Introduction

When Isaac Newton published the *Principia Mathematica*, he laid the foundation for modern celestial mechanics: the principle of universal gravitation. Simply stated, universal gravitation meant that every particle in the universe attracted every other particle in the universe, with a force that decreased as the inverse square of the distance between the two. From this simple law, Newton derived three laws of Kepler, using only the simplification that the mass of the planets was so small as to cause insignificant effects on the other planets. However, the fact that universal gravitation was *universal* meant that Kepler's three laws were only an approximation to the actual situation. In the three laws of Kepler's, the planetary orbits were permanently stable, yet even in Newton's time, it was known that there were minor variations (most notably in the motion of the Moon). Could universal gravitation cause long-term instability in the solar system?

In order for progress to be made, two factors had to be dealt with. First, and more simply, the proper mathematical basis had to be developed. Newtonian mechanics, essentially correct, was saddled with Newtonian mathematics, essentially geometric. When Newton wished to determine the variations in the motions of the Moon caused by the force of the sun, he had to resort to a very complicated geometrical argument, and even then the utility of the result was limited to a the determination of the mean value of the variations. Fortunately for the development of celestial mechanics, modern analysis was being developed on the continent, by such mathematical giants as the Bernoullis and Euler.

However, another factor conspired to prevent the powerful methods of analysis from joining with the correct system of mechanics. Universal gravitation required the sun to somehow affect the other planets, across millions of leagues of intervening vacuum. Most English scientists shrugged off the difficulty, following Newton's lead: the phenomenon exists, even if the physical basis is unknown. Continental scientists (particularly French ones) clung to another system that predated Newton: that developed by Rene Descartes.

To Descartes, space was filled with particles of an immaterial vortex, which whirled endlessly around the sun, carrying the planets like leaves in a stream. Variants of the Cartesian vortices persisted into the middle of the eighteenth century, hampering the acceptance of the essentially correct Newtonian system. Though the vortices as Descartes described them gradually faded into the background, it remained in the form of a tenuous, interplanetary medium, possibly the extended atmosphere of the sun and planets, possibly a new type of matter, but certainly something very different from empty space. This interplanetary medium was used to explain everything from the orbital motions of the planets, to the co-planarity of the planetary orbits, and even their sidereal rotations. Even if one treated the planets as being, somehow, attracted to the sun and each other by the Newtonian inverse square law, the effects of this interplanetary medium had to be taken into account before any question of the long-term stability of the solar system could be answered.

If universal gravitation could account for the motions of the planets to a high degree of accuracy, then even the notion of action at a distance could be set aside, and the general validity of the Newtonian system accepted by nearly all. Unfortunately, three phenomena could not be accounted for by gravitation.

The first was the progression of the Moon's apse. This point was observed to advance at a rate of approximately three degrees an orbit, but Newton could account for only about half the motion, a sizable discrepancy.

Second was a secular acceleration in the mean motion of the Moon, discovered in 1695 by Newton's friend (and financier of the publication of the *Principia*, Edmond Halley. After comparing eclipse records of Ptolemy in the second century and Albategnius in the ninth, Halley concluded that observational error alone could not account for the discrepancies between Ptolemy's predicted times and the times recorded by Albategnius. However, the discrepancy could be accounted for if one assumed a slow acceleration in the Moon's mean motion. Such an acceleration could be caused if the Moon was somehow approaching the Earth, but gravitation alone seemed to offer no mechanism by which this could happen. If one assumed, like the Cartesians, a resisting interplanetary medium, this approach was aunderstandable, for as the Moon lost momentum by moving through a resisting interplanetary medium, it would gradually approach the Earth.

The third was not discovered until 1718. In that year, the French astronomer Maraldi noted (again using ancient observational data) that the mean motion of Jupiter and Saturn appeared to be different than it was in the past; specifically, Jupiter seemed to be accelerating and Saturn appeared to be decelerating. Again, this could be accounted for if the two planets were changing their mean distance from the sun. The case of Saturn was particularly vexing, for while a resisting interplanetary medium might explain the acceleration of Jupiter (and the Moon), such an explanation would not work for Saturn.

Historically, the first problem was the most important; until it was solved, scientists retained doubts about the essential accuracy of the Newtonian system. Even those, like Euler, who believed in universal gravitation, questioned its application. Euler in particular raised, time and again, the question that while the inverse square law might hold for infinitesimal bodies, and for perfectly homogeneous, spherial bodies, the planets were neither, and the discrepancies between observation and calculation could be accounted for by suitable modifications of the law of gravitation.

The first problem also attracted the most attention. A slow acceleration in the mean motion of the Moon did not affect its position except on a time scale of hundreds of years. However, the rapid motion of the Lunar apogee meant that predicting the position of the Moon for even short periods of time was a very difficult problem. Moreover, in this case, knowledge of the position of the Moon had a very practical (and lucrative) benefit. The reason was longitude.

Briefly, the best method of determining a ship's position at sea was to observe the stars. Determining latitude was almost trivial: as one moved north or south, different stars could be observed, and by noting which of certain prominent stars appeared at what angle of declination, one could find one's latitude. Longitude was more difficult. To determine longitude, one had to observe the time that a known star passed a certain position (often the zenith) and compare that with the time the star was supposed to pass the same position at home. The difference between the two times was directly proportional to the difference in longitude between the ship and home.

Unfortunately, this required one to know what the time at home was, and before the invention of an accurate marine chronometer, one had to rely on some other method. Because hundreds of lives (and millions of pounds) were at stake, the British Admiralty offered an enormous prize for the "discoverer of longitude".

There are two obvious approaches to "discovering longitude". The first is to build a clock that can keep time on a ship that rocks and rolls unpredictably, encounters varying extremes of temperature and humidity, and can survive the occasional swamping. In order to win even the smallest portion of the prize, it also had to be accurate to within fifteen minutes over the course of a long sea voyage.

The other approach was to use the "clock in the sky": the regular motion of the planets. Much earlier (to the King of Spain) Galileo suggested using the timing of the eclipses of the moons of Jupiter for this very purpose. However, in addition to the difficulty of using a telescope on a moving ship, there was also the fact that the finite speed of light made the eclipses appear at different times during different parts of the year.

Much closer to home was the Moon itself. Unless the sky was completely obscured by clouds, the Moon's position could be measured accurately and with little difficulty. Combined with an accurate table of the Moon's position, the time at home could be determined, and the longitude could be found. However, the motion of the Moon did not have the even regularity of the moons of Jupiter; Newton himself claimed the problem of the Moon's motion was the only one that ever made his head ache.

In January of 1749, the French astronomer Alexis Claude Clairaut finally solved the problem of the motion of the Moon's apse (though not the general problem of the Moon's motion) by taking into account second order terms in the approximation of the motion of the Moon. In December of 1749, the Imperial Academy of the Sciences at St. Petersburg offered, for their first contest, a prize for determining if the motion of the Moon's apse could be accounted for by universal gravitation alone, or if other factors needed to be taken into account. Clairaut's entry won the prize.

Nearly simultaneous with Clairaut's determination of the motion of the Moon's apse, Euler won the 1748 French Academy prize for determining the motion of Jupiter and Saturn (though could not explain the secular variations in their mean motion). Using universal gravitation by means of the inverse square law alone, Euler could determine the position of Jupiter and Saturn to within $5'$ of arc, derived directly from universal gravitation.

The methods of both Clairaut and Euler involve a perturbation of the system of differential equations. First, they solved the unperturbed (or two-body problem), which has an exact solution for the radius r in terms of the true anomaly θ. Then, they assumed that the true orbit would be a perturbation of r and through various stratagems, found a series solution to the perturbed value of r.

These two triumphs of the predictive power of universal gravitation established the general validity of Newtonian mechanics. However, neither explained the secular variations in the motion of the Moon, Jupiter, and Saturn. This caused two schools of thought to develop regarding the remaining discrepancies between observation and prediction. The first, typified by Clairaut and the German astronomer Tobias Mayer (who would later win a portion of the Admiralty Prize for his lunar tables), was that the remaining discrepancies could be accounted for by improving the degree of approximation, just as Clairaut accounted for the full motion of the Moon's apse by using a higher approximation. The second, supported by the likes of Euler and d'Alembert, was that additional factors besides gravitation by means of the inverse square influenced the motion of the planets.

Of these additional factors, two played a major role. One was a variation of the inverse square law, possibly caused by the non-sphericity of the planets, to which Euler attributed the remaining discrepancies between prediction and observation. The other was a vestige of Cartesian philosophy: an interplanetary medium, which, by resisting the motion of the planets, would cause them to spiral slowly towards the center of their orbits.

In 1762, the French Academy of the Sciences offered an award for determining whether the planets moved through a resisting medium; the Academy actually awarded two pieces that determined the actual motion of the planets through such a medium. Both the pieces of Euler's son, Johann Albrecht Euler (who won an Award of Merit from the Academy) and the Abbé Bossut assumed it almost certain that the planets moved through a medium (both, for example, suggested that the light of the sun itself filled space with particles, and Euler hearkened back to the notion of the Cartesians, suggesting that without *some* matter in the space between the planets, gravitation could not be transmitted from sun to planet). Like Euler and Clairaut's work, both assumed the solution to the differential equations of motion to be a small perturbation of a simpler case.

The 1762 work of Bossut and Euler marked the final solution to the question of a resisting interplanetary medium. Both established what its precise effects would be, if it existed. However, neither demonstrated its existence and soon the notion of a resisting interplanetary medium would be discounted as an important factor in the motion of the planets. Instead, the mutual attraction of the planets for one another would become the dominant feature of celestial mechanics.

The change in viewpoint was slow in coming. Newton initially discounted the attraction of the planets for one another, but later, in a famous quote, wondered whether their mutual attraction might cause a disruption of this "most beautiful system". Others, like Cassini, wondered the same, and Euler's work clearly demonstrated the need to take into account the perturbing effects of the other planets (especially that of Jupiter, the largest planet).

Preliminary steps were taken by Charles Euler, who won the 1760 prize from the French Academy of the Sciences for answering the question of whether the planets, through their mutual attractions, could affect their mean motions — an important question that relates to the overall stability of the solar system. Euler's method can be traced back to a method suggested by his father and developed by his brother: analysis of the elements of orbit.

Rather than try and determine a solution to the radius vector r, the method of the Eulers involves examining the actual elements of orbit, six of which completely determine the elliptical path of the planet. Three of these (the semimajor axis, the eccentricity, and the initial position) determine the general size and shape of the ellipse, while the other three (the inclination, position of the nodes, and position of the apse) determine the ellipse's position in space. By rewriting the differential equations of motion in terms of orbital elements rather than in terms of a body's space position, Charles Euler obtained a differential expression for a, the semimajor axis, which completely determines a planet's mean motion. Euler then demonstrated that, to a first degree approximation in the masses, that the mutual attraction of the planets cannot cause any long term variation in the mean motions, but only periodic variations.

Even more important work was done by Joseph Louis Lagrange. Lagrange further extended the method of the Eulers, and developed the modern method of variation of parameters in a 1766 paper devoted to solving differential equations with particular application to the motion of Jupiter and Saturn. Then, in 1774, Lagrange made another innovation. Instead of looking at differential equations for individual elements of orbit, Lagrange introduced quantities of the form $s = \theta \sin \omega$ and $u\theta \cos \omega$, where θ is the tangent of the inclination and ω is the position of the nodes. The introduction of these new variables allowed him to greatly simplify the differential equations. Lagrange demonstrated that, to a first degree approximation in the tangent of the inclination and the sine or cosine of the nodes, the ilne of the nodes will vary (in a fairly complicated pattern in the case of three or more planets), but — importantly for the stability question — the tangent of the inclinations is bounded, being a finite sum of sines and cosines.

Though traditionally given credit for establishing the stability of the solar system, it is only after Lagrange's work that Laplace made his first major contribution to the theory of the stability of the solar system. Following Lagrange's lead, Laplace introduces variables $x = e \sin L$ and $y = e \cos L$, where e is the eccentricity of the orbit and L is the aphelion position, and shows, like Lagrange, that the eccentricity is bounded (and the aphelion position varies in a complicated pattern in the three or more planet case).

Lagrange also investigated the remaining problems in celestial mechanics: the accelerations in the mean motion of the Moon, Jupiter, and Saturn. For the 1772 Academy Prize, Lagrange examined whether the non-sphericity of the Earth was responsible for the secular acceleration of the Moon. Lagrange eliminates all reasonable sources of the acceleration save one: observational error. Since the evidence for the acceleration was based on eclipse observations two thousand years old, Lagrange felt it reasonable to assume that it was poor observational data, and not any actual physical phenomenon, that caused the Moon's apparent secular acceleration.

Lagrange further examines the situation in 1776, and, using the method of the Eulers to its best advantage, shows that *provided* that the mean motions of the planets are incommensurable, then there can be no secular variations in the semimajor axis (and hence, no secular variations in the mean motion of the planets). To Lagrange, this conclusively proves that neither the secular acceleration of the Moon, nor the similar variations in the mean motions of Jupiter and Saturn, can be caused by Newtonian gravitation. Lagrange extended his results in a two part essay published in 1781 and 1782, and for this he was lauded by his contemporaries as having proven the stability of the solar system, a feat nowadays credited mainly to Laplace.

What remained to be done after Lagrange's work of 1782 was minor polishing, primarily the work of Laplace. In 1784 and then again in 1787, Laplace summarized all of the results into his "celebrated equation" that limited the variations in the orbital elements to bounded quantities, provided that the major axes of the planets were constant.

Chapter 1

Celestial Mechanics To Newton

1.1 Celestial Mechanics To Copernicus

One of the first natural regularities to strike man was probably the motion of the celestial bodies, from the daily rising of the sun, to the monthly changing of the phases of the Moon. As man observed the heavens more carefully, he discerned regularities in the motions of the five planets known to the ancients, and constructed elaborate methods of predicting their position in the indefinite future. These methods were essentially empirical, matching the motions of the planets to some "function" that, with the correct computations, would yield the future position of a planet, and were some of the earliest mathematical computations required by man.

In most traditions, the lasting regularity of the planets was but a temporary feature; at some point, perhaps in the near future, the hand of God or the gods would disrupt the regular harmony in the heavens, as a prelude to the destruction of the universe. However, in practice, since the date of this occurrence could not be calculated, it was not included in any theory of the motions of the planets.

As astronomy grew in sophistication, an elaborate theory of cycles and epicycles, eccentrics and deferents, developed, to provide better agreement between theory and the motion of the planets. The first major advance in celestial mechanics came with the recognition that the Earth was just another planet, moving around the sun in some path or another.

At first, astronomers were unwilling to break free of the notion of perfectly circular motion; thus, when Nicholas Copernicus (1473-1543) reintroduced the heliocentric theory in 1543, he retained the cycles and epicycles of the Ptolemaic theory, but removed their center to the sun. The introduction, not written by Copernicus, claims that the theory was not meant to describe the actual state of affairs, but rather to provide a simpler mechanism for calculating the positions of the planets.

1.2 Kepler

Several important points were established by Johannes Kepler (1571-1630). In the beginning of the seventeenth century, Kepler announced three laws, which marked the beginning of modern celestial mechanics (op. cit. Wilson [1972). These laws are: planets move in elliptical orbits with the sun at one focus; the radius vector between the planet and the sun sweeps out equal areas in equal times; and that the square of the period of a planet is proportional to the cube of its major axis, and is related to no other quantity.

This ellipse has five parameters which completely determine it: its semimajor axis; its eccentricity; the inclination of the ellipse to a fixed plane (usually the ecliptic, or the plane of the Earth's orbit); the position of the line of intersection between the plane of the ellipse and the ecliptic (the line of the nodes); and the position of the major axis (usually measured from the line of the nodes). The angle formed by the perihelion (or, before 1800 A.D., aphelion) position of the planet, the sun, and its current position, with vertex at the sun, is called the true anomaly. Two more quantities of importance are the mean anomaly[1], and the eccentric anomaly E.[2] A ninth quantity, the mean distance, is the semilatus rectum of the ellipse, and equal to $a(1-e^2)$.

Kepler's laws provided means of predicting the future positions of the planets more accurately than ever before. However, there were anomalies that Kepler's three laws did not explain, most notably in the motion of the Moon. One of these came to be known as the Variation, and was discovered by Tycho Brahe (1545-1601), Kepler's mentor (Moulton [1914], 350) Tycho noticed that the angular distance between the sun and the moon affected the rate at which the Moon's true anomaly progressed. Kepler went further and in his introduction to his *Commentaries on Mars*, maintained that this variation was caused by the joint action of the sun and Earth, a glimmer of things to come.

1.3 Descartes

Though Kepler speculated that the planets affected each other, his system lacked a method to calculate these perturbations *a priori*. The first of the two models of the universe that would vie for scientists' attention during the early part of the eighteenth century was introduced in 1644 by the French polymath René Descartes (1596-1650), in his *Principia Philosophiae*. In Book III of that work, titled "Of the Visible World", Descartes gives his conception of the structure of the physical world.

[1] The position that a planet would be in provided it moved in a circular orbit whose radius was equal to its semimajor axis.

[2] The mean anomaly M and the eccentric anomaly E are related by "Kepler's equation", $M = E - \sin E$.

Descartes postulates that "empty space" is actually filled with particles, moving in vortices that whirl around the stars, the Earth, and Jupiter (Descartes [1644], 97). These vortices sweep the planets (and satellites) along, which are at rest within the vortices (Descartes [1644], 94). To explain the laws of Kepler, Descartes assumes that the vortices nearer to the sun move faster (Descartes [1644], 96). Meanwhile, the planets, are held in position by the resistance of the particles of the vortex (Descartes [1644], 115).

Descartes attempts to explain the motion of the planets in Propositions 140-45. The vortex particles, like a stream, can spread out and slow down (proposition 141), causing the planet's motion to do likewise; where the vortices "bunch up," in places where several planetary orbits approach each other, the corresponding vortices must accelerate, and hence the motion of the planets through those sections of the orbits must likewise be sped.

With this theory, Descartes attempts to explain the variation of the Moon. When the Moon is closer to the sun, it is in part of the sun's vortex that moves faster than the Earth, and thus the Moon itself moves faster in conjunction. Meanwhile, at quadrature, when the Earth and Moon are at the same distance from the sun, the Moon is thus not sped (Descartes [1644], 175).[3]

The presence of a physical mechanism that explains the motion of the planets provides more flexibility, and the possibility (which does not materialize) that the motions of the planet can be calculated from first principles. However, it also provides the first suggestion that, even without Divine intervention, the system of the planets might not be permanently stable. Descartes is the first to suggest that the simple laws of physics could cause the solar system to be unstable: he suggests the possibility that a vortex surrounding a "star" might be absorbed by the surrounding vortices, causing it to become a "comet" (Article 115) (Descartes [1644], 147) Once it does so, it can go wandering about the heavens (Article 127) (Descartes [1644], 156-7).

1.4 Newton

At the end of the seventeenth century, a competing explanation for the physical world appeared: universal gravitation. So prevalent was the doctrine of vortices that Newton felt compelled to spend a section of his work pointing out that "the hypothesis of vortices is pressed with many difficulties." (Newton [1726], 369)

Newton's key contribution was the mathematization of nature. Rather than try and debate the merits of a theory, based on rhetoric and attacks on opposing theories, Newton's mathematical methods provided a new means of evaluating theories: if a theory's predictions, based on its mathematics, were accurate, it was to be commended; otherwise, it would be amended or discarded.

This appears most strikingly in Book III, which can be divided into two sections. The first, larger section, is devoted to demonstrating that gravitation

[3]Unfortunately for Descartes' glib explanation, the Moon should be moving *against* the solar vortex at conjunction, and hence ought to move slower, not faster.

alone was sufficient to explain most celestial motions.[4] For example, Newton determines, in Book III, Proposition XXXII that the annual motion of the nodes is $19°18'1''23'''$, compared to the observed motion (in Newton's time) of $19°21'21''50'''$(Newton [1726], 313). Other calculations of Newton regarding the Moon's motion (with the conspicuous absence of the motion of the Moon's apse, or apogee) are equally close to the actual, observed values.

Newton's method involved geometric construction to analyze the motions of the planets and especially the Moon, and in light of later methods, is very complex. The key idea behind Newton's method is assuming that the Moon moves in an ellipse, the elements of which are continuously being changed by universal gravitation; translated into analytic terminology this will be the foundation on which Lagrange and later Laplace build their proofs of the stability of the solar system.

The most notorious of the Moon's inequalities is the motion of the Mon's apogee. To determine this quantity, Newton assumes that the Moon moves in an ellipse, which was itself turning about one of its foci (the Earth). This method was used by Jeremiah Horrocks (1618?-1640) in deriving his lunar tables, which were much favored by Newton; in calculating the Moon's longitude, Horrocks used a librating ellipse with variable eccentricity (Grant [1852], 424), though Newton claims the observation that the Moon's orbit is an ellipse revolving about one of its foci is from Halley.[5]

In Book I, Section IX (separated from the remaining calculations on the inequalities of the Moon's motion), Newton examines the motion of particle moving along a rotating ellipse. First, in Proposition 44, Theorem 14, Newton determines that in order to make an ellipse precess in such a way that the ratio between the true anomaly in the stationary ellipse and that of the moving ellipse is a constant, one needs an additional central force inversely proportional to the cube of the altitude (Newton [1726], p. 93).[6] Then, if the ratio between the true anomalies of the stationary ellipse with the moving ellipse (measured to the same line of reference) is $F : G$ (i.e., the angular velocities around the center have that ratio) then the force needed is proportional to

$$\frac{FF}{A^2} + \frac{RGG - RFF}{A^3}$$

where A is the distance between the body P at the center C, and R is the length of the semilatus rectum (Newton [1726], 95).[7]

---

[4]The second, shorter section deals with the deficiencies in the theory of vortices.

[5]Compare (Gaythorpe [1925], 859). In Horrocks' system, the position of the apse moved in a sinusoidal fashion. Newton credited Halley with determining, from observation, that the superior (empty) focus of the Moon revolves around the Earth with a uniform motion. (Gaythorpe [1925]) cites the insufficiencies in Horrocks' theory as the prime reason that Newton gave Halley credit for this, which was implicit in Horrocks' theory. One should not take Newton's attempt at history too seriously.

[6]The reason is that an object on the moving ellipse experiences centrifugal force due both to its motion about the ellipse, and the ellipse's motion about the center.

[7]Newton obtains this equation by considering the centripetal forces acting on the particle in the moving ellipse; the first portion, $\frac{FF}{A^2}$, is the centripetal force caused by the motion of

If the ellipse is very nearly circular, and letting T be the semimajor axis, X be the difference between T and A, one arrives at the force equation:

$$\frac{RGG - RFF + TFF - FFX}{A^3}$$

Thus, $F : G$ can be computed, which will be the ratio between the motions of the stationary and the moving ellipse, by the following method: Newton supposes that the force is given in terms of the distance A. Then, using $A = T - X$, where (since the orbit is very nearly circular) X is supposed to be zero, and $T = R$, one then compares the coefficients of the above equation with the force equation.

For example, if the central force is a constant (and thus independent of the distance to the center), e.g., as $\frac{A^3}{A^3}$, Newton determines that the ratio $G : F$ is $1 : \sqrt{3}$, and thus if the body in the stationary orbit makes an angle of 180 degrees (i.e., moving from the upper apse to the lower one), the body in the moving orbit makes an angle of only $\frac{180}{\sqrt{3}}$ degrees — hence the line of apses regresses.[8] In a manner typical of post-Newtonian science (using mathematics to make points about the physical universe), Newton then uses similar calculations to prove that there can be no law of attraction greater than the inverse cube, as that would cause the orbiting body to spiral inwards towards the central body, or or outwards to infinity.

If the central force is not a simple inverse power, but of the form $\frac{bA^m + cA^n}{A^3}$, then the ratio $G : F$ will be $1 : \sqrt{\frac{mb+nc}{b+c}}$. At the end of this section, Newton considers, without explanation, the case $b = 1, m = 1, n = 4$, and $c = \frac{100}{357.45}$. This choice of values will produce a motion of the apses of $1°31'28''$.

the body around the focus, and the second portion, $\frac{RGG-RFF}{A^3}$, is the additional force caused by the motion of the ellipse.

[8]If the additional force is a constant, then it can be written

$$\frac{A^3}{A^3} = \frac{(T-X)^3}{A^3} = \frac{T^3 - 3TTX + 3TXX - X^3}{A^3}$$

which is proportional to the force

$$\frac{RGG - RFF + TFF - FFX}{A^3}$$

Newton then sets the constant and variable parts proportional to each other; hence:

$$RGG - RFF + TFF : T^3 = -FFX : -3TTX + 3TXX - X^3$$

the latter of which is

$$= -FF : -3TT + 3TX - XX$$

Since the orbits are very nearly circular, Newton then lets X be nothing, and thus these ratios become:

$$GG : T^2 = -FF : -3TT$$

(since when X is zero, then $R = T$, as the semilatus rectum and the semimajor axis will be the same). Or, rearranging terms:

$$GG : FF = TT : 3TT = 1 : 3$$

which is the desired result.

Why these peculiar numbers? The succeeding statement offers a misleading clue: “The apse of the Moon is about twice as swift.” (Newton [1726], 100) This statement led many, such as Euler and d’Alembert (as we shall see below) to believe that Newton tried — and failed — to compute the motion of the apogee, and that additional work needed to be done on the lunar theory.

1.5 The Stability Question Raised

The Cartesians, along with many of their contemporaries, were looking for an all-encompassing “theory of everything”, and they criticized Newtonian mechanics because it could not explain such diverse phenomena as the daily rotation of the planets or the inclination of their orbits to the ecliptic. In particular, universal gravitation provided no physical reason why the planets maintained their places: they could orbit the sun at any inclination, with any eccentricity, and at any distance. What could prevent a disruption of “This most beautiful system of the sun, planets, and comets”? (Newton [1726], 369) Moreover, universal gravitation could not yet account for all the anomalies of the planetary orbits. These insufficiencies led to two major (and one minor) school of thought concerning the source of the remaining inequalities.

The first, an outgrowth of Cartesianism, held that the physical theory was inadequate. These inadequacies centered around two very real problems. First, universal gravitation provided no mechanism by which the sun could affect the Earth, through an intervening vacuum. Leibniz derided this “occult action”, and in fact, this mysterious action at a distance was the major objection to Newtonian mechanics. This could easily be solved if space was filled with some sort of matter, such as the particles of the Cartesian vortex. But these particles could not help but have some sort of effect on the motions of the planets, and any useful theory of astronomy would then have to take this into account.

Even if one accepted that point masses attracted each other by the inverse square law of attraction, and that homogeneous spheres did so likewise, one still faced the problem that the actual planets are neither point masses nor homogeneous spheres: hence, the question arose whether the simple inverse square law sufficed.

The second school of thought held that universal gravitation based on the inverse square law alone is sufficient to explain the various inequalities, but the mathematical methods of approximation are insufficient and need to be improved. For example, Newton’s attempt in the *Principia*, Book I, to calculate the motion of the apse was based on the assumption that the perturbative forces were manifested only as a change in the central force, which itself depended only on the distance to the body in question, and, moreover, Newton used a mean value for this perturbation; even the most basic consideration of the factors involved would suggest that the actual perturbative force would somehow have to depend on the position of the moon in its orbit. Additionally, Newton began with a strong hypothesis on *how* the ellipse moved, and thus the general applicability of the method could easily be questioned.

The third school of thought, a minor one, might be called the observational. If, after extending the mathematical as far as could be done, there still remained discrepancies between observation and calculation, then it was the *observations* whose validity should be questioned. Before one dismisses the observational school too readily, it should be pointed out that many of the observations contradicting Newtonian gravitation by means of the inverse square law alone involve the positions of the planets as recorded by astronomers a thousand or more years in the past, and many questions could be raised about their accuracy.

Historically, the physical school attained early prominence, and, indeed, the mathematical school did not really make an appearance until mid-century, after Clairaut discovered that the inverse square law *alone* was sufficient to explain the motion of the Moon's apogee. Thereafter the prestige and influence of the physical school declined, and by century's end, problems in celestial mechanics would be solved by more mathematics, not more physics.

