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What's New in Kepler's New Astronomy?

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Kepler's achievements are well known and can be stated succinctly, or so it seems. Yet he was a complex thinker who responded in unusual ways to many intellectual currents in astronomy and other disciplines. To set Kepler in his intellectual context I offer a few preliminary remarks on the historical development and the inner logic of the derivations of his laws of planetary motion and sketch some of his theological and methodological commitments that affected his astronomical arguments.

The Standard View of Kepler's Achievements in Astronomy

1. Kepler's contribution to astronomy consists in his three laws: the first two laws (the ellipse and the Area Law) that were derived in the *Astronomia Nova* (1609), and the third or Harmonic Law (relating planetary periods and heliocentric distances) in the *Harmonices Mundi* (1619). By introducing elliptical planetary orbits, Kepler "had overthrown for all time the 2000-year-old axiom . . . [of] uniform circular motion" (see Caspar 1962, 140).

2. Kepler abandoned all previous astronomical theories because of the discrepancy of 8 minutes of arc between Brahe's observations of Mars and values computed from the "best" equant model.

3. The ellipse and the third law were just lucky guesses or "approximations."

4. Kepler's views on matters other than his three laws can be ignored as "irrelevant to the progress of astronomy" (e.g., his introduction of regular solids between the "planetary orbs"; his use of analogy between the heavens and the trinity; and his appeal to principles of harmony, astrology, and magnetism). For example, Dijksterhuis (1961, 322) wrote that "the story of the discovery of Kepler's first two laws is significant not only historically but psychologically, because it clearly reveals the curious jumble of rational and irrational elements from which great discoveries tend to spring."

In addition to the anachronism of referring to Kepler's three laws (to which I have no objection insofar as they are understood as a summary of his contributions), the standard view fails to take into account the historical context and the conceptual framework in which Kepler operated. In other words, all four claims need to be nuanced.

Toward a Reevaluation of Kepler's Astronomical Contributions

1. Between 1596 and 1621 Kepler produced a series of books on astronomy, adhering to the program he had outlined in his first major publication, the *Mysterium Cosmographicum* (Duncan 1981), and he achieved his stated goal of finding what he took to be the physical causes of planetary motion. The content of these books as well as the rhetoric employed had many new features. For example, the full title of the *Astronomia Nova* includes the expression "Physics of the Heavens": Kepler had invented a new genre of astronomical writing—a technical treatise unifying astronomy and physics.

2. When Kepler wrote the *Mysterium*, he began by assuming that Copernicus's results were satisfactory and merely sought another way to reach the same conclusions. Kepler described Copernicus's method as a posteriori (i.e., based on observational data), and hoped to find the same results a priori (i.e., based on a set of first principles). Kepler's initial motivation for research in astronomy did not depend on observational data, and he only sought such data when he found an inadequate fit between his a priori reasoning and the Copernican distances that his teacher, Maestlin, derived for him.

The intellectual context in which Kepler operated was set in Aristotelian physics as understood in the late sixteenth century. In the *Mysterium*, chap. 1 (Duncan 1981, 77), Kepler wrote, "Nor do I hesitate to affirm that everything which Copernicus inferred a posteriori and derived from observations, on the basis of geometrical axioms, could be derived to the satisfaction of Aristotle, if he were alive (which Rheticus repeatedly wishes for), a priori without any evasions." In chap. 2 he added (ibid., 97):

For what could be said or imagined which would be more remarkable or more convincing than that what Copernicus established by observation, from the effects, a posteriori, by a lucky rather than a confident guess, like a blind man, leaning on a stick as he walks (as Rheticus himself used to say) and believed to be the case, all that, I say, is discovered to have been quite correctly established by reasoning derived a priori, from the causes, from the archetype of creation?

We can compare this to what Rheticus had proposed in the *Narratio Prima*, the first account of Copernican astronomy published in 1540 (Rosen [1939] 1959, 142): "Now in physics as in astronomy one proceeds as much as possible from effects and observations to principles" (this seems to be what Kepler meant by a posteriori in Copernicus). According to Buchdahl (1972, 275f), Kepler's a priori reasoning allowed him to convert the Copernican theory which only had the status of a "guess" into an "actual truth."

3. Kepler's debt to Tycho Brahe was not confined to observational data, for he used Brahe's models for the Sun (or the Earth, as Kepler preferred) and Mars as preliminary hypotheses in the *Astronomia Nova*. But Kepler also accepted Brahe's arguments, based on considerations of parallax, that the New Star of 1572 and the comet of 1577 were in the heavens rather than in the sublunary realm. Furthermore, from the path of the comet Brahe argued in his treatise published in 1588 that there were no solid planetary orbs, and concluded that Aristotle's distinction between the celestial and sublunary realms had to be abandoned. Kepler drew the conclusion that *models* for planetary motion were inappropriate since it had been assumed that the planet lay on a moving solid orb; for Kepler the planet was simply moving in a fluid. For some time Kepler held to the view that the planets were self-moved "like birds and fishes," but then withdrew intelligence from among the attributes of the planets and considered their motions as due simply to forces. It is surprising that Kepler was the only professional astronomer of the time to have noticed the consequence of Brahe's discovery, namely, that constructing planetary models no longer made sense. Indeed, even later in the seventeenth century this point was not understood by many members of the astronomical community.

