KEPLER'S LAWS OF PLANETARY MOTION: 1609-1666*

By J. L. Russell

Synopsis

Historians of seventeenth-century science have frequently asserted that Kepler's laws of planetary motion were largely ignored between the time of their first publication (1609, 1619) and the publication of Newton's *Principia* (1687). In fact, however, they were more widely known and accepted than has been generally recognized.

Kepler's ideas were, indeed, rather slow in establishing themselves, and until about 1630 there are few references to them in the literature of the time. But from then onwards, interest in them increased fairly rapidly. In particular, the principle of elliptical orbits had been accepted by most of the leading astronomers in France before 1645 and in England by about 1655. It also received quite strong support in Germany, Belgium and Holland.

The second law had a more chequered history. It was enunciated in its exact form by a few writers and was used in practice by some others without being explicitly formulated, but the majority, especially after 1645, preferred one or another of several variant forms which were easier to use but only approximately correct. The third law attracted less interest than the others, chiefly perhaps because it had no satisfactory theoretical basis, but it was correctly stated by at least six writers during the period under review.

Between about 1630 and 1650 Kepler's *Epitome Astronomiae Copernicanae* (in which all three laws were clearly formulated) was probably the most widely read work on theoretical astronomy in northern and western Europe, while his Rudolphine Tables, which were based upon the first two laws, were regarded by the majority of astronomers as the most accurate planetary tables available.

Kepler's work certainly did not receive all the recognition it deserved, but the extent to which it was neglected has been much exaggerated.

The history of planetary theory between the publication of Galileo's *Two World Systems* in 1632 and that of Newton's *Principia Mathematica* in 1687 has been somewhat neglected by historians of science. This is understandable in view of the fact that there were no outstanding individuals and no major discoveries to record. Nevertheless, the period was by no means without interest. It was a time of quiet but fairly steady progress, in which the Copernican system was establishing itself and the new knowledge of the heavens brought by the telescope was gradually being assimilated. And, in particular, it was the period when the ellipse displaced the epicycle and eccentric circle as the standard pattern for the movements of the planets. From this point of view it was dominated by the great and universally respected figure of Kepler.

Many modern historians of science have failed to appreciate the importance of Kepler's ideas in the mid-seventeenth century, and have given the impression that these were largely unknown, or at least ignored,

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before the time of Newton. The following extracts from three recently-published works are typical:

(1) 'Between the time of their publication and the publication of Newton's *Principia* in 1687, there are very few references to Kepler's laws.'[1]

(2) 'Inutile de dire que l'astronomie officielle et scolaire a complètement négligé les théories révolutionaires du mathématicien impérial [Kepler] . . . Dans la première moitié du siècle Gassendi et Boulliau sont a peu près les seuls qui le citent.'[2]

(3) 'If Descartes ever isolated Kepler's theorems from the otherwise (to him) repellent farrago of weird notions in Kepler's books, he paid no attention to them. In which he was no more unjust than virtually all his contemporaries.'[3]

As long ago as 1932, J. Pelseneer[4] drew attention to what he regarded as the mystery of what happened to Kepler's laws before the time of Newton, and recommended this as a useful field for research, but no one seems to have taken up the challenge. The present paper does not claim to be an exhaustive survey, but it is based on a fairly extensive sampling of seventeenth-century astronomical literature and will suffice to show that Kepler's influence was, in fact, much greater than the above quotations would imply.

To begin with, it will be useful to recall the main points of Kepler's own work. His three planetary laws are as follows:

(1) Every planet travels round the sun in an elliptical orbit, with the sun at one focus. The moon, in the same way, travels in an ellipse round the earth, though in this case he recognized that the ellipse was not perfect.

(2) The velocity of a planet varies with its distance from the sun in such a way that a line joining the planet with the sun sweeps out equal areas in equal times.

(3) The square of the time taken by any planet to make a complete orbit is proportional to the cube of its mean distance from the sun.

We shall be concerned with four of Kepler's works which, in order of publication, are: (1) *Astronomia nova* (Heidelberg), 1609. It was more usually known to his contemporaries by its sub-title, *Commentaria de motibus Stellae Martis*. (2) *Harmonices Mundi* (Harmony of the universe), Linz, 1619. (3) *Epitome astronomiae copernicanae*; published in three parts from 1618 to 1621 (Linz and Frankfurt). It had a second edition, Frankfurt, 1635. I shall refer to it simply as the Epitome. (4) *Tabulae Rudolphinae*—Kepler's astronomical tables based upon Tycho Brahe's observations, published at Ulm, 1627, with several later and more or less modified editions after his death.[5]

[1] I. B. Cohen, *The Birth of a New Physics*, London, 1961, p. 145.

[2] A. Koyré, *La revolution astronomique*, Paris, 1961, p. 364.

[3] A. R. Hall, *From Galileo to Newton, 1630-1720*, London, 1963, p. 280.

[4] *Isis*, xvii, 1932, p. 201.

[5] The first three of these have been republished in *Johannes Kepler, Gesammelte Werke* (ed. M. Caspar, 1938- ; vols. 3, 6, 7 respectively. *Tabulae Rudolphinae* has not yet appeared in this edition.

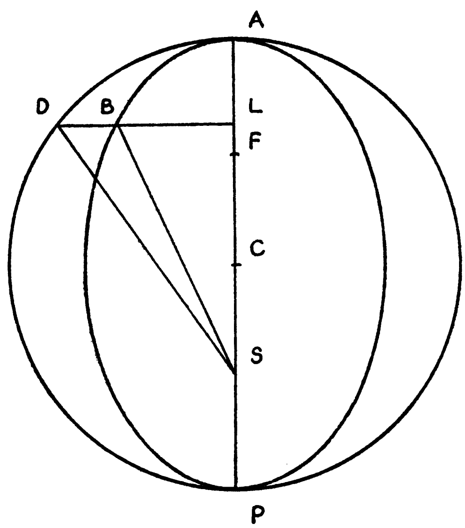
The first law was clearly stated in each of these works. In 1609 it was explicitly formulated only for Mars, but in the other three works it was applied to all the planets. The third law was enunciated for the planets in *Harmonices Mundi* (1619) and was repeated in the following year in Book IV of the much more widely-read Epitome, where Kepler extended it also to the four known satellites of Jupiter.

