Tychonic theory, and gave animated expression at every opportunity to his reverent and thankful acknowledgment of everything for which he was indebted to Brahe and his observations.

Two days after Brahe's death, Barwitz, an imperial adviser, came to Kepler and brought him the glad tidings that the emperor had decided to transfer to him the care of the instruments and incompleted works of Tycho Brahe. He was informed of a salary and he was ordered to make application for the corresponding sum. Therewith he was appointed as successor to the man on whom he had hitherto been dependent. He was imperial mathematician himself. He had a position such as he had long hoped for and dreamt about. Now he could work freely. The rich store of observations was at his free disposal. For he was dependent thereon, since he had to complete Brahe's works and especially had to carry out the great tables, for which barely any preparations had been made. That here lay a source of vexations and difficulties he was, to be sure, soon to discover. But at any rate, Kepler, with good cause, took charge of the observations in which were put down Brahe's lifework. The great observer had fulfilled his task; he was called away. Now the great theoretician took over—he, who from these observations had to solve the secrets which they concealed. So the sadness over Brahe's passing mixed involuntarily in Kepler's breast with the joy over his own promotion. Hafenreffer and Rollenhagen,[1] the well known poet of the "Froschmäuseler" (The Frogs and Mice), sent congratulations. Herwart particularly took joyful part in the favorable turn in Kepler's fortune. He was convinced that there was no mathematician other than Kepler in all Germany, indeed in all Europe, who was able to succeed Brahe. He voiced this conviction directly to Barwitz, the imperial adviser, to whom he wrote: "I, as one informed about these matters and having also some experience, know very well that at this time as far as one can judge from the works that have been published (*ex operibus editis*) no one can be found who can be compared both in intellectual power and in mathematics (*et ingenio, et fundamentis artis Matheseos*) with this Master Kepler, let alone be preferred to him, so that I have no doubt whatever that when it is brought to the attention of His Majesty most graciously and most humbly he will not let him go for any amount of money." While he said he knew a position for Kepler in the university in Lauingen, yet it would be best for all parties if he were to remain in Prague. Only one would then also have to "direct things in such a manner that he" would be "reimbursed for his past as well as his present expenses." Herwart was quite at home in questions of salary and particularly knew

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[1] Ed. Note. Georg Rollenhagen, 1542–1609, German satirist and clergyman.

how they were handled at the imperial court. Therefore, right from the beginning, he advised Kepler: "My friend should not be satisfied with a small and pinched salary but should ask for a large and comfortable one. And the estimate of it should not be based on bodily needs (*pro quantitate corporis*) nor on the "living standard" (*et tenuitate victus*) but on the greatness of your mind (*sed pro magnitudine animi tui*) and of the subject matter (*et rei subjectae*) and you should make an effort to receive an adequate down-payment at once." How justified Herwart's warning was, soon became apparent. Advised by men near him, Kepler left the fixing of his salary to the emperor, who granted him an annual sum of 500 gulden, beginning October 1, 1601. However, he had to dance attendance for months before at last, on March 9, 1602, he received the first payment. The sum appeared very trifling compared to that which Brahe had received. However, it must be borne in mind that, for the observations which constituted the wealth of the departed one, the latter's heirs made great demands which had to be complied with. Soon Kepler moved into a new dwelling in the New Town opposite the Emaus cloister, nearly an hour's distance from the castle, at which he frequently had to present himself.

The deeper one penetrates into the events from Kepler's first expulsion up to his appointment as imperial mathematician, into the motives which guided the people concerned, into the temporal relationships which they bore to each other, into the significance which they possess for the history of astronomy, that much more clearly does one recognize the hand of a higher guidance. In order that the paths in life of the two great astronomers who uniquely supplemented one another could unite, it was necessary that both be displaced from their widely separated residences, in order to meet at the court of an emperor whom history reproaches for having neglected the affairs of government for the sake of his astrological and alchemical bents. Kepler himself expresses his conviction of the rule of a divine decree in these events when he writes: "If God is concerned with astronomy, which piety desires to believe, then I hope that I shall achieve something in this domain, for I see how God let me be bound with Tycho through an unalterable fate and did not let me be separated from him by the most oppressive hardships."

3. Astronomia Nova; *and the second and first planet laws*

Even in the midst of the hardships and afflictions in which his life abounded, Kepler was very seldom forsaken by his remarkable ability to plunge into and bury himself in studies and speculations. Similarly, during the suspension and fear of the last year, his ever active mind made keenest use of the pauses, which care and illness allowed him, to pursue his scholarly researches. If we examine these researches, we immediately arrive at the beginnings of his most renowned accomplishment, the discovery of the planet laws. We see him in the months of his first visit to Tycho Brahe and then later when concerned with other works, occupied above all with the foundation of the glorious structure of his *New Astronomy*. It is not, however, as though he had laid this foundation according to a precise preconceived plan. The work that was being formed grew out of him in accordance with the unfathomable laws of gifted creation. Assuredly, he appears as the active one, the agent, the calculator, the meditative one, the designer, the constructor, but he was the sufferer, the stimulated one, the hunted one, since his genius directed his mind, led his hand, showed him the trail which he must follow, called him back when he made a mistake, spurred him on and left him neither rest nor quiet until everything was completed and he, who had carried it out, finally regarded with amazement the work in which he had succeeded. For anyone whose mode of thinking is not so unassuming that he operates only with complete results but who is able to derive pleasure and profit from considering the multiplicity of the divine manifestations of life, it is uncommonly fascinating to follow, in this unique example, the separate phases in the development of revolutionary opinions in the models of nature, and to follow step by step the work of the genius who introduced this revolution. In the example at hand, this is all the more possible since the extant sources give us all the information desired. Obviously, for various reasons, this undertaking cannot be completely carried out within the bounds of a biography. The difficulties of the material are not insignificant in the case before us because of the astronomical and mathematical knowledge which must be assumed. However, a biographer would fulfill his task badly, if he were to let these difficulties restrain him from giving the reader something more than only the last formulations of the final results. In the introduction to the great work, in which Kepler informs the world of his brilliant conclusion, he compares his voyage of discovery with those of a Columbus and a Magellan in whose narratives we find great entertainment. Whereas in reading we take no part at all in the hardships of the travels of the Argonauts, the reader of Kepler's works would get an opportunity to trace the obstacles and thorns on his paths of thought. However, so he asserted, this is the common lot of all mathematical books. As some people find pleasure in that, others in something else, so there will also be some who,

having overcome the intellectual difficulties, will be filled with joy to have before their eyes at one time the whole series of his discoveries.