4. We now come to Kepler's debt to his teacher, Michael Maestlin. Of particular interest is Maestlin's (1578) treatise on the comet of 1577–1578, ten years earlier than Brahe's major treatise on the same theme. Near the end of this treatise Maestlin presented a day by day ephemeris of the comet from 5 November 1577 to 10 January 1578 where the last three columns list the longitude and latitude of the comet followed by its distance in terrestrial radius from the center of the earth (pp. 52–53; see Brahe's treatment of the coordinates of the same comet in his publication of 1588 in Dreyer 1922, iv:177–79). Maestlin put the comet in a heliocentric orb just beyond Venus (i.e., between Venus and the Earth), but he did not suggest that such a three-dimensional approach should be applied to planets—nor had anyone else up to that time. (Note that Brahe's orbit for the comet in his publication of 1588 had many of the same features as that of Maestlin, but in a geostatic, rather than a heliostatic, framework; see Dreyer 1922, iv:160.) Nevertheless, Kepler may have concluded that if it was worth the effort to calculate the path in three dimensions of an ephemeral object (as a comet was thought to be), it would be reasonable to give as much attention to planetary paths—including the distances. On the title page of Maestlin's treatise, a figure displays the path of the comet in longitude and latitude against the background of the fixed stars (reproduced in Jarrell 1989, 25); similar figures had already been used by Apian for the comets of 1531 and 1532 (see Barker 1993). But surprisingly little attention was given to planetary distances in the period immediately preceding Kepler; this issue was not prominent in the debates over the Copernican system (except for the vexed question of the parallax of Mars). Yet here Maestlin treated a comet as if it were a planet with a heliocentric orb. It has been claimed (Westman 1972a, 23–24; Jarrell 1989, 26) that Maestlin failed to understand the equivalence of the Ptolemaic and Copernican models when he decided on a heliocentric orb for this comet. But Maestlin would have been aware that according to Ptolemy's nesting hypothesis there would be no room for a comet around the orb of Venus, whereas the Copernican orbs had some space between them (as Rheticus had already noted; see Rosen [1939] 1959, 147; Brahe's 1578 German treatise on the comet of 1577 in Dreyer 1922, iv:388 [translated in Christianson 1979, 136, but this passage seems to have been misunderstood]). In this sense the two systems were not equivalent. Indeed, in the preface to the first edition of the *Mysterium* (Duncan 1981, 63), Kepler considered adding a new

and invisible planet between Jupiter and Mars, and another between Venus and Mercury, taking advantage of the gaps in the Copernican system. Maestlin's discussion of the comet of 1577 was a key factor leading to Kepler's acceptance of the Copernican system, as he tells us in chapter 1 of the *Mysterium* (ibid., 79), even though he later rejected Maestlin's view on the path of comets. (See notes to the *Mysterium*, ed. 1621, in Duncan 1981, 87.)

Methodological Considerations

1. Kepler used the term *physics* in two senses which Westman (1972b, 247) has called "descriptive" and "causal" – I consider them both causal and distinguish between causes which for Kepler involve forces acting on bodies and causes which depend on the cosmic plan of creation. The failure to recognize the importance of this distinction has led to many misunderstandings in Kepler's arguments, since he does not always draw the distinction clearly. Kepler's appeal to a priori reasoning is based on his conviction that he can derive the details of planetary motion from an analysis of the plan of creation which is governed by theological considerations concerning the nature of God and His intervention in the world – for which no observational data are needed. The data, which are the starting points for a posteriori reasoning, only confirm Kepler's a priori reasoning or require modifications of it since, according to Kepler's methodology, a priori and a posteriori reasoning must agree.

2. Let us now turn to Kepler's view of God's plan of creation, which forms part of the groundwork for his astronomy. Though I have not found an explicit statement by Kepler of his indebtedness to Melanchthon, a leading theologian who worked with Luther, I suspect that Melanchthon's view of the Bible, directly or indirectly, had a serious impact on Kepler as a student of theology in a Lutheran seminary (see Kusukawa 1992, 44 and Westman 1975; 1980, 121, 142). According to Melanchthon, the Bible is a coherent document, and all its parts are equally authoritative as the word of God. Melanchthon was prepared to exclude the apocryphal books from the Bible because, in his view, they were incompatible with the coherence of the Bible (although he allowed such books to be studied in the same way that other non-Christian classics were studied). Moreover, Melanchthon accepted the view that God reveals himself in nature. Indeed, the pagan Greeks had

come to the knowledge of moral order from contemplating the order in nature, and from this order they arrived at knowledge of God, even if that knowledge was partial and inadequate (see, for example, Schneider 1990, 103f, 119f, 244). Kepler went further by asserting that the world, created in the image of God is also a coherent whole, just as Scripture is coherent as the word of God, and that the two are complementary. Kepler's conviction, that applying theological reasoning to the study of the natural world is appropriate, is not at all surprising in his intellectual environment. Although others at the time advocated such a fusion of disciplines, Kepler was the only one to seek *quantitative* results from it.

Kepler's image of the trinity in the plan of creation can be seen in the following passage from chapter 2 of the *Mysterium*, "The image of God the Three in One [is found] in a spherical surface, that is of the Father in the center, the Son in the surface and the Spirit in the regularity of the relationship between the point and the circumference. For what Nicholas of Cusa attributed to the circle . . . I reserve solely for a spherical surface" (Duncan 1981, 93). Although Kepler referred only to Cusa here, the application of this image to the heavens may be a response to Rheticus's *Narratio Prima*:

> First, . . . [Copernicus] established by hypothesis that the sphere of the fixed stars, which we commonly call the eighth sphere, was created by God to be the region which would enclose within its confines the entire realm of nature, and hence that it was created fixed and immovable as the place of the universe. Now motion is perceived only by comparison with something fixed. . . . Then in harmony with these arrangements, God stationed in the center of the stage His governor of nature, king of the entire universe . . . the sun. (Rosen [1939] 1959, 143)

As we shall see, Kepler also grounded astronomy in theology.