Out of these schools came the two forms of the stability question. The first was that non-gravitational causes, particularly the resistance of the interplanetary medium, affected the planetary orbits. Newton raised and then dismissed this issue, but for others, it remained open. For the majority of the eighteenth century, in fact, this *was* the stability question, and by 1762, it was tacitly assumed that there was a resisting interplanetary medium, and that it would affect the orbits of the planets: the only question was what the results would be.

The other form of the stability question was that the planets themselves, by mutual gravitation alone, might disturb their orbits "until the system wants a reformation," a point most famously raised by Newton in *Optics*. Because of the uncertainty of the validity of universal gravitation, this problem could not even be asked until after mid-century, but thereafter, progress was rapid and by 1760, a preliminary answer was obtained by Charles Euler, followed, over the next two decades, by the works of Lagrange and Laplace.

Chapter 2

The Presence of a Resisting Medium

The publication of the *Principia* is often portrayed as a great revelation that swept aside contemporary physical theories.[1] One is often left with the impression that Newton was such a giant, towering over all who came before him and many who came after, that though everyone recognized his ideas were brilliant, they could do but little with them. Yet this obscures the fact that, for nearly half a century after its introduction, Newtonian mechanics had few converts outside of England. Cartesian vortices were simpler, more intuitively appealing, and had the virtue (in France, at least) of being invented by a Frenchman.

The continued survival of Cartesian vortices (and the modifications wrought upon them by Neo-Cartesians, like Huygens) should not, however, be seen as mere conservatism or national pride. The *Principia* replaced vortices with a complicated mathematical system that few could understand in detail, and even fewer could use productively. Even worse, the system of Newton introduced one concept that was intuitively unacceptable: action at a distance. *How* could the gravitational force make itself felt across the distances between the sun and planets? Vortices, at least, did not require action at a distance, and for no other reason than this, were seen as being more acceptable. The major supporters of vortices always returned to action at a distance as the fundamental flaw in the Newtonian system.

Newton himself felt this was not a problem: he was concerned with *what*

---

[1] As a prime example, consider Pope's *Epitaph for Sir Isaac Newton*:

> Nature and Nature's laws lay hid in Night
> God said, Let Newton be! and all was Light.

Brewster gives, as reason for the long delay between the *Principia* and the first important works of celestial mechanics in the eighteenth century, "the imperfect state in which the differential calculus was left by Newton and Leibnitz" (Brewster, p. 348). D'Abro says "The importance of Newton's method was so obvious that it could not fail to impress his contemporaries." (D'Abro, Vol. I, p. 11).

happened, not *why* it happened. His philosophy is concisely summarized in Rule I of Book III:

> "*We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances.*" (Newton [1726], 270)

Moreover:

> "In experimental philosophy we are to look upon propositions inferred by general induction from phenomena as accurately or very nearly true, notwithstanding any contrary hypothesis that may be imagined, till such time as other phenomena occur, by which they may either be made more accurate, or liable to exceptions." (Newton [1726], 271)

Thus (see above), since universal gravitation seemed to be able to explain the motions of the planets, by that fact alone universal gravitation ought to be an acceptable theory. The fact that it must make itself felt across a vast emptiness is not a problem:

> "But hitherto I have not been able to discover the cause of those properties of gravity from phenomena, and I frame no hypotheses; for whatever is not deduced from the phenomena is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy." (Newton [1726], 371)

In effect, Newton is admitting he does not know the answer — but knowing the *cause* of the phenomena is not a part of science. This argument will not sit well with the Cartesians, however, and much of the early resistance to Newtonian mechanics is the lack of an explanation for the phenomenon.

2.1 Newton

The spectre of "action at a distance" led the Cartesians to postulate some sort of interplanetary medium, by which the planets and sun could affect each other. Introducing this medium, however, meant that to account for the full motion of the planets, one had to examine its effects.

Newton concerned himself with the existence of traces of resistance, which would necessarily slow the motions of the planets and cause them eventually to fall into the sun. This may be the reason behind all of Section IV in Book II, entitled "The circular motion of bodies in resisting mediums" (Newton [1726b], 407) In this section Newton supposes a case very suggestive of the solar system: the circular motion of bodies in resisting media (Newton [1726b], 407). While the other sections of Book II can be seen as modelling projectile motion through the atmosphere, or through water, it is difficult to envision a situation *except* for the solar system whereby bodies may move through a resisting medium, attracted to a central point. In this section, Newton supposes a force directed towards a fixed center, and experiments with various laws of resistance, and finds the motion of the bodies as they go around the center of attraction (Newton [1726b], 409, 415, 416, 417).

Of particular interest is Proposition XV, Theorem XII: "If the density of the medium in a place is reciprocal to the distance to the fixed center, and the centripetal force is in duplicate ratio to the density…" (Newton [1726b], 409). This would make the centripetal force inversely proportional to the distance to the center, precisely the case in the solar system, according to Newtonian gravitation.

Yet what could be a source of resistance? It was well known, by the time of Newton, that above a certain height the Earth's atmosphere thinned out to a vacuum, so while air resistance might be an important factor in calculating Earthly motions it was certainly not a factor in celestial motions. The sun might have an atmosphere, but if it was like that of Earth, it, too, should thin out to near nothingness in the region of the planets. And while comets appear to give off the matter which forms their tails, this matter is very thin and amounts to almost nothing.[2]

Thus, neither the atmosphere of the sun or planets, nor the exhalations of cometary tails, could provide a resisting interplanetary medium.[3] As for any other source of a resisting medium, Newton uses the evidence of the comets to support the notion of an empty interplanetary space. In Corollary 3 of Lemma IV of Book III ("The comets are beyond the Moon and orbit in the region of the planets") (Newton [1726b], p. 685), he writes:

> "Corol. 3. Thus it is manifest that the heavens are lacking in resistance. Because the comets in oblique paths and sometimes contrary to the course of the planets, move completely freely, and their motion prevails for a long time, even against the course of the planets" (Newton [1726b], 693).[4]

[2]For proof, Newton points out that stars can easily be seen through the tails of the comets.

[3]Newton switches back and forth between his views on the amount of mass represented by cometary tails. However, he recognizes the difference between a hypothesis, like the one above regarding the tails being the source of water "maintaining" the lakes and oceans, and the fact of the low density of the tails, based on the *observation* that the tails of a comet have less effect on the light of the sun and stars than our own atmosphere [Newton [1726b], p. 741-2]. However, it is instructive to compare the method of Newton and his followers (especially Clarke, below), to that of Descartes and his followers. On the one hand, a thick cometary tail would explain the "maintenance" of the oceans and lakes. On the other hand, it would cause other effects, such as the refraction or dimming of the light of the stars. Since this effect is not seen, then the idea of a thick tail must be rejected, *unless* one wishes to claim *also* that this tail is transparent, which is piling on additional assumptions. As we shall see, the Cartesians resorted to the additional hypotheses, while the Newtonians rejected the original hypothesis.

[4]Newton will later cite this freedom as one of the proofs against the existence of a vortex (see below).

Then Newton introduces what might be deemed the first proof of the permanence of the planetary orbits. In Book III, Proposition X, Theorem X, Newton writes: “The motion of the planets in the heavens can be preserved for a long time” (Newton [1726b], 584).

After citing his experiments with the resistance of air to the motion of a sphere of ice,[5] he goes on to conclude (using Jupiter as an example, and his previous work with the density of the interplanetary space):

> “And thus the planet Jupiter revolving in a medium of the same density as that superior air, in a time of 1000000 years, would not lose from the resistance of the media one part in ten hundred thousand.” (Newton [1726b], 585)

By extension, then, the other planets would persist in their motions for a similarly long period of time. As far as Newton was concerned, this was all that need be said.

With the exception of the Moon, Newton nowhere in the *Principia* considers in detail the mutual gravitational attraction of the planets; rather, he dismisses it as trivial (see below), though in one of his own copies of the first edition, Newton first adds then deletes a change to the proposition which reads “The undisturbed motion of the planets in the heavens can be preserved for a long time” (Newton [1726b], 584n). Newton may have been referring to gravitational perturbations, and only slowly did he come to consider them important.[6]

2.2 Huygens

Yet if there is no interstellar medium in the sense of an atmosphere (of the planets or, as Newton suggests elsewhere, the extended atmosphere of the sun), there is the possibility of some other type of matter filling interplanetary space: the particles of the Cartesian vortices.

Newton’s statement about the freely moving comets was meant as an attack on the Cartesian vortices. If the comets crossed the vortices (either obliquely, or, worse, “against the course of the planets”) they would suffer enormous resistance, and ought to quickly spiral into the sun. Newton points this out in the General Scholium:

> “The motion of the comets are completely regular, and observe the same rules as the motion of the planets, and vortices are unable to explain this. Comets are carried in exceedingly eccentric motions in all parts of the sky, which cannot be made except by abolishing vortices.” (Newton [1726b], 759)

[5]This is one of the few places in the *Principia* where he refers to an actual experiment he conducted. It is also one of the few places where he refers to Book II, probably the most contentious and least defensible of the three books of the *Principia*.

[6]This slow change in viewpoint will be considered below.

But if the free and unhindered motions of the comets was the rallying point of the arguments against vortices, action at a distance was the focus of the arguments against Newtonian gravitation. In order to avoid it, the Dutch physicist Christiaan Huygens (1629-1695) wrote, in 1690, "Discourse on the Cause of Gravity". To explain both the transmission of light and the force of gravity, Huygens postulated that space was filled with ethereal matter, resurrecting the vortices of Descartes. To explain gravity and action at a distance:

> "...I suppose that in spherical space, surrounding the Earth and the bodies all around it, there is a material fluid which consists of very tiny particles, and which is greatly agitated in every sense, and with great rapidity." (Huygens [1690], 135)

It might seem that Huygens is postulating a different rule than in a pure Cartesian system. Unlike Descartes, Huygens' particles are moving in "every sense": in all directions, rather than in a circular whirlpool around the sun. However, there is still a vortex moving in a circular motion about the sun and planets, which Huygens explains as follows:

> "The reason for this circular movement is that the matter contained in such a space, moves more easily in [a circle] than in a straight line, since it will be deflected by other particles (because the matter can not leave the space which contains it), [which will] change [their motion] to circular." (Huygens [1690], 135)

Thus, in Huygens' system, the matter of the planets *still* moved through a medium consisting of particles. Yet how can this be? Though Newton does claim that the doctrine of vortices is "pressed with difficulties", in fact there is only one: the utter lack of evidence of the resistance of the material of the vortex to bodies moving through it.

> "This is the only reason that M. Newton rejects the vortices of Descartes, and needs the celestial space to contain only very rarefied matter, so that the planets and comets meet [only] the least obstacle in their courses. If such rarity is supposed, it seems possible to explain neither the action of gravity, nor that of light..." (Huygens [1690], 161)

To explain the lack of effects of "filled" space, Huygens is forced to rely on another supposition: the particles possess almost no resistance:

> "But I believe that, without considering its rarity, the great agitation of the ethereal matter can contribute greatly to its penetrability. For if the small movement of water particles renders it liquid, and much less resistant, to bodies that pass through it, whether sand or fine particles; does it not mean that a matter more subtle and infinitely more agitated [is] all the easier to penetrate?" (Huygens [1690], 162)

Thus, the particles of the vortex have the seemingly contradictory properties of enough resistance to move the planets in circles around the sun, but no resistance to their motion whatsoever.

2.3 Newton Again

The Newtonians fought back against this apparent revival of the vortices of Descartes. Newton himself joined the fray, and stressed, time and again, that interplanetary space *had* to be empty to explain the *observed* motions of the planets, regardless of whether or not the motion could be *explained*.

In an undated manuscript, probably written before the publication of Newton's *Theory of the Moon* in Gregory's *Elements of Physical and Geometric Astronomy* (1702), Newton reiterates his view that the interstellar medium must be exceedingly rare:

> "For it follows from this [the calculation of the motions of the Moon by universal gravitation] that the revolutions of all the planets are ruled by gravity, and that not only are solid spheres[7] to be resolved into a fluid medium, but even this medium is to be rejected lest it hinder or disturb the celestial motions that depend upon gravity." (Scott [1959], v. 4, 3)

Of particular importance is Newton's method of argument: he argues that because universal gravitation alone was able to account for the motions of the Moon, then no other cause need be admitted to the system of the world.

This is repeated in *Optics*, where Newton says:

> "And, therefore, to make way for the regular and lasting motions of the planets and comets, it's necessary to empty the heavens of all matter, except perhaps some very thin vapours, steams, or effluvia, arising from the atmospheres of the Earth, planets, and comets.[8] A dense fluid can be of no use for explaining the phenomena of Nature, the motions of the planets and comets being better explained without it. It serves only to disturb and retard the motions of those great bodies..." (Newton [1717], 528)

Note the difference in the arguments of Newton and Huygens. Huygens argues that *because* the action of gravity cannot be explained by empty space, space is filled with particles. Meanwhile, Newton counters with the fact that *if* space was filled with particles, there should be observable effects — yet there are none. This is the key difference between the followers of Newton and the followers of Desscartes the Cartesians add hypotheses; the Newtonians eliminate them.

[7] I.e., the Ptolemaic cycles

[8] In the 1717 edition, Newton adds the line, "from such an exceedingly rare aethereal medium as we described above." This is doubtless in part due to the Leibniz-Clarke correspondence; see below for more details.

The emptiness of space may have influenced Newton's conception of light: light had to be particles, for otherwise how could it cross empty space? And, in *Optics*, he notes the similarity between the action of light at a distance to that of "an attractive force":

> "Qu. 29. Are not the rays of light very small bodies emitted from shining substances? For such bodies will pass through uniform mediums in right lines without bending into the shadow, which is the nature of the rays of light. They will also be capable of several properties, and be able to conserve their properties unchanged in passing through several mediums, which is another condition of the rays of light. Pellucid substances act upon the rays of light at a distance in refracting, reflecting, and inflecting them, and the rays mutually agitate the parts of those substances at a distance for heating them; and this action and reaction at a distance very much resembles an attractive force between bodies."(Newton [1717], 529)

Exactly what the "resemblance" is between light and attraction (Newton also includes magnetism in this category) Newton does not say. However, this is the first time Newton suggests a mechanism for gravity, something he was averse to doing in the *Principia*.

2.4 Leibniz and Clarke

The argument over whether or not space was empty (and could thus permit the planets to pass without resistance) or filled with particles, and how such a filled space would allow the resistance-free motion of the heavenly bodies, occupied portions of the fourth and fifth letters and replies to pass between Gottfried Wilhelm Leibniz (1646-1716) and Samuel Clarke (1675-1729); in his fifth and last letter (August 18, 1716), Leibniz remarks on the necessary existence of what we would call the ether:

> "For, it is a strange imagination to make all matter gravitate, and that towards all other matter, as if each body did equally attract every other body according to their masses and distances; and this by an attraction properly so called, which is not derived from an occult impulse of bodies; whereas the gravity of sensible bodies towards the centre of the earth, ought to be produced by the motion of some fluid. And the case must be the same with other gravities, such as is that of the planets towards the sun, or towards each other. [A body is never moved naturally, except by another body which touches it and pushes it; after that it continues until it is prevented by another body which touches it. Any other kind of operation on bodies is either miraculous or imaginary.]" (Alexander [1956], 66)

Clarke, in a typically Newtonian fashion, does not worry about the transmission of the gravitational force; he summed up the general attitude towards the problem in his reply (October 29, 1716):

> "That the sun attracts the earth, through the intermediate void space; that is, that the earth and sun gravitate towards each other, or tend (whatever the cause of that tendency) towards each other, with a force which is in a direct proportion to their masses, or magnitudes and densities together, and in an inverse duplicate proportion of their distances; and that the space betwixt them is void, that is, hath nothing in it which sensibly resists the motion of bodies passing transversely through: all this, is nothing but a phenomenon, or actual matter of fact, found by experience. That this phenomenon is not produced (§118) *sans moyen*, that is without some cause capable of producing such an effect; is undoubtedly true. Philosophers therefore may search after and discover that cause, if they can; be it mechanical, or not mechanical. But if they cannot discover the cause; is therefore the effect itself, the phenomenon, or the matter of fact discovered by experience, (which is all that is meant by the words attraction and gravitation,) ever the less true?" (Alexander [1956], 118)

Here is the essential difference between the Newtonians and the Cartesians. The Newtonians refused to allow for an ether, since if it existed, it would cause resistance, which their observations did not support. The Cartesians, on the other hand, demanded an ether, since without the ether, how else could light and especially the force of gravity be transmitted across a vacuum? The Newtonians deduced the lack of an ether from the lack of resistance, while the Cartesians deduced its existence because the force of gravity could not be explained: the former is the hallmark of modern science, while the latter was a holdover from the scientific theorizing of the ancient Greeks.[9]

[9]Newton is not completely free from making *ad hoc* explanations of phenomenon; see his explanation of the secular acceleration of the Moon, below. Additionally, one might consider that according to the laws of Kepler, the planets move in ellipses with the sun located at one of the foci, but the celestial mechanics of Newton does not predict this; rather, it is the center of mass that is situated at the focus. Also, if the hypothesis that *every* body in the universe gravitates towards *every* other body is correct, then the motion of the planets must be considerably more complicated than Kepler's three laws admits. Newton — without being able to more exactly calculate the slight variations — is forced to claim that the amounts are so small as to be ignored (cf. Book III, Proposition XIV, Theorem XIV, and below).

One might be tempted to compare Huygens' claim that the interplanetary medium is virtually resistance-free, since no resistance can be detected, and Newton's claim that the variations in the motion of the planets must be small, since no variations can be detected. Both are *ad hoc* arguments raised to reconcile observation with speculation. Without the added impetus of the *mathematical* argument that the variations are small — as they are in the case of the Moon, whose variations (except for the motion of the apse) Newton computes — there is no distinction between the two. It is the mathematical argument, and the mathematical argument alone, that distinguishes the two added suppositions.

2.5 Maclaurin

On the European continent, Newtonianism was championed by the philosopher Voltaire, who in 1738 published a French and English edition of *The Elements of Newton's Philosophy*. Voltaire found it necessary to refute Cartesianism in this work, and devoted an entire chapter to debunking Descartes. Most of the work, however, was devoted to popularizing Newton's less mathematical *Optics* than the more difficult *Principia*, and hence I shall not discuss it here.

In England, Newtonianism was championed against Cartesianism by Colin Maclaurin (1698-1746), whose *Account of Sir Isaac Newton's Philosophical Discoveries*, published posthumously in 1748, is a review of the more mathematical *Principia*, and not the easier *Optics*. Though by Maclaurin's time Newtonianism was well established in England (and in the process of being established in France), Maclaurin raised a number of salient points in his work.

Maclaurin, who was staunchly Newtonian, was a member of the mathematical school, and refused to entertain the thought that there was anything wrong with the physical theory. For Maclaurin, the inverse square law alone, with no modifications, was all that was necessary to explain all the motions of the planets, and neither a variation of the inverse square law, nor a change in physical theory (such as would be required if one assumed a resisting interplanetary medium or, worse, the vortices of Descartes) were necessary to resolve the differences between theory and observation. What discrepancies there were could be attributed to a lack of mathematics.

Maclaurin reiterates Newton's results from Section IX, Proposition XLV, and considers the alternatives to an inverse square law: an inverse cube would cause a non-circular orbit to spiral asymptotically inward; his own calculations showed that inverse fourth or greater power would cause collision with the central body in less than half a revolution (Maclaurin [1748], 312-3). Furthermore, for any power higher than an inverse square, the line of apses would move; even a very small deviation from a perfect inverse square would cause the line of apses to move sensibly (Maclaurin [1748], 314-5). Since the orbits of the planets show no change in their lines of apsides, Newton's law must hold (Maclaurin [1748], 315).[10] The motion of the Moon's apse must be caused by the attraction of the sun, though Maclaurin does not, in his work, go into the full details of showing how the action of the sun produces the motion of the Moon's apse.

At several points, Maclaurin shows concern for the fate of the solar system, especially in the event that there is some resisting interplanetary medium. In one instance, he suggests the solar system will eventually "run down":

> "The true general principle on this subject, is, that when any number of bodies, moved by their gravity, are connected together in any manner so as to act upon each other while they move, the ascent of their common centre of gravity, in their vibrations or revolutions, will be always found to be either equal to its descent, or less than it,

[10]Maclaurin simplifies the situation immensely: the line of the apsides does move, a fact known to Newton.

> but never to exceed it. And, from this principle, the impossibility of a perpetual motion is justly derived. For it appears, that, in such vibrations and revolutions, the successive ascents of the centre of gravity must continually diminish, in consequence of the attrition of the parts of bodies, and the resistance of the medium; since the ascent of the centre of gravity being never greater than the descent (tho' often less than it,) there can be no gain of force to overcome those resistances. All motion, therefore, must be abated and gradually languish in our mechanical engines, unless they be supplied by new and repeated influences of the power." (Maclaurin [1748], 196-7)

On the other hand, it was necessary to deny the existence of vortices, since they are incompatible with the Newtonian system:

> "[the density of the air] must be still less at the distance of the moon, which therefore, meeting with no resistance, continues to revolve for ever in her orbit, without any impediment or diminution of motion. As for a more subtile medium than the air, no experiments nor observations shew that there is any here, or in the celestial spaces, from which any sensible resistance can arise." (Maclaurin [1748], 294)

Maclaurin, like Newton, is concerned only with the effects of resistance on the motion of the planets, and not with their gravitational interactions.

The above contains the crux of the Newtonian argument, and in the true spirit of modern science: there is no evidence for an interplanetary medium, and that is all that need be said.

Maclaurin switches back to a perceptible resistance in the case of comets that approach the sun:

> "We are not to expect that the motions of the comets can be so exact, and the periods of their revolutions so equal, as those of the planets; considering their great number, and their great distance from the sun in their aphelia, where their actions upon each other must have some effect to disturb their motions. The resistance which they meet with in the atmosphere of the sun, when they descend into the lower parts of their orbits, will also affect them. By the retardation of their motion in these lower parts, their gravity will be enabled to bring them nearer the sun in every revolution, till at length they fall into him, and supply fewel to that immense body of fire." (Maclaurin [1748], 374-5)

The last sentence contains a foreshadowing of Helmholtz's theory that infalling meteorites produce the heat that powers the sun; a little later Maclaurin suggests that the limited number of comets necessarily imposes an upper limit on the age of the Earth and sun (Maclaurin [1748] 375-6).

However, if a theory is judged on its ability to make predictions, then a relatively straightforward test would determine who was correct. If the Newtonians were right, in that space was empty and that the force of gravity was not to be explained by invoking an ether, then the motion of the planets through the vacuum should be unchanged over the centuries. If the Cartesians were right, and that the force of gravity required the existence of an ether to make its effects felt, then as the planets moved through the interplanetary medium, they would encounter resistance, which would cause them to fall nearer to the sun and increase their mean motion. The amount might be small, but accurate observations of the positions of the planets over the thousands of years for which records are available should reveal if such an acceleration exists.

Remarkably, such a piece of evidence came to light in the last years of the seventeenth century, but neither the Cartesians nor the Newtonians made anything of it: the Cartesians did not point to it as proof their system of the world was correct, and the Newtonians did not deny its existence as an artifact of imperfect observation.

2.6 The Moon's Secular Acceleration

Around 1693, Edmond Halley (1656-1742) discovered an apparent acceleration in the mean motion of the Moon. Halley based his conclusion on a comparison of eclipse records dating back to the Babylonians. The Journal Books of the Royal Society record Halley's first mention of the problem. On October 19, 1692, Halley read a paper before the Royal Society of London trying to prove that the resistance of the ether was in fact sensible.

> "Hence, he argued, that the Motions being retarded must necessarily conclude a final period and that the eternity of the World was hence to be demonstrated impossible. He was ordered to prosecute this Notion, and to publish a discourse about it." (MacPike [1937], 229)

Halley's paper has not been preserved. However, in later works he does give hints of his thoughts. His main line of arguments involves the timing of eclipses, both solar and lunar. First, on October 25, 1693, Halley read his paper, "Emendationes ac notae in vetustas Albatenii Observationes astronomicas, cum restitutione Tabularum Lunisolarium ejusdem Authoris", regarding his translation of Albategnius, an Arabic astronomer who flourished around 890 A.D. (op. cit Armitage [1966]), 105-6).

Halley compared the calculated times of solar and lunar eclipses, based on the tables of Ptolemy, with four eclipses Albategnius observed in 891 (solar), 901 (solar), 883 (lunar), and 901 (lunar). In every case, Ptolemy's predicted times were later than the actual eclipse, by times ranging from 50 minutes to three and a quarter hours (Halley [1693], 918-9).

In this paper, Halley did little more than point out the anomaly in the timing of several eclipses. The fact that Ptolemy's predicted times were *consistently* later than what modern tables would predict, however, suggested to him that the cause was not poor observation by Albategnius or miscalculation on Ptolemy's part, but must involve something more fundamental. If the Moon's motion was accelerating at a slow rate, it would resolve the discrepancies between Ptolemy's predictions and Albategnius' observations.

Halley did not formally announce this conclusion, however, until 1695, when in the November/December issue of *Philosophical Transactions*, he appended his hypothesis to an archaeological article entitled "Some Account of the Ancient State of the City of Palmyra, with Short Remarks upon the Inscriptions found there."

The statement about the secular acceleration is almost an afterthought; it seems that Halley is not as certain about the existence of the acceleration as he was in 1692. This may be due to his uncertainty of the magnitude of the acceleration, caused by the uncertainty in the positions of the location of Arecca — displacing the city further east would make the eclipse occur earlier — although, in one case, the discrepancy was over three hours and could not be explained by any reasonable shift in position.

> "And if any curious Traveller, or Merchant residing there, would please to observe, with due care, the *Phases* of the *Moons Eclipses* at *Bagdat, Aleppo* and *Alexandria*, thereby to determine their Longitudes, they could not do the Science of *Astronomy* a greater Service: For in and near these Places were made all the Observations whereby the Middle Motions of the *Sun* and *Moon* are limited: And I could then pronounce in what Proportion the *Moon's* Motion does Accelerate; which that it does, I think I can demonstrate, and shall (God willing) one day, make it appear to the Publick." (Halley [1695], 174-175)

It might seem that the existence of such an acceleration is proof positive that there exists an interplanetary ether, and strong evidence in favor of the Cartesian world view (as extended by Huygens), especially since even a trace of resistance would remove the necessity of the most implausible assumption of the Cartesian system (which is to say, a fluid matter that offers no resistance to moving bodies). However, there are explanations from the strictly Newtonian side as well; Newton suggested one of them, which appeared in the second edition of the *Principia* (1713):

> "Now the vapors, which originate from the sun and fixed stars and heads of the comets, can fall into the planets' atmospheres by gravity and there condense and convert into water and humid spirits, and thereafter by slow heat into salts, sulfurs, and tinctures, and mud and mire, and clay, and sand, and stones, and coral, and little by little change into other terrestrial substances." (Newton [1726b], 758)

A similar statement occurs in all editions of the *Principia*; however, it is only in the second edition (published nearly twenty years after Halley's discovery of the secular acceleration) that Newton includes the two sentences:

> "However, the decrease in the body of the sun slows the mean motion of the planets around the sun little by little, and as the Earth increases, the mean motion of the Moon around the Earth will be increased. And indeed by collecting Babylonian eclipse observations with those of Albategnius and the present, our Halley put together the mean motion of the Moon with the daily motion of the Earth, and was first of all to determine it was little by little accelerated." (Newton [1726b], 758-759n)

Newton put forth this hypothesis sometime before 1710.[11] If Newton's hypothesis was correct, it muddied the line between clear evidence for an ether, and clear evidence against an ether, for now both would cause an acceleration of the mean motion of the Moon: Halley's discovery is double-edged.[12]

Weak as Newton's argument is, it provides a test, for cometary exhalations and resisting media will have different effects on the motion of the planets. A resisting medium would manifest itself as an acceleration in the mean motion of all bodies around their primaries, as resistance caused them to lose energy and spiral towards their central bodies: the Moon would accelerate its mean motion around the Earth, and in turn, the Earth would accelerate its mean motion around the sun.