It was already noted that previously, during Kepler's first visit, the elaboration of the theory of Mars was assigned to him by Tycho Brahe. Because he did not find there, as he had hoped, ready values for the sizes which he needed for corroboration and correction of his harmonic speculations, he had to set about calculating these sizes himself. Now then, what happened at closer range with these calculations? It is common knowledge that Mars, like the two other "upper" planets, Jupiter and Saturn, advances in the ecliptic from day to day from west to east until, in about 780 days, it has completed one synodic revolution. When it gets near to being in opposition to the sun, thus some time before it culminates at midnight, it is stationary, just as though it wanted to ponder, even moves back a bit, in order to continue its journey in the old direction sometime after the opposition. It is known that Copernicus had demonstrated in a startlingly simple manner that this remarkable loop was a reflection of the motion of the earth, from which we observe without perceiving this motion. Even if no account is taken of this loop, nevertheless another irregularity is still perceptible. The times between two oppositions, that is the synodic periods of a planet, are not exactly equal, as the observations of several centuries have established in detail. Because people, in their curiosity, want to know where the planets would stand at a particular time in the future, the calculation of this irregularity had to be taken into consideration. How was this possible? In the presentation of these motions of the heavens the ancients began with the principle that a natural retrograde motion must of necessity be a uniform circular motion. Supported in particular by the authority of Aristotle, an axiomatic character was given to this proposition, whose content, in fact, is very easily grasped by one with a naïve point of view; men deemed it necessary and ceased to consider another possibility. Without reflecting, Copernicus and Tycho Brahe still embraced this conception, and naturally the other astronomers of their time did likewise. In order to master that irregularity mathematically, the center of the universe (the earth according to Ptolemy, the sun according to Copernicus) was assumed to be somewhat away from the center of the orbit. The distance between the two points was called the eccentricity, the circular path the eccentric, the axis connecting the centers of the universe and of the orbit the line of apsides, and the intersections of this straight line with the orbit the apsides (according to Copernicus perihelion or nearest to the sun, aphelion or farthest from the sun). Even if the motion in the orbit proceeds uniformly, it still appears to an observer in the center of the universe as irregular, namely quicker at perihelion, slower at aphelion.

Yet since this simple aid did not manage to save the appearances, as it was expressed, additional assumptions were made. Only with the orbit of the sun (according to Ptolemy and Tycho Brahe) or of the earth (according to Copernicus) was it considered possible to get along without such supplements; here a simple uniform circular motion on an eccentric was retained. What those supplementary assumptions rested on is only hinted at: Ptolemy assumed a point on the line of apsides (equalizing or compensating point or *punctum aequans*[1]) from which the motion on the eccentric should appear uniform, so that in reality it is non-uniform. Copernicus, whom Brahe followed in this, sought to reach the same end by the superposition of two uniform circular motions. For what follows it is not necessary to go into this in greater detail. Only note that some astronomers in Kepler's time saw the chief merit of Copernicus precisely in the fact that his theory does greater justice to that axiom of uniform circular motion than does that of Ptolemy.

To develop the theory of Mars meant, consequently, to calculate the position of the line of apsides and the value for the eccentricity. Since a circle is defined by three points, to solve this problem it was necessary that three points on the planet's orbit be known. These are obtained from the observations of the opposition, because (to use the words of Copernicus) at an opposition it is immaterial whether one observes from the moving earth or the stationary sun, since in this configuration planet, earth and sun lie in a straight line. Now Tycho Brahe had a series of ten such Mars oppositions from the years 1580–1600 (later in 1602 and 1604 Kepler added two more). They formed the material which Kepler had before him when he set to work on the task set for him. Naturally the result had to be the same each time, no matter which group of three oppositions was taken as a basis if, yes, if, the assumptions as to the form of the path and the form of the motion were correct. Let it also be noted that the calculations were entirely carried out solely with the mathematical aids supplied by the geometry of Euclid and trigonometry.

This scanty sketch of the fundamental ideas of the earlier planet theories marks the runway from which Kepler started his flight into new regions. From the very beginning he set to work at his task with optimistic impetuosity. He believed that in eight days he would master the difficulties which had stumped Longomontanus. He even made a bet that he would accomplish this. When it did not go that fast, he kept hoping from day to day that he would reach a happy conclusion. He was madly bent on his calculations. The purpose for which he wanted the results receded into the background. Immediately after returning from his first visit to Benatky, he wrote to Herwart: "I would already have concluded my researches about world harmony, had not Tycho's astronomy so shackled me that I nearly went out of my mind." It was clear to the honorable seeker after the truth: "Those speculations may not *a priori* run counter to obvious empirical knowledge, rather they must be brought into agreement with it." Now it was a question of testing the empirical knowledge, of cornering nature and forcing her to answer prudent questions. At the new task, new abilities developed in him. The Kepler who speculated made way for the Kepler who computed and weighed critically. That he went straight to Mars, was a most propitious piece of good luck. For since this one of the three outer planets has by far the largest eccentricity, it alone did not fit the earlier theories and so made possible the new discovery. "I consider it a divine decree," writes Kepler, "that I came at exactly the time when Longomontanus was busy with Mars. Because assuredly either through it we arrive at the knowledge of the secrets of astronomy or else they remain forever concealed from us."

Now how did Kepler take hold of this work? Certainly he must have started by the traditional method. However, the vain attempts of Brahe and Longomontanus had already demonstrated that the conventional procedure was found wanting if all the assumptions made there were retained. Therefore, he had to drop or change one or another. In the first place his criticism was directed toward the supplementary assumptions discussed above. From the very beginning he rejected the superposition of two uniform circular motions, as introduced by Copernicus. Introducing an equant (or *punctum aequans*) was more satisfactory because then the motion of the planets in reality is irregular and appears uniform only from this mathematical point. This will be seen to have fitted into his fundamental concept from the very start. Previously, without any evidence, a very precise assumption about the position of this point had been taken as the start for the calculations. Kepler abandoned this procedure and left the position of the point on the line of apsides open. Since he thus introduced one more degree of freedom, the task naturally became more complicated. Instead of three points of the path, as hitherto, he now had to make use of four so as to be able also to calculate the position of the equant. Accordingly, he selected a favorable quadruple of four observations of oppositions, by the use of which he carried through the very cumbersome calculation. The solution of the task was possible only by a

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[1] Ed. Note. Equant.

procedure of approximations. Not less than seventy times, as he informs us asking for sympathy, had he had to carry out the entire series of difficult separate calculations which the solution required before everything agreed sufficiently for him to be content. And the result? He checked the path so calculated against the other available observations and saw that for all, the calculation fitted well with the observation within the limits of accuracy of two minutes which conformed to the Tychonic observations. Since those were distributed over the whole ecliptic, he had good cause to conclude that he possessed a means of calculating the position of Mars for any desired moment within those limits of accuracy. How he could triumph with such a result! Was not his problem thus solved?