In the *Paralipomena* or the *Optical Part of Astronomy* of 1604, Kepler explicitly related the created world to God, "The creator in his great wisdom found nothing more perfect or more beautiful or more excellent than himself. This is why, thinking of the corporeal world, He gave it the form most like Himself" (Chevalley 1980, 107). This form turns out to be the sphere: defined by a surface, a center, and the interval between them which is everywhere symmetric and filled by straight lines. Later, in the *Harmonices Mundi* of 1619, Kepler replaced the sphere by the circle, but the correspondence remained. Kepler conceived of constructible polygons inscribed in an archetypal circle whose vertices define a set of arcs; harmonies are then defined by the ratios of those arcs. In this way Kepler succeeded in basing arithmetic ratios or harmonies on geometric figures.

This three-in-one-some is the "archetype" of the world, where there is also a "trinity" that serves as the background against which the planetary motions take place. In the preface to the 1596 edition of the *Mysterium* Kepler proclaimed "the splendid harmony of those things that are at rest, [namely], the Sun, the fixed stars, and the intermediate [space], with God, the Father, and the Son, and the Holy Spirit" (Duncan 1981, 63). The trinity is represented by the motionless parts of the heavens in contrast to the moving planets. In this context it is important for Kepler to reconcile Scripture with science:

> Certainly God has a tongue, but he also has a finger. . . . Therefore, in matters which are quite plain, everyone with religious scruples will take the greatest care not to twist the tongue of God so that it refutes the finger of God in nature. (Notes to the *Mysterium*, chap. 1, ed. 1621, in Duncan 1981, 85)

3. For Kepler the job of the astronomer was to study the path of a planet in space, not to construct models (called "hypotheses" at the time) from which the path might be determined. This path was to be understood in three dimensions: heliocentric longitude, latitude, and distance (which could then be transformed to geocentric coordinates, if desired). In contrast, the observations discussed by previous astronomers were episodic, and they were used for determining the parameters of models, not paths.

Kepler claimed on the verso of the title page of the *Astronomia Nova* that he had met the challenge proclaimed by Ramus (d. 1572): to construct an astronomy without hypotheses (i.e., models). On the other hand, Brahe thought this challenge was meaningless because astronomers were supposed to construct hypotheses to account for planetary motions (see Blair 1990, 368). Already, in the *Mysterium*, Kepler paid special attention to the planetary paths, arguing that the planet's linear velocity along its path varies, as is the case in Ptolemy's equant model (considered by Copernicus to be a fault of that model). In the *Astronomia Nova*, chap. 1, Kepler illustrated the complexity of the geocentric path of Mars from 1580–1596 (see figure 1.1), displaying this trajectory in "depth" in the plane of the ecliptic—no such figure can be found in the previous astronomical literature (though it became a commonplace subsequently). By contrast, the heliocentric

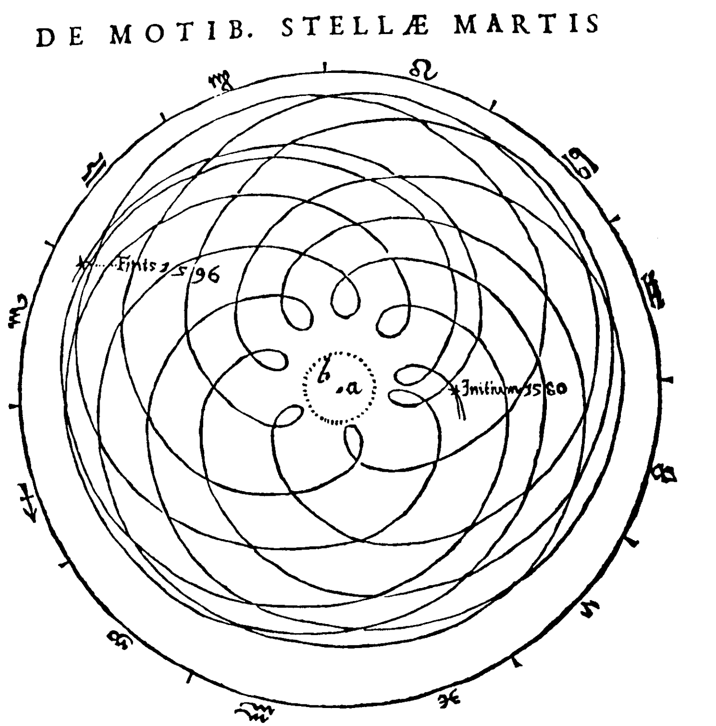

Figure 1.1 Kepler's figure to illustrate the geocentric trajectory of Mars for the period 1580–1596; *Astronomia Nova* ([1609] 1968, chap. 1).

path of a planet is an eccentric circle (or an oval) which is much simpler. In this way Kepler suggested a new reason to prefer a heliocentric over a geocentric system: on the basis of the planetary paths that are entailed by them.

4. In the *Apologia* (1600; see Jardine 1984), Kepler distinguished three tasks of an astronomer: (1) to record the apparent *paths* of the planets and their motions: the practical and mechanical part of astronomy; (2) to determine the true and genuine *paths*: the contemplative part of astronomy; and (3) to decide by what circles and lines certain images of these true motions may be depicted: the inferior tribunal of geometers. Again we see the crucial role of *paths* for Kepler (e.g., the apparent path is the "history" of a planet). Geometers provide tools for astronomers, and this is the role that Apollonius filled according to Kepler:

> Apollonius was not an astronomer by profession, but a geometer. And he did not himself, as the office of astronomer requires, apply in practice what he demonstrated from a problem derived from astronomy in order, having adopted this hypothesis, to infer and demonstrate from observation the motion of some planet. Rather, he handed over to astronomers a merely geometrical demonstration, as a retailer hands over keys or an axe to an architect in case someone should need these things for his work. (Jardine 1984, 191f)

This was precisely the role that Viète (d. 1603) saw for himself as the "Apollonius Gallus" (see Swerdlow 1975).