On the other hand, as Newton pointed out, exhalations from the comets and the sun would result in an increase of the Moon's mean motion about the Earth (as both increased in mass) while *decreasing* the mean motion of the Earth around the sun (as the latter lost mass). Hence, both theories predict an acceleration in the Moon's mean motion, but predict opposite effects for the Earth's mean motion. In order for a decision to be made, it was necessary to obtain better figures on the mean motions of both the planets and the Moon.

Unfortunately, Halley was unable to determine the magnitude of the secular acceleration, and seems to drop the idea entirely; no further mention of it appears in any of his published or unpublished manuscripts, and he makes no references to it in his letters. Newton might have been relieved at this lack of evidence, for his explanation of its cause was feeble, and he deletes the offending sentences from later editions of the *Principia*. (Indeed, the two sentences in the *Principia* are the only places where Newton, in print, suggests the possibility of these solar exhalations affecting the motion of the planets in any significant fashion.)

More surprising is the fact that none of the Cartesians ever raised the issue, either, even when it would significantly bolster their case (especially that of M.

[11] Based on Flamsteed's mention of the hypothesis in his February 11, 1710 letter, below.

[12] To be sure, the line is not that muddy. Here, Newton commits the same error that the Cartesians committed earlier: in order to explain a phenomenon that contradicts his theory, Newton adds an additional assumption to his system. Then, because that additional assumption would produce effects which are at odds with observation, Newton then must go to almost absurd lengths to reconcile theory and observation. In this case, while the matter given off by the sun and comets is enough to cause an observed acceleration in the mean motion of the Moon, at the same time the amount of matter must be almost nothing — see Proposition XLI, Problem XXI, especially pages 740-741 in Newton [1726b].

Bouguer, below). Thus, aside from the brief article by Halley and even briefer mention of the problem by Newton, the Moon's secular acceleration will vanish from scientific thought for half a century.

2.7 Variations in Jupiter's Mean Motion

To make things worse, another secular acceleration in the motion of the planets was discovered which, unlike the secular acceleration of the Moon, remained an important scientific controversy, spanning the gap between the publication of the *Principia* and the revival of celestial mechanics under the physical system of Newton.

Even before the second edition of the *Principia* appeared, there were questions about the changing mean motion of the planets Jupiter and Saturn (op. cit. Wilson [1985]). Part of the correspondence between the English mathematician Abraham Sharp (1651-1742) and the Astronomer Royal John Flamsteed (1646-1719) concerned inequalities of planetary motions. On January 6, 1710, Sharp wrote a letter (now lost) to Flamsteed, presumably asking questions or offering speculations about the inequalities in the planetary motions. Flamsteed's reply, on February 11, 1710, begins by discussing the variations in the motion of the Saturn, which he finds to move slower than Kepler and Bullialdus made it (Baily [1966], 274). However, Flamsteed does not seem to believe that this implies a secular acceleration:

> "I must add concerning Saturn that, whereas Sir I. Newton suggested to me that all the planets increased in their bulk continually, by an accession of matter from the tails of comets passing near them, and resolutions of matter from the ether about them, this now seems not probable. Mr. Halley had told him that the motions of Saturn were slower, this last 100 years, much than formerly. I have tables of Saturn by me, of his making, presented to Sir J. Moore, wherein he makes Saturn's motion in 100 years 26 minutes slower than 'tis in the *Caroline Tables*. Now, if the planets grow slower in their motions, they must consequently remove farther from the sun, and there is no reason for their removing farther from the sun except they increase in bulk and weight: but I do not find that Saturn moves any slower now than he did almost 2000 years ago. Which makes me think our earth, and the other planets, have gained little or nothing from the tails of comets ..." (Baily [1966], 274)

As the letter is dated 1710, it places the origin of Newton's idea concerning the Moon's secular acceleration to be no later than late 1709. (Flamsteed himself says nothing about Halley's discovery, which is not surprising considering the oftentimes acrimonious relationship between the two.) The reference to the planets "removing farther from the sun except they increase in bulk and weight" is cryptic, for it does not form part of Newton's ideas as developed above. It is likely that Flamsteed misinterpreted an idea Newton communicated to him, as it is clear from Newton's statement in the second edition of the *Principia* that it is not the increase in bulk and weight of the planets that causes them to recede from the sun, but the decrease of the sun's mass caused by its "exhalations".

The Italian astronomer Giacomo Felippi Maraldi (1665-1729) seems to have believed in the acceleration, though he was much more cautious about it. In a paper read to the Royal Academy of the Sciences at Paris, read December 14, 1718, he examines the evidence.[13] Maraldi examines Saturn's position as determined by Kepler's tables, and as measured by modern astronomers, and finds that Saturn is slightly — on the order of 10 to 20 minutes of arc — behind where Kepler's tables would put it.

By then it was well established that the sun, Jupiter, and the Earth reached very nearly the same relative positions every 83 years; in observational terms, this means that if one observes Jupiter to be near a certain star, then 83 years later, it will be near that same star once again. There is a small excess of motion, which Maraldi compared. Thus, the most ancient observations were the conjunction of Jupiter and the star in Cancer called Asinus Borealis[14] in 240 B.C., and a conjunction with that same star observed in December, 1717, a time interval of 1957 years; from this, Maraldi determines a mean motion of $4'16''$ over the 83 year cycle (Maraldi [1718], 320).[15]

For observations at adjacent conjunctions in the cycle, he cites a number of different observations: from April 1634 to April 1717, Jupiter has moved $15'0''$ (over a full cycle through the zodiac); from Bouillaud's observations of a conjunction near Regulus on October 12, 1623 to October 1706, a $21'$ motion; from the conjunction of Hortensius (a Dutch astronomer) in 1627 to 1710, a difference of $12'50''$; Gassendi's 1633 conjunction and a modern, 1716 observation $15'0''$; and finally Bouillaud 1634 to modern 1717, a $20'$ difference (Maraldi [1718], 320-322). Note that if one ignores Bouillaud's first observation, Jupiter travels slightly further in each cycle than before, suggesting that Jupiter's mean motion is accelerating. However, Maraldi is cautious and does not want to jump to conclusions, as moving away from the hypothesis of a constant mean motion is a major break from any existing theory of the motions of the planets. He states:

> "However, despite these observations, one has reason to doubt any acceleration in the mean movement of Jupiter, [for] we do not believe that [one] should to move from the hypothesis of the equality of the mean motion, without complete evidence. One can only have this evidence by a great number of exact observations, made in different centuries, which are comparable to one another, giving from century to century a sensible acceleration and such that we will have

[13]This was his second paper on the subject. The first, "Comments on the Theory of the Planets", was read November 26, 1704; in it Maraldi expresses his distrust of using "ancient observations" to determine secular acceleration, because of the uncertainty in the accuracy of their observations.

[14]γ Cancri

[15]Maraldi does not explain how he arrives at this figure.

> determined it by comparison to the observations made in centuries past." (Maraldi [1718], 326)

In 1718, celestial mechanics was still in a state of flux, and thus these observations, which could be explained neither by the Cartesian vortices nor universal gravitation would play little immediate role in the development of celestial mechanics.

2.8 The Academy Prize of 1732

Two of the prize contests for the Royal Academy of the Sciences in Paris illustrate the pervasive Cartesian influence as late as the mid-1730s, and how difficult it was for Newtonian gravitation, which did not *explain* phenomena, to gain a foothold. Since a resisting interplanetary medium, such as the particles of the Cartesian vortices, would have to have *some* effect, these prizes also illustrated the lengths to which the neo-Cartesians went to avoid the problems born from resistance. Finally, though the secular acceleration of the Moon would be observational evidence in favor of a resisting medium, none of the Cartesians show any acknowledgement of its existence, suggesting this piece of evidence was little known.

The prizes also demonstrate one other significant difference between physical scientists on the continent, and those in England. Those on the continent (particularly France) demonstrated a much more secular character; "God" was not part of their vocabulary. Meanwhile, for English scientists, "God" was very much an integral part of the system, and would remain so until the 1800s (as we shall see).

For example, in 1732, the Royal Academy sponsored a prize contest to explain the cause of the inclinations of the orbits of the planets. Newton's explanation, as it appears in the General Scholium of the *Principia* was, of course, entirely unsatisfactory:

> "The six primary planets are revolved about the sun in circles concentric with the sun, and with motions directed towards the same parts, and almost in the same plane. Ten moons are revolved about the earth, Jupiter, and Saturn, in circles concentric with them, with the same direction of motion, and nearly in the planes of the orbits of those planets; but it is not to be conceived that mere mechanical causes could give birth to so many regular motions, since the comets range over all parts of the heavens in very eccentric orbits; for by that kind of motion they pass easily through the orbs of the planets, and with great rapidity; and in their aphelions, where they move the slowest, and are detained the longest, they recede to the greatest distances from each other, and hence suffer the least disturbances from their mutual attractions. This most beautiful system of the sun, planets, and comets, could only proceed from the counsel

> and dominion of an intelligent and powerful Being." (Newton [1726], 369)

In other words, the planets have small inclinations because God made them that way. This is neither the first nor the last time that Newton points to a physical phenomenon as an indication of the existence of God.

Among the entrants for the 1732 Academy Prize was Pierre Bouguer, a convinced Cartesian (who felt, however, that Newton "extended" Descartes) (Bouguer [1732], preface).

By this time, the vortices of Descartes are no longer the passive medium carrying the planets along; had they been, then they would be unable to cause any resistance in the motion of the planets, as the planets moved in step with the vortex.[16] Bouguer, on the other hand, suggested that it is the impact of the ether on the planets that causes the change in their motions (Bouguer [1732], 32-3).

Bouguer revised and greatly extended his essay and published it in 1748 (and thus places himself among the last of the Cartesians). Using an idea of gravity he credits to Varignon, which is similar to that given by Huygens (i.e., gravity is caused by the agitation of particles that fill space), he notes dramatically:

> "In any case, if the two impulses [from opposite directions] are not perfectly equal, the planet feels some resistance in continuing its course; it loses its movement little by little, and it finds gravitation too great, [so] the planet begins to approach the sun, into which it falls in the end, after having made a great number of revolutions. Nothing assures us that the planets are not subject to this slow progress to the center of their period." (Bouguer [1748], 62-3)

It would be possible, he suggests, to determine if this is happening by examining records of the sun's apparent size, for as the Earth approached the sun, the sun would appear to enlarge. However, he points out, the records are not good enough to tell us whether such a portentious event has happened. But the Moon could provide a better study:

> "This secondary planet need be struck on all sides by M. Varignon's corpuscles; but, however, it will be covered a little from impacts underneath; since the Earth protects it from an infinite number of collisions. Therefore the Moon, less pressed underneath than above, tends to fall to the Earth, and it falls effectively if the velocity of its revolution does not give it a centrifugal force which sustains it." (Bouguer [1748], 63)

This statement is remarkable for what it omits. If Bouguer knew about Halley's discovery of the secular acceleration of the Moon, this would be the perfect time to raise the issue, since the phenomenon would then be seen to be a *natural*

[16] However, the vortices themselves would have experienced resistance from their surrounding vortices, and that would eventually bring them to a halt; cf *Optics*, Query 31.

consequence of the vortex. Yet Bouguer seems to be unaware of the secular acceleration of the Moon.

2.9 Academy Prize of 1734

The Academy was not satisfied with any of the entrants, so in 1734 they repeated the question, offering a double prize (Recueil, vol. III, introduction). Jean Bernoulli (1667-1748) and his son, Daniel Bernoulli (1700-1782), entered the contest.

2.9.1 Jean Bernoulli

The elder Bernoulli's piece, "Exposition of a New Celestial Physics, To Explain the Principal Celestial Phenomena, Particularly the Physical Cause of the Inclination of the Orbits of the Planets in Relation to the Equatorial Plane of the Sun", explained the planetary motions by a vortex different in many essential respects from that of Descartes.[17]

First, the elder Bernoulli makes several pointed remarks about the difference between the geometer and the physicist:

> "A geometer, likewise, is not obliged to explain the origin of things: he can suppose it, provided that, for discussing the properties, he reasons just on established hypotheses." (J. Bernoulli [1734], 3)

One is reminded of Clarke's defense that, even if the cause of gravitation is unknown, one cannot deny the existence of gravitation. Bernoulli continues, saying that while the geometer need not concern himself with the origin of phenomena, it should be a major concern of the physicist (J. Bernoulli [1734], 3) Then he launches into an attack of the Newtonian system:

> "The inconveniences which result from these two principles incomprehensible to physicists, the void and attraction [without an intervening medium], are not the only [ones] which prevent admission into physics of the system of M. Newton: there are others, related to some phenomena, which remain inexplicable, even if one accepts these principles; some of these are, for example, the rotation of the planets around their axes; as well as the common direction of their revolution around the sun, making each of the signs of the zodiac [move] from west to east." (J. Bernoulli [1734], 4-5)

Here again, one sees the dissatisfaction of the Cartesians with the inability of Newtonian physics to *explain* certain phenomenon.

[17]Notice the emphasis on the *explanatory* aspect, rather than the predictive aspect. Cartesian theory was preferable then because it could explain physical phenomenon, while Newtonian theory is preferrable today because it can *predict* physical phenomenon. This is the key difference between the science of the ancient Greeks and modern science.

> "The gravitation of the planets to the center of the sun, and the gravity of bodies to the center of the Earth, has for its cause neither the attraction of M. Newton, nor the centrifugal force of the matter of the vortices according to M. Descartes, but to the immediate impulses of matter, which, in the form of a torrent that I call *central*, throws continually all the circumference of a vortex on its center, and impresses in consequence to all the bodies which it contains on its path, the same tendency to the center of the vortex." (J. Bernoulli [1734], 8)

In making this suggestion, he claims to be extending Huygens' idea on gravitation (J. Bernoulli [1734], 9-10) He makes some weak attempts to avoid the existence of the "dense fluid" that Newton argued against in *Optics*. After supposing that this fluid (matter of the first element) is infinitely divisible, he then says:

> "The matter of the first element is perfectly liquid and has at no point coherent particles, [and] one sees that it does not make any resistance to bodies which move in it; because the resistance of fluids comes only from the inertia of the particles of which the fluid is composed, and a body that moves through these particles needs to in all movements move and displace a certain numbers, which cannot be done without communicating to them a part of the body's movement and causing [the body] to lose in consequence as much as all [of their motion]." (J. Bernoulli [1734], 13)

Thus, since a resisting interplanetary medium would be unacceptable, the interplanetary medium had no resistance. This echoes Huygens' argument of nearly half a century before.

What are the properties of this stream of matter? Bernoulli supposes that at the sun's surface, it is moving at the same speed and the same direction, and that its velocity elsewhere is inversely proportional to the square root of the distance. (Though he does not state it, this is equivalent to Kepler's third law) Since the sun rotates on its axis once in $25^1/_2$ days, this allows him to calculate the period of the vortex in which each of the planets move:

Saturn	6744 years
Jupiter	2715 years
Mars	428 years
Earth	230 years
Venus	140 years
Mercury	54 years

(J. Bernoulli [1734], 39) Unlike the vortices of Descartes, Bernoulli's torrent does not move at the same speed as the planets, but much slower — and thus, in answering one question, the elder Bernoulli raises another: why do the planets move as *quickly* as they do? At least in the Cartesian system, the vortices moved at the same speed as the planet, carrying them along, and one may answer that the planets move at the speeds they do because the vortices move at that speed. With the planets overtaking the torrents, moving hundreds of times faster, this is no longer possible: now is it necessary to explain both why the planets move at their speeds and the vortices at another speed. Bernoulli does not explain this apparent contradiction.

The whirling vortex of Bernoulli is then made to explain the rotation of the planets: as the planet moves around the sun, it overtakes the vortex, which flows like a stream around it. However, the vortex particles on the sunward side are moving faster around the sun, and thus slower relative to the planet. This unequal velocity imparts a counterclockwise rotation to the planet, which is what is observed for Earth, Mars, Jupiter, and Saturn (J. Bernoulli [1734], 46-7).[18] Of course, at this time it was not known that Venus rotates in a direction opposite to its orbit around the sun — Venus was believed to rotate in either 23 days (according to the observations of Bianchini) or 23 hours (according to Cassini pére) (see J. Cassini [1735], below)

Bernoulli explains the inclination of the orbit of the planets as caused by the interaction between the ethereal matter, which orbits in the same plane as the sun's equator, and the non-spherical shape of the planet and the inclination of its axis to the plane of the solar equator (J. Bernoulli [1734], 68).

It would greatly strengthen Bernoulli's case if he demonstrated a correlation between axial tilt and inclination, but he does not do this, since there is no such correlation. Lest it be thought that no such evidence was available, earlier in the year, Jacques Cassini had written, "The Inclination of the Plane of the Ecliptic and the Orbit of the Planets in Relation to the Solar Equator" read to the Royal Academy of the Sciences in Paris on April 3, 1734 (Cassini [1734], p. 107). Cassini includes the following table:

Planet	Inclination of Orbit to Solar Equator
Saturn	$5°55'0''$
Jupiter	$6°22'0''$
Mars	$5°50'0''$
Earth	$7°30'0''$
Venus	$4°6'0''$
Mercury	$3°10'6''$

(Cassini [1734], 112) Cassini himself supplies the epitaph for the Bernoullian "vortex", just a year later:

> "It will suffice to remark that the time that the planets take to make their revolutions about their axis is not in the least proportional to

[18]Bernoulli took this idea from Mairan, who read to the Academy a paper, "New Thoughts on the Cause of the Diurnal Rotation of the Earth About its Axis From the East To the West", on August 6, 1729. Mairan concluded, based on the theory of vortices, that a planets rotation is propotional to its diameter, and inversely proportional to its distance from the sun (Mairan [1729], p. 54), an idea Cassini will reject, below.

> their size, nor to their distance from the sun ..."(Cassini [1735], 453).[19]

However, at the time, the elder Bernoulli's work was sufficiently worthy to win half of the double prize. The other half went to his son, Daniel.

2.9.2 Daniel Bernoulli

The paper by Daniel Bernoulli, "Astronomical and Physical Researches on the Problem Proposed for the Second time by the Royal Academy of Sciences at Paris", is hardly more satisfactory. While the elder Bernoulli attempted to answer the question of why the inclinations *existed* by suggesting a mechanism that could cause them by driving a planet from the plane of the sun's equator into an inclined orbit, the younger Bernoulli focused on the generally low inclinations of the orbits, and sought a mechanism that would "edit" out all but the small inclinations evidenced in the orbits of the known planets. This Bernoulli found the solution in the atmosphere of the sun.

Using a number of somewhat dubious assumptions, Bernoulli arrives at the formula for the density of the solar atmosphere:

$$D = \frac{x^2}{e^{n(x-r)r^2}}$$

where r is the radius of the sun, x the distance to the sun's center, and n is a constant.

Bernoulli's conditions suggest that the density of the sun's atmosphere need not be greatest at the surface (i.e., at $x = r$). Bernoulli assumes first that the density is greatest in the vicinity of Venus, where x is 150 solar radii. Given that, one may determine the density of the extended atmosphere of the sun at the distances of the planets:

Surface	=	1
Mercury	=	2200
Venus	=	3000
Earth	=	2600
Mars	=	1300
Jupiter	=	0, 40
Saturn	=	0, 000006

(D. Bernoulli [1734], p. 106) Under this assumption, Bernoulli notes:

> "XII. In this hypothesis the density of the solar atmosphere becomes nearly equal in the regions of Mercury, Venus, Earth, and Mars, but

[19] This is probably aimed at the work of the elder Bernoulli, rather than Mairan, since the higher profile of the former's work. At this time, Cassini is also a Cartesian. His work on the subject will be discussed below. However, note Cassini's method of attack: he produces observations that conflict with the predictions of the theory. Though he is still a Cartesian at this point, Cassini has the attitude of a Newtonian.

> around Jupiter and especially Saturn, the matter becomes quite rare, [and] it can no longer produce any sensible effect. There is therefore reason to believe that the place of greatest density is beyond the region of Venus. If one supposes it to be in the region of Mars, then the densities are in this proportion:

On the surface of the sun	=	1
In the region of Mercury	=	4170
Venus	=	8910
Earth	=	12300
Mars	=	14400
Jupiter	=	1310
Saturn	=	15

> XIII. If the greatest density is supposed to be around Jupiter, the solar atmosphere becomes still more uniform from Mercury to Jupiter; and this proposition appears the most probable to me: because a great number of phenomena, common to all the planets, could appear to me deduced from the solar atmosphere, [and] it is very appropriate that the density of this atmosphere can, in the full extent of the planetary regions, not be excessively unequal, as is the atmosphere of the Earth in moderate differences of altitude." (D. Bernoulli [1734], 106)

Bernoulli has touched upon a very important point, one that will cause much consternation when the secular accelerations of Jupiter and Saturn are more fully elaborated: if there is some pervading physical phenomenon affecting all the planets, the effects on the different planets must be qualitatively the same. Thus, he discards the first possibility, which causes a dramatic difference between the medium surrounding Saturn and that surrounding Venus; the second is only slightly better, still yielding an enormous disparity between the inner and outer planets, while making the solar atmosphere densest near Jupiter makes all the planets move through virtually the same medium — and to experience the same effects.

Though Bernoulli suggests the extended solar atmosphere could explain many phenomena, he does not give any indication of how the phenomena could be deduced. Neither does he justify his assumption that the atmosphere reaches its greatest density in the regions of the planets, other than his requirement that the effect on the planets be uniform, and thus the density of the atmosphere at various distances be comparable. A much more reasonable assumption would be that the greatest density of the atmosphere occurred at the surface, which is the case for the Earth's atmosphere; in this case, the density of the atmosphere in the region of the planets would likewise be virtually the same — but nearly zero everywhere.

As for the inclination of the orbits:

> "The movement of the solar atmosphere causes, first of all, and ignoring *solar gravity*, bodies to tend to travel either in the [plane of the] solar equator, or in a plane parallel: and if these bodies move obliquely, they arrive little by little to accommodate to the aforementioned direction, but can never perfectly reach it, without an infinite [amount of] time." (D. Bernoulli [1734], 109)

The implication is that the more extreme inclinations have been wiped out by the continuous passage of the planets through the plane of the sun's equator. Again, Bernoulli gives no indication of how this has occurred; the implication is that the solar atmosphere is densest in the plane of the solar equator, though the density equation is spherically symmetric.

Such was the state of celestial mechanics in the first part of the eighteenth century that these two Bernoullis split the prize that year (Recueil, vol. III, introduction) This despite the obvious problems with the piece of the elder Bernoulli (such as the lack of correlation between axial tilt and inclination), and the lack of justification for the assumptions in the piece of the younger Bernoulli.

Daniel Bernoulli's work seems to have drawn many more supporters than his father's. This might be taken as an indication that the theory of vortices was on the wane, especially since it required the existence of yet another type of matter. Meanwhile, the existence of a solar atmosphere was unquestioned, and it might conceivably have the properties that Daniel Bernoulli required of it.

2.10 Jacques Cassini

One of the more interesting figures of the time period is Jacques Cassini (1677-1756), who began life as a Cartesian, but later changed his mind and became a solid supporter (if not a promoter) of Newtonianism. Cassini was dissatisfied with the works of some of the Cartesians, and on July 23, 1735, he read a paper that attacked the ideas of the vortex-induced rotations of Mairan and Jean Bernoulli. However, he introduces his own notions, explaining the rotation of the planets by *their* extended atmospheres.

Cassini's dissatisfaction stems from the discontinuity between the velocity of the vortex (calculated by Kepler's laws) near the surface of a planet, and the actual velocity (measured by the planet's rotation) of the planet's surface. The difference varies from 3 times faster (in the case of Jupiter), to 17 times faster (in the case of the Earth), to 228 times faster (in the case of the sun) (Cassini [1735], 455.[20] Why should this sharp discontinuity exist? If the surface of the

[20] These numbers are, of course, the same numbers that the elder Bernoulli obtained for his motion of the vortex assuming it moved at the same speed as the surface of the body, and correspond to the ratio between a surface-skimming satellite of the planet and its rotational velocity.

planet is in contact of a vortex moving at tremendous velocity, why should the surface not gain some of that velocity?[21]

Cassini supposes that the atmospheres of the planets extend far beyond their surface, and that these atmospheres rotate at the same rate as the planet (Cassini [1735], 456).[22] Then he calculates the distance at which the planet's atmosphere is moving at the same speed as the surrounding vortex (which moves in accordance with Kepler's Laws): for example, in the case of Earth, he establishes its atmosphere extends 10,000 leagues into space (Cassini [1735], 458)[23]

Thus, the vortex and the atmosphere are moving at the same rate, and there is no sharp discontinuity between the speed of the vortex and the speed of the planet.[24] Also, Cassini has freed himself of the problem of explaining the difference in rotation rates: different planets rotate at different rates because their atmospheres have different heights — and there is no *a priori* method of calculating the heights of the atmospheres. Two planets identical in every respect except the depth of their atmosphere will, according to Cassini's theory, rotate in different times.[25] This done, he says:

> "It suffices to us to have shown in this Memoir, that one can reconcile easily in the system of vortices, the period of the revolution of the planets around their axes, with [the period] the fluid which surrounds them needs to have following the rules of Kepler, which one has regarded as a very strong objection to this system." (Cassini [1735], 464)

If these pieces can be considered representative of the attitude of the time, then celestial mechanics is under the following assumptions: in order to explain the attraction of gravitation, there *must* be a medium extending from the sun to the planets. In the elder Bernoulli's piece, the Earth is moving over 200 times faster than the "whisps" of matter that form the vortex, while the younger Bernoulli does not seem to believe it rotates at any significant velocity.[26] This medium is dense enough to explain such diverse observations as the diurnal

[21] This objection is new with Cassini; Newton does not draw attention to it, focused as he is on the difficulty of the motion of the comets.

[22] Cassini speaks of the planet being "the same body" as the atmosphere, noting that if this was not the case, tremendous winds would sweep across the Earth.

[23] One league is three miles; Cassini gives the circumference of the Earth to be 9,000 leagues. Cassini has calculated the height of a geosynchronous orbit here, possibly the first such determination in history.

[24] One might compare the elder Bernoulli's calculations of the speed of his "matter of the first element", which is moving at the same angular velocity that the sun is, with Cassini's planetary atmospheres. The key difference is that Cassini is not suggesting a new type of matter, but rather new uses for a well-known type.

[25] Cassini then calculates the heights of the atmospheres of Jupiter and Saturn; he cannot compute similar quantities for Mars, Venus, and Mercury, owing to their lack of satellites (Cassini [1735], p. 464) — effectively making their synchronous orbital periods unknown.

[26] The extended solar atmosphere of Cassini would have this same problem, if one assumed that the atmosphere extended beyond its heliosynchronous position. One gets the impression from Cassini, however, that the atmosphere only extends as far as necessary, then blends into the vortex.

rotation of the planets and their inclinations to the solar equator. On the other hand, the medium has some peculiar property that prevents it from exerting any sensible retardation of the motion of the planets.

Note also the level of proof required by the Parisian Academy of the Sciences at this time. Neither the Prize-winning pieces, nor the earlier piece by Bouguer, sought to prove their cases using mathematics to *demonstrate* how their vortices or their solar atmospheres would cause the observed effects. Necessity was sufficient reason to justify a new phenomenon.[27]

Likewise, the vortices themselves were needed to explain why the force of attraction could be felt across a vacuum. Because no resistance was known, they were deemed to have no resistance, despite the ability of their impacts on the planets to affect their orbits. Only by piling assumption upon assumption were the Cartesians able to maintain the doctrine of vortices; their arguments were based on rhetoric, not arithmetic, though Cassini did offer one mathematical prediction which could be confirmed or denied: the extent of the Earth's atmosphere.[28] (In a broader sense, the extent of the solar atmosphere of Daniel Bernoulli could also be measured, but it would be much more difficult, since it required the knowledge of the density of the solar atmosphere at many points, rather than determination of a single figure)

Still, even in the rhetorical vein, there were ways of proving or disproving a theory such as the vortices or the extended solar atmosphere. In either case — the new vortices of Jean Bernoulli, or the solar atmosphere of Daniel Bernoulli and Jacques Cassini — there ought to be evidences of secular accelerations as the planets lost motion through whatever resistance the void offered, and fell towards the sun and thus increased their mean motions. And while the secular acceleration of the Moon could provide powerful evidence for just such an acceleration, neither the Bernoullis, nor Bouguer, nor Cassini, ever gave any indication that they were aware such a phenomenon existed.