Indeed, anyone else would have been satisfied, but not Kepler. He wanted to be absolutely certain of his results and accordingly sought further confirmation. Like a possessed collector, he sat before the accumulated treasure of observations and with the eye of a connoisseur selected a few rare pieces which enabled him to calculate the eccentricity of the orbit directly, in a highly original manner. However, he obtained, not a confirmation, but a contradiction which was so large that at the maximum there was a difference of eight minutes for the planet's position. The triumph was too early. Such a difference was not to be neglected. Here observation confronted observation, both indubitable. Logic decided: there must be an error in the suppositions regarding the form of the orbit and the form of the motion. One or the other or both assumptions were wrong. So much Kepler clearly stated. "These eight minutes showed the way to a renovation of the whole of astronomy." Kepler was undaunted. The sincerity and purity of the purpose which guided him in his inquiries is expressed when he seals the negative result with the incomparable lovely words: "After the divine goodness had given us in Tycho Brahe so careful an observer, that from his observations the error of calculation amounting to eight minutes betrayed itself, it is seemly that we recognize and utilize in thankful manner this good deed of God's, that is we should take the pains to search out at last the true form of the heavenly motions."

Now the scene changes. On the stage appear two thoughts, which had long been standing behind the wings and were barely able to wait for their turn to play their parts. After all, they had advanced once before. Both figures sharply criticized antiquity, both for the same reason. Copernicus in his picture of the universe had, it is true, placed the sun in the center. However, since he relied entirely on Ptolemy whom he esteemed highly, he had, in presenting the planet theories, always assumed as center of the universe, not the sun itself but rather

the center of the earth's orbit, which was somewhat to the side of the sun, and referred all his calculations to this. Tycho Brahe, in his system, had made an assumption corresponding exactly to this. Because both of them, on the strength of these assumptions, erected their planet theories on oppositions to the so-called mean sun instead of to the true one, inaccurate figures naturally entered the calculations from the very beginning. Very early Kepler rightly took exception to that. He required that all values be referred to the true sun.

The second thought which he introduced concerned the earth's orbit. Copernicus had assumed that the earth moves uniformly in her circular orbit; he had not found any of the supplementary assumptions necessary here as contrasted with the paths of the upper planets. In this regard, too, he followed Ptolemy as did in turn Tycho Brahe. Why, Kepler now asked, should a different theory be valid for the earth than for the other planets? It was not solely a deduction by analogy which put this critical question into his mind. Behind this question stood an important positive thought. It was the same thought from which the previous consideration had also grown: The sun is the seat of a force which moves the planets in their course, and, what is more, the motion is so much the quicker the nearer the planet is to the source of the force. If that is the case, then the sun itself, the body of the sun, must be the middle point of the whole planet system, not an empty point like the center point of the earth's orbit. And if the effect of the force decreases and increases according to the distance, then the earth, in its eccentric orbit, must also move faster when nearer the sun, slower when farther away.

In the sun there is the seat of a moving force. This was the great new guiding thought which from now on shone in front of him in his inquiries and led him to the discovery of his laws, the great theme which he henceforth varied to the utmost and tried to found out of observed facts with all the consummate skill with which he was able to utilize the observational material. He now wanted to abandon the old beaten track and adopt new ways. He was no longer willing to be satisfied with a kinematic and pure geometric presentation of the motions; he wanted to explain these by their causes. As he rightly said, if the earlier masters, and Copernicus and Tycho as well, always had proceeded *more Ptolemaico mutatis*, so he now intended to clean house, getting rid of the entire furnishing of epicycles, and demonstrate the planetary system as governed by inner laws, regulated by physical forces. Even in his student days, he had such thoughts and we have seen similar physical considerations emerge several times. Now the time had come to introduce them systematically into the science of the heavens, to shape astronomy into celestial mechanics. With this Kepler had set himself a difficult task. The mathematical resources of his time no longer sufficed to accomplish it. Also, we see how in the solution of his problems, which later was relatively simple to accomplish with the tools of mathematical analysis, he slaved unceasingly, without quite reaching the goal which he had set for himself.

However, before introducing his physical notions into the theory, Kepler still wanted to prove empirically that his supposition in regard to the motion of the earth was correct. More exact information about it was therefore necessary, because of course all observations are made from the moving earth, so that an error made in regard to this motion necessarily also creeps into the working up of the observations. For this reason Kepler perceived in the correct theory of the earth's orbit the "key to a more deeply penetrating astronomy." As daring and rich in fantasy as he was in his speculation about the universe, just as thoroughly and carefully did he now proceed, taking no step without gathering authorization and confirmation from the observations. Indeed, while following his Mars researches, one almost gets the impression that sometimes he deals with individual tasks and proofs out of pure delight and pleasure in the observations.

But now, how could he get more exact knowledge of the earth's orbit? In any event, not by conscientiously employing the old methods; even though Brahe, also assuming a uniform circular motion, succeeded, with the aid of his accurate observations, in doing justice to the phenomena within the limits of accuracy set by him. Now here again Kepler's inventive genius was active and suggested an ingenious trick. Hitherto the point of view had been from the earth to Mars; now Kepler wanted to follow the earth in its course from a point on the orbit of Mars "as from a watchtower." He, so to speak, transposed his eyes to a particular position of Mars' orbit and from there found out directly the relative values of the distances from sun to earth. Since the sidereal period of Mars was accurately known, such points of time, when it was in the same positions in its orbit, could easily be specified. Kepler chose three. Since, naturally, at these points of time the earth occasionally was to be found at various sufficiently accurately known positions in its orbit, he succeeded in calculating, by elementary geometrical means, the relative distances of the earth from the sun for these three points of time. But in this he mastered the assumed circular orbit of the earth and could calculate the distance of the sun from the central point of the orbit, that is the eccentricity of the earth's orbit. Out of this was derived a value for this eccentricity which beyond doubt corroborated his surmise that the theory of the earth's motion is the same as that of the upper planets, namely that the earth, too, moves non-uniformly in its orbit. This extremely ingenious procedure was still further profitable for his later researches; to wit, it also provided him with the relation between the radius of the earth's orbit and the distance of Mars from the sun at that place in its orbit which was in question, that is a relative distance of Mars from the sun.