5. Kepler also made a sharp distinction between astronomical and geometrical hypotheses in astronomy. He claimed, for example, that an oval shape for the moon's path would be an astronomical hypothesis—but when an astronomer shows by what circles this oval can be constructed he uses geometrical hypotheses (Jardine 1984, 153). Kepler presumably was referring to the oval shape of Ptolemy's deferent for the Moon; this fact was not mentioned by Ptolemy but it had already been noted in Reinhold's commentary on Peurbach's *Theoricae novae* (first ed. 1542; I consulted the 1557 Paris ed., fol. 38b–39a), a work that Kepler cited in a letter to Herwart von Hohenberg in 1599 (see Jardine 1984, 62) and in the *Astronomia Nova*, chap. 46 (see Donahue 1992, 467); Reinhold in turn probably depended on Brudzewo's commentary on Peurbach's *Theoricae novae* (dated 1482, printed 1495; ed. Birkenmajer 1900, 124, "Similiter etiam centrum epicycli Lunae infra unum mensem non circularem, sed etiam fere ovalem, propter descensum et ascensum suum, describit figuram"; this reference was kindly given to me by J. L. Mancha, Seville). Kepler was also aware that Copernicus's double epicycle for the Moon produced an oval (*Astronomia Nova*, chaps. 4 and 43), but he did not recognize that this oval is an ellipse (see Swerdlow and Neugebauer 1984, 197).

6. For Kepler the fundamental principle that governs planetary motion is the distance-velocity relation, that is, that the linear velocity of a planet varies inversely with its distance from the center of motion. This principle was not new with Kepler, indeed, Kepler cited Aristotle's *De Caelo* (see Duncan 1981, 197), and Copernicus (1543, 7v) referred to Euclid's *Optics* for it. But Kepler used it in a new way. It first served to support Ptolemy's equant models against Copernicus's models based on uniform circular motions, and then to support an equant model for the Earth. In Ptolemy's solar model (and correspondingly in Copernicus's terrestrial model), the variation in angular velocity is an optical

effect, due to the observer's location at a point eccentric to the center of motion. But Kepler insisted that the linear velocity of the Earth should vary with its distance from the Sun and that it is not an optical effect. Kepler drew an important distinction between angular velocity, a familiar concept in astronomy, and linear velocity along the path of a planet. For measuring angular velocity Kepler used angles per unit time, whereas for linear velocity he preferred times (*morae*) per unit distance (instead of distance per unit time, as we might expect). Indeed, for Kepler distance is prior to motion, and the distance-velocity relationship came as a natural consequence of this commitment. In the *Astronomia Nova* ([1609] 1968, chap. 33, 168; see Donahue 1992, 377), Kepler remarked:

> Distance from the center is prior both in thought and in nature to motion over an interval. Indeed, motion over an interval is never independent of distance from the center, since it requires a space in which to be performed, while distance from the center can be conceived without motion. Therefore, distance will be the cause of intensity of motion (*causa vigoris in motu*), and a greater or lesser distance will result in a greater or lesser amount of time (*morae*).

It seems to me that Kepler's claim that the period of a planet varies with its distance from the Sun (which ultimately led to his Third Law) is also related to this commitment.

7. In sum, Kepler's basic tools for finding the laws of planetary motion in agreement with the observational data understood as the paths of the planets through space were (1) the plan of creation and the notion of archetypal reasoning associated with it, and (2) the distance-velocity relationship along with the priority of distance over motion.

Kepler's Laws of Planetary Motion

1. This section deals with the inner logic of Kepler's derivations of his three laws rather than the precise way he reached them. Several scholars have argued that Kepler misled many of his readers by introducing in some of his works (including the *Astronomia Nova*), for rhetorical purposes, a fictional order of discovery. Therefore, reconstructing the actual steps in his discoveries depends on a careful reading of his correspondence and manuscripts (see Donahue 1993, 1994). However, that reconstruction will not be discussed here because, in my view, Kepler's conceptual framework is sufficiently accessible through his published works.