Meanwhile, Cartesianism, which reached its apex with Bernoulli and Bouguer, would undergo a decline for the next two decades, and by mid-century, the Newtonian world view had gained an unquestioned supremacy. Bouguer's publication of his revised "Entretiens" was a last gasp of Cartesianism. Even Maclaurin's *Account* of 1748 came years after Newtonian mechanics was generally accepted.

By 1750, no one was in a mood to question universal gravitation as being *the* operative factor in the motion of the planets, with all their motions either being traceable to the perturbing effects of the other planets, or to the resistance of actual, interplanetary matter, and not some mysterious "ether" required to transmit forces.

This turn of events came about in part due to Clairaut, who cleared up one of the major discrepancies between a prediction of universal gravitation and

[27]Cf. D. Bernoulli's argument that the solar atmosphere had to be densest not in the region of Venus, but in the region beyond Jupiter, because of the apparent uniformity of planetary motions.

[28]Of course, how to *measure* the extent of the atmosphere is another question entirely, since there is no sharp dividing line between atmosphere and interplanetary space.

observation. With such a tremendous victory of universal gravitation, as well as the decline of Cartesianiasm coupled with the immense weight of its additional assumptions, the last vestiges of the vortices of Descartes would vanish by 1760.

Chapter 3

Celestial Mechanics to Clairaut

The prime failure of Newtonian gravitation in the period before 1750, and the key factor that prevented its universal acceptance, was its inability to fully predict the motions of the Moon. While the vortices of Descartes might be criticized for requiring assumption upon additional assumption, or the particles of the vortex might, on the one hand, be called upon to provide enough resistance to rotate the planets and change their orbits so they revolve in the same plane, but not enough resistance to affect their motions, all these things could perhaps be dealt with.

Against this, Newtonian gravitation offered three things: difficult mathematics, "occult" action at a distance, and an inability to produce the full, observed motion of the Moon's apse. A mathematically difficult theory that produced incorrect results could hardly be expected to replace another theory, however laden with assumptions, that appealed to intuition and national pride. Hence, until the discrepancy between the calculated motion of the Moon and its observed motion could be accounted for, universal gravitation could not expect to win many converts. The solution of the discrepancy was found by Clairaut [1749], who took into account terms in the perturbation of the second order.

3.1 The Admiralty Prize

If the discrepancies in the motion of the Moon were of purely academic interest, it is likely that little would have come out of the irregularities for quite some time: mathematicians had many more promising problems to solve. However, determining the position of the Moon was an eminently practical problem — one that was both mathematically and monetarily rewarding.

The basic problem was that of determining a ship's position in the middle of the ocean, which entailed determining the longitude and latitude. Determination of latitude was simple: the angle between the horizon and the north celestial pole gave one's north latitude, and the position of the north celestial pole could be determined by finding the location of any three stars whose position relative to the pole was known.

The determination of longitude was the real problem. The basic method of determining longitude involved knowing the time. For simplicity, suppose one knew the precise time at which a star passed zenith at home. If one measured the position of the same star at the same time but in a different location, the angle between zenith and the star gives the difference in longitude between the current location and the home longitude.

In 1714 the English Parliament established "a Publick Reward for such Person or Persons as shall discover the Longitude at Sea" (Cohen [1975], 42). The prize varied from a minimum of $10,000$ pounds if the determination was accurate to within one degree, to twice as much if the accuracy was good to within $30'$ — worth over a million dollars in today's money (Cohen [1975], 42-3).

Since the essential problem involved finding the time, there were two obvious solutions. First, one could invent an accurate, marine chronometer. Since a pendulum clock would be worse than useless on a pitching, swaying ship, the chronometer would necessarily be based on a spring which, unless carefully designed, would vary its properties constantly due to temperature, humidity, and other factors; the chronometer would also have to be reasonably water resistant. Despite these technical difficulties, this method would be the one that proved ultimately successful when, in 1735, John Harrison produced such a chronometer.[1]

The other method was to use the "clock in the sky": the regular motions of the astronomical bodies. Galileo had long before suggested using the moons of Jupiter as a method of finding the time, though this method had several disadvantages: the finite speed of light would make the observed positions of the Galilean satellites — assuming they could even be seen, given clouds or the difficulty of training a telescope on rocking, rolling ships — depend on both their real position and the distance between the Earth and Jupiter, requiring a complicated seasonal correction.[2]

A nearer astronomical body would be much easier to use, and the one astronomical body (besides the sun) that would be simple to locate was the Moon. As a result, the development of accurate tables of the Moon's position at a given time was the second method by which the prize might be obtained (Cohen [1975], 43). To this latter end, the Royal Observatory at Greenwich was established (Cohen [1975], 44).

However, the problem turned out to be much more difficult than anticipated. Newton was able to calculate, to a high degree of accuracy, many of the irregularities in the Moon's motion, but remained unable (in work published during

[1]Though Harrison's first chronometer fit the requirements for winning the prize, he would be required to build three more, each of increasing accuracy but subject to ever more rigorous tests. Finally, George III intervened, and forced parliament to pay Harrison his deserved prize money in 1773. See David S. Landes, *Revolution in Time*, especially pp. 150 ff. for details.

[2]In fact, it was discrepancies in the timing of the eclipses, noticed by R"omer in 1675, that led to the first determination of the speed of light.

his lifetime) to determine the full quantity of the Moon's apse from universal gravitation alone. Between 1713 (the date of the English publication of the *Principia* and the 1740s, no significant work was done in explaining the lunar inequalities by universal gravitation alone.

It is difficult to say the cause of this gap; Brewster cites the lamentable state of analysis as a reason. Certainly the mathematics of the *Principia* made it an unpopular work; Descartes' principles, devoid of serious mathematics, were greatly favored (Grant [1852], 41). Newton's methods were quite difficult to implement as well; virtually ignoring his own method of the calculus and relying mainly on the works of Apollonius and Euclid (Cohen's introduction to Opticks, p. xxi).

Yet this cannot be the sole reason; progress was possible, even using geometric methods. Newton himself was later able to give a correct calculation of the motion of the line of apses, in a manuscript discovered in 1872 (Moulton [1914], 363).[3] Likewise, after he had calculated analytically the motion of the aphelia of the planets, Joseph Louis Lagrange gave a geometric analysis of the problem (Lagrange [1784]); it is likewise possible to show that the equilateral three body solution (Lagrange points) can be determined using purely geometric methods (Suzuki [1996]). To be sure, the latter knew the answer before starting the problem, and the first was Newton.

3.2 David Gregory

A minor contributor to celestial mechanics during this period was David Gregory (1659-1708). In his *Elements of Physical and Geometrical Astronomy*, published in 1726, he copies Newton's work in the *Principia*, Book I, Section IX ("The Motion of Bodies in Movable Orbits; and the Motio of their Apsides") practically verbatim, as Proposition VI of Book IV (Gregory, [1726B], 485ff).

Gregory drew together two pieces of Newton's work which are widely separated in the *Principia*: Newton's calculation of how much the apse would move under a given force, and how much of a force would be needed to make the apse move a given amount (Gregory [1726B], 489-90). In his calculation, he assumes a central force of the form $\frac{1}{A^n}$, where A is the distance to the object. (Gregory calculates that n would have to be $2\frac{4}{242}$ to obtain this amount of progression (Gregory [1726B], 489). As three degrees an orbit is roughly the rate of progression of the Moon's apse, it seems probable that Gregory was thinking of the case of the Moon.

Overall, Gregory's book was significant only in that it included "Sir Isaac Newton's Theory of the Moon" in Volume II, pages 563-571. Unfortunately, this Theory, though resonably accurate (but not enough so to win the Admiralty prize), did not provide details on how Newton arrived at the conclusions he did; hence, in terms of mathematical astronomy, this work is not a particularly valuable addition to the solution of the problem of three bodies.

[3]However, as Whiteside has shown, in order to arrive at this "correct" value Newton introduced a number of *ad hoc* assumptions (Whiteside [1974], VI, 508ff, especially 519n.)

3.3 Euler's Early Work

In 1747, Euler published at Berlin his essay, "Recherches sur le Mouvement des Corps Celestes en General", where he tackled the problems of celestial mechanics using the method of undetermined coefficients. Recall that as of 1747, the motion of the lunar apogee had not yet been determined from first principles, and in [1747], Euler expresses many doubts on the general validity of the law of the inverse square, a reservation which he will retain for many years. Indeed, most of the work in the 1747 piece involves solving the general problem of the motion of bodies under a force that varied not quite as the inverse square of the distance. He devotes some time to outlining his conviction that the inverse square law is alone insufficient to account for the inequalities of the planets. One of the major spurs for this research was the slow motion of the aphelia of the planets which could not be allowed under a simple inverse square rule using the first order approximations thought sufficient at the time:

> "As it is certain, that if there is no other force that acts on the planets, than that, which is directed to the sun, [and] decreases exactly in the ratio of the square of the distances, then neither the aphelia of the planets, nor their nodes, are subject to any change, [thus] it necessarily follows, that the force by which each planet is actually drawn, is not directed precisely towards the center of the sun, or that it does not follow exactly the reciprocal ratio of the square of the distance, or that there are other forces beyond this one, which perturb the motion of the planets, and cause this observed change in their aphelia and nodes." (Euler [1747], 2)

Euler is correct in what he says: there must be something involved other than an inverse square law of attraction towards the center of the sun. Rather than assume it is the mutual perturbations of the planets, however, Euler decides:

> "Because of the researches and reflections that I have made, on the origin of these forces, [i.e.,] those of the perturbations, that one finds in the motion of the Moon and of the greater planets, I am led to believe that these forces support [the idea] that the planets attract each other, not exactly according to an inverse square of their distances." (Euler, [1747], 3)

In what becomes his favored means of supporting the insufficiency of the inverse square law, he cites the nonsphericity of the planets:

> "Thus, in the case where the planet is nonspherical, it is not difficult to prove by the calculus that the resulting force of attraction neither decreases by the [inverse] square of the distance nor is directed towards the planet's center." (Euler, [1747], 4)

Euler expands on this idea in a letter written to Tobias Mayer, on December 25, 1751, which is reminiscent of Clairaut's suggestion of the addition of an inverse quartic term:

> "According to the theory of Newtonian attraction, the centripetal force of the Earth, owing to its spheroidal shape, ought certainly to decrease in about the inverse ratio of the distance squared, and when z denotes the distance, the formula for this force must therefore be $\frac{A}{ZZ}+\frac{B}{Z^4}$ when the Moon is in the equator ... Hence I am almost sure that the lunar inequalities that still remain [after Clairaut's work], which could not be determined through any theory, arise from this reason." (Forbes [1971], 45)

It seems clear that Euler's [1747] was not meant as a mere attempt to solve a general question of the motion of objects under a variant force law; in every variant force law he solves, Euler derives a formula for the motion of the apse, which strongly suggests that Euler was in particular concerned about the motion of the Moon's apse.

Before continuing, one might speculate on why Euler (and so many others) seized upon the idea of a variant force law, instead of examining the real cause: the perturbing effects of the other bodies in the solar system. A very simple reason might be advanced: the n-body problem is too complicated.

More precisely, the differential equations of orbit that are needed to solve the two-body problem are, in polar coordinates:

$$\frac{d^2r}{dt^2} - r\Big(\frac{d\theta}{dt}\Big)^2 = -f(r)$$

$$r\frac{d^2r}{dt^2} + 2\frac{dr}{dt}\frac{d\theta}{dt} = 0$$

where $f(r)$ is the force law, assumed to be a function only of the distance. If one varies the force law, one affects only the first equation, which can be solved (or at least approximated) by a simple perturbation procedure. It is this procedure that Euler develops.

This particular essay by Euler should be seen as the translation of Newton's methods in Book I of the *Principia* to the language of analysis. Euler examines a number of problems. The second problem deals with the motion of a body attracted to a fixed center by a force varying as the inverse square of the distance (Euler [1747], 15). The third problem he examines is:

> "Given a force, acting on the body M and directed towards the point C, considered fixed, which is only nearly proportional to the reciprocal of the squares of the distances, find the movement of the body, supposing that the orbit differs little from that of a circle." (Euler [1747], 23)

His method is as follows: the two differential equations of motion are:

I. $$2dr d\phi + r dd\phi = 0$$

II. $$ddr - r d\phi^2 + \frac{aV}{\Pi} d\omega^2 = 0$$

where r is the distance to the fixed point, ϕ is the angle measured from a fixed line, a is the mean distance (i.e., the length of the semimajor axis of the orbit), Π is the force of attraction at that distance, ω is the mean motion, and V is the force of attraction (Euler [1747], 24). Thus, if one assumes that at least part of the central force is given by the inverse square law, one has:

$$\frac{aV}{\Pi} = \frac{mc^3}{rr} + R$$

where R is a given function of r (Euler [1747], 24). Note that in this case the perturbing force depends only on the distance to the central point.

Euler assumes without explanation that the perturbed equations will satisfy:

$$d\omega = \alpha dv(1 + k\cos v)$$

$$r = c(1 + k\cos v + s)$$

where s is a very small quantity (Euler [1747], p. 24) (For the unperturbed equations, i.e., where R is zero, one would have $\alpha = 1$ and $s = 0$) Euler arrives at the equation:

$$0 = dds(1 + k\cos v) + kdsdv\sin v - 2\alpha\alpha msdv^2 + \frac{3\alpha\alpha nnsdv^2}{1 + k\cos v}$$

$$+\frac{\alpha\alpha}{c}Rdv^2(1+k\cos v)^3 - kdv^2\cos v - k^2dv^2 + \alpha\alpha mdv^2 + \alpha\alpha mkdv^2\cos v - \alpha\alpha nndv^2$$

which depends on a linear approximation to $\frac{1}{(1+k\cos v+s)^n}$ (Euler [1747], 25). This is the equation that is to be solved for the values of s and α, once R is known. The method Euler uses here is easily recognized as the method of unknown constants, and while he may have used a similar method in *Inquisitio physica in causam fluxus ac refluxus maris*, a 1740 essay on the tides, this is his first clear, widely spread dissemination of the method.

Euler's first example is particularly important, for he introduces the notion of solving the perturbed equations by means of a trigonometric functions. Euler assumes a perturbing force of the form $R = \frac{\mu c^{\nu+1}}{r^n}$ and since r was assumed to be of the form $r = c(1 + k\cos v + s)$, one can then write:

$$R = \frac{\mu c}{(1 + k\cos v + s)^\nu}$$

using a first order approximation (Euler [1747], 26).[4] Carrying out the binomial expansion to third order, this can be written:

$$\begin{aligned} R &= \mu + (3-\nu)\mu k\cos v + \tfrac{1}{2}(3-\nu)(2-\nu)\mu k^2\cos^2 v \\ &\quad + \tfrac{1}{6}(3-\nu)(2-\nu)(1-\nu)\mu k^3\cos^3 v - \tfrac{\mu\nu s}{(1+k\cos v)^{\nu-2}} \end{aligned}$$

[4]Assuming R is a function of s. Then from the above, one has the linear approximation for R near $s = 0$ of:

$$R \approx \frac{\mu c}{(1 + k\cos v)^\mu} - \frac{\nu\mu c}{(1 + k\cos v)^{\nu-1}} \cdot s$$

This can be expressed as the above equation.

This is then substituted into the above differential equation, yielding:

$$\begin{aligned} 0 \;=\; & \tfrac{dds}{dv^2}(1+k\cos v)+\tfrac{kds}{dv}\sin v-2\alpha^2 ms+\tfrac{3\alpha^2n^2s}{1+k\cos v}-\tfrac{\alpha^2\mu\nu s}{(1+k\cos v)^{\nu-2}}\\ & -k\cos v-k^2+\alpha^2 m+\alpha^2 mk\cos v-\alpha^2n^2+\alpha^2\mu+(3-\nu)\alpha^2\mu k\cos v\\ & +\tfrac{1}{2}(3-\nu)(2-\nu)\mu\alpha^2k^2\cos^2 v+\tfrac{1}{6}(3-\nu)(2-\nu)(1-\nu)\mu\alpha^2k^3\cos^3 v\\ & -\tfrac{\mu\nu s}{(1+k\cos v)^{\nu-2}} \end{aligned}$$

Since when $\mu=0$, $s=0$, and thus the terms

$$-k^2+\alpha^2 m-\alpha^2n^2+\alpha^2\mu+\frac{1}{4}(3-\nu)(2-\nu)\mu\alpha^2k^2$$

$$-k\cos v+\alpha^2 mk\cos v+(3-v)\alpha^2\mu k\cos v+\frac{1}{8}(3-\nu)(2-\nu)(1-\nu)\mu alpha^2k^3\cos v$$

$$+\frac{1}{4}(3-\nu)(2-\nu)\mu\alpha^2k^2\cos 2v+\frac{1}{24}(3-\nu)(2-\nu)(1-\nu)\mu\alpha^2k^3\cos 3v$$

must be identically zero (Euler [1747], 26).[5] By examining the coefficients of the constant terms and the $\cos v$ terms, Euler obtains the result:

$$n^2=m(1-k^2)+\mu-\frac{1}{4}(3-\nu)(2+\nu)\mu k^2-\frac{1}{8}(3-\nu)(2-\nu)(1-\nu)\mu k^4$$

which can be used to eliminate the n^2 terms in the differential equation, yielding:

$$\begin{aligned} 0 \;=\; & \tfrac{dds}{\alpha^2dv^2}(1+k\cos v)+\tfrac{kds}{\alpha^2dv}\sin v-2ms+\tfrac{3n^2s}{1+k\cos v}-\tfrac{\mu\nu s}{(1+k\cos v)^{\nu-2}}\\ & +\tfrac{1}{4}(3-\nu)(2-\nu)\mu k^2\cos 2v+\tfrac{1}{24}(3-\nu)(2-\nu)(1-\nu)\mu k^3\cos 3v \end{aligned}$$

To solve this, Euler supposes

$$s=A+B\cos v+C\cos 2v+D\cos 3v$$

and obtains a set of recursion equations for s by differentiation. While Euler assumes s has a finite trigonometric series expansion here, Euler has, elsewhere in [1747], used infinite trigonometric series expansions (Euler [1747], 20), and the extension to the use of such a series to the perturbation is a natural idea. Indeed, the only rationale for limiting the above expression of s to the cosine of $3v$ is that the expansion for the force equation was carried out only to the third degree in the binomial expansion. The direct descendant of this work will be Euler's 1748 entry for the French Academy of Sciences, for an determination of the motions of Jupiter and Saturn from the principles of universal gravitation alone.

In a later article, *De Perturbatione Motus Planetarum ab Eorum Figura non Sphaerica Oriunda*, for the St. Petersburg Academy of the Sciences for 1750/1 (published in 1753), Euler examined the effects of nonsphericity in detail, not

[5] Euler has expanded the $\cos^2 v$ and $\cos^3 v$ terms to obtain this result

of the attracting body, but rather of the object being attracted. The analytical method of the problem is not substantially different from that of his 1747 article.[6]

Euler considers the motion of a barbell-shaped object around a center of attraction. The inverse square law is obeyed for each of the ends (which Euler assumes are spheres). Might the gravitational attraction cause the observed motion of the apogee? Euler demonstrated that in order for the non-sphericity of the Moon to cause the observed change in the Moon's apogee the long axis would have to be two and a half times as great as the short axis (Euler [1753], 174). This, of course, is absurd. But if the oblateness of the Moon is not the cause, what is?

> "But if this oblong figure is seen as intolerable, and if some smaller spherical figure defects can [not] produce the acceleration of the apogee, certainly other forces act on the Moon, [and] the complete motion of the apogee is produced by them: or it may be decided that the Earth pulls on the Moon not eactly proportional to the reciprocal of the square of the distance." (Euler [1753], 174)

It might be thought that Clairaut's determination of the full motion of the apogee based on extending the approximation to a higher degree would eliminate Euler's suspicions about the insufficiency of universal gravitation; however, Euler's subsequent remarks do not bear this out, as we shall see.

3.4 d'Alembert

Aside from Euler, another prominent supporter of the physical school (though he would surely deny it) was d'Alembert. In the introduction to his *Reseraches on Various Points of the System of the World*, published in 1754, he writes:

> "[Euler and Clairaut] believed they could draw from this [the motion of the apse] some consequence against the law of gravitation decreasing by the inverse square of the distance. For me, I have always thought that it was not necessary to decide to quickly abandon this law, and this for two reasons that I will only indicate, which are developed more at length in this work. The first is based on a principle which is equally dangerous to employ when the phenomena oppose it, and to neglect when it is not opposed; this is that all other laws substituted for the law of squares are not as simple, since then the ratio of attractions no longer depends simply on the distances; the second, is that the substituted law can not serve, as some people have thought, to explain all the times the phenomena of gravitation,

[6]The problem may have been suggested to Euler by a letter from Buffon, in one of his responses to Clairaut's suggestion to changing the inverse square law. Buffon suggested that the Moon might be "strongly irregular" in shape: perhaps an ellipsoid with the long axis several times the shorter axes. Clairaut discounted this notion (op. cit Waff [1976], 130 ff.).

> and that of the attraction that one recognizes or that one supposes between terrestrial bodies." (d'Alembert [1754], xxxvii-xxxviii)

Surprisingly, d'Alembert includes Clairaut among those who would abandon Newtonian gravitation![7] Thus, he says:

> "M. Clairaut wrote to the public assembly of the Academy on 15 November 1747 a memoir in which he claimed that the movement of the apogee of the Moon found by theory is half as slow as that given by observation. From this he concluded that the gravitational force is not proportional to the inverse square of the distance ...
>
> ... I did not believe it as important as it had appeared to others. Not only did this contradict the Newtonian system not at all, likewise it did not give, to speak properly, any threat to the fundamental law of this system, since it is obvious that one can attribute in part the movement of the apogee to some particular force, different from that of gravity. It is because of this that I believed no need to draw from my calculations any conclusion against the system of M. Newton." (d'Alembert [1754], 113-4)

D'Alembert thus places himself firmly within the physical school, by arguing that it is not surprising that Newtonian gravitation alone cannot account for the full motion of the apogee, since other forces besides gravity are "obviously" at work. Later, d'Alembert writes:

> "But when the movement of the apogee found by the Theory does not exactly conform to that given by observation, this was not in my opinion a sufficient reason for changing the law of attraction in the planetary system." (d'Alembert [1754], 184)

d'Alembert then gives a number of reasons for not changing the law of attraction.

> "This function which expresses the law of attraction, and by which one supposes renders the reason of the movement of celestial bodies, can not explain other phenomena ... the formula of attraction which differs little from the law of the square at the distance of the Moon, is very different from this law at the surface of the Earth ... one agrees that the formula $\frac{1}{xx} + \frac{b}{x^4}$ cannot serve to express this law of attraction." (d'Alembert [1754], 185)

A key difference between Euler and d'Alembert is that while Euler always retained a vestige of Cartesianism, d'Alembert is solidly Newtonian. While Euler will suggest vortex-based explanations of planetary inequalities, d'Alembert says:

[7] Or perhaps not so surprisingly. The relationship between d'Alembert and Clairaut was far from cordial; the former's writings are filled with thinly veiled attacks on Clairaut's work. Thus, despite the fact that Clairaut eventually backed away from the belief that the inverse square law needed to be amended, after holding it for only a few brief years, d'Alembert still seems to classify Clairaut among those who would change what Newton wrought.

> "I believe therefore, without changing a thing of the law of gravitation, that one has only to adjoin some particular forces to it, the nature of which I absolutely refrain myself from declaring.[8] M. Newton has already suspected such things, and although he had not introduced these forces in the calculation of the movement of the apogee, it is possible that they produce a part [of its movement]; this is enough at least to suspend our judgment on this point." (d'Alembert [1754], xxxviii)

Though historically the work of Jean le Rond d'Alembert (1717-1783) follows that of Clairaut, it is really a throwback; like Euler's [1747], d'Alembert's work on the motions of the Moon, *Recherches sur differens points du System du Monde*, published in 1754 and 1756, attempted to write the perturbing force as a single, slightly modified *centripetal* force, and not (as Simpson and Clairaut) two forces, one parallel to the radius vector, and the other perpendicular to it. Thus:

> "It is certain that the orbit AL described by virtue of the forces Ψ and Π could also be described by virtue of a single force Q which tends always to the center or the fixed point T.
>
> ...
>
> "Therefore we go to find by the ordinary methods of equations that ought to determine the orbit if it is described by the single force Q, then we find a value of Q in terms of Ψ and Π, and we have the equation of the orbit desccribed by virtue of the forces Ψ and Π." (d'Alembert [1754], 12-3)

While this may seem superficially similar to Clairaut's method, whose Ω is a function of both the perpendicular and the parallel forces, d'Alembert's procedure is vastly different: he reduces the force Π, perpendicular to the radius, to a change in the centripetal force Ψ. The method is entirely equivalent to Newton's reduction of the perturbing force of the sun on the Moon to a variation in the centripetal force of the Earth on the Moon.

3.5 Clairaut

Perhaps the most amazing thing about the problem of the motion of the apogee was that it existed at all. Newton's inability to account for the full quantity of the motion is peculiar. At one point (Book III, Proposition XXV, Problem VI) he tries to determine the mean value of the perturbing force of the sun on the motion of the Moon, and comes up with the amount of one part in $178\frac{29}{40}$ of the Earth's gravitational force on the Moon (Newton [1726], 300). Yet earlier, in Book I, Section IX, Proposition XLV, Problem XXXI, where he tries to calculate the motion of the apsides of an orbit under a perturbing force, he uses a value

[8] One is reminded of Newton's "I frame no hypotheses."

of one part in 357.45 to calculate the motion of the apse, and obtains the rate of $1°31'28''$ — then concludes by saying "The apsis of the moon is about twice as swift." (Newton [1717], 100)

This value of one part in 357.45 is precisely half the value he obtains in Book III; the full, mean value which he calculated originally would very nearly account for the entire motion of the apse. Why did Newton use the smaller value when, as Clairaut will point out, by using the full value he could have obtained the full motion of the apogee?[9]

Euler, for example, seemed to believe that Newton calculated the motion of the apogee and found it wanting, for in 1753 he wrote:

> "Why he [Newton] omits [the calculation] of the motion of the apogee, he seen to advance no other reason except that he noticed this motion, [calculated] according to the theory advances [and] conforms too little to observation. For from it [the theory], which Newton brought forth in his immortal work on the motion of the apsides, one can with no great difficulty determine the motion of the apogee of the Moon: against expectation this truth came out, that the annual motion of the [apogee of the] Moon with difficulty was discovered to have been about 20°, when however from observation it is established the apogee of the Moon during one year advances more than 40°." (Euler [1753B], 71)

From this statement, it is clear that Euler is referring to Newton's lack of calculation of the motion of the apogee in Book III — where he calculates the motion of the Moon's nodes, inclination, and variation, but *not* the motion of the apogee — and his calculation in Book I, where he does calculate the motion of the apogee under a force $\frac{1}{357.45}$ the strength of the central force.

Euler expresses dissatisfaction with his own attempts to provide a theory of the Moon — and suggests, in fact, that the doctrine of vortices might provide an answer. In a letter to Clairaut, dated September 30, 1747, he writes:

> "M. Bradley communicated to me a great number of observations, from which I can already determine these coefficients in such a way, that the calculation never differs more than $5'$, from observation; and as these errors could not be attributable to observation, I do not doubt, that a certain derangement of forces, as one supposes in the theory, is the cause. This circumstance renders very probable to

[9] Newton's value higher value is close to what a modern determination of the mean central perturbing force of the sun would be. This can be obtained by means of the integral:

$$\frac{1}{2\pi}\int_0^{2\pi}\left(\frac{M}{(R-r\cos\theta)^2}-\frac{M}{R^2}\right)\cos\theta d\theta$$

where R and r are the distances between the Earth and Sun, and the Earth and Moon, respectively, and M is the ratio between the mass of the sun and the mass of the Earth (roughly 330,000). To first order in $\frac{r}{R}$ (roughly 1/375), this is $\frac{Mr}{R^3}$. This is approximately $\frac{1}{160}$.