Now, however, the moment had come to introduce his physical conceptions. The procedure just now sketched, as a more exact inquiry proved, had demonstrated not only the irregularity of the motion of the earth in its orbit, in general; from it could also be derived a measurement for the points at which the earth has its greatest and smallest distance from the sun. It was shown that at these places, that is at aphelion and perihelion, the speed of the earth is inversely proportional to its distance from the sun. This measurement he immediately extended to the whole orbit and thus advanced the general proposition which he had already had in his mind for a long time: The rate at which the earth moves in its orbit, as a consequence of the force issuing from the sun, is inversely proportional to its distance from the sun. And he introduced still another generalization. What holds for the earth, holds also for the other planets. Naturally, as Kepler well knew, experience still had to prove whether these inductive conclusions were admissible.

But how can one calculate with this proposition, that is solve the problem set by astronomy, to ascertain the place of the planet in its orbit at a given moment? This was a difficult matter for Kepler. (The motion of a point which travels on a circle in such a way that its rate is inversely proportional to its distance from a given eccentric point leads, according to modern analysis, to an elliptical integral.) Yet Kepler was not frightened away. He divided half of the circular orbit beginning at one apside into 180°, calculated the distance to the sun of each one of these little graduated arcs (letting the semi-diameter of the orbit equal one) and added these 180 numbers. The sum gave him the measure of the time it takes the earth to travel around half its orbit. If he wanted to calculate the time when the earth had moved 50° from the apside, he added the first fifty values of the distances. The ratio, then, of this sum to the previous one is the same as that of the time sought to half the period of revolution. Thus was solved the problem of computing the time which it takes the earth to reach a given point on its orbit. The inverse problem, to be sure, of calculating the position of the earth at a given moment of time, could only be solved by interpolation with the help of a table, constructed in accordance with the previous procedure.

Now, however, calculating with the sums of the distances was exceedingly bothersome. And Kepler immediately looked around for a suitable short cut. He himself tells about his next step: "Since I was cognizant of the fact that there are infinitely many points on the orbit and correspondingly infinitely many distances, the thought came to me that all these distances are contained in the plane of the orbit. For I remember that once Archimedes also divided the circle in the same manner into infinitely many triangles, because he tried to find the ratio of the circumference to the diameter." So now Kepler, tempted by this consideration, not mathematically indisputable, replaced the sums of the distances by the corresponding areas and succeeded in finding the time it takes a planet to pass over a particular section of its orbit. He did this by measuring the area bounded by the rays from the sun to the end points of these sections. In that way he obtained what is today called the second planet law: *The radius vector describes equal areas in equal times.* Since the areas, which he thus introduced, were easy to calculate, he thenceforth used this proposition as the working hypothesis for further research. He was nevertheless completely cognizant that the two propositions, the distance proposition and the area proposition, are not identical. Immediately after he had accomplished the change from the one to the other, he himself pointed out the difference with mathematical precision; the applications he made in this connection constitute a specimen of his clever mathematical way of thinking, as well as his accuracy and thoroughness. The difference in the results of the two propositions in their application to the motion of the earth was irrelevant considering the limits of accuracy of that time. Only experience with other planet orbits of greater eccentricity could establish which proposition was the correct one.

Although calculating those areas was simple, Kepler still could only solve the problem of calculating the position of a planet for a given point of time indirectly as previously, because the statement led to a so-called transcendental equation. Every mathematician well knows the great significance of this famous "Keplerian problem" in the further development of the theory of functions.[1]

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[1] ED. NOTE. This problem was well stated by Robert Small in *An Account of the Astronomical Discoveries of Kepler* . . ., London: Mawman, 1804, p. 296, as follows: "Having the area of part of a semi-circle given, and a point in its diameter, to determine an arch of the semi-circle, and an angle at the given point, such that the given area may be comprehended by the lines including the angle, and by the required arch: or, to draw from a given point in the diameter of a semi-circle, a straight line dividing the area of the semi-circle in a given ratio." Geometers have been unable to achieve a rigorously accurate solution. For a modern solution with calculating machine, see Jens P. Møller, "On the Solution of Kepler's Equation," *Festschrift für Elis Strömgren*, Copenhagen: Munksgaard, 1940, pp. 163–74.

These researches clarified the form of the motion for Kepler. Now it was a question of testing the other assumptions of the earlier theories; those regarding the shape of the orbit. He began with the information that the orbit cannot possibly be circular. This truth he demonstrated by referring to the distances of the sun to Mars, which were known to him from his earlier detailed researches. If the orbit were circular, then he would always have to come up with the same orbit whatever triplet of distances he were to use. Now, however, the negative was demonstrated, since a different result appeared depending on the choice of the three distances. But how to proceed now? His physical concepts which hitherto had brought him rich returns now enticed him on an extremely burdensome and very long detour. As soon as it had become clear to him that the orbit departs laterally inward from a circle, he believed it possible to furnish a physical cause for this phenomenon. He prepared a certain mechanism showing the motion due to the force issuing from the sun. The mechanism led to an egg-shaped orbit with the blunt end at aphelion and the point at perihelion. Conquering this picture of the motion mathematically gave Kepler infinite trouble. He calculated the breadth of the "moonlets" which lie between a circular orbit and his oval one. He sought to ascertain the area of the ovoid. Then again its perimeter. He attempted to solve the problem now with the sums of the distances, now with the areas. For all 180° he recalculated the distances of the sun to Mars provided by his mechanism. For in no other way could he complete his integration problem. When the result did not agree with the observation he changed his preliminary statement; at least forty times, so he remarks, he carried out such a calculation for all 180°. If only he had succeeded in ascertaining the area of an ovoid by geometry, without its being necessary to reckon repeatedly "in smallest divisions." Yes, if the orbit were a perfect ellipse, he wrote at that time to a friend, so the problem would already have been solved by Archimedes and Apollonius. Only the picture of the motion, with which he had fallen in love, did not admit of such a thing. Then what was the fault in these disagreements? Kepler sought it in his area proposition, he sought it in an erroneous use of the proposition of distances. The only place he did not seek it was where it lay, in his picture of the motion. Only when all possibilities were exhausted did he decide, with heavy heart, to desist from this. He had again triumphed too soon. Later he joked about the over-great haste, with which he had taken hold of his problem. "Hasty dogs bear blind young."