The second law was originally formulated, in 1609, in two different forms: the correct form which I have already given, which I shall call the area law, and an alternative form which stated that the velocity of a planet varies inversely as its distance from the sun. I shall call this the inverse distance law. At that time Kepler regarded them as mathematically equivalent, although in fact they are not; the inverse distance law is a good approximation for ellipses of small eccentricity, but is not exact. By 1621, however, when the last part of the Epitome was published, he had come to realize that the two laws were not identical and that the area law was correct.

The area law was, and still is, difficult to apply in practice since it provides no exact, directly calculable relationship between the position of a planet and the time. Thus, if ABP is a planetary ellipse, S is the sun, B the position of the planet and AB its path from aphelion A, there is no directly calculable relationship between the area of the sector ASB and the angle ASB (Fig. 1). Kepler was able to make the problem a little more tractable by showing that the ellipse could be replaced by its circumscribed circle ADP, since for any point B on the ellipse there is a constant ratio between the areas ASB and ASD, where DBL is perpendicular to AP. That is to say: if SB sweeps out equal areas in equal times, so also does SD. Kepler therefore posed his famous problem to all mathematicians: given a point S on the diameter AP of a circle and given the area of ASD, to find the angle ASD. Kepler rightly surmised that no exact geometrical solution is possible, and he could only use a rather clumsy method of trial and error in order to determine the position of a planet at a given time. Later mathematicians—Wallis, Newton, Euler and many others—took up 'Kepler's problem', as it came to be called. They were able to improve on his methods, but they, like him, could give no direct solution and the calculations remained difficult and tedious.

This will help to explain the fact that many very competent astronomers who accepted Kepler's first law with enthusiasm, nevertheless rejected the second and substituted some simpler but less accurate alternative. It was not normally due to ignorance or obscurantism, but to their conviction that such a mathematically untidy law, depending essentially on trial and error or successive approximation for its application, could not represent the ultimate truth about the movement of the heavenly bodies. It was not until Newton showed that the area law was derivable from a much simpler and more ultimate set of exact mathematical laws that it could become scientifically 'respectable'. Even those who accepted the area law in principle, often used simpler but less accurate variants in practice.

The difficulty of the area law must always be borne in mind when assessing the reaction of Kepler's contemporaries to his ideas. Many historians have seriously misjudged the problem by overlooking this point. They have given the impression that the area law is simple and aesthetically satisfying; they have therefore explained its comparative neglect by supposing that seventeenth-century astronomers were so repelled by Kepler's style and by his 'mystical' tendencies that they failed to notice the gold hidden among the dross. This is true only to a very limited extent. The chief complaint levelled against his planetary theory was that the area law was 'ungeometrical' and that in order to use it one had to resort to devices which were unworthy of a mathematician. Although Kepler's style is not easy, its difficulty has been exaggerated. In particular the Epitome—the most systematic and widely-read of all his works—is much clearer than, say, the *Astronomia Nova*. One finds occasional complaints about his style (such as Peter Crüger's, reproduced below), but the main difficulties concerned the mathematical techniques



as such. These could hardly have been made pleasing to the seventeenth-century mind by any writer before the time of Newton, however persuasive his style.

The inverse distance form of the second law is even more difficult to use than the area law. I know of no astronomer who attempted to apply it in practice except, in one heroic instance, Kepler himself.[6]

[6] *Astronomia Nova*, c.40.

Many writers quoted this form and made no mention of the area law, but it is generally impossible, in such cases, to know whether they intended to assert it as an exact law or whether the relation was intended only qualitatively: i.e. the nearer a planet is to the sun the faster it moves, and conversely. In the former case the assertion would be incorrect; in the latter it would be correct but incomplete. The inverse distance formulation is simpler to state and to understand than the other, and would normally have been regarded as sufficient in a non-technical account, even by one who knew and accepted the area law. It is possible, therefore, that the exact form was in fact known to many who never actually stated it.

Kepler's laws were formulated within the framework of a definite physical theory of planetary motion. According to this, the sun sends out 'quasi-magnetic' rays or fibrils into space, which entrain the planets and carry them around. As the sun rotates on its axis the fibrils rotate with it, something like the spokes of a very flexible wheel, and thereby keep the planets in an orbit. If this were the sole mechanism we should expect the orbit to be circular, whereas in fact it is elliptical, the planet being sometimes nearer to the sun and sometimes more distant. This was explained by postulating that the force was sometimes attractive, sometimes repulsive, depending on the orientation of the planet relative to the sun; it was attractive during the movement from aphelion to perihelion and repulsive during the other half of the orbit. If the planets were weightless they would all move round the sun with the same period of rotation, which would be equal to that of the sun around its own axis. Owing to their inertia, however, they resist the tractive force of the fibrils, the resistance increasing as the distance of the planet increases. This explains why the more distant planets move more slowly than those which are closer to the sun.

When Kepler published his theory in 1609 the sun's rotation had not yet been observed. He therefore postulated on purely theoretical grounds that the sun must rotate in the same sense as the planets, with a period of less than three months—this being the time taken by Mercury, the planet nearest the sun, to complete its orbit.[7] When, a few years later, Galileo's observations on sunspots showed that the sun does rotate in the required direction, with a period of about 28 days, Kepler naturally regarded this as a strong confirmation of his theory.

In discussing Kepler's influence, it will be necessary to distinguish between his laws and his physical theories, since some of his supporters accepted both laws and theories, others accepted one or more of the laws but rejected his theory, while others accepted the theory but ignored the laws.

7 *Astronomia Nova*, c.34.

Kepler's Laws: 1609-1630

Astronomia Nova attracted little attention when it was first published. It is a difficult book to read. It is diffuse, and much of it is simply a record of Kepler's early unsuccessful attempts to solve the problem of the orbit of Mars. It is not until page 284 (out of a total of 337) that the first two laws are finally enunciated. Kepler and his contemporaries were quite unfamiliar with the properties of ellipses; his mathematical approach to them is therefore clumsy and unsystematic, while his readers were even less qualified than he was to understand their properties and to apply them to astronomical calculations. Up to about 1630, the references which I have been able to find to his ideas are few and far between.

One of the earliest readers of *Astronomia Nova* was the English astronomer and mathematician, Thomas Hariot, who received a copy soon after publication and recommended it to another mathematician and pupil of his, William Lower. We know of Lower's reaction from a letter which he wrote to Hariot in February 1610.[8] He clearly found the work almost intolerably difficult, but at the same time intensely stimulating. He readily accepted many of Kepler's ideas, including the elliptical orbits, but felt that he needed further help from Hariot. 'Indeed I am so much delighted with his booke, but he is so tough in manie places as I cannot bite him. I pray write me some instructions in your next, how I may deale with him to ouermaster him for I am readie to take paines . . .' It appears from Lower's letter that Hariot himself accepted Kepler's ideas, at least in substance, though he does not seem to have published anything on the subject.