> me [that] vortices or some other material cause of these forces [are the source of these inequalities], since it is then easy to conceive, that these forces need be altered when they are transmitted through some other vortex. Thus I suspect that the force of the sun on the Moon is altered considerably in the opposition, since it then passes through the vortex of the Earth. And likewise I believe that the force of the sun on the superior planets is perturbed by the passage through the atmosphere or vortices of the inferior [planets], and therefore by the same reason, the force of Jupiter on Saturn in the opposition need be considerably deranged." (Bigourdan [1926], 29)

Likewise, it is clear that d'Alembert believed Newton was trying to determine the motion of the apogee based solely on Universal Gravitation. He writes:

> "M. Newton in the first edition of his Principia in 1687, said that he had calculated by the law of attraction the movement of the apogee, and had found it to conform well enough to the observations. But not only did he not give the method which he had followed for reaching this [conclusion], he avowed simultaneously that his calculation is inexact, and that this is the reason that he did not detail the preceding. In the second edition, the Scholie where one finds that [disavowal] which we speak of is replaced by another, where M. Newton neglects and omits the very first calculation, and speaks no more of the movement of the lunar apogee by observation. But in another place in this second edition, he says, without bringing in the proofs, that the action of the sun on the Moon, as far as it is directed to the Earth, is such that it gives the apogee its movement; however, it is very certain that the part of action by the sun, which is proportional to the distance from the Moon to the Earth, and which in the principles of M. Newton causes the movement of the apogee, is only half of that which is needed to give the necessary movement to the apogee." (D'Alembert [1754], xlix-xl)

The "another place in this second edition" that d'Alembert refers to can only be the calculation in Book I, Section IX.[10]

The motion of the Moon's apogee would eventually be solved by the one mathematician who recognized Newton's calculation in Book I, Section IX, for

[10] Some more recent historians of astronomy also treat this peculiarity as a problem; regarding Newton's work in the *Principia*, Ernest W. Brown notes in his *An Introductory Treatise on the Lunar Theory*:

> "The result which he [Newton] obtains for the mean motion of the perigee is only half its observed value ..." (Brown [1896], p. 237)

Moulton writes:

> "The value of the motion of the lunar perigee found by Newton from theory was only half that given by observations." (Moulton [1914], p. 363)

what it was: a bad example. In a footnote added to his 1747 paper, Clairaut says:

> "In this example, he [Newton] puts for the coefficient of the second term which expresses the [additional] centripetal force, only half the perturbing force of the sun, and it is not surprising that since one used only half the force destined to produce the movement of the apogee, one finds only half of its movement." (Clairaut [1747A], 354n).[11]

Clairaut would be chagrined to discover that, using the analytical method and the full value of the sun's perturbing force, one still did not obtain the full motion of the apse, which was the start of his work that would ultimately lead him to the prize for the Russian Academy of the Science's first prize contest in 1750. In fact, a seemingly more accurate approximation leads to a less accurate prediction of the motion of the apogee.

This surprising lack of predictive ability led Clairaut to raise questions about the sufficiency of the simple inverse square law. These doubts he expressed in a paper he read on November 15, 1747 before the French Academy of the Sciences. The paper, entitled "On the system of the world, under the principal of universal gravitation", spends much time discussing Clairaut's arrival at the idea of the insufficiency of the simple inverse square law:

> "After having extensively examined the theory of Mr. Newton, without drawing the conviction that I expected, I determined myself to take no more from him, and to find directly the celestial movement on the sole supposition of their mutual attraction ..." (Clairaut [1747A], 334-5)

Immediately afterward Clairaut points to the key problem with Newtonian gravitational theory:

> "Of all the inequalities that affect the movement of the Moon, that which has appeared the most essential to examine, and at the same time that which Mr. Newton treats the most obscurely, is the movement of the apogee: this is where one needs to employ the greatest corrections in the movement of the Moon." (Clairaut [1747A], 335)

Recall that Newton never did publish a full account of the gravitational theory of the Moon's motion. When Clairaut re-examined the problem, considering the full effect of the sun's force on the motion of the Moon, he discovered he could account for only half of the observed motion of the Moon's apse (about three degrees a revolution). At first he has harsh words for Newtonian gravitation:

> "If this theory does not gives the movement of the apogee, or gives one very distant from the actual [value], the difference of which cannot be attributed to observational errors, it should be henceforth

[11] The date of the footnote is obscure, but is certainly earlier than the Spring of 1748.

> condemned without appeal, since one is by its method no closer to the truth than at the time of the first astronomers, who supposed the moon moved uniformly in a circle around the Earth." (Clairaut [1747A], 336)

However, noting the success of Newtonian gravitation and its correlation with Kepler's three laws, the motion of the nodes and other astronomical phenomena, as well as in determining the tides, he decided to attempt to retain it (Clairaut, [1747A], 337). However:

> "The moon without doubt expresses some other law of attraction than the [inverse] square of the distance, but the principal planets do not require any other law.
>
> "It is therefore easy to respond to this difficulty, and noting that there are an infinite number of laws which give an attraction which differs very sensibly from the law of the squares for small distances, and which deviates so little for the large, that one cannot perceive it by observations. One might regard, for example, the analytic quantity of the distance composed of two terms, one having the square of the distance as its divisor,and the other having the square square." (Clairaut, [1747A], p. 337)

Clairaut's suggestion was immediately attacked by Georges Louis Leclerc, Comte de Buffon (1707-1788), beginning a series of heated exchanges between the two, Buffon being strongly on the side of the simple inverse square law. The controversy died out in any case, when Clairaut demonstrated that the simple inverse law could, indeed, account for the full motion of the Moon's apogee, provided the mathematical approximations used were of a higher degree.

By January 21, 1749, he completed his re-examination and discovered that previously neglected terms would account for nearly all of the remaining discrepancy between the observed motion of the apogee and its predicted motion under a simple, Newtonian law. He deposited a paper with the French Academy of the Sciences, entitled "On the Orbit of the Moon, Including The Squares of the Forces of the Perturbing Forces", but did not read it at that time.[12]

In December of 1749, the Imperial Russian Academy of the Sciences announced its first international prize question (Kopelevich [1966], 656). Out of five questions anonymously proposed by Euler, the Academy selected the question asking:

> "to demonstrate whether all the inequalities observed in lunar motion are in accordance with Newtonian theory — and if they are not, to demonstrate the true theory behind all these inequalities, so that

[12] As a side note of interest, the 1749 paper may be the earliest occurrence of Hebrew letters in mathematics, antedating Cantor's $\aleph$ by more than a century and a half. Clairaut used the letters waw, beth, 'ayin, pe, and qoph to represent certain terms in the expansion of the perturbation function. (Clairaut [1749], p. 424)

> the exact position of the moon at any time can be computed with the aid of this theory." (Kopelevich [1966], 654)

Of the major figures working on the theory of the Moon at the time, Euler (as a member of the Academy) was ineligible; d'Alembert dropped out in February, and only Clairaut entered an essay, in December (Kopelevich [1966], 657). There is no indication that Simpson, who was also working on a theory of the Moon (included in his *Essays* [1757]) even knew about the contest.[13] On June 5, 1751, Euler sent out the official announcement that Clairaut won the contest, by a large margin, since the other essays were, in Euler's words, "categorically rejected, since they completely fail to comprehend the essence of the matter ..." (Kopelevich [1966], 658).

Clairaut demonstrated that one could not ignore the terms of the second order of the perturbation, which all previous workers had considered negligible. Instead, they added a motion to the apse almost as large as the motion added by the first order terms. Thus, the problem of the total motion of the apse *could* be explained in the purely Newtonian fashion, without changing the law of gravitation or invoking some other cause.[14]

A brief sketch of Clairaut's method will give some insight into the practices of celestial mechanics at mid-century. Clairaut begins by breaking down the

[13] Simpson is an unusual figure whose work merits further study. Though from England, he is not hostile to the Leibnizian differential notation; in the introduction to his *Miscellaneous Tracts on Some Curious, and Very Interesting, Subjects in Mechanics, Physical Astronomy, and Speculative Mathematics* of 1757, he writes:

> "I have chiefly adhered to the analytic method of Investigation, as being the most direct and extensive, and best adapted to these abstruse kinds of speculations. Where a geometrical demonstration could be introduced, and seemed preferable, I have given one: but, tho' a problem, sometimes, by this last method, acquires a degree of perspicuity and elegance, not easy to be arrived at any other way, yet I cannot be of the opinion of Those who affect to show a dislike to every thing performed by means of symbols and on Algebraical Process" (Simpson, [1757], n.p.)

Simpson goes farther, in fact, and decries the slavish adherence to geometric and Newtonian methods among English mathematicians:

> "And it appears clear to me, that, it is by a diligent cultivation of the Modern Analysis, that foreign Mathematicians have, of late, been able to push their Researches farther, in many particulars, than Sir Isaac Newton and his followers here, have done" (Simpson, [1757], n.p.)

This, of course, marks Simpson as an apostate. It is clear that he is not completely taken with the analytic method, and seems concerned with some aspects of the way that the "foreign Mathematicians" have applied it:

> "tho' it must be allowed, on the other hand, that the same Neatness, and Accuracy of Demonstration, is not everywhere to be found in those Authors; owing in some measure, perhaps, to too great a disregard for the Geometry of the Antients." (Simpson, [1757], n.p.)

In this is also included Simpson's lunar theory, which follows the Euclidean development, and not Clairaut's method.

[14] The timing is peculiar, to say the least, for Clairaut had already answered — but not announced — the solution to the question before the Prize was offered.

perturbing force into two components, Π, perpendicular to the radius vector, and Σ, parallel to it. (Clairaut considers the Earth, Sun, and Moon to be in the same plane) Through a series of manipulations, Clairaut ends with the following equation relating the radius vector r to the true anomaly v:

$$\frac{f^2}{Mr} = 1 - g\sin v - c\cos v + \sin v \int \Omega dv \cos v - \cos v \int \Omega dv \sin v$$

where M is the mass of the Earth, v is the true anomaly, $\frac{f^2}{M}$ is the semilatus rectum, and Ω is a quantity that depends on the perturbing force, which in turn depends on the true anomaly v.[15]

First, the solution to the unperturbed ($\Omega = 0$) case is

$$\frac{f^2}{Mr} = 1 - g\sin v - c\cos v$$

where g and c are constants.[16] The perturbed solution is given by

$$\frac{1}{r} = \frac{1}{p} - \frac{e}{p}\cos v + \Delta$$

where

$$\Delta = \sin v \int \Omega \cos v dv - \cos v \int \Omega \sin v dv$$

Ω is a function of the true anomaly, and as such (Clairaut assumes) it can be represented by a trigonometric series. Therefore, Δ is also some trigonometric series. Clairaut assumes that this solution can be reduced to the form

$$\frac{1}{r} = \frac{1}{k} - \frac{e}{k}\cos mv$$

for suitable constants k and m, which he proceeds to do, comparing coefficients of the two series.

A simple, illustrative case that Clairaut uses occurs when one adds a force varying as the inverse cube of the distance to the central force, and no force perpendicular to the radius vector. In this case, $\Phi = \frac{hM}{r^3}$ and $\Pi = 0$, making $\Omega = \frac{h}{r}$.

The solution to the perturbed equations is:

$$\frac{1}{r} = \frac{1}{p} - \frac{e}{p}\cos v + \Delta$$

[15] Ω is the expression:

$$\Omega = \frac{\frac{\Phi r^2}{M} + \frac{\Pi r dr}{M dv} - 2\rho}{1 + 2\rho}$$

where

$$\rho = \frac{\int \Pi r^3 dv}{f^2}$$

where $\frac{f^2}{M}$ is the semilatus rectum, or parameter.

[16] The eccentricity of the orbit is given by $\sqrt{g^2 + c^2}$, and the position of the apogee is given by $\arctan \frac{g}{c}$

Clairaut assumes that this can be reduced to the form:

$$\frac{1}{r} = \frac{1}{k} - \frac{e}{k}\cos mv$$

Note this is the equation of a precessing ellipse, whose apse moves in the ratio of $1 - m$ to 1. (This may have provided Clairaut with the justification for saying that m is never 1, since then the apse would not precess). Of interest is the resemblance of this method to Lagrange's development of the method of variation of parameters; Lagrange was well acquainted with Clairaut's work and he may well have taken his inspiration from him.

In any event, one can substitute this into the equation for Δ, since:

$$\Omega = \frac{h}{k} - \frac{eh}{k}\cos mv$$

This yields:

$$\frac{1}{r} = \frac{1}{p}(1 + \frac{h}{k}) + \frac{he}{pk(m^2-1)}\cos mv - \frac{1}{p}(c + \frac{he}{k(m^2-1)} + \frac{h}{k})\cos v$$

based on a lemma Clairaut proved earlier (Clairaut [1765], 13).[17] Note that while m is an unknown, the coefficient of $\cos mv$ is related to m. In order to determine m, Clairaut sets the two coefficients $-\frac{e}{k}$ (from the initial solution) and $\frac{he}{pk(m^2-1)}$ equal to each other, and determines that $m = \sqrt{1 - \frac{h}{p}}$.

The essential features of Clairaut's method, for the future of celestial mechanics, are that he obtains a solution to the unperturbed case, a formal solution to the perturbed case, then an assumed actual solution with undetermined coefficients. By comparing the coefficients, he then obtains the actual solution.

Euler congratulated Clairaut in strong terms; in a letter, dated April 10, 1751, he writes:

> "I congratulate you for determining the cause [of the motion of the apogee], and on this happy discovery, and I dare say, that I regard this discovery, as the most important and profound [one], which has ever been made in mathematics." (Bigourdan [1928], 36)

Later, on June 29, 1751, Euler heaped additional praise upon Clairaut:

> "and the more I consider this excellent discovery, the more important it seems to me, and in my opinion this is the greatest discovery in the theory of astronomy, without which it would be absolutely impossible to succeed in determining the derangements, that the planets cause upon one another in their movement. Because it is very certain that from this discovery one can regard the law of attraction [by the] inverse ratio of the square of the distance as solidly established, on which depends all the theory of astronomy." (Bigourdan [1928], 36-37)

[17] As in the original manuscript. The c should be an e, and the $\frac{h}{k}$ should not be part of the coefficient to $\cos v$.

This is a remarkable change to Newtonianism, from the Cartesianism expressed in Euler's letter to Clairaut just four years previously (see above): now, it would seem, Euler is convinced that the inverse square law for finite bodies need no modification to account for the remaining discrepancies in the orbits of the planets. (However, as we shall see, Euler's conversion is not complete)

Clairaut's method was seized upon by many prominent physical astronomers, who applied it to other bodies in the solar system. Bailly applied Clairaut's method to Jupiter's satellites in a paper he read on March 27, 1762 (Bailly [1763]) J. J. Lalande made wide use of the method, applying it to the motion of Mars (with Jupiter as the perturbing body),(Lalande [1758]) and to Venus (with Earth as the perturbing body) (Lalande [1760])

Though some would, despite Clairaut's work, question the general validity of the inverse square law for gravitation, very few after Clairaut would continue to question the essential correctness of Newtonian gravitation, and all succeeding work on problems in celestial mechanics would use, as their basis, Newtonian gravitation as their starting point. However, Clairaut was the primary defender of the idea that universal gravitation alone, by the law of the inverse square alone, could account for the inequalities (in the motions of the Moon and, presumably, the other planets). In the introduction to the second (and last) edition of his *Theory of the Moon* he writes:

> "This dissertation has all the success that I could desire . . . not only in making it obvious that the law of Newtonian attractions is sufficient to explain all irregularities of the moon, but in furnishing better tables of the movement of that heavenly body." (Clairaut [1765], preface p. 6)

The weight of authority was on the side of the physical school, however. Clairaut as a mathematician could not measure up against d'Alembert and Euler; though many astronomers followed Clairaut's route, many mathematicians were convinced, along with Euler, that additional forces besides universal gravitation were at work, perturbing the motions of the planets. In particular, the interplanetary medium was viewed as a source of inequalities — and this time, it would be examined under the microscope of analysis.

Chapter 4

Revival of the Interplanetary Medium

Clairaut's vindication of universal gravitation was very timely. Only a few years before, Halley's discovery of the secular acceleration was revived and announced to the world. Had Cartesianism been a viable force at the time, this would surely have been seized upon as evidence of particles of the Cartesian vortex in interplanetary space, and the paradoxical assumption about the resisting properties of the vortex particles could be done away with. However, thanks to Clairaut's work, there was no question that the *dominant* factor in the motions of the planets was universal gravitation. In 1762, Bossut wrote:

> "The system of universal gravitation, born in England, fought at first in France, finally adopted in all the Academies, is today regarded as a most incontestable experimental truth." (Bossut [1762], 5)

Despite Clairaut's best efforts, there still remained many small discrepancies between predictions based on the inverse square law of Newton and the observed positions of the planets. Most notable of these were the secular variations observed in the mean motions of the Moon, Jupiter, and Saturn. These discrepancies might be accounted for by improving the accuracy of the approximations. The more popular view, however, was that Newtonian gravitation alone could not explain the discrepancies between theory and observation. Though Cartesianism was effectively moribound, one vestige remained: an interplanetary medium.

Hence, at mid-century, the physical school dominated astronomical thought. Dunthorne obtained a quantiative value for the Moon's secular acceleration, establishing it as an additional anomaly that, seemingly, could not be explained by the simple force law. Examinations of variant force laws made up the core of Euler's work at mid-century, and in 1762 the French Academy offered a prize for determining if the planets moved through a resisting medium.

4.1 Richard Dunthorne

Key to the belief that an interplanetary medium existed was the existence of a secular acceleration in the mean motion of the Moon, which could be explained by assuming that the Moon's mean distance was decreasing — which, in turn, could be explained as an effect of a resisting interplanetary medium.

Halley's discovery of 1695 was revived a few years after Halley's death in 1742, by the Reverend Richard Dunthorne.[1] At first, he was well satisfied with the correlation between the predictions of the Moon under universal gravitation and its observed position. On February 5, 1747, he read his results before the Royal Society:

> "From a Comparison of such Observations I obtained the Moon's mean Longitude, which came out $1'$, at least, greater than in the Tables, and very nearly as *Newton* has it in the last Edition of his *Principia*." (Dunthorne [1746], 413)

By this, it is clear that Dunthorne refers toNewton's calculations in Book III, where he determined the various motions of the Moon caused by the mutual gravitation of the Earth and Sun, and *not* as he calculated them in his *Theory of the Moon's Motion*. The comparison of the observation to the *Principia* in particular is important, for it was in the *Principia* that Newton explicitly calculated the motions of the Moon under only the assumption of universal gravitation (as opposed to his *Theory of the Moon's Motion*, where he provides an algorithm for calculating the Moon's position without explaining how he arrived at the formula).

In this first letter, Dunthorne expressed general satisfaction with the computed tables. Somewhat later, Dunthorne examined the observations in greater detail and communicated his results to the Royal Society in a letter read June 1, 1749.

> "I proceeded to examine the mean Motion of the Moon, of her Apogee, and Nodes, to see whether they were well represented by the Tables for any considerable Number of Years, and whether I should be able to make out that Acceleration of the Moon's Motion which Dr. *Halley* suspected." (Dunthorne [1749], 162)

(Dunthorne gives no indication of how he was made aware of the secular acceleration discovered by Halley). Although the second edition of the *Principia* was the most prominent announcement of Halley's discovery, Dunthorne refers to using the "last" edition of the *Principia*, presumably the 1726 edition, where the secular acceleration is nowhere mentioned. First, he compared the eclipse times observed by Tycho with those computed by his own tables:

[1] It is interesting to notice how many religious figures enter into the history of celestial mechanics. Another reverend, Richard Bentley, would suggest to Newton the possibility of a gravitational disruption of the solar system, as we shall see below. The connection between religion and the long term stability question is another topic which seems to warrant investigation.

> "[I] found them agree full as well as could be expected; considering the Imperfection of his [Tycho's] Clocks, and the Difficulty there must commonly have been in determining the Middle of the Eclipse from the Facts observed, as published in his *Historia Caelestis*. Indeed the small Distance of Time between *Tycho Brahe* and *Flamsteed* render'd *Tycho*'s Observations but of little Use in this Enquiry." (Dunthorne [1749], 162-3)

Next, he used the observations of Bernard Walter and Regiomontanus (two German astronomer/mathematicians of the fifteenth century):

> "Upon comparing such of their Eclipses of the Moon, whose Circumstances are best related with the Tables, I found the computed Places of the Moon were mostly $5'$ too forward, and, in some, considerably more, which I could hardly persuade myself to throw upon the Errors of Observation; but concluded, that the Moon's mean Motion since that time, must have been something swifter than the Tables represent it; though the Disagreement of the Observations between themselves is too great to infer any thing from them with Certainty in so nice an Affair." (Dunthorne [1749], 163)

The third piece of evidence that Dunthorne used was Albategnius' observations.

> "Then I compared the four well-known Eclipses[2] observed by *Albategnius* with the Tables, and found the computed Places of the Moon in three of them considerably too forward: This, if I could have depended upon the Longitude of *Aracta*, would very much have confirmed me in the Opinion, that the Moon's mean Motion must have been swifter in some of the last Centuries than the Tables make it; though the Differences between these Observations, and the Tables, are not uniform enough to be taken for a certain Proof thereof." (Dunthorne [1749], 163)

The remainder of his evidence consists of some scattered observations by various astronomers, especially Ptolemy. Dunthorne includes two eclipses of the sun and one of the Moon, observed in Cairo between the time of Albategnius and Regiomontanus, which Tycho refers to, and an eclipse of the sun observed by Theon in Alexandria, between the time of Albategnius and Ptolemy (Dunthorne [1749], 163-4). As for Ptolemy's observations in particular:

> "The Eclipses recorded in *Ptolemy* in his *Almagest*, are most of them so loosely described, that, if they shew us the Moon's mean Motion has been accelerated in the long Interval of Time since they happened, they are wholly incapable of shewing us, how much that Acceleration has been. There are indeed two or three of them attended with such lucky Circumstances as not only plainly prove,

[2]Presumably the ones Halley wrote about.

> that there has been such an Acceleration, but also help us to guess at its Quantity." (Dunthorne [1749], 169)

One of these "lucky Circumstances" he describes in detail, for he recognizes it as one the most convincing evidence of a secular acceleration:

> "One of these is the Eclipse, said by *Hipparchus* to have been observed at *Babylon*, in the 366th Year of *Nabonassar*, the Night between the 26th and 27th Days of *Thoth*, when a small Part of the Moon's Disk was eclipsed from the North East, half an Hour before the End of the Night, and the Moon set eclipsed. This was the Year before Christ 313, *Decem.* 22. The Middle of this Eclipse at *Babylon* (supposing with *Ptolemy* the Meridian of that Place to be 50′ in the Time East of the Meridian of *Alexandria*), by my Tables was *Dec.* 22, 4h4′ apparent Time; the Duration was 1h37′, *Ptolemy* makes it 1h30′ nearly; whence the Beginning should have been about 8h15′ after Midnight: According to *Ptolemy*, the Night at *Babylon* was at that Time 14h24′ long, and therefore Sun rise at 7h12′ after Midnight; and as the Moon had then South Latitude, and was not quite come to the Sun's Opposition, her apparent Setting must have been something sooner, *i.e.* more than an Hour before the Beginning of the Eclipse, according to the Tables; whereas the Moon was seen eclipsed some Time before her Setting; which, I think, demonstrates, that the Moon's Place must have been forwarder, and consequently her Motion since that Time less than the Tables make it by about 40′ or 50′."

This is very convincing, for such an observaton cannot be explained by any reasonable amount of observational error: an observer might mistake the time, or the position, or the location, by a considerable amount, but is far less likely to be mistaken about whether the Moon set eclipsed or not.

Dunthorne uses another eclipse, observed in Babylon on March 19, 721 B.C., which began shortly after Moonrise, to place an upper limit on the amount of the acceleration (since if the acceleration was too great, the Moon would rise partially eclipsed); he concludes that the difference between observation and computation was no more than 50′ at that time — which corresponds to an acceleration in the mean motion of the Moon of 10″/100 years (Dunthorne [1749], 171)

This time, secular acceleration of the Moon would not drop out of scientific thought until the problem was finally solved, at first by Laplace, and then more thoroughly by Adams. And, while even two decades earlier, the existence of such a secular acceleration would have been viewed as positive proof of the existence of the "subtle matter" of the Cartesian vortices, thanks to the triumph of universal gravitation at the hands of Clairaut, there was no question that if gravitation could not alone explain it, then there existed matter in the interplanetary spaces — real matter, akin to that of Earthly substances, and not some other form of matter as postulated by the Cartesians.[3]

4.2 Euler

Euler cites observations, by Bernhard Walther, that imply the year is becoming shorter; in other words, the Earth itself is increasing its mean motion around the sun. In a letter to the Royal Society, written June 28, 1749 and read before the society on November 2, 1749, he writes:

> "For having carefully examined the modern Observations of the Sun with those of some Centuries past, although I have not gone farther back than the fifteenth Century, in which I have found *Walther's* Observations made at *Nuremberg*; yet I have observed that the Motion of the Sun (or of the Earth) is sensibly accelerated since that Time; so that the Years are shorter at present than formerly: The Reason of which is very natural; for if the Earth, in its Motion, suffers some little Resistance (which cannot be doubted, since the Space through which the Planets move, is necessarily full of some subtile Matter, were it no other than that of Light) the Effect of this Resistance will gradually bring the Planets nearer and nearer the Sun; and as their Orbits thereby become less, their periodical Times will also be diminsh'd." (Euler [1749A], 204)

Euler's certainty that space must contain some matter allows no then-current interpretation of light to avoid resistance: here, Euler views light as matter (one of Newton's particles, for example), which would cause resistance.[4] If one views light as a wave, as in Huygens' view, then the medium through which it moves must provide resistance; under *either* the particle *or* the wave theory, space must have some resistance.

Euler's belief in the secular acceleration of the Earth's orbit fit in well with his belief in God, for it implied that the solar system was *created* at some point in time:

> "Thus in Time the Earth ought to come within the Region of *Venus*, and in fine into that of *Mercury*, where it would necessarily be burnt. Hence it is manifest, that the System of the Planets cannot last for ever in its (present) State. It also incontestably follows, that this System must have had a Beginning: For whoever denies it, must grant me, that there was a Time, when the Earth was at the Distance of *Saturn*, and even farther; and consequently that no living Creature could subsist there. Nay there must have been a Time, when the Planets were nearer to some fixt Stars than to the Sun;

[3] One might speculate that the reason for Dunthorne's success in reviving the question was largely due to his quantitization of the amount — something that Halley was unable to accomplish — which went along with the Newtonian ideal of mathematizing nature.

[4] Later, Euler will change his view and depict light as a wave.

> and in this Case they could never come into the Solar System. This then is a Proof, purely physical, that the World, in its present State, must have had a Beginning, and must have an End." (Euler [1749A], 204-5)

Despite the fact that the secular acceleration of the Moon would be a second example to support his notion, Euler questions its existence. In his next letter, dated December 20, 1749, and read March 1, 1749,[5] Euler writes:

> "The late Dr. *Halley* had also remark'd, that the Revolutions of the Moon are quicker at present than they were in the Time of the ancient *Chaldeans*, who have left us some Observations of Eclipses. But as we measure the length of Years by the Number of Days, and Parts of a Day, which are contained in each of them; it is a new Question, Whether the Days, or the Revolutions of the Earth round its Axis, have always been of the same Length." (Euler [1749B], 357-8)

What Euler means by this is suggested in the last quoted paragraph of the letter:

> "And yet I have some Reasons, deduced from *Jupiter*'s Action on the Earth, to think, that the Earth's Revolution round its Axis continually becomes more and more rapid. For the force of *Jupiter* so accelerates the Earth's Motion in its Orbit round the Sun, that the Diminution of the Years would be too sensible, if the diurnal Motion had not been accelerated nearly in the same Proportion. Wherefore, since we hardly at all remark this considerable Diminution in the Years, from thence I conclude, that the Days sufer much the same Diminution; so that the same Number will answer nearly to a Year." (Euler [1749B], 358-9)

It is to this peculiar set of circumstances — the shortening year caused by the resistance of the ether and by Jupiter, combined with a shortening day that somehow marches in step with the shorter year giving us almost as many days in the year as before — that Euler seems to attribute Halley's secular acceleration of the Moon.