After this lack of success, Kepler once more took up where he had let himself be pushed aside from the correct path. He began to calculate distances and, indeed, very thoroughly. So, at last, he had marked off a great many points on the orbit of Mars. Indeed, he now had the parts at hand but lacked the picture which comprehends and puts these parts together. Excited, he was on the lookout for a solution. Now was it chance or a good fairy which set him on the correct track? He had calculated the width of the "little moon," which his oval produced. In his trials he had found that this width may be only half as big. The number stuck in his head. Then he accidentally hit upon the idea that precisely in the ellipse, whose eccentricity is equal to that of the orbit of Mars, the difference between the semi-major and the semi-minor axis was half as big as the width of that "little moon." It was for him, as he says, as though he were awakened out of sleep and saw a new light. It was clear to him that the rule, by which the distances change from point to point of the orbit, proved correct precisely for this ellipse. The question of the shape of the orbit was solved. What held for Mars, must also hold for the other planets. The law was announced: *Planets move in ellipses with the sun at one focus.*

It was a steep and long path which Kepler had to retrace in order to scale the summit which he had seen from the distance. Some reader to whom the subject is foreign may already be nearly out of breath from the attempt to become acquainted with this path as it has been described in the previous statements of particulars. And yet here everything is put in the simplest form, all mathematical detail being left out, and only the main lines shown. Anyone who penetrates deeper into Kepler's exposition finds himself transplanted into a confusion of calculations and deliberations. What sounds exceedingly simple in our arguments divides into difficult single problems, for whose solution Kepler himself had to contrive a method because up to then no one had set or carried out such problems. It is necessary to force a way with him through the thicket of his numbers, to share in his detours, to overcome the difficulties of his abstruse style. Yet it is worth the trouble. The power of the logic which impels him forward is captivating, the ability with which he masters every difficulty is admirable, the rich flashes of ideas which streamed in on him are pleasing, every new outlook which he opened is enjoyable. His prodigious industry, his inventive genius, his mathematical sense, his unhesitating sense of fact are to be marveled at. The same man who came to Prague to complete his *a priori* structure of the universe we now see calculating, for months, for years, because the observations required it. It is always the observations which chain him, which he forces to answer his questions. The problem which he had mastered should be made clear. The numbers giving the position of Mars at the times of the observations are on

many pages in Brahe's journals. A confused muddle! Kepler brought order out of this chaos. He had hunted out the laws uniting these numbers, so that they no longer stand together unrelated but rather each can be calculated from the other. In this connection one circumstance still deserves special mention: that is the limits of accuracy of the Tychonic observations. That these limits were narrow enough so that Kepler could not afford to neglect those very important eight minutes, we have already seen. But had they been considerably narrower, he would certainly have been caught in a fine meshed net, because in many of his calculations he would no longer have been permitted to overlook certain inaccuracies, as was necessary for the progress of his research. Thus, theory and practice harmonized remarkably with one another.

It is a new land which is glimpsed from the position next to Kepler on his summit. He left far behind him not only Ptolemy but also Copernicus and Tycho Brahe. Perhaps it seems that it makes little difference whether the planet orbit is a circle, or an ellipse deviating little from the circular shape. Yet Kepler's prodigious step forward consists precisely in the fact that with his ellipse proposition he had overthrown for all time the two-thousand-year-old axiom, according to which every motion retrograde in itself must of necessity be a uniform circular motion. By that step he had made the orbit free for a new development of astronomy. And nothing is more difficult in science than to set aside such deep-rooted opinions. People who have not read Kepler often tell the story as though Kepler had found his laws in a purely geometrical way, so to speak by trials. It is naïve to believe that a fortress as strong as that axiom indicates can be taken by such means. No, everywhere in the solution of the problem confronting him, physical concepts were in the background and drove him forward. They became more and more intimately intertwined with his astronomical thinking. In 1605 he wrote in a letter: "I admit that for at least five years past I have used for physical considerations at least half the time left me by the affairs at court." When he was reproached for having a passion for innovation because he wanted to mix together such heterogeneous sciences as astronomy and physics, he explained: "I believe that both sciences are so closely bound with one another that neither can achieve perfection without the other."

Nowadays we are so accustomed to seeing mechanical forces operating in the planetary motions that it is difficult for us to think that it was once different. And yet Kepler ran up against rejection and lack of understanding on all sides. Maestlin, Fabricius, Longomontanus and others shook their heads. Even many years later Maestlin advised his former pupil to leave physical causes and hypotheses entirely out of the question and to explain astronomical matters only according to astronomical method; geometry and arithmetic alone are the vibrations of the knowledge of the heavens. It is Kepler's greatest service that he substituted a dynamic system for the formal schemes of the earlier astronomers, the law of nature for mathematical rule, and causal explanation for the mathematical description of motion. Thereby he truly became the founder of celestial mechanics. The goal that he pursued he summarized clearly: "My goal is to show that the heavenly machine is not a kind of divine living being but similar to a clockwork in so far as almost all the manifold motions are taken care of by one single absolutely simple magnetic bodily force, as in a clockwork all motion is taken care of by a simple weight. And indeed I also show how this physical representation can be presented by calculation and geometrically." If magnetic force is here replaced by the designation attractive force and the limitation "almost" is omitted, then with these words the great problem of classical celestial mechanics is formulated. In historical accounts it is repeatedly stated that it was Galileo who founded the Copernican theory physically. While fully appreciating Galileo's accomplishments in the domain of mechanics, it must still be emphatically pointed out that he completely failed to comprehend the idea of a celestial mechanics. In none of his works did he take notice of Kepler's planet laws although he certainly knew them. Not once in his famous *Dialogue* about the systems of the world, which appeared a quarter of a century later, did he speak of them, although they surely should have played a central part. Yes, as though Kepler had spoken into the wind, Galileo praised Copernicus in this work, because he understood how to present the planet motions by uniform circular motions; he sticks here throughout to the old Aristotelian distinction between "natural" and "violent" motion. So it was Kepler first of all, not Galileo, who freed astronomy from the bonds of Aristotelian physics.[1]

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[1] Ed. Note. Although it is not advisable to go into detail at this place concerning Galileo's failure to mention Kepler's laws of planetary motion, it is important that the reader's attention be drawn to certain recent contributions to the literature concerning this subject even though they can be treated only superficially here. In this regard see:

Erwin Panofsky, *Galileo as a Critic of the Arts*, The Hague: Nijhoff, 1954, especially pp. 20 ff.