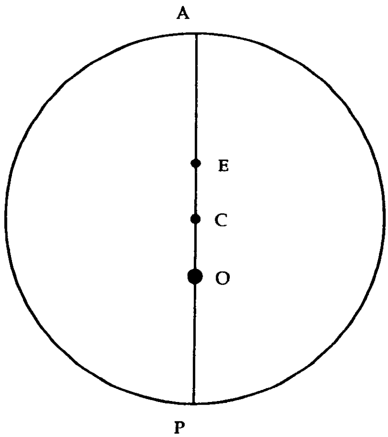

Figure 1.2a

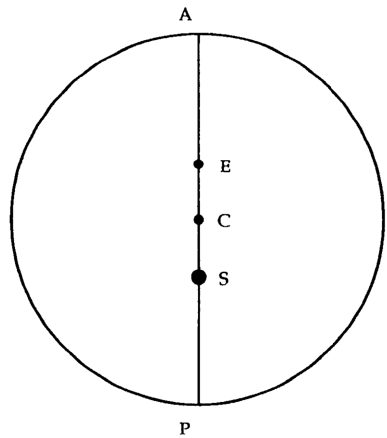

Figure 1.2b

2. As is well known, in the early chapters of the *Astronomia Nova* Kepler showed the inadequacy of all three alternative planetary models that were worthy of consideration: the models of Ptolemy, Copernicus, and Brahe. Instead, Kepler's "vicarious hypothesis" which was based on Ptolemy's equant model, the very model that Copernicus had found so objectionable, best reproduced the observational data. Let us first consider Ptolemy's equant model for the deferent of the outer planets (Mars, Jupiter, and Saturn): Let C be the center of the deferent circle *AP*, *E* the equant point, and O the observer, such that $EC = CO$ (see figure 1.2a). Uniform motion takes place about *E*; hence the linear velocity on the circle is not uniform, as Ptolemy realized. In the *Astronomia Nova*, Kepler modified this model by replacing O with *S* (for the Sun), and allowing $EC \neq CS$. The models he derived from observations of Mars were two equant models: (1) where $EC = CS$ that accounted for the observed latitudes (see figure 1.2b), and (2) where $EC \neq CS$ that accounted well for the observed longitudes (to within 2′ of arc). No single equant model could account for all the observed data; the equant model with bisected eccentricity led to a discrepancy of 8′ of arc between theory and observations at the octant points, but it represented the distances from the Sun to the planet reasonably well (see Stephenson 1987, 42ff). The equant model with nonbisected eccentricity became Kepler's "vicarious hypothesis" (i.e., a substitute for the true theory yet to be discovered) because it was useful in computing the longitudes of Mars even though it was wrong in other respects.

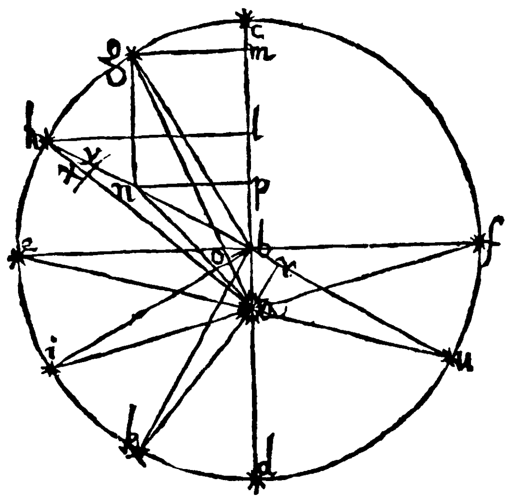

Figure 1.3 Kepler's figure to illustrate his derivation of the area law; *Astronomia Nova* ([1609] 1968, chap. 40, 193). The sun is located at point *a*, and the planet travels along circle *cedf* about center *b*.

3. Kepler's first derivation of the Area Law comes in the *Astronomia Nova*, chap. 40 (see Donahue 1992, 417ff), and his central argument referred to an eccentric circular orbit (see figure 1.3). Curiously, this derivation follows a chapter in which Kepler argued against the circular path for planetary motion.

To clarify Kepler's discussion we introduce figure 1.4: Consider the area A_1 defined by lines drawn from the Sun to the ends of a small arc s, representing the motion of a planet. Note that the Sun is not at the center of the circle on which the planet is supposed to move. Then, at aphelion and perihelion, where the motion of the planet is perpendicular to the radius vector drawn from the Sun, the area of this small triangle A_1 will be proportional to the product of the arc s and the length of the radius vector d_1. Strictly this relation will be valid only at the aphelion and perihelion (as Kepler was aware).

Next construct the motion of the planet around a portion of its orbit by adding small segments like that already defined (see figure 1.5). We then have i arcs of length s, and we seek the variable time intervals that correspond to each of these equal arc-lengths. Kepler calls these time intervals *morae*; in effect, they represent the time it takes a planet to move a unit distance along its trajectory. Following the proportionality already established, the sum of the areas A_1, A_2, A_3, . . . A_i will be proportional to the sum of $sd_1, sd_2, sd_3, \ldots sd_i$, where $d_1, d_2, d_3, \ldots d_i$, are the lengths of the corresponding radius vectors; that is:

$$A_1 + A_2 + \ldots + A_i \propto sd_1 + sd_2 + \ldots + sd_i. \qquad (1.1)$$

It follows from expression (1.1) that the proportionality holds without the quantity s on the right side; we shall discuss the consequences of eliminating s later in this section. Now Kepler believed that the linear velocity of a planet is inversely proportional to its distance from the Sun. Hence, in each term in the above series we may replace the distance d_j (where $1 \leq j \leq i$) by the reciprocal of the corresponding velocity v_j, producing a quotient s/v_j. Each of these quotients represents the distance travelled by the planet along a small portion of its orbit divided by the velocity with which it traverses that portion of the orbit, and thus defines the time taken to traverse that portion of the orbit. That is:

$$A_1 + A_2 + \ldots + A_i \propto \frac{s}{v_1} + \frac{s}{v_2} + \ldots + \frac{s}{v_i}; \qquad (1.2)$$

$$\propto t_1 + t_2 + \ldots + t_i. \qquad (1.3)$$

Therefore, the ratio of the sum of the areas A_j making up a given segment of the orbit to the area of the whole orbit (A) will be equal to the ratio of the sum of the corresponding times t_j to the time required

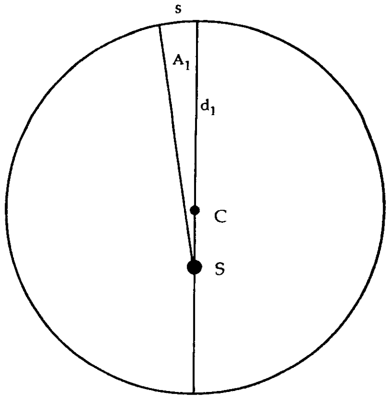

Figure 1.4

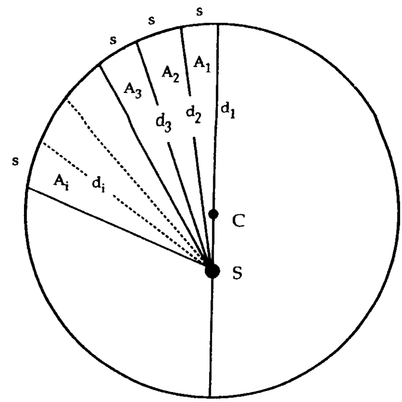

Figure 1.5

for the planet to complete one orbit that is the period of the planet (T). That is:

$$\frac{A_1 + A_2 + \ldots + A_i}{A} = \frac{t_1 + t_2 + \ldots + t_i}{T} \qquad (1.4)$$