Why does Euler not believe in the secular acceleration of Dunthorne and Halley? Euler may have a personal reason for questioning Dunthorne's arguments. Dunthorne believed that this was too great an error to be attributed to poor observation. Euler, on the other hand, felt that a $5'$ error was easily attributable to observation. For example, for the 1748 Academy Prize (which will be discussed below), Euler will eventually produce tables giving the locations of the planets Jupiter and Saturn; when he compares his calculations with the

[5] This would be March 12, 1750 everywhere else in Europe; England did not switch to the Gregorian calendar until 1752. Elsewhere, the year began on January 1, but in England, the year began on the traditional date of March 25.

observations, he too finds a $5'$ difference between observation and computation, about which he says:

> "These positions determined by calculation, appear to me sufficient to prove the preference of my tables of Saturn over those which have served until now; [you] see that on average the error only rarely surpasses $5'$; and it is probable that in these cases, a good part [of the error] can be attributed to observations..." (Euler [1749], 157)

Of course, in his letter to Clairaut of September 30, 1747 (cited above), Euler declared that Bradley's observations of the Moon yield a difference of $5'$ from computation — and since he was criticizing the *physical* theory behind the calculations, "these errors could not be attributable to observation" (see above).

4.3 Tobias Mayer

The secular acceleration of the Moon occupied a portion of the letters that passed between Euler and Tobias Mayer, in the years between 1751 and 1755. Mayer disbelieved in Euler's acceleration of the Earth. In a letter dated August 22, 1753:

> "Your view that the solar years may be unequal has always appeared so fundamental to me that I would not have departed from it if I had not been compelled to do so through a more accurate investigation both of the ancient observations and of the correctness of the common chronological time-scale. It is first of all quite certain that from the time in which the observations of Hipparchus and Ptolemy were made, neither more nor fewer days have passed than one is normally used to count." (Forbes [1971], 73)

He attributed the discrepancies between Ptolemy and modern calculations to errors in Ptolemy's determination of the equinox (Forbes [1971], 74).[6] In order to intercalate a day between Ptolemy and ourselves (in effect, take into account the effects of a secular acceleration in the Earth's mean motion), Mayer argued that one must also make the synodic months longer in the days of Ptolemy, because Ptolemy's observation connects lunar eclipses with the equinoxes. Thus:

> "The general hypothesis [of constant mean motions] would therefore differ from the improved one [of secular acceleration] at new and full moon by about a day from the times given by Ptolemy; however, at the present epoch, both hypotheses would agree with one another. It follows further from that, that these two hypotheses must be different by half a day from one another around the times given by

[6] Mayer was not alone in believing Ptolemy's observations were in error; cf. Lalande [1757], p. 421. Indeed, if Ptolemy's observations are discarded, then the length of the solar year is virtually uniform.

> the Arabic astronomers, which fall approximately half-way between Ptolemy's and our epoch; that is, from the one hypothesis a given solar eclipse may have occurred about these mean times by day, from the other, however, by night. And then it is easy to perceive which of the two may be in accordance with the truth. It will namely be the one from the calculation of which the eclipse results as it then was actually observed by Albategnius and several other Arabic astronomers. This is afforded, however, by the general hypothesis; on the contrary, according to the alleged improved [hypothesis] a certain visible solar eclipse, such as that which, e.g., Albategnius at Aracta and simultaneously another [astronomer] at Antioch have observed, should have been observed about 12 hours earlier, i.e. by night. No matter how one may tackle this problem it is impossible to account for a whole day or 13° in the motion of the Moon. I have tried to arrange the lunar tables in accordance with the hypothesis of the intercalation of one or two days, and to ascribe the 13° or 26° difference in the mean motion of the Moon to an acceleration; only it was not possible for me even to bring the observations from the two most recent centuries into agreement with it, without committing an error of half or even a whole hour, with eclipses which were certainly accurately observed to within a few minutes." (Forbes [1971], 74)

As for secular variations in the rest of the planets:

> "I could quote various arguments against the secular acceleration of the motion of the planets and in particular Saturn, whose motion is not only unaccelerated but even retarded; only time and space are both used up on this occasion, and if you permit me, I will reveal my doubts next time." (Forbes [1971], 76)

Frustratingly, Mayer does not quote his various arguments. Whatever they were, Euler's (unfortunately lost) response convinced Mayer that the secular acceleration was real. However, Euler's views on the subject may be gleaned from some of earlier work on the matter, especially his 1747 essay. Recall in this essay Euler examined the effects of a variant central force, Euler's favorite explanation for many of the observed inequalities in the solar system. Here he says:

> "I still have nothing to say about the resistance, that the planets undergo by all appearance, in passing through the ether. This fluid, however subtle that it could be, is known nearly to oppose some resistance to the movement of the planets; and I believe [I] have already proven evident the effect of this resistance on the motion of the Earth. From which it follows, as I have moreover demonstrated, that the periodic times of the planets are subject to a continual diminution, and that their mean distances become insensibly smaller. This circumstances renders the determination of periodic

> times extremely difficult; since it is no longer permitted to compare the modern observations with the ancient [ones], in determining the time of a revolution, without having regard to this same diminution, which could have been caused in this interval of time. However because I have made seen, that the position of the apsides suffers no changes from this resistance, and that the excentricity is at no point altered: the research of these forces, of which I want to speak, is not already greatly halted by this fact. Finally, whatever law that these forces follow, it differs but little from the inverse duplicate ratio of the distance, that in the calculation one can without fault regard this aberration as infinitely small, [and] this can greatly contribute to overcoming the other obstacles." (Euler [1747], 7)[7]

Here, Euler says that the resistance has little effect on the short term motion of the planets — though over historic time, it renders the comparison of the positions of the planets inaccurate and unusable.

Mayer responded on November 25, 1753, and indicates that he was not completely convinced of the certainty of the secular acceleration of the planets.

> "The reasons with which you have supported the resistance of the aether are indeed so important that the small doubts which I have had against it have now been quite abolished. However, so much seems to be certain, that the effect of this resistance on the Earth's motion may either be very small, or augmented by the attraction of the other planets." (Forbes [1971], 76)

Suddenly Mayer's "various arguments" against the resistance of the ether is converted to "small doubts". However, the last sentence above suggests that he is being polite, rather than truly convinced: the effect of the ether, he seems to say, would be counteracted by universal gravitation.

> "From these observations I again found the year to be just as long as the new observations give it; at least, the differences were not so considerable that one could ascribe them to an acceleration in the motion of the Earth, particularly because [not only] did these sometimes show an excess but sometimes [also] a deficiency, and are consequently to be attributed rather to the incorrectness of the observations." (Forbes [1971], 76-7).[8]

Mayer gives a further hint as to what he meant by saying the effect of resistance is "augmented by the attraction of the other planets". After he examined the motions of the Earth (through the solar tables), he notes:

[7]Euler's claim to having already determined the effects of motion of the planets through a resisting medium probably refers to his piece in *Opuscula varii argumenti* (Berlin 1746), p. 245-276.

[8]Mayer ignores a second option, that there are variations in the length of the year, but they are periodic, rather than secular.

> "However, what I have particularly noticed hereby is this, that it seems as if the eccentricity of the Earth's orbit has previously been much larger than it is now. Not only Hipparchus' equinoxes, for which the difference can easily be ascribed to another and immediate cause, but especially the old observations of lunar eclipses show traces of this, as the latter are in better agreement with the lunar tables if one assumes a larger eccentricity. Should this conjecture, which still demands more detailed investigation, become established, it would perhaps cause the motion of the Earth to be retarded through the attraction of Jupiter or also of Venus, just as such a retardation in Saturn is combined with a diminution of its eccentricity. And since the observations show no actual retardation for the Earth, one must therefore conclude that the resistance of the aether again compensates such [an effect]." (Forbes [1971], 77)

Thus, even if there is a secular acceleration due to the presence of a resisting medium, other effects conspire to mask its existence.[9] Although this might be true for the Moon, Mayer is still against the idea of introducing a resistance of the ether:

> "The very marked acceleration in the Moon's motion may now also be best accounted for by this resistance. Yet I think that the cause might also lie in the attraction of the sun, from which the rest of the inequalities of the Moon's motion originate. For just that reason, some of these inequalities become appreciable, because their periods are very long; thus it could happen that in the combination of so many angles, if the approximation would be carried very far, an inequality would be found whose period may be very long and [even] infinitely large. And in these instances, this inequality would transform itself into a continuous acceleration. Meanwhile, it appears to be very difficult or quite impossible to derive this [conclusion] from the theory itself." (Forbes [1971], 77)

Here is the first suggestion that an inequality with a very long period would appear to be a constant, secular variation.[10]

In a letter of February 26, 1754, Euler responded to Mayer's notion of long term periods — and in the end (at least of the Euler-Mayer correspondence), it is Euler, not Mayer, who is swayed to the other's viewpoint, as Euler backs away from the notion of a resisting ether:

[9] Compare this with Euler's argument, above, in [1749B] that the shortening length of the day stays in step with a shortening year.

[10] Mayer considers only periods of "infinite" length; however, to appear as a constant, secular acceleration, the period need only be longer than any period of observations — e.g., of historical length. Ultimately, Laplace will show that an extremely long period variation in the Earth's eccentricity will be the source of about half the apparent acceleration in the Moon's mean motion; the remainder will be attributed, by Adams, to the decrease in the length of the Earth's day, due to tidal effects.

> "That you have also by means of the Sun found my lunar equation to be valid has also pleased me not a little, and regarding the resistance of the aether (which still appears to me well established and necessary) I would be rather dubious since I must confess that the discovered accelerated motion of the mean Moon could easily be the effect of an inequality whose arguments may have a period of many centuries. With regard to the Sun, I am completely convinced of my error; it is nevertheless to be regretted that one has no older and more accurate observations." (Forbes [1971], 79)

Here, Euler has come around to Mayer's view of the "secular" acceleration actually being one of a very long period. Euler gives no hint as to why the ether — even if it is no longer needed to explain the Moon's secular acceleration — is "well established and necessary".[11]

4.4 Lalande

Joseph Jerome de Lalande (1732-1807) provides a survey of the known secular accelerations in the mean motions of the planets and the Moon in his 1757 article. His comments about the existence of such a secular acceleration of the Earth's mean motion once again point to the existence of a resisting medium consisting of known types of matter:

> "This acceleration of the Earth gives rise to a disastrous consequence for humanity, announcing to us nearly the time & the manner in which it will end: in effect, if the Earth accelerates its movement thus, this is a certain proof that proves a resistance on the part of the ether or the subtle matter which fills the universe, not only that of light ..." (Lalande [1757], 413)

Lalande is cautious about the secular acceleration of the Earth's mean motion. Like Mayer, he is suspicious about Ptolemy's observations, since they are so divergent from more recent observations.[12] In the end, he concludes that, given the dubious nature of the evidence, the Earth has undergone no secular acceleration in its mean motion (Lalande [1757], 421) As for the other planets, Lalande notes the acceleration of Jupiter (Lalande [1757], 438), though no secular acceleration is apparent for Mars (Lalande [1757], 445), and Saturn is the

[11] One might speculate, however. The question of the transmission of the force of gravity across a vacuum has not yet been solved — and the existence of the ether would be a simple explanation. And if such an ether existed, light might be a wave in such an ether, which would explain phenomena such as diffraction and refraction, which the particle theory of Newton could not. It seems that unlike Newton, whose theory of light was forced upon him by the necessity of empty space, Euler's theory of "empty space" was forced upon him by the observations on light.

[12] He says: "Thus it is that nearly all astronomers find Ptolemy in error" (Lalande [1757], p. 421) when discussing Ptolemy's determination of the solar year, which is seven minutes longer than the best information available to Lalande. Bossut, on the other, uses this same data as evidence that the Earth's year is decreasing. Cf. with Mayer, above.

most unusual case of all, undergoing a *deceleration* (Lalande [1757], 439). If the planets were moving through a resisting ether, they all ought to be accelerating; if some uniform change other than the ether was the cause, such as a steady decrease in the mass of the sun, they ought to be undergoing the same type of change.

4.5 Le Monnier

In the same year that Lalande published his observations of the secular variations of the planets, Pierre Charles Le Monnier (1715-1799) published observations on variations in the inclinations of the five Saturnian satellites. He hearkens back to the extended solar atmosphere of Cassini and Daniel Bernoulli, but does not revive the doctrine of vortices:

> "The small inclination that affects the orbits of the six planets which turn around the sun, has first of all made it suspected that, as they tend to return to the same common plane, the solar atmosphere needs to be be extended to as far as Saturn. Likewise [the atmosphere] of these planets [extends] as far as their satellites; but within the limits that the retrograde comet of 1682 & 1607 (the same whose return we await this very year)[13], together with some other comets less inclined, could indicate to us in the following manner." (Le Monnier [1757], 88-9)

Le Monnier's suggestion about Halley's comet is that it could be used to determine the limit of the sun's atmosphere, presumably by the effects (or lack thereof) of the atmosphere on the path of the comet. In a similar manner the satellites of a planet could also be used to determine the limits of the atmosphere of their primary. However, Le Monnier warns:

> "In effect, independent of the other uses of these satellites [of Jupiter and Saturn], the research of the position of their orbits requires us to clarify more and more the limits of the atmospheres [of their respective planets]." (Le Monnier [1757], 90)

In other words, *if* the planets and their satellites are moving through a resisting medium, *then* simply improving the accuracy of the approximations to their motion under the purely Newtonian attraction — as Clairaut did — would not yield more accurate tables of planetary positions.

Thus, by the late 1750s, evidence was accumulating that universal gravitation by itself was not the sum of all forces: both Jupiter and the Moon seemed to be undergoing a secular acceleration, while Saturn was decelerating. A resisting medium might be made to explain the former, though not the latter; in order to clarify the effects of such a medium, in 1762 the Academy posed the question of *whether* the planets moved through a resisting medium; what they received and awarded prizes for was the *effects* of such a medium.

[13] Halley's comet

4.6 Johann Albrecht Euler

Euler's second son, Johann Albrecht Euler (1734-1800) entered with "Investigation Examining If the Planets Move Through a Medium Whose Resistance Produces Some Sensible Effect On Their Movement". Like his father, the younger Euler points to light as being proof that the interplanetary medium is far from empty:

> "Besides all other reasons, the sole one of light proves sufficiently, that all the regions of the sky are filled with the subtle matter of which the rays of light are made." (J. Euler [1762], 4)

Euler retains a vestigial Cartesianism, mentioned only in the introductory remarks and never referred to thereafter:

> "However, when one reflects on the cause of gravity, though it is unknown to us, it seems that one will find it only in the pressure of an extremely subtle fluid, which passes freely, even through the least pores of bodies." (J. Euler [1762], 9)

Euler then examines the effects of a resisting medium, whose force is proportional to the distance travelled. Euler eventually concludes:

> "Thus all the effect of the resistance of the ether on the movement of the planets reduces to the diminution of their periodic times, since we have seen that neither their eccentricities nor their aphelia undergo any sensible change." (J. Euler [1762], 43).

Even this shift is, according to Euler, barely noticeable. For example, Euler calculates that the eccentricity of the Earth's orbit will decrease by less than one part in two billion every year, and thus, even after thousands of revolutions, it will not be noticeable (J. Euler [1762], 37).

Perhaps the most interesting part of Euler's work is that he clearly considered an historic time scale, and not a geologic or astronomic one. This is not too surprising, since the antiquity of the Earth had not yet been established, however. Even Buffon's speculation that the Earth was as much as 75,000 years old was still 17 years in the future, and it would not be until after Lagrange and Laplace produced the bulk of their work that James Hutton's *Theory of the Earth* would be published (in 1785), which suggested that the Earth might be still older.[14]

The work of Euler won an award of Merit from the Academy.[15]

[14] The coincidence of this great revision in geological estimates of the age of the Earth and the proofs by Lagrange and Laplace of the essential stability of the solar system merits further investigation.

[15] The younger Euler's results agree completely with the elder Euler's results of 1746, in the *Opuscula varia* referred to earlier. One wonders how much of a hand Euler *pére* had a hand in the work of this Euler *fils*.

4.7 Abbé Bossut

The winning entry was by the Abbé Bossut, with "Investigations on the Alterations In the Mean Movement of the Planets That Could Be Caused By The Resistance of the Ether". Given the difficulty in making tables for the motion of the Moon, and especially Clairaut's 1749 discovery that it was neglecting supposedly negligible terms that led to the poor agreement between the prediction, based on universal gravitation, of the motion of the lunar apogee and observation, Bossut notes:

> "However, as the actual imperfection of Analysis does not permit resolving in all rigor the problems concerning the movement of the planets, and [from the solutions] one is obliged to change a little, after a certain time, the mean place of the planets, for making perfect accord [between] the observations [and] the tables constructed after Newtonian theory, one doubts that these slight alterations of the mean movements are uniquely due to the small quantity neglected in calculation, [thus] one might not be able to ignore the role of the resistance of the medium in which the planets move. It is for clarifying these doubts that the Royal Academy of Sciences of Paris asks today *if the planets move in a medium whose resistance produces some sensible effect in their movement.*"(Bossut [1762], 6, italics his.)

In other words, is the observed secular acceleration due to a non-gravitational cause, or is it a hitherto unsuspected consequence of universal gravitation?

It might seem from Bossut's statement that he intends to examine the work done on calculating the positions of the planets using universal gravitation, and see if taking additional terms into account would result in a secular acceleration (thereby avoiding the necessity of a resisting medium). However, Bossut does not do this. Rather, he examines the motion of bodies moving through a resisting medium.

Bossut obtains the equation:

$$ddz + zdx^2 - \frac{M + 2nMX}{ffmm}dx^2 = 0 \tag{G}$$

where z is the inverse of the radius, dx the change in the true anomaly, and M, m, f are related by:

$$m^2 = \frac{2M(f+c)}{f(2f+c)}$$

where f is the initial distance and m the initial velocity, M the mass of the sun, and c the distance betwen the foci (Bossut [1762], 10). Finally, n is a constant arising from the resistance of the interplanetary medium.[16] This is, according to

[16] Bossut lets the resistance K be given by:

$$K = nr^{\rho}\left(\frac{ds}{dt}\right)^2$$

Bossut, "the fundamental equation of the problem of the resistance of the ether to the movement of the planets." (Bossut [1762], 13). To solve this, Bossut

where r is the distance between the planet and the sun, and $\frac{ds}{dt}$ is the planet's velocity. (Bossut [1762], p. 11) introduces a reduction of order technique (Bossut [1762], 13).[17]

---

[17] Suppose one has the equation

$$ddz + Azdx^2 + Bx^p dx^2 + Cdx^2 = 0$$

where A, B and C are constants, and p is a positive integer. Let

$$Az + Bx^p = y$$

and thus, differentiating

$$Adz + pBx^{p-1}dx = dy$$

and

$$Addz + p(p-1)Bx^{p-1}ddx = ddy$$

This last equation is of the form

$$ddy + Dydx^2 + Ex^{p-2}dx^2 + Fdx^2 = 0$$

and so one can make the substitution

$$Dy + Ex^{p-2} = s$$

and continue, reducing the x term by two degrees every time. Eventually the x term will vanish, leaving

$$ddq + Gqdx^2 + Hdx^2 = 0$$

a differential equation that can be solved directly, as

$$dx = \frac{dq}{\sqrt{2L - Gqq - 2Hq}}$$

which can then be substituted back through the series of equations.

In the case at hand,

$$ddz + zdx^2 - \frac{dx^2}{f} + \frac{cdx^2}{2ff} - 2nxdx^2 = 0$$

one begins with

$$z - 2nx = q$$

and $G = 1, H = -(\frac{1}{f} - \frac{c}{2ff})$ and thus

$$dx = \frac{dq}{\sqrt{2L + (\frac{2}{f} - \frac{c}{ff})q - qq}}$$

where L is a constant of integration. To determine its value, note that at $x = 0$ (perihelion), $z = \frac{1}{f}$, $\frac{dz}{dx} = 0$ and thus $\frac{dq}{dx} = -2n$ by differentiatiation of $z - 2nx = q$ to obtain $dz - 2ndx = dq$. Thus, $2L = 4nn - \frac{1}{ff} + \frac{c}{f^3}$ and thus

$$dx = \frac{dq}{\sqrt{4nn - \frac{1}{ff} + \frac{c}{f^3} + (\frac{2}{f} - \frac{c}{ff})q - qq}}$$

This is integrable as:

$$q = \left(\frac{1}{f} - \frac{c}{2ff}\right) - \sqrt{4nn + \frac{cc}{4f^4}}\cos(x + C)$$

where C is a constant of integration. Once again, at $x = 0$, $z = \frac{1}{f}$ and thus

$$\cos C = \frac{-c}{\sqrt{16n^2f^4 + c^2}}$$

and

$$\sin C = \frac{4nff}{\sqrt{16n^2f^4 + cc}}$$

Thus, one can rewrite $\cos(x + C)$ in terms of $\sin x, \cos x, \cos C$, and $\sin C$ to obtain:

$$z = 2nx + (\frac{1}{f} - \frac{c}{2ff}) + \frac{c}{2ff}\cos x - 2n\sin x$$

Bossut arrives at the equation for r[18]:

$$r = f + \frac{c}{2} - 2nffx - \frac{c}{2}\cos x + 2nff\sin x$$

Clearly, as x increases, the distance r must decrease without end, the logical prediction of a slight resistance of the ether.

To determine the position of perihelion, one must find the point where $\frac{dr}{dx} = 0$. Differentiating, one obtains

$$\frac{dr}{dx} = -2nff + \frac{c}{2}\sin x + 2nff\cos x$$

which has solutions at $x = 0, 360°, 2 \cdot 360°, \ldots$. Thus, like Euler, he concludes, "if this point [perihelion] has some movement, the resistance of the matter of the ether is known not to be the cause." (Bossut [1762], 16) Bossut is implying that some other force is responsible for causing any progression of the perihelion; later in his paper, he attributes all remaining inequalities of the planets to their mutual gravitational interaction.[19]

To bolster support for the existence of such a secular acceleration, he notes:

> "All the observations of Ptolemy indicate an acceleration in the mean movement of the Earth. According to this astronomer, the mean tropical year is 365 days, 5 hours, 55 minutes, compared with the best modern observations, [in which] it is simply 365 days, 5 hours, 48 minutes and 48 to 50 seconds." (Bossut [1762], 47)

Bossut's citation of Ptolemy is remarkable, for many astronomers were then of the opinion that his observations, being so disparate from the observations of other astronomers, were not to be trusted (cf. Lalande [1757]). However:

> "The alterations of the mean movement of the Moon are more exactly determined than those of the movement of the principal planets.
>
> "It is indubitable that the mean movement of the Moon is accelerated in a sensible manner. This acceleration is demonstrated completely by comparison of the intervals of eclipses observed more than twenty five centuries ago with those which occurred a thousand years ago and with those we observe at the present..." (Bossut [1762], 48-49)

The dates Bossut gives — a thousand years ago, and twenty five centuries ago — are precisely those given to Dunthorne; there is little doubt that Bossut

[18]Obtained by inverting the previous equation for z, and expanding to first order

[19]For example, as Clairaut demonstrated, the motion of the apogee of the Moon could very nearly be determined by the action of the sun. Any remaining inequalities, Bossut is certain, must be caused by mutual gravitation. In particular, Bossut seems to be discarding the possibility — so cherished by Euler — that the inverse square law is an insufficiently accurate approximation to determine the motion of the Moon.

here is using Dunthorne's figures. Bossut then gives justification for suspecting a resisting medium exists in the first place, even without direct proof of its existence:

> "1st. In the formula that the geometers have derived from the principle of attraction for determining the place of the Moon, there are no terms which produce an acceleration of the mean movement. This acceleration can not be any more attributed to the passing attraction of some comets, since it is constant and that it is in consequence produced by a force [that is] always present and always acting. Therefore it appears until now inexplicable by the principle of attraction." (Bossut [1762], 49)

As with the Eulers, Bossut cites the "matter" of light:

> "2nd. The existence of a material ether in the celestial spaces is not doubtful; because even when one refuses to admit an atmosphere around the sun, such as that which surrounds the Earth, there remains always in the sky the fluid which forms light. Now it is impossible to conceive of any fluid, however rare one wants to suppose [that] does not pose some resistance to the movement of bodies which traverse it." (Bossut [1762], 49)

It is important to note that by "ether", Bossut means *any* substance in the interplanetary space: if not the atmosphere of the sun and planets, then the matter of light, as Euler himself suggested.

Bossut ends in utter certainty about the source of the secular acceleration of the Moon and the Earth:

> "There results from these preceding researches a type of supplement and confirmation of the system of universal gravitation. The acceleration of the mean movement of the Moon, until now *inexplicable* by attraction, appears to be an effect of the resistance of the ether: the other inequalities of the Moon, inexplicable by Cartesian vortices and by the resistance of the ether, are produced by attraction. The acceleration of the mean movement of the Earth, which is akin to that of the mean movement of the Moon, seems to be indicated by observation." (Bossut [1762], 54)

As for the other planets, particularly Jupiter and Saturn, Bossut returns to an idea of Newton's:[20]

> "The action of the ether on the movements of Jupiter and Saturn can not become sensible until after a long period of ages. Thus if

[20] In Book III, Proposition X, Theorem X, cited above, where Newton argues that the resistance of an interplanetary medium would not slow Jupiter one part in a million after a million years.

> the alteration of the mean movements of these two planets are such as some astronomers imagine, they are produced by attraction." (Bossut [1762], 54)

The essays of Bossut and Euler actually marked the end of the first part of the stability problem. Though the resistance of an ether might explain, adequately, the secular acceleration of the Moon, the behavior of the other planets, especially Saturn, did not support the existence of a resisting ether. As methods of celestial mechanics improved, more and more of the remaining inequalities in the motions of the planets could be accounted for by gravitational attraction alone, and the ether, as a material substance, dropped further into insignificance.

In fact, the 1762 essays would be the last time that the resisting ether would play a major role in the determination of long term stability during the classical era. Both Laplace and Lagrange will dismiss its effects as insignificant, and neither will seriously suggest it as a solution to an unresolved discrepancy in the motions of the planets.

Chapter 5

Resisting the Medium

Though both Bossut and the younger Euler based their work on the assumption of a resistant, interplanetary medium, and the elder Euler found it consonant with his theological beliefs, actual evidence for such a medium was virtually non-existent. Aside from the zodiacal glow[1], the arguable acceleration of the Moon[2] and the very dubious observations of Ptolemy[3], there was only one solid indication of a resisting medium: the secular acceleration of Jupiter. This was balanced by the secular *deceleration* of Saturn, which could not be explained by a resisting ether. This lack of observational evidence supporting a resisting ether led mathematicians to re-examine the possibility that gravitational perturbations were the cause of the various remaining inequalities in the motions of the planets.

5.1 Newton

Initially, Newton was unconcerned with the effects of gravitational perturbation. In the *Principia*, Book III, he considers the effects of gravitational interaction to be so small they may be ignored:

> "Proposition 14, Theorem 14: The aphelions and nodes of the orbits of the planets are fixed.
>
> "The aphelions are immovable by Proposition 11, Book I; and so are the planes of the orbits, by proposition 1 of the same book. And if the planes are fixed, the nodes must be so too. It is true that some inequalities may arise from the mutual actions of the planets and comets in their revolutions, but these will be so small that they may be here passed by." (Newton [1726], 287)

[1] Which Cassini used to argue for the existence of an enormously extended solar atmosphere.
[2] Which Lagrange will later claim to be an artifact of imperfect observation.
[3] Which were accepted as sufficient proof by some astronomers, but rejected by most others.