Alexandre Koyré, "Attitude esthétique et Pensée scientifique," *Critique*, Sept.–Oct., 1955, pp. 835–47, which is a critical review of the Panofsky pamphlet listed above. (Prof. Koyré's earlier *Etudes Galiléennes* (1939) is cited by Prof. Panofsky in the 1954 pamphlet.)

———, "A Documentary History of the Problem of Fall from Kepler to Newton; *De Motu Gravium Naturaliter Cadentium in Hypothesi Terrae Motae*," *Transactions of the American Philosophical Society*, new ser., vol. 45, part 4, 1955.

Erwin Panofsky, "Galileo as a Critic of the Arts: Aesthetic Attitude and Scientific

Thought," *Isis*, XLVII (1956), 3–15, which is an "abridged and somewhat revised version" of the author's pamphlet listed above and which uses suggestions made by Prof. Koyré in his review.

Edward Rosen, Review of Panofsky, *Galileo as a Critic of the Arts*, 1954, in *Isis*, XLVII (1956), 78–80.

Erwin Panofsky, "More on Galileo and the Arts." *Isis*, XLVII (1956), 182–5, discussing Prof. Rosen's review.

As is known by all three of the authorities cited above, there is available (Galileo, *Opere*, Ed. Naz., XI (Florence, 1901), 365–7) a letter dated July 21, 1612, from Frederico Cesi to Galileo which refers to the ellipses of Kepler as a matter of common knowledge. Since Galileo was not ignorant of Kepler's laws, why did he ignore them? As Prof. Panofsky says (1954, pp. 24–5) "At the very beginning of the *Dialogue*, Galileo unequivocally endorses the belief...in the perfection...of the circle not only from a mathematical or aesthetic but also from a mechanical point of view. According to him the qualities of uniformity and perpetuity, reserved to rectilinear motion in post-Galilean dynamics, exclusively belong to the circular movement which Huygens and his successors have taught us to consider as vectorially accelerated." Prof. Panofsky goes on to say (p. 25) that Galileo says that before the world was created rectilinear motion may have had some use but that thereafter only circular motion is naturally appropriate to the bodies constituting the universe. Prof. Panofsky believes that the "haunting spell of circularity," borrowing a phrase from Prof. Koyré, "made it impossible for him [Galileo] to visualize the solar system as a combination of ellipses." "Kepler, on the other hand," says Prof. Panofsky (1954, p. 26), "did break the 'spell of circularity'...he considered the rectilinear...movement as privileged as far as the physical world is concerned:..." As Prof. Rosen points out (*Isis*, XLVII, 79), Kepler abandoned the traditional circular motion because no circle would fit the observations at his disposal. These were the Tychonic observations on whose accuracy Kepler justifiably relied. Prof. Panofsky points out that both the famous astronomers attempted to support their celestial mechanics by a comparison of the motions of the stars with those of the human body, and in this also arrived at opposing views, Kepler believing that all muscles move in accordance with the principle of rectilinear movement and Galileo that all human movements can be reduced to a system of circles.

Prof. Koyré says (1955, pp. 842–3) that Galileo rejected the Keplerian ellipses for the simple reason that they were ellipses, and that for Galileo the Keplerian astronomy was an astronomy of Mannerism. Galileo very probably had the same aversion for Kepler's symbolism and use of cosmotheological reasoning that he had for the allegorism of Torquato Tasso (1955, p. 846).

Prof. Rosen (*Isis*, XLVII, 79) finds a reason for Galileo's neglect of Kepler's laws in Kepler's obscurity, prolixity and mysticism which were so repugnant to Galileo that he had no desire to seek out "the nuggets of real gold hidden away in Kepler's heap of dross."

The question, which was more modern, Galileo or Kepler, brings out the fact that both men had prejudices, although different ones. Kepler was able to substitute celestial dynamics for celestial cinematics because he clung to Aristotle's interpretation of motion as a "process," believing that the planets would cease to move if the force emanating from the sun ceased to act on them. Galileo, on the contrary, considered motion as a "state." Prof. Panofsky concludes his 1955 article with the statement that "as Galileo ignored—and, in a sense, was bound to ignore—Kepler's ellipses, so did Kepler ignore—and, in a sense, was bound to ignore—the principle of inertia quite clearly (though restrictedly) stated in Galileo's *Second Letter on the Sunspots* of 1612." It may be trite to add what Prof. Caspar, because of his desire to emphasize the importance of Kepler's overthrow of the age old axiom of circular motion, did not include here, that the Newtonian synthesis rested upon the works of both Galileo and Kepler, that both the Keplerian celestial dynamics and the Galilean dynamic physics were needed.

An unusually interesting and comprehensive treatment in English of the physics and metaphysics of Kepler's universe should be mentioned here because it distinguishes the different aspects of Kepler's approach to the structure of the universe: Gerald Holton, "Johannes Kepler's Universe: Its Physics and Metaphysics," *American Journal of Physics*, XXIV (May, 1956), 340–51.