Now let us define

$$\alpha_i = A_1 + A_2 + \ldots + A_i$$

and

$$\tau_i = t_1 + t_2 + \ldots + t_i .$$

Then, we can rewrite equation (1.4) as

$$\frac{\alpha_i}{A} = \frac{\tau_i}{T} \qquad (1.5)$$

The correlation established here between areas and time intervals is the same one we recognize, for the case of an elliptical orbit with the Sun at one focus, as the second law of planetary motion.

The derivation in chapter 40 suffers from a number of defects, some real, others alleged. The first defect is that the basis on which the small areas are calculated gives a good approximation only when the arc along which the planet is moving, and hence its velocity, is perpendicular to the radius vector drawn from the Sun. This condition is only satisfied at the apses, as Kepler recognized. Indeed, he gave an extensive analysis of the error introduced by this consideration, including a graphical representation of an exact solution. As Aiton (1969, 90), among others, has pointed out, Kepler presented a correction to the distance-velocity relation some years later in the *Epitome* of 1621: "The component of the velocity perpendicular to the radius vector is inversely proportional to the distance from the sun" (Aiton's paraphrase of the text in Kepler, *GW* 7:377).

A second defect is that Kepler seems to be appealing to infinitesimal values of s and summing an infinite number of radius vectors. Indeed, Kepler's language suggests this to be the case, and virtually all previous commentators on this chapter have pointed to this difficulty. But Kepler here explicitly appeals to Archimedes for his use of infinitesimals, and Archimedes took infinitesimals like s to be very small but still finite. In his only example, Kepler in fact took s to be 1°, far from an infinitesimal quantity. Moreover, when recasting this argument in the *Epitome* of 1621 (*GW* 7:377), Kepler was more careful and said that the orbit is to be divided into "the most minute equal parts" ("*in particulas minutissimas aequales*"). The difficulty in chapter 40 arises from Kepler's omission of some steps in his procedure whereby s could be eliminated altogether. Let us divide the entire circumference of the circle into n equal arcs of length s. Then, according to expressions (1.1) and (1.4),

$$\frac{A_1 + A_2 + \ldots + A_i}{A} = \frac{sd_1 + sd_2 + \ldots + sd_i}{sd_1 + sd_2 + \ldots + sd_i + \ldots + sd_n}$$
$$= \frac{s(d_1 + d_2 + \ldots + d_i)}{s(d_1 + d_2 + \ldots + d_i + \ldots + d_n)}$$
$$= \frac{d_1 + d_2 + \ldots + d_i}{d_1 + d_2 + \ldots + d_i + \ldots + d_n}. \qquad (1.6)$$

Thus s has been eliminated by a proper procedure. We then combine equations (1.5) and (1.6) with the following result:

$$\frac{\tau_i}{T} = \frac{d_1 + d_2 + \ldots + d_i}{d_1 + d_2 + \ldots + d_i + \ldots + dn}. \qquad (1.7)$$

In chapter 40 Kepler divided the circle into 360 equal parts and considered evaluating equation (1.7) for each value of i. Then, instead of pursuing this "tedious" method, he returned to equation (1.5), and took this as the principal result in Part III of the *Astronomia Nova*.

Let us return to the procedure in chapter 40. Kepler's goal at the outset was to approximate the motion on a circular orbit about an eccentric Sun according to the distance-velocity principle which he took to follow from physical causes. By the end of the chapter, this principle had been replaced by the Area Law as an approximation to it. Along the way, he proposed a method for taking the ratio of sums of radius vectors (see equation 1.7, above) to approximate the Area Law and then abandoned it as too cumbersome even though it appears to be much closer to the original distance-velocity relationship.

4. An argument similar to the one for the Area Law yields Kepler's third law (first presented in *Harmonices mundi* of 1619 and elaborated in the *Epitome* and in the notes to the *Mysterium* of 1621 [see Duncan 1981]). Let us begin by recalling that in a Copernican model, the length of the path of a planet about the Sun is simple to calculate, whereas in the Ptolemaic or Tychonic models the length of the planet's path about the Earth is no simple matter. The path of a planet in a circular orbit about the Sun is $2\pi R$, where R is its mean distance from the Sun. Let T be its period. Then its linear velocity

$$v = 2\pi R/T. \tag{1.8}$$

For two planets, P_1 and P_2, where P_2 is farther from the Sun than P_1,

$$\frac{v_2}{v_1} = \frac{T_1{}^*R_2}{T_2{}^*R_1}. \tag{1.9}$$

If $v_1 = v_2$, then $\frac{T_2}{T_1} = \frac{R_2}{R_1}$, (1.10)

that is, $T \propto R$. Now let $v_2 < v_1$ (i.e., a planet farther from the Sun moves more slowly in linear velocity), and $T \propto f(R)$. For example, let $T \propto R^2$. Since both R and T are known, one can check whether this relationship holds. It is then easy to determine that T does not increase so rapidly. Hence the compromise is $T \propto R^{3/2}$ (Kepler's third law), which can be verified directly from the data.