Newton amplifies upon the effects of mutual action in the Scholium following Proposition 14, Theorem 14:

> "Since the planets near the sun (viz., Mercury, Venus, the earth, and Mars) are so small that they can act with but little force upon one another, therefore their aphelions and nodes must be fixed, except so far as they are disturbed by the actions of Jupiter and Saturn, and other higher bodies. And hence we may find, by the theory of gravity, that their aphelions move forwards a little, in respect of the fixed stars, and that as the $^3/_2$th power of their several distances from the sun. So that if the aphelion of Mars, in the space of a hundred years, is carried forwards $33'20''$, in respect of the fixed stars, the aphelions of the earth, of Venus, and of Mercury, will in a hundred years be carried forwards $17'40'', 10'53''$, and $4'16''$ respectively. But these motions are so inconsiderable, that we have neglected them in this Proposition." (Newton [1726], 287)

In the *Principia*, as we have seen, Newton's only concern is that a resisting medium might slow the motions of the planets, though he dismisses this readily as insignificant.

By the time he published *Optics*, however, Newton has changed his mind. At the end of *Optics*, he appends a list of Queries, which were made "in order to [suggest] a further search to be made by others." (Newton [1730], 339) In the first edition of *Optics*, published in 1706, Newton states in the last and longest query, number 31:

> "For while comets move in very eccentric orbs in all manner of positions, blind fate could never make all the planets move one and the same way in orbs concentric, some inconsiderable irregularities excepted which may have risen from the mutual actions of comets and planets upon one another, and which will be apt to increase, till this system wants a reformation." (Alexander [1956], 180)

This is Newton's first public statement that the possibility existed that the mutual gravitational interactions of the planets could affect each other's orbits to the point of instability.[4]

5.2 Flamsteed

What caused Newton, so casual about gravitational influences in the *Principia*, to be worried about them in the *Optics*? One possible source of Newton's disquiet may have occurred due to his attempts to determine the motion of Saturn. As John Flamsteed, first Astronomer Royal at Greenwich, reports to a Mr. Bossley on January 11, 1699:

[4]It is interesting to note as well that Newton is *certain* that the system will "want a reformation". Undoubtedly this has to do with Newton's theology: the very fact that the solar system is so orderly now implies the intervention of some sort of "Divine Counsel".

> "I gave Mr. Newton, about 4 years ago, the same places of Saturn and Jupiter I gave you on our first acquaintance. He tells me he had tried to answer those of Saturn, and found he could do it nearly by only liberating his aphelion ..." (Baily, p. 169-170.)

This sounds as if Newton could compute the place of Saturn only by assuming its aphelion mobile, as opposed to the immobility suggested in Book III, Theorem 14, Proposition 14 of the *Principia*; the method is suggestive of what Clairaut would eventually derive for his theory of the Moon.[5] Newton adds that the place of its aphelion "liberates very oddly". It would be interesting to see what Newton meant by this; however, he never published the relevant works.

Some of Flamsteed's communications with Sharp also discuss the irregularities in the motions of the planets, inequalities that a Newtonian would hope would be accounted for by universal gravitation.

Sharp's letters to Flamsteed began sometime after his return to Yorkshire after a brief absence (1694) until Flamsteed's death in 1719; Sharp continued to correspond with Joseph Crosthwait, Flamsteed's assistant, until his own death in 1730 (Cudworth [1889], 34). While Sharp retained copies of his letters, they were written in an archaic form of shorthand and made lavish use of his own abbreviations; only a few have been deciphered (Cudworth [1889], 35-6). As a result, few of Sharp's letters are available, and their content must be inferred from Flamsteed's replies.

Sharp apparently wrote a letter (now lost) to Flamsteed on January 8, 1708, which concerned the perturbation of the planets and possibly raised questions about the cumulative effects of their mutual interactions. Flamsteed's reply, on March 2, 1708, says:

> "Your thoughts concerning the restitution of the planets' motions are just: they act one on another, and since their actions are as the squares of their distances reciprocally, it will be difficult, and require a great deal of consideration, to disentangle them, and find what the effects of their actions have been in several ages. For certainly they must cause secular inequalities in the superiors: and though the inferiors, being less in bulk, cannot have so great effects on each other, yet approaching each other much nearer, their effects must be sensible and perplexed. I think I feel them both in Mars and Venus; and then our Earth, that moves betwixt them, must be involved with the same.

[5] Recall that Clairaut started by assuming that the perturbed orbit

$$\frac{1}{r} = \frac{1}{p} - \frac{e}{p}\cos v + \Delta$$

could be expressed as

$$\frac{1}{r} = \frac{1}{k} - \frac{e}{k}\cos mv$$

where m is a constant to be determined.

> "And for [Jupiter's] satellites, I doubt not but their motions are all liable to inequalities; but my age and infirmities suffer me not to examine them as I would. All I can do is, to lay in a good stock of observations, as I have done, for the primary planets; whereby posterity may be enabled to proceed where I am forced to leave off. . . "(Baily [1966], 266)

Flamsteed does not use the term "secular" in the modern sense of non-periodic variations. Rather, Flamsteed means long period variations, as the following letter, dated July 14, 1710, indicates:

> "Since my last to you I have been frequently employed in correcting the motions of Saturn, Jupiter, and Mars: as for Saturn, I find I cannot make any numbers, as yet, that will represent all my own observations nearly; but they will be too slow in Bernard Walter's by 10 or 15 minutes, and about 8 minutes, in Hevelius, of time too fast; which intimates a secular intention and remission of his mean motion, and another inequality arising from his position in respect of Jupiter." (Baily [1966], 276)

Flamsteed is certain that gravitation is the sole cause of the variations in the motions of the planet that he observes, and not some other cause, such as the resistance of the ether. In his letter of September 20, 1710, to Sharp he writes:

> "On the whole, I plainly perceive there is a cause that vitiates their motions; and it seems to be their mutual gravitations on each other, and pretty regular. So that I do not doubt but to solve them: for, their gravitations being reciprocally as the squares of their intermutual distances, and directly as the bulk of their bodies, it will not be difficult to determine how much they draw the remoter planet inward towards the sun, or force the nearer from him." (Baily [1966], 277)

Flamsteed's "it will not be difficult" is, of course, a great understatement, as the solution of the problem will take over half a century and no fewer than four prize-winning essays from the French Academy of the Sciences.

5.3 Bentley

Even before Flamsteed, however, Newton communicated with Reverend Richard Bentley (1662-1742), who may have been the first person to suggest one of the dire possibilities of universal gravitation: the planets, by their own actions, might disturb their orbits. Bentley wrote to Newton sometime before late 1692, apparently asking about many of the topics of the *Principia*. It was Bentley's intent to gather "scientific" proof of the existence of God, to incorporate in a series of lectures he was giving; Bentley wanted to clarify certain points with

Newton. Certainly Newton was an apt choice for such support; as he makes clear in the *Principia*, he feels the orderly structure of the universe necessarily requires the existence of a Deity (see above). Indeed, as Newton tells Bentley, one of the reasons for writing the *Principia* was to use it as a vehicle for demonstrating the existence of God. On December 10, 1692, he writes:

> "When I wrote my treatise about our Systeme I had an eye upon such Principles as might work with considering men for the beleife of a Deity & nothing can rejoyce me more then to find it usefull for that purpose." (Turnbull [1959], v. III, 233)

This was the first of four letters to pass from Newton to Bentley, which were instrumental in helping Bentley to give a series of sermons entitled "A Confutation of Atheism" (Turnbull [1959], v. III, 236). The letters deal with many things, mostly showing the providence, planning, or necessity of a God. For example, Bentley's sermon on December 6, 1692, the eighth in the series of "A Confutation of Atheism", begins with a statement reminiscent of the General Scholium, but Bentley continues the argument even beyond what Newton had suggested:

> "Such an apt and regular harmony, such an admirable order and beauty, must deservedly be ascribed to divine art and conduct: especially if we consider that the smallest planets are situated nearest the sun and each other; whereas Jupiter and Saturn, that are vastly greater than the rest, and have many satellites about them, are wisely removed to the extreme regions of the system, and placed at an immense distance one from the other. For even now, at this wide interval, they are observed in their conjunctions to disturb one another's motions a little by their gravitating powers: but if such vast masses of matter had been situated much nearer to the sun, or to each other, (as they might as easily have been, for any mechanical or fortuitous agent,) they must necessarily have caused a considerable disturbance and disorder in the whole system." (Bentley [1836], 180)

Here is the earliest written suggestion that merely by the power of their own gravitational attraction, the planets might be able to disrupt the harmony and order of the solar system.[6]

Whether he realizes it or not, Colin Maclaurin, in his spirited (albeit somewhat unnecessary) defense of Newtonianism against Cartesianism, gives an argument for the stability of the bodies in the solar system almost identical to that of Bentley:

> "If the great planets, *Jupiter* and *Saturn*, had moved in the lower spheres, their influence would have had much more effect to disturb

[6] Unfortunately, as the original letters from Bentley to Newton have been lost, it is difficult to say whether or not Bentley aired this idea directly to Newton.

> the planetary motions. But while they revolve at so great distances from the rest, they act almost equally on the sun and on the inferior planets, and have the less effect on their motions about the sun, and the motions of their satellites are at the same time less disturbed by the action of the sun." (Maclaurin [1748], 304)

An important aspect of Bentley's argument is that the universe must have been created at some point in time. The key to the argument is this: in the minds of many, the creation of the universe necessarily gives rise to the question of how it was created – and suggests the existence of a Creator. If one assumes that the universe was not created at some point in time, however, one can sidestep the existence of a Creator by denying the existence of a creation. Therefore, it is important to Bentley (and Newton) to prove that the solar system *was* created at some particular time in the past. Euler will give one argument, based on the existence of a secular acceleration of the Earth's orbit. Bentley gives another:

> "For let them [atheists] assign any given time, yt Matter convened from a Chaos into our System, they must affirm yt before ye given time matter gravitated eternally without convening, which is absurd." (Turnbull [1959], v. III, 249)

Newton replies (February 25, 1693) to Bentley's letter, and extends it:

> "And tho all ye matter were at first divided into several systems & every system by a divine power constituted like our's: yet would the outward systemes descend towards ye middle most so yt this frame of things could not always subsist without a divine power to conserve it. Which is your second Argument, & to your third I fully assent." (Turnbull [1959], v. III, 255)

Maclaurin also furnishes arguments against the eternal prior existence of the solar system, using the comets. He believed (as seen above) that the comets, passing through the solar atmosphere, were retarded in their motions and eventually fell into the sun, providing fuel for its fire. However:

> "The argument against the eternity of the universe, drawn from the decay of the sun, still subsists; and even acquires a new force from this theory of the comets: since the supply which they afford must have been long ago exhausted, if the world had existed from eternity. The matter in the comets themselves, that supplies the vapour which rises from them in every revolution to the perihelium, and forms their tails, must also have been exhausted long ere now. In general, all quantities that must be supposed to decrease or increase continually are repugnant to the eternity of the world; since the first had been exhausted, and the last had grown into an infinite magnitude, at this time, if the world had been from eternity: and of both kinds there seems to be several sorts of quantities in the universe." (Maclaurin [1748], 376)

Maclaurin points out (as Euler did) that this implies not only an end, but a beginning as well:

> "It appears, however, not to have been his [God's] intention, that the present state of things should continue for ever without alteration; not only from what passes in the moral world, but from the phaenomena of the material world likewise; as it is evident that it could not have continued in its present state from eternity." (Maclaurin [1748], 387)

Hence for Bentley, Newton, and Maclaurin, and Euler later, the existence of a secular variation which would limit the permanence of the solar system was not only permissible, but even desirable, for it furnished proof of the existence of God.

5.4 Leibniz and Clarke

Responding to Newton's suggestion that the system might want a "reformation", Leibniz criticizes Newton and his followers for imagining a God so slipshod that He would have to intervene and fix His work from time to time. In a series of letters exchanged between Leibniz and Samuel Clarke, Leibniz expresses his dissatisfaction with Newton's system. In the very first letter, dated November, 1715, Leibniz writes:

> "4. Sir Isaac Newton, and his followers, have also a very odd opinion concerning the work of God. According to their doctrine, God Almighty wants to wind up his watch from time to time: otherwise it would cease to move. He had not, it seems, sufficient foresight to make it a perpetual motion." (Alexander [1956], 11)

Clarke makes a weak argument that perfection in a human workman (whose device would run without interference) is different from that of God (with whom, as the source of all things, "nothing is done without his continual government and inspection.") (Alexander [1956], 14) Leibniz, of course, objects to this and strengthens his statement:

> "A true providence of God, requires a perfect foresight. But then it requires moreover, not only that he should have foreseen every thing; but also that he should have provided for every thing before-hand, with proper remedies: otherwise, he must want either wisdom to foresee things, or power to provide against them. He will be like the God of the Socinians, who lives only from day to day, as Mr. Jurieu says. Indeed God, according to the Socinians, does not so much as foresee inconveniences; whereas, the gentleman I am arguing with, who put him upon mending his work, say only, that he does not provide against them. But this seems to me to be still a very great

> imperfection. According to this doctrine, God must want either power, or good will." (Alexander [1956], 19)

Leibniz repeats this in a letter to Conti, around December, 1715:

> "As to the dynamics or the doctrine of forces, I am astonished that Newton and his followers believe that God has made his machine so badly that unless he affects it by some extraordinary means, the watch will very soon cease to go." (Alexander [1956], 185)

Unfortunately, the necessary tools to answer this question of the gravitational disruption of the solar system were not at hand. In England, mathematicians acknowledged the correct physical theory, but were hampered (except for Simpson) by a difficult notation and complicated, geometric proofs. In continental Europe, mathematicians utilized better notation and procedures, but for the first half of the century, were laboring under an incorrect physical theory. In addition, the calculus of trigonometric functions was but poorly understood, at least until Euler worked out the theory behind their variations.

5.5 Jacques Cassini

However, by 1740, the climate was changing, and Newtonian mechanics were gradually being accepted in continental Europe. One particularly interesting study is Cassini, who began life as a neo-Cartesian, but around 1745 changed his mind and became a convert to the Newtonian system. In the introduction to his *Elements of Astronomy* (1740), considered by Lalande to be one of the principal works on physical astronomy (Lalande [1792], lvi), he indicated a number of areas where research in physical astronomy has yet to yield definitive answers. Cassini at first notes:

> "...but also I do not presume that it [astronomy] has been carried to the highest perfection to which it can aspire." (Cassini [1740], viii)

After this, he raises a number of unsolved questions in astronomy.

> "But in addition to the variations which can be produced by the reciprocal action of the bodies on one another, can there not be some other natural cause of these variations, of which we have no knowledge?" (Cassini [1740], viii-ix)

Even putting aside "unknown" forces, though:

> "Without finding other causes of the derangement of the course of the heavenly bodies, we can be assured they are subject to, as one recognizes, different inequalities, [so will] they always persevere in

> the same degree of movement, without alteration after the passage of centuries?
>
> "It is true that most of the ancient observations compare with our own, [and] seem to support this sentiment, which is generally accepted by all astronomers, and we should not depart from it without having good, complete proof to the contrary; however many other observations appear to not be in accordance [with the notion of the invariance of the mean motions of the planets]." (Cassini [1740], ix-x)

Though Cassini nowhere mentions universal gravitation, his talk of "reciprocal action", coupled with the fact that, within a few years, he will be an ardent convert to Newtonianism, makes it seem certain that what Cassini speaks of here is nothing but the derangement of the orbits of the planets by their mutual gravitational effects. In effect, he has resurrected Newton's Query Number 31.

Cassini adds in the other elements of orbit aside from just the mean motion (which depends only on the major axis):

> "...can one be assured that the orbits that the planets describe always retain the same figure and the same eccentricity, and that they always have the same inclination with regard to one another ...?" (Cassini [1740], x-xi)

A special case of the inclination of the orbits is that of the ecliptic (the plane of the Earth's orbit):

> "Likewise, the ecliptic, the plane on which the movement of the sun or the Earth is made, has it always had the same inclination with respect to the [solar] equator?" (Cassini [1740], xi)

This is the first clear statement of the nature of the problem: there is no force that *requires* the planets' major axes, or their eccentricities, or their inclinations to remain within certain limits, even if the resisting ether is ignored. The reciprocal action of the planets *alone* might be enough to disrupt their orbits, and whether or not this will occur is a problem to be investigated by physical astronomers.

After the motion of the Moon's apogee was explained by Clairaut, there remained the irregularities in the motions of Jupiter and Saturn. If universal gravitation by means of the inverse square law alone could explain these, then no further assumptions were necessary. But could it?

On April 27, 1746, Cassini read to the Academy "On The Two Conjunctions of Mars With Saturn, Which Occurred in 1745, With Some conjectures on the Causes of Observed Inequalities in the Movements of Saturn and Jupiter" (op. cit. Wilson [1985). Once a Cartesian, Cassini shows an admirably Newtonian outlook in suggesting that:

> "...since in their conjunction where they find themselves at the same time in opposition to the sun, they are as close as possible to one another, their mutual actions need, according to the hypothesis of gravitation, cause some variation in their movement and in their distance, and it is necessary for the perfection of their theory to be able to find that quantity." (Cassini [1746], 466)

Thus, Cassini is advocating the use of universal gravitation alone to predict the positions of the planets. Moreover, he recognizes that it is only by providing a correct determination of the effects of their mutual attraction that the theory reach "perfection". He continues, expressing great (and premature) confidence in the ability of universal gravitation to account for planetary inequalities:

> "The inequalities of the movement of the Moon, caused by these different aspects with regard to the sun, are very considerable for having been perceived for a long time by astronomers; and M. Newton has shown that they conform to the law of gravitation." (Cassini [1746], 466)

Recall in 1746 the motion of the apogee of the Moon was still unexplained, so Cassini's statement that Newton demonstrated their conformity to the law of gravitation is premature, though it shows great faith in the system itself.

Cassini notes that Saturn's mean motion could be reconciled with the ancient observations by assuming it was decelerating at the rate of $2'$ per century (Cassini [1746], 473) and Jupiter's was accelerating at the rate of $50''$ per century (Cassini [1746], 474). Was a resisting interplanetary medium necessary to explain these changes? Cassini discounts the notion (erroneously) by arguing that, while it was reasonable to believe that a planet could slow by moving through a resisting medium (the case of Saturn), he could not see how it might accelerate (the case of Jupiter). Though this argument is wrong in its application (motion through a resisting medium would cause a secular acceleration as the planets spiralled closer to the sun, increasing their mean motions), Cassini did hit upon an important point: if one pointed to a "global" cause for the acceleration of Jupiter and the Moon, how could one explain the opposite effect observed in the motion of Saturn?

In any event, Cassini does not feel an additional assumption is necessary:

> "However, as one can have difficulty admitting this hypothesis, if it is not supported by evidence which has at least some reasonableness, I have examined if this variation in the movement of these planets, can be explained by the law of gravitation, which, as M. Newton has shown, accords with the movements of the planets, whatever the cause of this gravitation." (Cassini [1746], 474-5).[7]

[7] Note Cassini's "whatever the cause of this gravitation": this is a very Newtonian viewpoint, and very much in line with post-Cartesian science. Cassini's inclusion of such a proviso could be interpreted to indicate that he was concerned about resistance to the idea of explaining astronomical phenomena by Newtonian gravitation.

Thus, in Cassini's viewpoint, *unless* universal gravitation is unable to explain the variations in the motions of Jupiter and Saturn, an additional hypothesis should not be introduced.

Despite this Newtonian outlook, Cassini's arguments remain, essentially, non-mathematical, and reminiscent of his earlier, Cartesian arguments (op. cit. Wilson [1985]). Like Newton's lunar theory, Cassini's table of the acceleration and decelerations of the motion of Jupiter and Saturn at different (angular) distances to their conjunction appears from nowhere, without explanation of how the values were determined (Cassini [1746], 482)

5.6 Euler

On April 30, 1746, Le Monnier (*fils*) read a paper before the Academy that definitively established the inequalities in the motions of Jupiter and Saturn. The Academy thought the problem was of such importance that the next year, they announced that the 1748 prize would be:

> "A theory of Saturn and Jupiter, by which one can explain the inequalities that these two planets appear to cause each other mutually, especially at the time of their conjunction." (Euler [1749], 45)

Euler's entry, "Recherches sur la Question des Inegalites du Mouvement de Saturne et de Jupiter", won the prize and was subsequently published, with his tables for Jupiter and Saturn, in 1749. By this time, the Academy is solidly Newtonian: the inequalities in the motions of the planets *must* be caused by mutual attraction: the suggestion of a resisting ether is not even raised by Euler.

> "Now, first of all, there is no doubt that the Royal Academy has in mind the theory of Newton, founded on universal gravitation, which has worked thus far so admirably well in accordance with all celestial movements, [and] whatever the inequalities which are found in the movement of the planets one can always boldly claim that the mutual attraction of the planets is the cause." (Euler, [1749], 45)

That is to say, universal gravitation must be the source of the irregularities. However, there are still problems with universal gravitation. (Recall that in 1748, the sufficiency of the inverse square law to explain the motion of the Moon's apse was being called into question by Clairaut, so Euler's classification of Newtonian gravitation works "admirably well" is somewhat premature) Although he does not appear to question the inverse square law for infinitesimal bodies, Euler several times raises the question of whether it can be profitably applied to real bodies, and before relenting to this simplification, he devotes two pages of his work to his thoughts on the insufficiency of the inverse square law. His doubts center around the problem of the Moon:

> "Having compared very meticulous observations of the Moon with this theory, I have found that the distance of the Moon to the Earth is not so great as it needs to be according to the theory; it follows that the gravity of the Moon to Earth is a little less than that according to the inverse square of the distance: and some small irregularities in the movement of the Moon, which are not explained by this theory, have further confirmed me in this sentiment." (Euler, [1749], 49)

Euler's greatest support for the planets not operating in accordance with a simple, inverse-square law is the nonsphericity of the planets:

> "The greatest geometers of this century have shown that the gravity toward a spheroid is neither constantly directed toward its center, nor proportional to the inverse square of the distance. Thus, since the body of Jupiter is the most oblate of all the planets, its attractive force can differ considerably by this established reason, and the force of the sun similarly could vary a little." (Euler, [1749], 49-50)

Euler will continue to argue that the inverse square law is too simple to fully explain the motions of the planets for many years. In the case at hand Euler notes that, especially in the case of Saturn:

> "It seems to me, thus, very likely that the forces that govern the movement of Saturn, by reason of its bizarre figure, differ rather considerably from the general law." (Euler [1749], 50)

What does Euler mean by this? In the previous sentences, he discussed nutation[8] and other effects that occur when the gravitational force does not act on the center of the body, so it seems likely that Euler is postulating that Saturn could likewise undergo some sort of irregular motion, due to its ring system (its "bizarre figure").

However, Euler finally agrees to accept the inverse square law, for the duration of the problem at least:

> "Notwithstanding the reservations I have on the irregularities of the forces which act on Saturn, I follow in my theoretical researches exactly the system of attraction which is today adopted by all astronomers; and I suppose that the forces, from the sun and from the planets, decrease precisely in the ratio of the [inverse] square of the distance, and they act on the center of gravity of the body to which they are drawn." (Euler, [1749], 50)

[8] If the Earth's axis is extended to an imaginary celestial sphere, the axis traces out a circle over a period of about 23,000 years, a phenomenon known as precession, which is itself caused by the gravitational action of the Moon on Earth's equatorial bulge, a fact first demonstrated by d'Alembert. However, the circle is not quite smooth; the axis "bobs" as it prcesses. This bobbing is referred to as nutation

Thus, despite his misgivings, Euler intends to use the simple, inverse square law of attraction to determine, analytically, the variations in Jupiter and Saturn. His method is one of successive approximation:

> "As it is very wearying to surmount these difficulties all at once, I pass [to them] by degree; and I will suppose first the two orbits, always in the same plane, lack eccentricity; that is to say, the orbit of Jupiter will be circular, and that of Saturn will be as well, [as] if it is drawn only by the force of the sun. It is clear that this assumption will furnish us a type of inequality in the movement of Saturn, which will be similar to that in the Moon that astronomers call *Variation*. In the second place, leaving the two orbits in the same plane, and that of Jupiter circular, I will regard the eccentricity of Saturn, and I will determine the new inequalities which result. One will perhaps be surprised, to see that these surpass considerably those of the first hypothesis. In the third place, I will suppose the orbit of Jupiter eccentric, such as it is in fact, and this circumstances will furnish again new inequalities, which likewise will be most considerable. Finally, I consider the mutual inclination of the two orbits, and I will draw on it to determine all the successive changes, with the movement of the line of the nodes which result." (Euler [1749], 47-48)

The method used by Euler in [1749] differs considerably from Clairaut and Simpson. Whereas Clairaut and Simpson used an iterative procedure to produce successively better approximations of the same differential equation, Euler obtains solutions to each of the problems (coplanar, circular orbits; coplanar, one circular and one eccentric orbit; coplanar, eccentric orbits; and non-coplanar, eccentric orbits) and uses that solution as the approximation to the next problem. In essence, Euler is solving four smaller problems instead of large one, and treating each successive problem as a perturbation of the previous one.

A very brief summary of Euler's method follows. Euler first assumes the two planets are concentric and coplanar. Then, assuming that Saturn does not perceptibly influence the motion of Jupiter, he assumes the distance of Saturn to the sun is given by $z = f(1+nr)$, where f is the mean distance, nr a very small fraction, and r depends only on the angle of elongation ω between Jupiter and Saturn, as seen from the sun (Euler [1749], 59). By reducing terms involving r into infinite trigonometric series, and comparing coefficients, Euler is able to determine the form of r (Euler [1749], 69). This process is repeated for the case where the orbit of Saturn is eccentric, so for the second approximation, he assumes $z = f(1 + k\cos q + nr)$, where $z = f(1 + k\cos q)$ would give the unperturbed orbit (Euler [1749], 74). Repeating the process, he determines the quantity nr. This process is repeated assuming the orbit of Jupiter is eccentric, assuming Saturn's is circular (Euler [1749], 88). This method is essentially the same as the one he introduced in [1747], particularly with respect to his solution by means of infinite trigonometric series.[9]

[9]Euler pioneered the use of such series in this Memoir, according to Katz, though Euler introduced a finite series approximation in [1747], and the use of infinite series approximations

In the first two cases, Euler finds no secular variations in the motion of the planets. However, in the third case he came across the first indication that there might be a problem with the long term stability of the solar system. In the expansion of solution for r, the variation of the distance between Saturn and the sun (recall that the distance was given by $z = f(1 + k\cos q + nr)$, so the mean distance was f), Euler obtains a term of the form

$$T'' ep \sin(\omega - p)$$

where p is the eccentric anomaly and ω is the angular distance between Jupiter and Saturn. This is a serious problem:

> "75. Thus if this term $T'' ep \sin(\omega - p)$ enters in the expression of r, the inequality which results, increases each revolution, since after any of these revolutions, the value of p is augmented by 360 degrees: and however small the value of $T''e$, it must, in time, arrive absolutely, that the value of this inequality surpasses any given quantity: and as it is sometimes added to the distance of Saturn to the sun, and sometimes subtracted, it can happen that in the same period, Saturn approaches the sun, and then moves away to a distance incomparably greater than that which it draws away at present." (Euler [1749], 97)

This would seem to cast grave doubts on the stability of the solar system. However, this is not one of Euler's concerns; indeed, at the same time, Euler is expressing certainty that the solar system must come to an end (see above). In any, case, Euler is not seriously concerned with this problem, dismissing it readily:

> "However, one sees very well that as one such derangement becomes considerable, the approximation I am using here is no longer of any use, and I believe thus that the determination of one such movement surpasses the bounds of our intent." (Euler [1749], 97)

In other words, before the variation becomes too great, the approximation Euler used to obtain it in the first place will no longer be valid, and thus the expression will likewise be suspect; this is similar to the method Johann Albrecht Euler would suggest in [1759]. Such a "continuation" method, unfortunately, does not allow for prediction of long term behavior.