Of course Kepler did not reach the high goal of celestial mechanics which he was the first to set up and perceive. It was reserved for Newton's genius, by stating the law of gravity, to crown the structure which Kepler had begun, and to prove clearly that the planet laws follow as necessary consequences of this general law of nature. Yet various of his remarks show how close Kepler actually came to this law, for example, when he says the magnetic, that is the attracting, force of the sun spreads itself out like light and at another place proves that the intensity of the light diminishes with the square of the distance, or when he categorically asserts: "If one would place a stone behind the earth and would assume that both are free from any other motion, then not only would the stone hurry to the earth, but also the earth would hurry to the stone; they would divide the space lying between in inverse proportion to their weights."[1] That is an unprecedented speech in a time when the Aristotelian theory of weight was still universally recognized. Here the clever researcher positively has at hand the idea of universal gravitation. However, in the physical presentation of the planet motions he did not follow this concept but pictured the motion differently. He divided it into two components, a circular revolution around the sun and a deviation along the radius vector. The first motion is taken care of by the sun. The force issuing from the sun spreads out in the plane of the ecliptic and grows weaker with the distance away. Now the sun, since it rotates (an assumption which Kepler made *ad hoc* a few years before the actual discovery of axial rotation), by means of its likewise rotating rays of force, pulls the planets around in a circle. The phenomenon that the rate of the orbital velocity is less than the rate of rotation of the sun is to be explained by the inertia of the planet body which by nature inclines to rest. Stimulated by the very important work on magnetism published in 1600 by William Gilbert, the Englishman, Kepler explains the deviation in the radius vector by imagining the planet bodies polarized, that is consisting of parallel magnetic filaments one end of which is pulled by the sun while the other is being repelled. A vital force is supposed to hold these filaments continually in a parallel position, and perpendicular to the line of apsides. Now if the rays of force of the sun pull the planet around away from aphelion, where the effect of the sun is the same on both poles of the planet, then the end of the filaments, which undergoes an attraction, lies nearer to the sun than the other. Thus the planet steers toward the sun and, indeed, it does so until it reaches perihelion.

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[1] Ed. Note. *Johannes Kepler Gesammelte Werke*, XV (letter number 358), from Kepler to David Fabricius, from Prague, Oct. 11, 1605, pp. 240–80, especially p. 241. Similarly, see *ibid.*, III (*Astronomia Nova*), 25.

From that point on, the reverse takes place. The deeper reason why Kepler, in explaining the mechanism of planetary motion, could not penetrate to the knowledge which we owe to Newton is obviously that he lacked the conception that a mass remains in uniform straight line motion if no external forces act on it.[1]

In the summer of 1605 Kepler had collected those researches about the orbit of Mars, which we have followed above. The first part had already been finished during Tycho Brahe's lifetime. The area proposition was introduced soon thereafter, in the year 1602. Other works occupied the year 1603. Carrying through the oval hypothesis and rejecting it took up nearly the whole following year. In December, 1604, once more weighed down by thoughts of death, he considered depositing his manuscript, as far as it went, at the University of Tübingen. But he soon recovered from his depression and abandoned this plan. Final success was near; at Easter time, in 1605, he discovered his ellipse proposition. Justifiably he could give the work he had composed the proud title: *Astronomia Nova αἰτιολογητος Seu Physica Coelestis, tradita commentariis de Motibus stellae Martis.* It is the first modern astronomy book.

There were grave obstacles in the way of publication. The first difficulty Kepler encountered emanated from Brahe's heirs. After Brahe's death a strained relationship had developed between them and Kepler. This immediately came to the surface when, executing the imperial commission to publish the works left by Brahe, Kepler tackled the completion of the printing, already well along, of Brahe's great *Progymnasmata*. To this book which dealt with Brahe's solar and lunar theories as well as with the fixed stars and the new star of 1572, he composed an appendix[2] and here and there he made improvements. Now, to his annoyance, the "Tychonians," without his knowledge, had various notes printed, which he had written down for private use. They also kept him from correcting proof, so that many errors remained. The main subject of the fight between the two sides was Kepler's use of the Tychonic observations. After Tycho's death, the right to possess them was transferred to his heirs. The emperor wanted to acquire them along with the instruments and offered the heirs the

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[1] Ed. Note. See the footnote before last, concerning recent literature. Kepler wrote to Fabricius (...*Gesammelte Werke*, vol. XV...241), in a letter dated Oct. 11, 1605, that by nature every body inclines to rest, "Quodcunque materiatum corpus, se ipso aptum natum est quiescens, quocunque loco reponitur."

[2] Ed. Note. In a letter to Magini, Kepler explicitly stated that he was the author of this appendix, "Appendicis ad Progymnasmata ipse author sum: . . ." *Johannes Kepler Gesammelte Werke*, XVI, 279–80 (letter number 551), dated from Prague, Feb. 1, 1610, especially p. 279. The appendix, which was published as part of the book, can be read in *Tychonis Brahe Dani Opera Omnia*, III (1916), 320–3.

sum of 20,000 talers for them. Naturally, however, the imperial treasury had no money to satisfy them. Indeed, in the course of the year, they received a few thousand talers. This, however, was not sufficient to cover the accumulated interest. On the other hand, Kepler could not carry out the imperial order without free and unhampered use of the observations, of which, after all, he had taken charge immediately after Brahe's death. Besides, as a follower of Copernicus and an opponent of the Tychonic system of the universe, Kepler's researches led him further and further from the theoretical point of view of the man without whose observations he could not have carried out his investigations successfully. Out of this arose, spontaneously, the reproach on the part of Brahe's heirs, that Kepler was not using the observations in the sense intended by the man who had acquired them by many years of toil, and that he was looking out for his own fame and advantage. His chief opponent was Brahe's son-in-law, Tengnagel, who, in order to keep the upper hand, repeatedly promised a publication of his own on the basis of the observations, although he was not at all qualified for such a task. Indeed, he preferred not to be called a mathematician. Contrasting character traits made an amicable agreement more difficult. Kepler appropriately characterized the way his opponent wanted to guard Brahe's treasure by comparing him, following Aesop's well-known fable, with a dog in the manger who certainly eats no hay himself but also lets no one else near it. The chief plan to be realized was the working out of the *Rudolphine Tables*, whose completion was very important to Tengnagel and his family, partly because of the paternal glory, partly because of the sound of money clinking which they hoped it would bring. Whereas they were not at all aware of the difficulties of this problem, Kepler, on the other hand, fully understood that only someone full of self-confidence and willing to risk his scholarly calling could hope to solve this problem rapidly. To him it was clear that, before the work on the tables could be approached with a view to success, he first had to solve the problem in the midst of which he was already embroiled, and free the old planet theories from the faults clinging to them, that is found a new astronomy.