It has been suggested that Kepler proceeded by trial and error (see Koyré 1961, 341), but that does not do justice to his method. Already in the *Mysterium* (ed. 1596, chap. 20), Kepler expressed his conviction that T varied with R and, realizing that the proportionality was not linear, he proposed a relationship based on the arithmetic mean. Then in the *Astronomia Nova*, chap. 39 ([1609] 1968, 186), he proposed that T varied with the square of R: "supposing the same planet to be in turn at two distances from the sun, remaining there for one whole circuit, the periodic times will be in the duplicate ratio [i.e., square] of the distances or magnitudes of the circle" (Donahue 1992, 407). In 1618 (the precise date is recorded in the *Harmonices Mundi* V,3), Kepler discovered the third law, but the notice about it in the *Harmonices Mundi* is brief. A fuller account appears in the notes to the *Myste-*

rium of 1621 (*ad* chap. 20; Duncan 1981, 205): "from the principles adopted [in chap. 20 of the *Mysterium*, ed. 1596] the geometric mean was the legitimate conclusion . . . but the arithmetic mean came closer to the mean according to the 3/2 power than the geometric mean, or that according to the square" (see Duncan 1981, 249–50, where it is shown that the substitution of a geometric mean for an arithmetic mean leads immediately to the law: $T \propto R^2$). In this way Kepler linked his arguments in the treatises of 1596, 1609, and 1619. It is clear that from the very beginning he sought a rule such that T would vary with R; he did not have to consider all possible exponents of R since information was gained after each trial. His reasoning was based on the distance-velocity relationship, and was confirmed by the data. This reasoning is "archetypal," which is "physical" for Kepler, for it depends on the plan of creation as the cause of planetary motion.

In the *Epitome* (IV, Part 2,4) Kepler attempted to produce another kind of physical argument to justify this rule, based on forces, and to some it has seemed ad hoc. But this view needs to be nuanced. In the *Astronomia Nova*, Kepler stated that the solar force diminishes linearly with the distance from the Sun; as the force diminishes, the linear velocity of a planet diminishes and hence its period increases. But the period must also increase as the orbit increases in length (which also depends on R). Therefore, combining both effects, $T \propto R^2$ (as in the *Astronomia Nova*, chap. 39). So far no account has been taken of the effect of the planetary body. Kepler believed that the density of the planets varied inversely with the square root of their distances from the Sun (which he took to be the densest of the heavenly bodies: *Epitome*, IV, Part 1,4; *GW*, 7:486ff); the denser a planet, the more "sluggish" it is; and the more "sluggish," the greater its period. This, together with the previous relationships, yields the third law:

$$T \propto R^{*}R/\sqrt{R}; \tag{1.11}$$

$$\propto R^{3/2}. \tag{1.12}$$

Kepler gave a priori reasons for the density to vary inversely with the distance, but the third law of planetary motion led to the introduction of the square root. Only in this restricted sense may his argument be called ad hoc.

Kepler also considered the volume of a planet (*moles*) and the amount of its matter (*copia*) separately: the volume (rather than the diameter or the surface) of a planet varies directly with its distance from the Sun; and the amount of its matter varies directly with the square root of that distance. The greater the volume (V), the greater is the effect of the solar force and the shorter the period—an inverse proportionality; while the greater the amount of matter (M), the stronger is the resistance to motion and the greater the period—a direct proportionality (see Gingerich 1975). Again, we reach the third law:

$$T \propto \frac{R*R*M}{V} \tag{1.13}$$

or

$$T \propto \frac{R*R*\sqrt{R}}{R}. \tag{1.14}$$

Thus

$$T \propto R^{3/2} \tag{1.15}$$

as before.

5. The derivation of the ellipse is the most complicated case, and the one for which Kepler provided the most information concerning the path that led him to discover it. But this wealth of information and calculation has tended to obscure the principles that underlie his procedure. At a very early stage Kepler was prepared to accept ovals, for he already mentioned an oval for the deferent of the Moon in the *Apologia* written in 1600, and there was a long tradition among Ptolemaic astronomers concerning the oval deferents of the Moon and Mercury. Brahe had even considered an oval path for the comet of 1577 (see Dreyer [1906] 1953, 366). Moreover, in the *Astronomia Nova*, chap. 4, Kepler remarked that in *De Rev.* V, 4, Copernicus indicated that the path of a planet is not circular but "goes outside the circle at the sides," whereas for Kepler the planetary ovals should go "inside" the circle at the sides (Donahue 1992, 136f). On the other hand, no mention of an ellipse is made in Kepler's early astronomical writings, or in the works on which he depended for planetary theory. Yet, we

would be wrong to conclude that only Kepler was capable of making such a "conceptual leap," for an unpublished manuscript by Viète includes a mathematical discussion of an ellipse in a planetary model. Kepler did not cite this work (though he does allude to a published work by Viète; see Donahue 1992, 256), and there is no reason to suppose that Kepler was aware of it. Moreover, Viète made no attempt to produce physical arguments in favor of his ellipses, and he did not seek observational evidence to confirm his insights. In sum, Viète approached planetary theory from the point of view of a geometer who could show astronomers (particularly Copernicus) how to construct elegant mathematical models (see Swerdlow 1975).

In his derivation of the elliptical orbit of Mars, Kepler seems to suggest that this result came as a surprise to him in the course of investigating various preliminary hypotheses that were later discarded. Despite the difficulties involved in reconstructing Kepler's procedures in arriving at the ellipse (see, e.g., Wilson 1968), I believe that he was guided once again by the cosmic plan of creation and the distance-velocity relation, but detailed analysis must be left for another occasion.