In 1612 the Italian savant, Federico Cesi, a friend and patron of Galileo and fellow-member of the Lyncean Academy, in a letter written almost certainly to Galileo himself, mentioned Kepler's theory of planetary ellipses with approval.[9] This is important as showing that Galileo must have been aware of the theory, although he never mentioned it in his writings and certainly did not accept it. More important support came in 1615 when Giovanni Magini, Professor of Mathematics at Bologna, published his *Supplementum Ephemeridum* in which he used Kepler's laws in calculating ephemerides for Mars. However, apart from a general acknowledgement that he was applying Kepler's theory, he gave no details as to what the theory was.

The publication of *Harmonices Mundi* in 1619 did little to spread a knowledge of the author's ideas, at least among astronomers. It was apparently the least read of his major works and there are few references to it in subsequent astronomical literature. His next work, the *Epitome*

[8] Published in *Thomas Hariot: the Mathematician, the Philosopher, and the Scholar*, by Henry Stevens, London, 1900, pp. 120-124.

[9] The relevant passage is quoted in *Isis*, xlvii, 1956, p. 78.

Astronomiae Copernicanae, was a powerful defence of the Copernican system, in the course of which he gave a full account both of his own physical theories and of his three laws. It was published in three parts in 1618, 1620 and 1621, and eventually became very influential and widely read, but it made a slow start. Copernicanism was out of fashion at this time owing to the influence of Tycho Brahe, and the learned world was not much disposed to listen to its defence.

Kepler's theory of Mars was briefly noticed by two writers in 1622: Longomontanus in *Astronomia Danica* and Nathanael Carpenter in the second edition of *Philosophia Libera*. Both rejected the ellipses as they were unwilling to abandon the principle that planetary motions should be reducible to perfect circles. Their criticisms were repeated in the second edition of *Astronomia Danica* (1640) and the third edition of *Philosophia Libera* (1635). Kepler had, however, at least one disciple during the early 1620's: Philip Müller, Professor of Mathematics at Leipzig University. Müller does not seem to have published anything on the subject, but his general acceptance of his ideas is shown both in his letters to Kepler and in his correspondence with Peter Crüger, to be discussed later. There is some evidence that Willebrord Snel (1591-1626) also accepted the ellipses.[10] The German astronomer, Ambrosius Rhodius, in a foreword to Michael Havemann's *Astraea* (1624), warmly commended *Astronomia Nova* for its defence of the Copernican system. He was clearly well disposed to Kepler's ideas.

The turning point in Kepler's fortunes came with the publication of his last big work, the Rudolphine Tables, in 1627. This was an event for which the scientific world had long been waiting. They were based upon the first-class observational data accumulated by Tycho Brahe in the later part of the sixteenth century, and in them, for the first time, the laws were really put to the test. Henceforward, astronomers could compare the predictions of the tables with the actually observed positions of the sun, moon and planets, and could then compare the results with those of rival astronomical theories. They could then decide whether it was worth while to undertake the difficult and laborious work of mastering Kepler's methods and applying them in practice. In due time, a large majority of them would decide in his favour.

The impression made by these tables on Peter Crüger, Professor of Mathematics at Danzig, is vividly conveyed by extracts from his correspondence with Philip Müller, published by von Dyck and Caspar in 1927.[11] Only Crüger's side of the correspondence is available, but it is clear from this that Müller was a supporter of Kepler and, from about 1620 onwards, was urging Crüger to study his works. Crüger did so,

[10] He is said to have done so by G. Wendelin, *Teratologia Cometica*, 1652, p. 23.

[11] *Nova Kepleriana 4: Die Keplerbriefe auf der Nationalbibliothek und auf der Sternwarte in Paris.* Abh. der Bayerischen Akad. d. Wiss.; math.- naturwiss. Abt. xxxi, 1927, pp. 114.

but for long remained unimpressed. In 1622, after receiving the second part (Book IV) of the Epitome[12] he wrote:

'I have received the 4th book of Kepler's astronomy . . . I have read more than once what he says about the proportion of the orbits and the planetary bodies in the places referred to by you. The Poet says that to read a thing ten times is pleasing. But this work I do not yet understand after reading it a hundred times. The author seems, as usual, to obscure the matter deliberately. However, I will study all these things later at leisure with my whole strength, though I do not see what use this will be. These theories are based upon uncertain foundations and mere guesswork. Perhaps we shall find more certain principles in the Danish Astronomy [of Longomontanus].'[13]

And again in 1624:

'You say; we ought therefore to accept Kepler's astronomy. I recognise and admire the acuteness and subtlety of this man. But not every idea that is acute is correct . . . Hence I do not subscribe to the hypotheses of Kepler. I trust that God will grant us some other way of arriving at the true theory of Mars.'[14]

However, all this changed after the publication of the Rudolphine Tables. Writing to Müller in 1629 he says:

'First, concerning the Danish Astronomy, which you mention at the beginning of your second letter. You hope that someone will give these tables a further polishing and you say that all astronomers would be grateful for this. But I should have thought that it would be a waste of time now that the Rudolphine Tables have been published, since all astronomers will undoubtedly use these . . . For myself, so far as other less liberal occupations allow, I am wholly occupied with trying to understand the foundations upon which the Rudolphine rules and tables are based, and I am using for this purpose the Epitome of Astronomy previously published by Kepler as an introduction to the tables. This epitome which previously I had read so many times and so little understood and so many times thrown aside, I now take up again and study with rather more success seeing that it was intended for use with the tables and is itself clarified by them . . . I am no longer repelled by the elliptical form of the planetary orbits; Kepler's proofs, in his *Commentaria de Marte* [i.e. *Astronomia Nova*] have convinced me.'[15]

[12] This contained the first and third laws, but no exact statement of the second.

[13] 'Librum Astronomiae Kepplerianae quartum accepi . . . Legi, nec semel, quae de proportione Orbium et Corporum Planetariorum Kepplerus citatis a te locis infert. Lectio decies repetita placebit, ait Poëta. Sed haec vel centies repetita nondum intelligo. Et videtur autor more suo rem obscurare de industria . . . Considerabo tamen posthac per ocium omnia summis viribus, quamquam non video cui bono. Nituntur enim haec fundamentis lubricis et meris conjecturis. Certiora forte deprehendemus in Astronomia Danica' (p. 107).

