (ref. 4), 132–3. See also S. K. Heniger, "Pythagorean cosmology and the triumph of heliocentrism" in *La soleil à la Renaissance* (ref. 69), 35–53, p. 53. Wilson, on the other hand, characterizes Kepler's innovations and the resulting improvements in predictive accuracy as revolutionary ("From Kepler's laws" (ref. 6), 92, 122).
253. Halley in William Wotton, *Reflections upon ancient and modern learning* (London, 1694), 280.
254. Hall, *Revolution in science* (ref. 4), 139.
255. Norwood R. Hanson, "The Copernican disturbance and the Keplerian revolution", *Journal of the history of ideas*, xxii (1961), 169–84, p. 169.
256. Koyré, *Révolution astronomique* (ref. 3), 120.
257. An excellent summary of the central issues in this point of view is provided by Wilson, "Predictive astronomy" (ref. 43), 205.
258. See Bennett, "Cosmology and magnetic philosophy" (ref. 207), 168 for Foster; Donahue, *Dissolution* (ref. 11), 250 for Boulliau.
259. Today, for example, through socially legitimated norms and means, the 'community' passes judgement in order to determine membership, support research and determine publication; in the seventeenth century, of course, norms and means for these functions were quite different.
260. The Scottish universities were rather traditional in 1680, then moved rapidly to Newtonianism (John L. Russell, "Cosmological teaching in the seventeenth-century Scottish universities, Part 1", *Journal for the history of astronomy*, v (1974), 122–32; Part 2, 145–54).
261. Among those familiar with and to a certain extent adopters of a portion of Keplerian astronomy were Magini (1615), Cavalieri (1632), Riccioli (1651) and Cassini (1662) (Russell, "Kepler's laws" (ref. 33), 15). A clue to the Bologna story may be found in Kepler's novel effort at collaboration with Magini, who used data provided by Kepler for the construction of his ephemerides (Bialas, "Ephemerides" (ref. 97), 21–22).
262. This is suggested by Thoren, "Kepler's second law" (ref. 133), 251, n. 37 in connection with

Nicolaus Mercator, who was early a Keplerian and played an important role in clarifying Kepler's second law for English astronomers. But the professor at his alma mater, the University of Rostock, was Fabricius, a pupil of Tycho's (Joseph E. Hofmann, "Nicolaus Mercator (Kauffman), sein Leben und Wirken, Vorzugsweise als Mathematiker", *Abhandlungen mathematisch-naturwissenschaftliche Klasse d. Akademie d. Wissenschaft Mainz*, iii (1950), 45–103, p. 49, n. 18).

263. Westman, "Astronomer's role" (ref. 185), 120.

264. "The new disciplinary norms define a *widened domain of options* but they do not determine which must be chosen" (*ibid.*, 134). See also Westman's "Two cultures or one? A second look at Kuhn's *The Copernican revolution*", *Isis*, lxxv (1994), 79–115, pp. 104–11, where he provides a brief analysis of the selective reception of Kepler's ideas and of the role of his *Epitome* in attempting to create a novel disciplinary structure for astronomy in the context of both court and academic cultures.

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NOTES ON CONTRIBUTORS

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