Conclusion

Finally, we may note that Kepler's use of archetypal reasoning seems to have given more trouble to his modern readers than the complexity of his mathematical arguments. There is a related difficulty in interpreting his use of physical analogy which also deserves extensive treatment. Moreover, Kepler often interspersed archetypal reasoning with reasoning based on forces, adding to the confusion.

I have attempted to indicate that Kepler's religious and methodological commitments—as well as those that directly concern mathematics and natural philosophy—need to be exposed in order for us to appreciate fully his mode of thinking and to avoid anachronistic interpretations of his work. In the absence of such a global treatment of Kepler, even his technical achievements will remain unintelligible.

NOTES

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REFERENCES

Aiton, E. J . 1969. "Kepler's Second Law of Planetary Motion." *Isis* 60:75–90.

Barker, P. 1993. "The Optical Theory of Comets from Apian to Kepler." *Physis* 30:1–25.

Birkenmajer, L. A., ed. 1900. *Commentariolum super theoricas novas planetarum Georgii Purbachii in studio generali cracoviensi per Mag. Albertum de Brudzewo diligenter corrogatum A.D. MCCCCLXXXII.* Cracow: Jagellonian University.

Blair, A. 1990. "Tycho Brahe's Critique of Copernicus and the Copernican System." *Journal for the History of Ideas* 51:355–77.

Buchdahl, G. 1972. "Methodological Aspects of Kepler's Theory of Refraction." *Studies in the History and Philosophy of Science* 3:265–98.

Caspar, M. 1962. *Kepler: 1571–1630.* Trans. C. D. Hellman. New York: Collier Books.

Chevalley, C., trans. 1980. *Kepler: Les fondements de l'optique modern: Paralipomènes à Vitellion.* Paris: Vrin.

Christianson, J. R. 1979. "Tycho Brahe's German Treatise on the Comet of 1577." *Isis* 70:110–40.

Copernicus, N. 1543. *De revolutionibus orbium coelestium.* Nuremberg.

Dijksterhuis, E. J. 1961. *The Mechanization of the World Picture.* Oxford: Clarendon Press.

Donahue, W. H., trans. 1992. *Kepler: New Astronomy.* Cambridge: Cambridge University Press.

———. 1993. "Kepler's First Thoughts on Oval Orbits: Text, Translation, and Commentary." *Journal for the History of Astronomy* 24:71–100.

———. 1994. "Kepler's Invention of the Second Planetary Law." *British Journal for the History of Science* 27:89–102.

Dreyer, J. L. E. [1906]. 1953. *A History of Astronomy from Thales to Kepler.* Reprint ed. New York: Dover.

Dreyer, J. L. E., ed. 1922. *Tycho Brahe: Opera Omnia*, vol. 4. Copenhagen: Libraria Gyldendaliana.

Duncan, A. M., trans. 1981. *Kepler: Mysterium Cosmographicum: The Secret of the Universe.* New York: Abaris.

Gingerich, O. 1975. "The Origins of Kepler's Third Law." *Vistas in Astronomy* 18:595–601.
Jardine, N. 1984. *The Birth of History and Philosophy of Science.* Cambridge: Cambridge University Press.
Jarrell, R. A. 1989. "The Contemporaries of Tycho Brahe." In R. Taton and C. Wilson, eds., *Planetary Astronomy from the Renaissance to the Rise of Astrophysics; Part A: Tycho Brahe to Newton.* Cambridge: Cambridge University Press. Pp. 22–32.
Kepler, J. 1953. *GW* 7. Kepler: *Gesammelte Werke*, vol. 7. Edited by M. Caspar. Munich: C. H. Beck.
———. [1609] 1968. *Astronomia Nova.* Prague (facsimile ed. Brussels) Culture et Civilisation.
———. 1619. *Harmonices mundi libri v.* Linz.
Koyré, A. 1961. *La révolution astronomique.* Paris: Hermann.
Kusukawa, S. 1992. "Law and Gospel: The Importance of Philosophy at Reformation Wittenberg." *History of Universities* 11:33–57.
Maestlin, M. 1578. *Observatio et demonstratio cometae aetherae qui anno 1577 et 1578. . . .* Tübingen.
Reinhold, E. [1542] 1557. *Theoricae novae planetarum Georgii Purbachii. . . .* Paris.
Rosen, E. [1939] 1959. *Three Copernican Treatises.* Reprint ed. New York: Dover.
Schneider, J. R. 1900. *Philip Melanchthon's Rhetorical Construal of Biblical Authority: Oratio Sacra.* Lewiston N.Y.: Mellen Press.
Stephenson, B. 1987. *Kepler's Physical Astronomy.* New York and Berlin: Springer-Verlag.
Swerdlow, N. M. 1975. "The Planetary Theory of François Viète." *Journal for the History of Astronomy* 6:185–208.
Swerdlow, N. M. and O. Neugebauer. 1984. *Mathematical Astronomy in Copernicus's* De Revolutionibus. New York and Berlin: Springer-Verlag.
Westman, R. S. 1972a. "The Comet and the Cosmos: Kepler, Mästlin and the Copernican Hypothesis." *Studia Copernicana* 5:9–30.
———. 1972b. "Kepler's Theory of Hypothesis and the 'Realist Dilemma.'" *Studies in the History and Philosophy of Science* 3:233–64.
———. 1975. "The Melanchthon Circle, Rheticus, and the Wittenberg Interpretation of the Copernican Theory." *Isis* 66:165–93.
———. 1980. "The Astronomer's Role in the Sixteenth Century." *History of Science* 18:105–47.
Wilson, C. 1968. "Kepler's Derivation of the Elliptical Path." *Isis* 59:5–25.