[14] 'Quin igitur, inquis, amplectamur Astronomiam Keppleri. Huius viri acumen et subtilitatem merito miror. Sed non omnia acuta etiam recta . . . Non igitur hypothesibus Dn. Keppleri subscribo. Dabit, uti spero, Deus aliam rationem deprehendendae verae theoriae Martis' (p. 108).

[15] 'Initio tamen de Astronomia Danica, cuius in epistolae tuae posterioris vestibulo fit mentio. Optas aliquem, qui limam istis Tabulis adhibeat; gratum id fore toto choro Astronomorum. Ego vero putarim, hanc operam fore cassam, publicatis jam Rudolphinis, quibus omnes dubio procul adhaerebunt, . . . Ego jam, quantum per alias occupationes minus liberales possum, totus in eo sum, ut Rudolphinorum praeceptorum ac tabularum fundamenta penitus intelligere discam, idque ex Epitome Astronomica antehac prodromi loco a Kepplero edita. Illam inquam toties ante publicatas tabulas lectam, parum intellectam, saepe a manibus abjectam, nunc resumo tractoque paulo felicius, utpote ad Tabulas componatam iisque illustratam . . . Nec jam abhorreo a forma Planetariarum orbitarum elliptica, praesertim persuasus demonstrationibus Keppleri in commentariis de Marte' (p. 108).

Public support for the Rudolphine Tables came almost immediately after their publication from Jacob Bartsch, a pupil of Müller at Leipzig, later (1630) to become Kepler's son-in-law. In 1629 he published a volume of Ephemerides based upon Kepler's tables, but calculated for the locality of Strasbourg. In it he spoke of Kepler's theories in terms of high praise, but did not expound them. Instead, he referred the reader to the Epitome for the theoretical principles on which the tables were based.

The Rudolphine Tables were undoubtedly a great improvement on all preceding ones and one might have expected that they would have swept the board at once. Two reasons, however, conspired to delay their general acceptance. The first was their difficulty. Many contemporary astronomers complained on this score. Partly it was due to the inherent difficulty of handling ellipses, and especially the area law. The problem was, of course, all the greater since neither the calculus nor co-ordinate geometry had yet been invented. Many students were also repelled by Kepler's use of logarithms which, although time-saving for those familiar with them, were still unknown and rather alarming to most astronomers. In addition, his mathematical techniques were sometimes unnecessarily clumsy and the tables were marred by a number of errors and misprints.

The second main reason which militated against the Rudolphine Tables was the appearance, a few years later, of a rival set of tables compiled by the Dutch astronomer, Philip Landsberg (1631, 1632).[16] These were simpler than Kepler's; they were based on the traditional circular orbits and their author made extravagant claims for their accuracy which were at first accepted by many of his contemporaries. They were, in fact, much less accurate than Kepler's. Landsberg was familiar with Kepler's main works and frequently made use of his data, but dismissed his theories as absurd.[17] His tables were widely used during the 1630's, but thereafter fell more and more out of favour; he was even accused of having deliberately falsified some of his data in order to fit his theories.[18] He continued to find occasional supporters, however, at least up to 1662.[19]

The only other non-Keplerian tables of much importance after 1627 were those of Longomontanus in his *Astronomica Danica* (1622; second edition 1640). These, like Kepler's, were based on Brahe's data, but used eccentrics and epicycles. They had a few supporters in Northern Germany, but were not, on the whole, serious rivals to the other two.

16 The titles of works designated by author and date only, will be found in the bibliography at the end of this article.

17 He even borrowed (with acknowledgements) a diagram from Kepler's *Astronomia Nova* for use in his *Commentationes in Motum Terrae*, 1630, p. 12.

18 E.g. by Boulliau (1645), p. 16; Wing (1651), p. 58.

19 E.g. Malvasia (1662).

Kepler's Laws: 1630-1666

Kepler's Epitome attracted little attention when it was first published in 1618-1622, but by 1630 it was evidently being more widely read and was stimulating a renewed interest in the Copernican system. That this was the case is strongly indicated by the fact that in 1631 two anti-Copernican works were published by J. B. Morin in Paris and Libert Froidmont in Antwerp respectively. Both authors were disturbed by the growing prestige of copernicanism and both made the Epitome one of the two main targets for their attacks—the other being Landsberg's *Commentationes in Motum Terrae*, published in 1630.[20] Froidmont gave a short outline of Kepler's physical theories but barely mentioned the ellipses. Morin, on the other hand, was more sympathetic to them; he says that the elliptical path is 'the simplest and most ingenious and wonderfully pleasing; it would certainly have to be accepted were it not that, as proposed by Kepler, it denies that the earth is at rest'.[21] In 1633 Morin returned briefly to the same subject in the preface to his *Trigonometricae Canonicae*, where he expressed the hope that in a subsequent work he would have the opportunity to expand his ideas on planetary theory 'ex mente Copernici et Keppleri'. If this means, as it appears to, that he himself was inclining to Copernicanism at this time, then he changed his mind later (presumably as a result of the condemnation of Galileo in 1633) since in his *Coronis Astronomiae* (1641) he was again anti-Copernican. By this time, however, he had definitely accepted the planetary ellipses but within a Tychonic framework; i.e. he postulated that the five planets go round the sun and the sun round the earth, all in elliptical paths. He also described a simple geometrical method for determining the eccentricities of the orbits of the inner planets, Mercury and Venus, which had been communicated to him by the mathematician François de Beaune. Morin remained faithful to Kepler's ellipses; in 1650 he published a corrected and simplified version of the Rudolphine Tables which was reprinted in 1657 and appeared in an English edition in 1675.

In 1632 further support for Kepler's theories came from a German astronomer, Wilhelm Schickard. In the previous year Gassendi, following a suggestion of Kepler himself, had published an open letter to the astronomers of Europe asking them to observe the transit of Mercury across the sun which was due to take place on 7 November 1631. Schickard was one of those who co-operated. He found that Kepler's tables gave a more accurate prediction of the time of transit than any others, and he concluded that the theory on which they were based was sound. In a

[20] Both seemed to regard the Epitome as the more important of the two.

[21] 'Qui equidem motus [ellipticus] simplicissimus ac ingeniosissimus mireque arridens omnino recipiendus esset; nisi illum ut a Kepplero traditur, quies Terrae penitus respueret' (p. 19).

pamphlet published in Tübingen (1632) he gave a brief outline of Kepler's main ideas, including a statement of the first two laws—the second being only enunciated, however, in a qualitative inverse-distance form. He referred the reader to the Epitome and Rudolphine Tables for further details. Since this pamphlet was in the form of an open letter to Gassendi it must have been known to the latter and, presumably, to other French astronomers.