In the course of these disputes, the emperor's father confessor, the prelate Johannes Pistorius, was selected as the man to whom Kepler should now and then report on the use of his time and about his studies. Pistorius was favorable to Kepler and in the circumstances it might not have been too unpleasant for Kepler to consent. It was, however, less pleasant when in the year 1604 in return for the relinquishment of the Tychonic observations he had to agree in writing not to

publish anything based on them without Tengnagel's approval until the *Rudolphine Tables* were completed. This made the publication of the commentary on Mars dependent on Tengnagel's consent. Now the latter wanted to undertake the elaboration of the tables himself and orally promised the emperor that they would be completed within four years. However, he was in no hurry with this work, and hardly did or could set about them in real earnest. Consequently Kepler found himself in a disagreeable position. This did not improve when Tengnagel soon after was named imperial appeal counsel and joined the Catholic Church, further increasing his influence at court. Tengnagel, after taking office, could think less than ever about carrying out the work he had promised. Thus he had Kepler in his power. Accordingly, the latter foresaw further disputes because Tengnagel's sole object was to guard the fame of his father-in-law, whereas Kepler, on the contrary, had the freedom of research in view. In fact, Tengnagel threatened to prevent the printing of the commentary on Mars when Kepler fancied himself free of his agreement after his opponent failed to fulfill his promised term of four years. Nevertheless, an agreement was finally reached because Kepler declared himself ready to insert a preface by Tengnagel at the beginning of his work.

The second obstacle to publication was the delay of printing for financial reasons. At the end of 1606, Emperor Rudolph granted 400 gulden, "because for the extension of the fondness of patronizing astronomy, which is our custom and that of our predecessors in the Austrian House, we did not gladly leave untouched the previously mentioned book, in which so many glorious secrets of nature are included." However, since Tengnagel's approval had not yet been given, the printing could not be started immediately. In the meanwhile Kepler spent the money "in a great part otherwise and for household needs" because the payment of his salary stopped. With the remainder he got the printing under way in 1608 at Ernst Vögelin's in Heidelberg. Since the sum still at his disposal did not suffice and Kepler wanted to travel to Heidelberg, he had to ask the imperial Maecenas for further financial aid. As a consequence the latter bestowed an additional 500 gulden. Since everything moved slowly, the printing was not completed until the summer of 1609, while the author was in Heidelberg. The emperor had denied Kepler the public sale of the book and ordered that "he give no one a copy of it without our previous knowledge and consent." He reserved the ownership of the entire edition evidently because it was composed by Kepler in the pursuance of his office and printed with imperial money. In this order there is also implied a recognition of the great importance attributed by the emperor to the book which he wanted to distribute himself. However, since the imperial treasury remained continually in arrears with salary payments and the emperor's situation had in the meanwhile become so precarious that he could trouble himself but little more with such things, Kepler tried to recover his losses and in the end sold the whole edition to the printer. In make-up, the book corresponds completely to the importance due to the *New Astronomy*. In big folio format and lovely print, it is the most magnificent of all the works which Kepler published. Because only a small edition was printed, it is today by far the most expensive of the great astronomer's first editions.

4. Astronomia Pars Optica

It is reasonable to suppose that the planet orbits with their secrets and whims had so filled Kepler's thoughts that no room remained for other scientific research. And yet we see him at the same time busy with another comprehensive complicated question. This certainly touched upon the former but still, for the most part, had its own form and meaning, namely the subject of optics.[1] When he went from Graz to Prague, he already carried a sizable portion of the questions within him; others were aroused by the association with Tycho Brahe as well as by the exigencies of his work on the planet orbits. One can recall the eclipse observation which Kepler had made in July, 1600, with his own instrument, devised and constructed for this purpose, as well as the successful detailed considerations he made in this connection regarding the laws for pictures by a pinhole camera. Because of unfavorable circumstances he did not carry out his original intention of publishing immediately what he found here. But the problem which was here set had established itself in his thought and developed further. However well instructed a person was in general about the occurrence of solar and lunar eclipses of his time, there still arose a host of separate questions which needed explanation and solution as soon as he concerned himself more closely with these phenomena, often observed since antiquity. Yet such a dependable astronomer as Tycho Brahe had denied the possibility of a total solar eclipse. The many observations zealously gathered by Kepler both from ancient literature and from contemporary reports frequently did not correspond exactly with each other or would not agree with the calculations as to the passage of time and the size of the obscuration to the same degree which a good theory demanded. There could be various reasons for these disagreements. They could stem from the fact that the numerical values, on which were founded the calculations for the sizes and distances of the two heavenly lights, were inexact, or from the fact that the phenomena of the motions of the sun and moon had not yet been completely mastered. The cause could, however, also lie in too crude an observational procedure that did not take into consideration certain external circumstances and relied too much on estimates instead of on exact methods of measurement. In addition, still other questions appear, with which Kepler had been partly occupied previously. Whence comes the reddish light of the moon during a total lunar eclipse? What is the explanation of the reported luminous appearances around the sun at total solar eclipses? The question concerning the diminution of the lunar diameter during solar eclipses was already recalled. So many questions, so many problems. They all made Kepler restless and forced him to formulate a great plan. He wanted to write a book exploring and presenting the sizes and mutual distances of the sun, moon and earth, primarily supported by the phenomena of eclipses. Since he knew from a report by Theon of Smyrna that the great astronomer of antiquity, Hipparchus, had composed a work (since lost) dealing with the same subject, he wanted to give his book the title *Hipparchus*. However, the first step toward carrying out this plan was to examine and clear up those optical questions without whose solution the necessary exact and trustworthy foundation to the work could not be securely laid.

A further series of questions on optics, connected, indeed, with the above, but playing a part in all astronomical observations, are grouped about the subject of atmospheric refraction. The importance of this is perceived when it is remembered that it is precisely by the refraction of its rays in the atmosphere that the sun, when on the horizon, is raised by an amount approximately equal to the diameter of the solar disc, so that the sun seems to touch the horizon with its lower rim when in reality it is immersed just beneath the horizon. Certainly, the amount of refraction decreases with increasing altitude; nevertheless, it is still so great that it must always be taken into consideration if the precise star positions, determined with refined instruments, are to have meaning. Brahe's improvement of the art of observation consequently aspired to more exact knowledge of refraction. He himself

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[1] Ed. Note. The optical work of Kepler, in the setting of its time, is well handled by Dr. Vasco Ronchi, director of the Istituto Nazionale di Ottica in Florence, in "L'ottica del Keplero e quella di Newton" in *Atti Della Fondazione G. Ronchi*, anno XI N.3 (1956), pp. 189–202. See also Vasco Ronchi, *Optics: The Science of Vision*, translated from the Italian and revised by Edward Rosen. New York: New York University Press, 1957, pp. 40–51, 263–5.