In the same year, 1632, Kepler's first two laws were discussed in the *Directorium Generale* of Bonaventura Cavalieri. Cavalieri was one of the leading mathematicians of his time, best known for his pioneering work on infinitesimals; he was professor of mathematics at Bologna University and a friend and disciple of Galileo. The *Directorium* was an advanced textbook of spherical astronomy, designed mainly to teach the use of logarithms in astronomical calculations. He had obviously made a careful study of Kepler's works since many of his examples were based upon data contained in them. Although not primarily concerned with the theory of planetary orbits, he devoted several pages to a consideration of Kepler's ideas.[22] He stated the first law correctly, but the second only in the inverse distance form. He then explained an approximate method for calculating planetary positions which he claimed to be almost equivalent to Kepler's own.

Two more users of the Rudolphine Tables about this time may be mentioned: Adrian Vlacq in Holland (1632), and Laurence Eichstadius in Stettin, Northern Germany (1634). Vlacq used Landsberg's tables for his ephemerides for the sun and moon, and Kepler's for the planets, but only, as he explained, because Landsberg's had not yet been published. He clearly did not accept Kepler's theories. Eichstadius used Longomontanus's tables for sun and moon but, again, Kepler's for the planets. However, although he spoke of Kepler in terms of the highest praise and referred by name to all his main works, he does not seem to have regarded the ellipses as satisfactory. Like many others, he complained of the difficulty of the Rudolphine Tables and their frequent inaccuracies; by 1644, when he published the third part of his ephemerides, he was using Kepler for Mercury and Venus only. He cannot be regarded as a disciple.

In 1635 Kepler's theories received a further stimulus with the publication of a second edition of the Epitome at Frankfurt. This work is a substantial volume of nearly a thousand pages; the fact that it was worth republishing some five years after its author's death is good evidence that there was a lively interest in his ideas at this time.

The next astronomer to claim our attention is Pierre Herigone, professor of mathematics at Paris. Herigone had published, in 1634, a four-volume textbook of mathematics containing a section on astronomy

[22] Pp. 138-140; 148-152.

written on traditional lines and, on the whole, anti-Copernican with no mention of ellipses. In 1637, however, he issued a fifth volume in which he had become completely converted to Kepler and to Copernicanism. He gave in it a detailed exposition of Kepler's theories, including correct statements of the first and third laws (the first mention of the third law that I have been able to find, apart from Kepler's own), but the second only in the inverse-distance form. He also added a point-by-point refutation of the arguments he had advanced, three years before, against the Copernican theory. Finally, in 1642, he added a sixth volume, or supplement, in which he expounded Kepler's theories in yet more detail and included a correct statement of the area law. All six volumes were reprinted in 1644.

Roughly at the same time as Herigone, Kepler acquired another French disciple: Noel Durret (or Duret). In 1635 Durret had published a volume of ephemerides in which he had used Landsberg's tables. But in 1639 he issued a supplement to these, in which he pointed out that Kepler's tables had been found to be more accurate than Landsberg's. He then gave a short account of Kepler's first two laws and their use in planetary calculations. For the second law he did not actually mention the equal-area formulation, but he reproduced the mathematically equivalent construction based on the circumscribed circle (see Fig. 1). He should, therefore, be included among those who correctly stated the law. The supplement of 1639 was followed by a new volume of ephemerides in 1641 based wholly on the Rudolphine Tables. Durret said in his preface to this that his choice of Kepler rather than Landsberg was based upon 'the approval of nearly all the most competent astronomers'.

Durret's experiences were closely paralleled in England where the brilliant young astronomer, Jeremiah Horrox, started in 1633 by using Landsberg's tables, but by 1637 had become so dissatisfied with them that, on the advice of his friend William Crabtree, he turned to Kepler. From then on he, like Crabtree, became an enthusiastic disciple, accepting not only the Rudolphine Tables but the physical theories as well. Horrox, before his early death in 1641 at the age of 24, was working on a book in which Kepler's theories and tables were strongly supported and Landsberg's equally strongly criticized. In it, the first and third laws were correctly stated, but the second was given only in its qualitative form, though Horrox probably used the area law in his calculations. This book was unfinished at his death and was not published until some 30 years later in 1673 under the title: *Astronomia Kepleriana defensa et promota*. A shorter work of his, *Venus in Sole Visa*, had previously been published by Hevelius in 1662 as a supplement to his own work: *Mercurius in Sole Visa Gedani, Anno MDCLXII*. In this also, Horrox praised Kepler highly, but gave fewer details of his theories. He did not, in this case, explicitly state the third law, but he asserted that there is an exact relation between the period of a planet and the semi-diameter of its orbit; he then gave a reference to the relevant chapter of *Harmonices Mundi* for further details.

Crabtree does not seem to have published anything on Kepler, but he must have helped to spread a knowledge of his work since he corresponded with many astronomers and mathematicians of his day. In 1640, for instance, he wrote a letter to the mathematician William Gascoigne strongly recommending Kepler's theories, pointing out that his tables predicted the time of the transit of Venus in 1639 more accurately than either Landsberg's or Longomontanus's, and referring Gascoigne to the *Astronomia Nova* and Epitome for further information.[23]

One other reference to Kepler's tables in the 1630's may be added. Vincent Renieri, an Italian monk and friend of Galileo, published his *Tabulae Medicaeae* in 1639, in which he gave detailed instructions for the use of six different sets of tables: those of Kepler, Landsberg, Longomontanus, and three older ones: the Prutenic, Alphonsine and Ptolemaic. The rules were purely practical; there was no discussion on theory and no judgement on their relative usefulness, but the fact that Kepler's were put in the first place suggests that he regarded them as the most important. Clearly Renieri himself must have been familiar with the first two laws.

We may summarize at this point by saying that the decade 1630-1639 saw Kepler's ellipses accepted by Morin, Herigone, Durret and, presumably, de Beaune in France, by Schickard in Germany, Horrox and Crabtree in England, and either accepted or at least treated with respect by Cavalieri and Renieri in Italy. This, of course, is in addition to those, such as Müller and Crüger, who had already taken them up in the previous decade.

The 1640's saw steady progress in the spread of Kepler's ideas and this was certainly helped by the fact that Landsberg's tables were now falling into disrepute. The first public attack on them came from a fellow countryman, John Phocylides Holwarda (generally known to his contemporaries as Phocylides), professor at the university of Franeker in Holland. In 1640 he published his *Examen Astronomiae Lansbergianae* in which he subjected Landsberg's work to some very destructive criticism. He did not, in this book, expound his own views on planetary theory, but his occasional favourable references to Kepler's ideas strongly suggests that he accepted these. That he did so at least in the later part of the decade is clear from another work of his—*Philosophia Naturalis*—published posthumously in 1651. It is a non-technical work intended more for the general reader than for the professional astronomer, but he gave in it a systematic exposition of Kepler's physical theory of planetary

[23] A. B. Whatton, *Memoirs of the Life and Labours of the Rev. Jeremiah Horrox*, London, 1859, pp. 54-55.

motion, which he made his own, and included statements of the first and third laws, together with an inverse-distance formulation of the second law. Evidently, Holwarda had been teaching Kepler's theories to his students at Franeker for some years before his death in 1651.

In Belgium the first prominent supporter of Kepler was the well-known astronomer Gottefried Wendelin, who in 1647 mentioned and accepted the first law and in 1652 gave a quite detailed account of the third law. He proposed, however, some modifications of the first law for the moon, suggesting that its orbit was ovoid rather than elliptical and that the primary focus was not exactly at the centre of the earth, but was about 2,500 miles distant from it. Since the centre of gravity of the earth-moon system is in fact about 3,000 miles from the earth's centre, this modification probably represented a genuine improvement. Wendelin had a deservedly high reputation as an exact observer and corresponded with many of the savants of his time. His advocacy of Kepler's system is likely, therefore, to have exerted considerable influence on his contemporaries.

In Germany and Northern Europe there is little evidence of any widespread interest in Kepler during the 1640's. However, George Frommius published a pamphlet at Copenhagen in 1642 in which the ellipses were strongly commended. A much more important astronomer, Johann Hevelius, of Danzig, declared his acceptance of them in his *Selenographia* (1647, p. 169). Hevelius had been a pupil of Peter Crüger who, no doubt, introduced him to Kepler's ideas.

In 1650 we find an enthusiastic disciple in Maria Cunitia—the only notable woman astronomer of the seventeenth century. A native of Silesia, she published her *Urania Propitia* at Oels, 1650 (second edition, Frankfurt, 1851). This was a simplified set of tables based upon the Rudolphine and on Kepler's physical theories, with her own modification of the second law. The volume was mainly practical in intention and she gave only a brief summary of Kepler's ideas, making it clear that she accepted them whole-heartedly. Like other astronomical writers of the period she asserted, as an undisputed fact, that all the most eminent astronomers regarded the Rudolphine Tables as unquestionably the best. Two years later Daniel Lipstorp, of Lübeck, published *Copernicus Redivivus* (Rostock, 1652; second edition, Leyden, 1653) in which he referred to the elliptical theory of Kepler and Boulliau[24] with respect, but without much interest, though he seems to have accepted it. After this, I have been able to find very little from Germany. Abdias Trew, in a textbook of mathematics published at Altdorf (near Nürnberg) in 1657, mentioned Kepler's theories, including his first law, but referred his readers to the Epitome and to Maria Cunitia's work for a fuller account. He seemed well-disposed to them, but evidently considered Kepler's

[24] See p. 16.

ideas to be too difficult for beginners. Finally, in 1662, Johann Hecker published a volume of ephemerides at Danzig, based on Kepler's physical theories and the Rudolphine Tables. At about the same time Andreas Cellarius, rector of the College at Horn in Holland, fully accepted ellipses in his massive work: *Harmonia Macrocosmica* (Amsterdam, 1661).

In Italy there was little interest in Kepler during the 1640's; most astronomers apparently ignored his ideas completely. However, in 1651, the Jesuit astronomer, G. B. Riccioli, made an important contribution to the spread of Kepler's theories in *Almagestum Novum* (Bologna, two volumes, 1651; second edition, Frankfurt, 1653). This was a very complete work on astronomy which gave by far the most detailed exposition of these theories to be found anywhere outside the Epitome. He was one of the few who gave the area law in its exact form (Vol. I, p. 531) and he also clearly stated the third law (II, p. 532). The *Almagestum Novum* was widely read throughout Europe and must certainly have helped to spread a knowledge of the laws. Riccioli himself was anti-Copernican and did not, at that time, accept ellipses, but his exposition of Kepler's views was admirably objective and impartial. Later, in *Astronomia Reformata* (Bologna, 1665) he did come round to the use of ellipses in practice, though never convinced of their theoretical validity. He remarked incidentally, in his latter work, that 'from the time of Kepler all the followers of Copernicus have accepted . . . the ellipse in place of the eccentric circle'.[25] This is an exaggeration, since neither Galileo nor Landsberg used ellipses, but it had probably been substantially true for at least 20 years before he wrote.

One of the leading Italian astronomers, G. D. Cassini, used ellipses in his contributions to the planetary tables of Count Malvasia (1662). Malvasia himself, up to this time, had been using the methods of Landsberg. And, at the end of the period under review, G. A. Borelli published an important work on planetary theory—*Theoricae Mediceorum Planetarum* (Florence, 1666), based entirely on ellipses, in which Kepler's physical theories were modified and improved. It is worth noting that of the seven Italians already mentioned as having either accepted or at least carefully studied Kepler's ideas, four—Magini (1615), Cavalieri (1632), Riccioli (1651) and Cassini (1662)—were professors at Bologna. Evidently there was a more or less continuous Keplerian tradition at this university during the whole of the period.

It was in France that the most lively interest in Kepler was to be found during the 1640's. In the early years of the decade two well-known and influential scientists gave at least indirect support to him. First, Gassendi (1642) mentioned the first law and gave a brief account of his

[25] 'Omnes porro Copernici sectatores jam inde a Keplero amplexi sunt, una cum motu Telluris et diurno et annuo, ellipsim loco circuli eccentrici planetas deferentis' (p. 30).

physical theories with apparent approval, but without definitely committing himself. Five years later, he asserted positively that the moon and planets move in ellipses, in his *Institutio Astronomica*—a popular textbook which went through at least six editions. Secondly, Mersenne probably accepted Kepler's theories, at least in part. In 1636 he had referred vaguely to the possibility that the planetary paths were elliptical, but had expressed no opinion. In his *Universae Geometriae . . . Tractatus* (1644) he remarked that he did not propose to discuss planetary theory, but he recommended four books for those who wished to study the subject.[26] The first of these was Kepler's Epitome and the fourth was Boulliau's *Astronomia Philolaica* which was then in the press and which, as we shall see in a moment, strongly advocated the elliptical orbits. The second, it is true, was a more traditional textbook—Biancani's *Sphaera Mundi* which followed Tycho's system; while the third was Roberval's pseudo-Aristarchan *De Mundi Systemate* which did not discuss the precise paths of the planets at all. It seems fair to say that this bibliography was strongly weighted in favour of Kepler.

The most important planetary treatise of the 1640's was, however, Ismael Boulliau's *Astronomia Philolaica* (Paris, 1645), already referred to. This represents something of a watershed in the history of planetary ellipses. Until that time most, at least, of their supporters had followed Kepler quite closely, accepting one or other of his formulations of the second law in theory (though they generally devised some simpler modification in practice), and generally approving also of his physical theories. Boulliau accepted the first law but substantially modified the second in theory as well as in practice, and completely rejected Kepler's physical explanations. His view, vigorously asserted in the introduction to this book, was that we must look not for physical but for geometrical causes of planetary motion. By this he meant that the ultimate reason for the shape of an orbit was simply the exemplification of a geometrical form. Kepler had explained the orbit as arising from a physical interaction between planet and sun; Boulliau denied any such interaction. He postulated instead that each planet had an intrinsic tendency to move in an ellipse and did so in complete independence of what any other body was doing. One could say perhaps that Boulliau's approach was Platonic: the reason for the orbit is the actualization of an ideal mathematical form; whereas Kepler's was Aristotelean: he looked for a physical mechanism which could produce the observed effects. This difference in approach showed itself in several ways. Boulliau, for instance, held as a fundamental axiom that the planetary orbits must be perfect geometrical figures which must remain constant for all time. Kepler, on the other hand, regarding the orbit as the result of a complex interplay of forces, was quite prepared to admit some deviation from perfect ellipticity.

[26] Praefatio in Synopsim mathematicam, XI.

He thought also that the elements of the orbit might vary slowly with time.[27] He recognized, in fact, that the moon's orbit was not perfectly elliptical and that his theory could not exactly account for its path.[28]

One important consequence of Boulliau's geometrical approach was that it led him to reject Kepler's second law in principle. There were two reasons for this. In the first place he could not rest content with the untidy, ungeometrical trial-and-error methods which had to be used when the law was applied in practice. Secondly—and I think this was an important consideration even though he did not make it into an explicit axiom—he needed to assign some function to the empty focus of the ellipse. An ellipse is generated from two foci and if the ultimate reason for the planet's path is geometrical, we should expect that each of them would have some recognizable significance. One focus is obviously significant; it coincides with the sun. But the other focus, on Kepler's theory, is completely vacuous; nothing happens there. Boulliau solved the problem by making it an equant point. That is to say, a line joining it to the planet (FB in Fig. 1) rotates with a constant angular velocity as the planet moves in its orbit. For an observer stationed at this point, the movement of the planet would always appear uniform.

This modification was, in principle, a retrograde step. It is true that for ellipses of small eccentricity the empty focus is very nearly an equant point, but this is, at best, only an approximation. And for the more elongated ellipses of Mars and Mercury the error involved is quite significant. Kepler himself, in his early days, had examined and rejected the equant law; in the Rudolphine Tables he had repeated that the empty focus is not exactly an equant (p. 57). Boulliau did not, as a matter of fact, use the equant law in practice. Instead, he showed that an ellipse is mathematically equivalent to an epicyclic path of suitable construction and he then used this latter as the basis of his calculations. This gave a reasonably good, but by no means perfect, approximation to the true law.

Boulliau has been severely blamed by some historians for betraying Kepler's principles, but at the time there was much to be said for his theory. The area law had no satisfactory theoretical basis and, as has already been said, was very difficult to apply. Boulliau's methods were neater and much simpler. Many astronomers, less fanatically devoted to the search for absolute accuracy than Kepler, were prepared to overlook a small discrepancy between theory and fact for the sake of a more convenient mathematical technique. It should be added, however, that

[27] He mentioned this possibility, for instance, in a letter to Matthias Bernegger, June 1625. (*Ges. Werke*, vol. 18, 1959, p. 237.)

[28] He referred to the moon as the 'contumacious planet' since it would not conform to any rules that could be devised for it: 'Post consumpta omnium Artificum consilia, post tot inaequalitates Lunae prolatas in lucem, adhuc contumax sidus, legesque respuens, passim exorbitat minutule' (*Tabulae Rudolphinae*, 1627, p. 111).

Boulliau eventually came to recognize that his law was unsatisfactory. In 1657 he put forward a modified form of the equant which was much closer to the true law, though not exactly equivalent to it.

From 1645 onwards the proponents of ellipses fall into two fairly well-defined camps which can be called those of the physicists and geometers respectively. The physicists accepted Kepler's theories, in substance at least, and regarded the planetary orbits as a resultant of physical interactions. The geometers followed Boulliau in regarding the geometrical pattern as the sufficient reason for the planet moving as it did. The physicists tended to follow Kepler's methods or modifications of these; in many cases they may have accepted the area law in principle even though they rarely formulated it explicitly. The geometers, for the most part, used the equant law or some modification of it. On the whole, the geometric approach was more usually adopted in France and England; the physical in Germany. In the Low Countries Wendelin (1652) and Holwarda (1651) accepted Kepler's physical theories, although both knew and respected Boulliau's work.

After Boulliau, only two more names from France need to be mentioned. (1) Count Pagan (1657) was an extreme exponent of the purely geometric approach, taking credit to himself for being the first to eliminate physical causes completely from planetary theory. He accepted the first law and the equant form of the second law. He was aware that this did not agree perfectly with the available data, but assumed it was the observations which were at fault, not his theory. Pagan's work enjoyed some reputation for a few years, but then dropped out of sight. (2) J. B. Duhamel (1660) accepted Pagan's theory with slight modifications, but in a Tychonic framework similar to Morin's.

In England there was not much astronomical activity during the 1640's; interest in planetary movements being largely confined to the compilers of Almanacs and Prognostications. I have looked at some 15 almanacs for 1641, of which one, by Arthur Sofford, referred explicitly to the Rudolphine Tables and one, by Vincent Wing, to Landsberg's. The others gave no indication of the source of their data. Wing's subsequent progress can be traced in some detail. In his almanac for 1643 he was still using Landsberg's tables. In 1647 he had apparently abandoned them and was using partly Kepler's and partly those of the Italian astronomer Andreas Argoli. By 1649 Argoli had dropped out and he was referring only to the Rudolphine Tables. Finally, in 1651, he published a full-length treatise on planetary theory—the *Harmonicon Coeleste*—in which his conversion to Kepler was complete. In it he referred to Kepler as 'the most subtile mathematician that ever was' (Preface). And later: 'That most admirable mathematician John Kepler, by the help of Tycho's observations, did make the most admirable and best restauration of Astronomy, of any that ever did precede him' (p. 158). It is interesting to note that Wing, in his work, asserted that elliptical orbits were generally accepted. He says: 'It appears by observation that the Wayes of the Planets in the Heavens are Elliptical, the first vigilant observer thereof was the admired Kepler, since whom it is received as a general truth' (p. 46). In spite of his admiration for Kepler, however, Wing followed Boulliau in accepting a modified form of the second law, chiefly, it would seem, because of its greater simplicity. He was still doing so when he published his *Astronomia Britannica* in 1669.

After 1650 most English astronomers accepted elliptical orbits. Samuel Foster, Professor of Astronomy at Gresham College and a member of the 'Invisible College' certainly did so before his death in 1652, as we know from his *Miscellanies*, published posthumously in 1659.[29] In 1653 Jeremy Shakerley published a volume of astronomical tables based upon the theories of Kepler and Boulliau. In it he said: 'Some things we have set down according to the opinion of *Bullialdus*, but in most things we have credited *Kepler*' (p. 26). In the same year Seth Ward, Savilian Professor of Astronomy at Oxford, wrote a short work in which he criticized certain aspects of Boulliau's theory, while making it clear that he accepted Kepler's ellipses. Three years later he expounded them in more detail in his influential *Astronomia Geometrica* (1656). Ward accepted the equant law and was apparently the first to use it in practice. In 1657 John Newton published *Astronomia Britannica*, in which he also used ellipses and the equant law. The area law was enunciated, perhaps for the first time in England, by John Wallis, Savilian Professor of Geometry at Oxford. This was at the end of a short treatise on the properties of the cycloid, published in 1659, in which he demonstrated, *inter alia*, an improved method for applying the area law in practice. Wallis remarked in the course of it that he had discovered this method 'olim', which suggests that his interest in Kepler's ideas went back at least to the early 1650's.

Two years later appeared Thomas Streete's *Astronomia Carolina* (1661) in which planetary theory was again firmly based on the ellipse. The first and third laws were clearly stated; for the second he used Boulliau's later (1657) modification of the equant law. Then, in 1664, came Nicholas Mercator's *Hypothesis Astronomica Nova* which also contained the first and third laws and, for the second, a new variant of the equant law. Mercator was a German, but as he lived and published in England at this time I include him among the English writers. Finally, in 1665-1666 Isaac Newton was working on planetary theory at his home in Woolsthorpe. Dr. D. T. Whiteside's researches into Newton's papers have shown that at this time he definitely knew of Kepler's third law, probably from Mercator's *Hypothesis*, but almost certainly did not know the correct form of the second law, which was not mentioned in Mercator's work.

[29] *Of the Planetary Instruments*, p. 25.

This was unfortunate as it prevented him from carrying his investigations to a successful conclusion.[30] It was not until 1670 that Mercator himself stated the area law correctly and admitted that it agreed well with observation, though he apparently still hesitated to accept it.

CONCLUSION

Kepler's influence was exerted mainly through the *Epitome* which, from about 1630-1650 or beyond was almost certainly the most widely read treatise on theoretical astronomy in Europe. Most of the authors I have mentioned refer to it, often with specific page references which show that they must actually have read the work or parts of it. Many writers of the period sent their readers to it for further information about Kepler's ideas. No other work of any author is mentioned so frequently or, for the most part, with so much respect where planetary theory is concerned. It is true that up to about 1635 some of Landsberg's works were serious rivals and that by 1650 Boulliau's prestige was beginning to overshadow Kepler's here and there, but taking the period as a whole he was the dominating figure. Of his other works, the Rudolphine Tables were the most widely used astronomical tables from about 1640 until 1666 and beyond. *Astronomia Nova* was mentioned not infrequently in the literature; *Harmonices Mundi* much more rarely.

Kepler's laws and theories attracted little attention until the publication of the Rudolphine Tables in 1627. Thereafter, interest in them increased steadily. By 1645, or even 1640, almost all French astronomers must have known of the first law and most of them seem to have accepted it, apart from some of the traditionalists. The same is true in England perhaps by 1650 and certainly by 1655. In Germany and the Low Countries there was less unanimity, but the first law must have been fairly generally known by 1655. In Italy, apart from Bologna, the interest seems to have been much less.

The third law would have been known to anyone who read Book IV of the Epitome with attention, but it does not seem to have attracted much notice, probably because it had no convincing theoretical basis. It was, however, explicitly stated by Herigone (1637) in France, Wendelin (1652) in Belgium, Holwarda (1651) in Holland, Riccioli (1651) in Italy, Horrox (not published till 1673), Streete (1661) and Mercator (1664) in England. The second law, as we have seen, had a more complicated history. Of those whom I have read, only Herigone (1642), Riccioli (1651) and Wallis (1659) gave the correct area law

30 Whiteside's conclusions are summarized in a paper by M. A. Hoskin and Christine Jones, *Problems in Late Renaissance Astronomy*, to be published in *Actes du Colloque 'Le Soleil à la Renaissance', Bruxelles, 1963*. I should like to take this opportunity of thanking the joint authors for providing me with some valuable data for my own work.

before 1666, while Durret (1639) gave, instead, a correct geometrical construction for it. Of the others the physicists (in the sense already defined) generally quoted the inverse-distance relation and the geometers accepted the equant law or some modification of it. However, it must be remembered that some of those who gave the inverse-distance law may have intended it merely as a convenient qualitative description, and may have accepted the area law in principle. The latter figures prominently in both *Astronomia Nova* and the Epitome, so it must have been known to many of those who did not mention it.

The conclusion which emerges from this survey is, therefore, that the importance of Kepler's ideas during the period under review has been greatly underestimated. There was a time when these were largely ignored, but the tide began to turn about 1630; subsequently his influence steadily increased until, by the early 1650's, his work had become known to and, in varying degrees accepted by, most astronomers north of the Alps.

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[4] **Gilbert, Bacon, Galilée, Képler, Harvey et Descartes: Leurs relations**
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