The Academy offered the same prize question in 1752, and again Euler won. In the post-Clairaut era, questioning the validity of the inverse square law had dubious merit; in fact, Euler seems convinced that not only was the inverse square law essentially correct, but he evidences a switch to the mathematical school:

---

is implicit in the earlier article.

> "But since M. Clairaut made the important discovery, that the movement of the apogee of the Moon is perfectly in accordance with the hypothesis of Newton on the law of attraction, there remains no doubt on the generality of this proportion; and since this same proportion is exactly suitable for all the movement of the Moon, despite all the objections that one has a reason to make, one can now state boldly that the two planets of Jupiter and Saturn attract each other mutually by the inverse square of their distances, and that all the irregularities which one can discover in their movement are infallibly caused by this mutual attraction. Therefore we see the true cause of all these derangements of whatever nature they can be; and if the calculations one supposes to have drawn from this theory are not very well in accordance with observations, one will be always correct to doubt rather the accuracy of the calculations, than the truth of the theory." (Euler [1752], 4-5)

By this, Euler now places himself in the mathematical school: any remaining discrepancies must be due to inaccuracy in the *calculations*, rather than in the physical theory. As he did in [1749], Euler discovered a serious variation in [1752], one that threatened the stability of the solar system.

Euler begins by supposing the distances x and y between the sun and Jupiter and Saturn, respectively, are given by:

$$x = c(1+u), y = e(1+v)$$

and then that u and v are given by:

$$u = bl\cos(\omega + s) + A\cos\omega + C\cos 3\omega + \dots$$

$$v = l\cos s + A'\cos\omega + C'\cos 3\omega + M'l\cos(\omega + s) + \dots$$

where ω is the angle between Jupiter and Saturn (as observed from the sun), $A, C, \dots$ and $A', C', M', \dots$ are constants to be determined, and $ds = \lambda d\omega$, where λ is a constant (Euler [1752], 49). Substituting these into the differential equations of motion, Euler then attempts to solve for the coefficients. The coefficient of the $l\cos(\omega + s)$ term gives rise to an equation in b, μ, ν (where μ and ν are the masses of Jupiter and Saturn, relative to the sun). Since μ and ν are known, b may be obtained.

However, the best values for μ and ν available to Euler were $\frac{1}{1067}$ and $\frac{1}{3021}$, respectively. If these are substituted in, b has imaginary roots:

> "Since the value of b effectively becomes imaginary, when $\mu = \frac{1}{1067}, \nu = \frac{1}{3021}$, in order to avoid imaginary angles, which reduce to real exponential quantities, I have slightly changed the values of μ and ν, [hence] it follows that if these values of μ and ν are correct,[10] the variations that I have developed, never return to the same state, but will go to infinity" (Euler [1752], 76-7).[11]

[10] I.e., the original values of $\mu = \frac{1}{1067}, \nu = \frac{1}{3021}$.

[11] Modern values for μ and ν are $\frac{1}{1038}$ and $\frac{1}{3467}$)

As he did in 1748, he dismisses the problem:

> "But as it is only a question of their [Jupiter and Saturn] movement that they follow during the course of a small number of ages, I flatter myself that my method is perfectly good; as I have changed only a little the values of the letters μ and ν in the determination of b, this difference will not produce a sensible error in the space of a few centuries, whatever the error, which results over infinite time, [and] can become infinite." (Euler [1752], 77)

By increasing the agreement between observation and calculation, the methods of Clairaut and Euler were useful in reducing the need for other physical forces, like a resisting medium, to account for the inequalities in the motions of the planets: Clairaut's pivotal piece showed that the motion of the Moon's apogee could be accounted for, solely by universal gravitation, as Euler's 1748 prize winning piece demonstrated for the motions of Jupiter and Saturn. As for the remaining inequalities, we have already seen that Euler feels they are caused by insufficiencies in the physical theory. However, neither the method of Euler-I nor that of Clairaut represents the thread of development for celestial mechanics, which culminated in the first proof, in the late 1770s and early 1780s, of the stability of the solar system. These proofs relied on a different method, which was developed primarily through the efforts of the Euler clan.

Chapter 6

The Elements of Orbit

The method that would ultimately be used by Lagrange and Laplace for their proof of the stability of the solar system had its roots in Newton's original work in the *Principia*, when Newton examined the motion of the apogee of the Moon by assuming that the Moon's actual path was on a precessing ellipse. The new approach (which I designate Euler-Lagrange, after the four mathematicians who developed it) involved determining the elements of the orbit itself, particularly the semimajor axis, the eccentricity, or the mean distance. By the work of the Euler clan, this method would gradually grow in flexibility and lead to the first proof of the stability of the solar system, Charles Euler's prize winning piece of 1760.

6.1 Leonhard Euler

The first steps in this direction were taken by Leonhard Euler. In his 1748 piece, he allowed the nodes and inclinations to vary. The first portion of the essay assumes that the mutual inclinations of the two planets, and the position of the line of the nodes, remains the same. However, in section VI, Euler examined the variation in the nodes and the inclinations.[1] Letting the mean longitude of Jupiter be ζ, its true longitude θ, its mean distance a, its true distance y, and the corresponding quantities for Saturn be ϕ, f, z, and the angle between the two $\theta - \phi = \omega$, the longitude of the ascending node π, angle between the planes of Saturn and Jupiter ρ and the distance between the two planets be

$$v = \sqrt{\frac{zz}{\cos^2 \psi} + yy - 2yz \cos \omega}$$

[1]Recall that Euler broke the problem down into four sub-problems, and used the solution of each as the preliminary solution to the next. The fourth and final problem considered non-coplanar, eccentric orbits; the preceding three assumed the orbits to be coplanar.

where ψ is the latitude of Saturn, which is small ($1°15'20''$ in Euler's assessment), and hence $\cos\psi$ can be approximated by 1. Thus:

$$v = \sqrt{zz + yy - 2yz\cos\omega}$$

Euler had earlier expressed the differential equations relating ρ and π by:

$$\text{III.} \qquad d\pi = \frac{na^3 d\zeta^2 \sin(\phi - \pi)\sin(\theta - \pi)}{zd\phi}\left(\frac{1}{yy} - \frac{y}{v^3}\right)$$

$$\text{IV.} \qquad d\ln\tan\rho = \frac{na^3 d\zeta^2 \cos(\phi - \pi)\sin(\theta - \pi)}{zd\phi}\left(\frac{1}{yy} - \frac{y}{v^3}\right)$$

Euler gives no explanations of the source of these equations; he does not derive them in [1749]. However, he derives a similar set of equations in [1747] (Euler [1747], 44).

As before, Euler begins with the case of circular, concentric orbits: $y = a$, $z = f$, $v = \sqrt{aa + ff - 2af\cos\omega}$; $f = \lambda a$, $\frac{2\lambda}{1+\lambda^2} = g$, $\lambda(1+\lambda^2)^{\frac{3}{2}} = h$, $d\theta = d\zeta$, $d\phi = md\zeta$ and $d\omega = (1-m)d\zeta$ (where λ and m are constants that allow Euler to express f and ω in terms of a and ζ) (Euler [1749], 104) Finally:

$$\frac{1}{(1 - g\cos\omega)^{\frac{3}{2}}} = A + B\cos\omega + C\cos 2\omega + D\cos 3\omega + \ldots$$

These, substituted into III. and IV., with the substitution $\frac{h}{\lambda} - A = \alpha$ or $1 - \frac{\lambda}{h}A = \frac{\lambda}{h}\alpha$, give:

$$\text{I.} \quad d\pi = \frac{n}{mh}d\zeta\sin(\phi - \pi)\sin(\theta - \pi)\Big(\alpha - B\cos\omega - C\cos 2\omega - D\cos 3\omega - \ldots)\Big)$$

$$\text{II.} \qquad d\ln\tan\rho = \frac{n}{mh}d\zeta\cos(\phi - \pi)\sin(\theta - \pi)$$
$$\times\Big(\alpha - B\cos\omega - C\cos 2\omega - D\cos 3\omega - \ldots)\Big)$$

Then, "since the variability of π is very small, one can, without error, in the integration of these formulas, regard the quantity π as constant" (Euler [1749], 104)

Replacing $d\theta, d\phi, d\omega$ with $d\zeta$, $md\zeta$, and $(1-m)d\zeta$, respectively, and setting $d\pi = 0$ (making π constant), Euler then obtains formulas for π and $\ln\tan\rho$:

$$\begin{aligned}
\text{I.} \quad \pi &= C - \tfrac{n}{4mmh}B\phi + \tfrac{n}{mh}\Big(\tfrac{\alpha}{2(1-m)}\sin\omega - \tfrac{\alpha}{2(1+m)}\sin(\theta + \phi - 2\pi) \\
&\quad - \tfrac{B}{8(1-m)}\sin 2\omega + \tfrac{B}{8m}\sin 2(\phi - \pi) + \tfrac{B}{8}\sin 2(\theta - \pi)\Big) \\
\text{II.} \quad \ln\tan\rho &= D - \tfrac{n}{mh}\Big(\tfrac{\alpha}{2(1-m)}\cos\omega + \tfrac{\alpha}{2(1+m)}\cos(\theta + \phi - 2\pi) \\
&\quad - \tfrac{B}{8(1-m)}\cos 2\omega + \tfrac{B}{8m}\cos 2(\phi - \pi) + \tfrac{B}{8}\cos 2(\theta - \pi)\Big)
\end{aligned}$$

(a note from the editor indicates a mistake on Euler's part; the last part of the second equation should read $\frac{-B}{8m}\cos 2(\phi-\pi)-\frac{B}{8}\cos 2(\theta-\pi)$ (Euler [1749], 106). These two equations give the values of π (the position of the nodes) and $\ln\tan\rho$, where ρ is the position of the nodes:

> "87. We see therefore that the longitude of the node π is not fixed, but it undergoes, during each cycle, many variations, which are proportional to the sines of the angles
>
> $$\omega, \theta+\phi-2\pi, 2\omega, 2(\phi-\pi), \text{and} 2(\theta-\pi) (\text{Euler [1749], 106})$$

Not too long after [1749], Euler began to examine the variation of all of the orbital parameters, not just the inclination and the nodes. Furthermore, he adds what he omitted in [1749]: the derivation of π and $\ln\tan\rho$ (again, these were also derived in [1747], which in some ways is the most important work in celestial mechanics, in terms of the number of methods introduced in it which will form the basis of future celestial mechanics). The article he would write, "On the Motion of Celestial Bodies Perturbed By Any Force", appeared in 1758, in the 1752/3 volume of commentaries for the Academy at St. Petersburg.

Euler begins in a characteristically Eulerian way, approaching the problem by stages. The first problem is to determine the motion of a body under the assumption that it is attracted to a fixed point C by a force varying inversely as the square of the distance to the body CP, arriving at

$$x=\frac{b}{1+k\cos s}$$

$$d\zeta=\frac{ds}{(1+k\cos s)^2}\sqrt{\frac{Ab^3}{Ca^3}}$$

$$d\phi=ds$$

where x is the distance, b the semilatus rectum, k the eccentricity, s the true anomaly (measured in this case from perihelion), ϕ is the angle between the planet and a fixed line, ζ the mean anomaly, a is the mean distance between the Earth and the sun, $\frac{C}{xx}$ is the force from the center C to the planet and $\frac{A}{xx}$ is the force from the center C to the Earth (Euler [1758], 180) By expanding the right hand side of the second equation as an infinite series, and integrating term by term, Euler obtains a series for ζ (Euler [1758], 182-3).[2]

Problem 2 is to find the orbit of a body, given an initial impulse. This is a problem equivalent to finding the orbit of a body once the initial conditions of position and velocity are known.

---

[2]The series is one of the form

$$\zeta=(s-2k\sin s+Ak^2\sin 2s-Bk^3\sin 3s+\ldots)\sqrt{\frac{Ab^3}{Ca^3(1-k^2)^3}}$$

whose exact coefficients are not relevant to Euler's development.

Euler decomposes the initial impulse into the distances $M\mu$ (parallel to CM) and Mn (perpendicular to CM) the body would travel in "time" $d\zeta$, and thus sets $M\mu = md\zeta$ and $Mn = nd\zeta$. However, $Mn = xd\phi$ and thus $d\phi = \frac{nd\zeta}{x}$ (Euler [1758], 186). Likewise, $dx = mn = M\mu = md\zeta$ (Euler [1758], 186).

Knowing that x and $d\zeta$ follow the above equations, and that $d\phi = ds$ (as the position of the perihelion is unchanged), then $ds = \frac{nd\zeta}{x}$ (Euler [1758], 186). Euler then obtains

$$n\sqrt{\frac{Ab}{Ca^3}} = 1 + k\cos s$$

by substituting the value of $d\zeta$ and x (Euler [1758], 186).

Then, beginning with the equation for x and $dx = md\zeta$ Euler differentiates to obtain

$$dx = \frac{bkds\sin s}{(1 + k\cos s)^2} = \frac{nbkd\zeta\sin s}{x(1 + k\cos s)^2}$$

(the last by replacing ds with $\frac{nd\zeta}{x}$) and thus

$$m(1 + k\cos s) = nk\sin s$$

(using the equation for x to eliminate it) thereby obtaining a relationship between $m, n, k,$ and s (Euler [1758], 186-7). From these equations, Euler is able to determine all the constants of the orbit, from the initial forces n and m and the initial distance x. As it is of particular interest, the determination of b (the semilatus rectum or "parameter") will be detailed here: from $n\sqrt{\frac{Ab}{Ca^3}} = 1 + k\cos s = \frac{b}{x}$, one obtains $Annxx = Ca^3b$ and thus $b = \frac{Annxx}{Ca^3}$ (Euler [1758], 187). Note this quantity depends only upon the initial distance x and value of the initial impulse n.

In Problem 3 Euler adds the effects of perturbation:

> "17. If a body (fig. 2), whose regular motion along APM is known, reaches M and then, is struck with whatever impetus, so that its velocity and direction at M is from [this blow] suddenly changed, find the orbit, which it follows after this blow is struck." (Euler [1758], 190)

As before, Euler introduces the quantities $Mn = nd\zeta$ and $M\mu = md\zeta$:

$$n = (1 + k\cos s)\sqrt{\frac{Ca^3}{Ab}}$$

and

$$m = k\sin s\sqrt{\frac{Ca^3}{Ab}}$$

(the latter by replacing n in the equation $m(1+k\cos s) = nk\sin s$) (Euler [1758], 190).

Now Euler supposes additional forces $M\mu' = m'd\zeta$ and $Mn = n'd\zeta$ come into play. These forces change the orbit of the body from APM to $A'P'M'$, with new orbital elements (denoted by primes). (These forces are the old forces $M\mu$ and Mn, augmented by the new forces, so they are the total forces acting on the body at M) The new orbit is easily obtained. With regard to b', the new value for the semilatus rectum, as the mathematics are unchanged, is $b' = \frac{An'n'xx}{Ca^3}$, and thus $b' = \frac{n'n'}{nn}b$ (Euler [1758], 191-2).

This is the key difference between this work and previous works. Whereas previous attempts to solve the perturbation problem kept the orbital elements constant, Euler here allows them to vary. In previous attempts to solve the problem, the equation for the orbit of the planet yielded at no point a recognizable conic; here, the planet is always moving in some conic section.

Both of the preceding problems assumed that the planet underwent a single, discrete impulse at a single point in space. Euler then makes the step, in Problem 4, of allowing the additional impulse to become infinitely small, and thereby to determine the variation of the orbital elements.

Euler lets $Mn = (n + dn)d\zeta$ and $M\mu = (m + dm)d\zeta$ (where $n + dn$ is what used to be n', and likewise for the orbital constants b, k, s, ω, ϕ). In particular, for b, replacing b' with $b + db$ and n' with $n + dn$ in the equation for b', above, expanding and ignoring terms of order dn^2 he obtains:

$$db = \frac{2ndn}{n^2}b = \frac{2dn}{1 + k\cos s}\sqrt{\frac{Ab^3}{Ca^3}}$$

(the latter, again, by replacing n with $(1 + k\cos s)\sqrt{\frac{Ca^3}{Ab}}$ (Euler [1758], 193). A similar method is used to find the variation of the other elements of the elliptical orbit.

The next step is to allow the perturbing impulse to act continuously, as forces (accelerations, really) M and N, parallel and perpendicular to the radius vector, respectively. Because of the perturbing force, the body moves in time interval dt not a distance of $md\zeta$ (perpendicular to the radius vector), but the distance

$$md\zeta - \frac{1}{2}Mdt^2 = md\zeta - \frac{Ma^3}{A}d\zeta^2$$

the latter half of the expression because $\frac{1}{2}dt^2 = \frac{a^3}{A}d\zeta^2$ (Euler [1758], 199). Euler obtains this from his differential equations of motion:

I. $$2dxd\phi + xdd\phi = 0$$

II. $$ddx - xd\phi^2 + \frac{1}{2}dt^2 \cdot \frac{C}{xx} = 0$$

From the second equation, and assuming $x = a$ — a circular orbit at the mean distance of the Earth to the sun — one first obtains that $d\phi = d\zeta$ (the mean motion is the same as the true motion), and thus $\frac{1}{2}dt^2 = \frac{a^3}{A}d\zeta^2$.

In the same fashion, the motion of the body parallel to the radius vector is not $nd\zeta$, but $nd\zeta - \frac{Na^3}{A}d\zeta^2$. Thus:

$$dm = -\frac{Ma^3}{A}d\zeta$$

$$dn = -\frac{Na^3}{A}d\zeta$$

Euler gives some indication of the reasons for his work in his closing remarks:

> "Thus the quantity of the motion of Saturn to Jupiter, and the motion of Jupiter perturbed by the force of Saturn can be investigated with better success, than [with] other methods, which it has been customary to use until now. Then if the force of Jupiter extends to as far as Saturn, it is undoubtable, [that] from it [Jupiter] the motion of Mars and the Earth, and perhaps also the inferior planets are influenced not a small part.[3] Finally it is seen I have advanced the investigation of the inequalities of the motion of the Moon by this method, because one may correctly recover the forces of the sun [on the Moon] partly to the force of the rules [of gravitation], and partly to the forces perturbing M and N." (Euler [1758], 209)

At the time, there are still slight discrepancies between the predicted position of the Moon and its observed position, and this might be one of the inequalities Euler refers to. However, Euler seems to be suggesting *other* forces that perturb M and N. Since, at the time, Euler is well aware of the secular acceleration of the Moon, he may be considering these other forces to be either the resistance of an interplanetary medium (the existence of which Euler takes as a foregone conclusion, though he will vary in his assessment of its effects), or to a modification of the simple, inverse square law which could likewise produce a secular acceleration.

6.2 Johann Albrecht Euler

Euler's method was improved by one of his sons, Johann Albrecht Euler (1734-1800). Whereas in [1758], the elder Euler determined the changes in the parameters of the ellipse by evaluating a difference, then discarding terms of higher order (such as in db), in "Researches on the Perturbation of the Movement of a Planet caused another Planet or a comet", included in the 1759 volume of the *Memoirs* for the Berlin Academy of the Sciences (published in 1766), the younger Euler takes a more analytical approach, evaluating the change in the semilatus rectum directly.[4]

The younger Euler examines the two planet case, where one planet is assumed to be in an unperturbed orbit, and the other in a variable orbit. Assuming the position of the nodes is unchanging, Euler arrives at a differential equation for the semilatus rectum p:

$$dp = -2nuv^3 d\phi(\cos(\theta-\psi)\sin\sigma - \sin(\theta-\psi)\cos\sigma\cos\omega)(\frac{1}{w^3} - \frac{1}{u^3})$$

[3] Presumably, Euler means that if Jupiter can gravitationally influence Saturn, it can certainly influence the inner planets, which are closer to Jupiter than Saturn.

[4] Though the title suggested the method contained would determine the perturbations in the motion of a planet when affected by a comet, in practice the method outlined only works for nearly circular orbits — and thus to almost no cometary perturbations worth speaking of.

(J. Euler [1759], 274), where n is the ratio between the mass of the two planets and the sun, u the distance between the sun and the planet producing the perturbations, v the distance between the sun and the planet being perturbed, ϕ and θ respectively the true anomalies of the planet being perturbed and the perturbing planet, ψ the position of the node, σ the angular distance between the planet and the node, and ω the inclination of the plane of the planet's orbit (all for the pertured planet), and w the distance between the two planets.[5] Aside from the assumption of constant node position and invariability of the perturbing planet's orbit, Euler's quantities are exact. Unfortunately, the equation cannot be integrated; well aware of the difficulty of determining the actual value of the parameters of the orbit, Euler contens himself with finding their differentials. He suggests a method of approximating them, however:

> "Now, at least when these changes do not vary very rapidly, one can put for dt the time of a day, and thus calculate successively the changes for all the days following. After each day one can well correct the elements of the movement of the body Z and of its orbit by the change found for the preceding day; but at least when they are not too considerable one can always use the first elements, since the changes following at no point suffer sensible alteration. Then one has only to gather together to the end all the changes found for each element in a sum, to have the entire change, that each element has suffered during the proposed time." (J. Euler [1759], 275)

The suggestion of the younger Euler is reminiscent of the method of the older Euler for approximating solutions to differential equations.

This is clearly a step in the right direction. Though Euler is aware of the practical applications of this method, in general, knowledge of the orbital elements is less useful for observational astronomy — in this way, one sees the beginning of the split between celestial mechanics done for the purpose of mathematics, and celestial mechanics done for the purpose of astronomy.

One might see in this the germ of a proof for the stability of the solar system. As the differential for dp seems to contain only periodic terms, it might seem that this would imply that p, the mean distance, undergoes only periodic variations.

However, there are strong limits on this method, the most notable being that the differentials themselves depend on the changing values of the other elements of the orbit, which might not be periodic. In particular, the nodes change on a regular basis and cannot, in any realistic assessment of the problem, be assumed to be constant. Euler's suggested use would not work for a determination of stability, since the stability question requires a solution valid for all time, and not limited, as Euler suggests, to a short stretch of time.

[5] Euler expresses this more concisely at the end of his article as

$$dp = -\frac{2nuv^3 \Pi H d\phi}{c^3}$$

where Π is the quantity $\cos\eta\sin\sigma - \sin\eta\cos\sigma\cos\omega$, where $\eta = \theta - \psi$, and H is the quantity $\frac{c^3}{w^3} - \frac{c^3}{u^3}$.(J. Euler [1759], p. 280)

Euler utilized this method again in "On the Perturbation of the Motion of Comets Caused By the Attraction of the Planets", which appeared in the Commentaries for the St. Petersburg Academy of Sciences in September, 1762, though this second work adds little but the quantitative calculations of the changes in the orbit of Halley's comet of 1682 due to the gravitational interaction of Jupiter.[6]

6.3 Charles Euler

This new method found its first purely *theoretical* use in the work of a third Euler, and resulted in the first attempt to prove the long term stability of the solar system. In 1760, the French Academy of the Sciences offered a prize for the examination of the question of the constancy of the mean motion of the planets, notwithstanding questions of whether or not they moved through a resisting medium (a question which would be asked for the next prize, that of 1762): in other words, was gravitation *alone* the source of the many observed secular accelerations in the motions of the Moon and planets? An Italian, Paolo Frisi, won an Award of Merit for his work (Introduction to Recueil, Vol. VII, n.p.; Frisi's work is not contained in the collections). The winning entry was by Charles Euler, who extends the earlier development of the determination of the variation of the elements of the elliptic orbit to establish what might be deemed the first theorems on the stability of the solar system: the constancy of their major axes and, consequently, their mean motions. Charles Euler's piece is the first work to make a claim on the constancy of the mean motions (and consequently the long term stability of the solar system).

The evidence for some sort of variation in the mean motions is clear to Euler:

> "Indeed, for the Moon [one is] hardly now allowed to doubt, that its mean motion now is more rapid than in the past: especially as in Saturn and Jupiter the mean motion is sometimes known to change as the most ingenious examination of the celestial motion by Le Monnier has concluded. From [this discovery] one counts the dashing of the belief of the perpetual constancy of the [mean] motions." (C. Euler [1760], 5)

Euler here is probably referring to the paper that Le Monnier *fils* presented to the Academy in 1746 (see above), which sparked the 1748 prize. The evidence of secular changes in the mean motion of Jupiter and Saturn, in Euler's view, made the likelihood that the secular acceleration of the Moon (rediscovered by Dunthorne in 1749) was more real than apparent: where one secular acceleration existed, others could as well.

[6] This Euler's primary interest in celestial mechanics seemed to be comets: his other works always examined the case of extremely elliptical orbits, as were found in the case of the comets. This method in particular is well-suited for examining the perturbed orbits of the comets, because while the radius vector r varies greatly, due to highly elliptical orbit, making power series that depend on r converge slowly if at all, the major axis a can be expected to vary only a little.

In line with the new methods of celestial mechanics, pioneered by the elder Euler, and extended by Johann Albrecht Euler, Charles Euler proposed to examine the elements of orbit, particularly the transverse (major) axis; the question posed by the Academy entails determining:

> "whether the mutual gravitation of the celestial bodies causes any change in their [the planet's] mean motions or not? ... Certainly it [the mean motion] is disturbed, because the periodic inequality is often by no means small, even as is clear in the Moon, [and] our question is better seen from the transverse axis, and whose orbit is drawn [from it]." (C. Euler [1760], 6)

Since the transverse (major) axis completely determines the period, and hence the mean motion, then variations in the one directly affect the other.[7]

Recall that in the Euler I period, one approached the answer by successive approximation, and one of the most valuable assumptions was that the mean motion was very close to the true motion. Charles Euler suggests here that the difference is not insignificant, and thus the standard technique will not be sufficiently accurate. Rather than use this method, Euler instead notes that:

> "However the motion of the planets is perturbed, it always can be taken, nearly to lay in a conic section, whose position as well as species and quantity vary continuously: the motion otherwise conforms to the rules of Kepler." (C. Euler [1760], 6-7)

Let the position of the nodes of the orbit of Z be ψ, the inclination of the plane YRZ to be ω, $EAQ = \theta$ (the longitude of the perturbing planet), $AQ = u$ (the distance between the perturbing planet and the sun), $AZ = v$ (the distance between the planet being perturbed and the sun), ζ be the true anomaly, and ϕ be the projection of the true anomaly ζ onto the plane EAQ. Then the rectangular coordinates X, Y, Z of the planet Z are:

$$X = v\cos\zeta\cos\psi - v\sin\zeta\cos\omega\sin\psi$$

$$Y = v\cos\zeta\sin\psi + v\sin\zeta\cos\omega\cos\psi$$

$$Z = v\sin\zeta\sin\omega$$

[7]The major axis is elated to the other quantities of orbit in the following manner: if e is the eccentricity, a the semilatus rectum, r the length of the radius vector, and θ the true anomaly (measured from perihelion), then one has

$$r = \frac{a}{1 - e\cos\theta}$$

The transverse axis x (distance between perihelion and aphelion) is then

$$x = \frac{a}{1-e} + \frac{a}{1+e} = \frac{2a}{1-e^2}$$

Let the distance QZ (the distance between the planets) be designated τ. Then

$$\tau^2 = u^2 - 2u(X\cos\theta + Y\sin\theta) + v^2$$

from spherical trigonometry.[8]

Also,

$$X\cos\theta + Y\cos\theta = v\cos\zeta\cos(\theta-\psi) + v\sin\zeta\cos\omega\sin(\theta-\psi)$$

by substituting the value for X and Y, and noting that $\cos\theta\cos\psi + \sin\theta\sin\psi = \cos(\theta-\psi)$ and $\sin\theta\cos\psi - \cos\theta\sin\psi = \sin(\theta-\psi)$. This gives

$$\tau = \sqrt{u^2 - 2uv\Big(\cos\zeta\cos(\theta-\psi) + \sin\zeta\cos\omega\sin(\theta-\psi)\Big) + v^2}$$

First, assuming ψ (the longitude of the nodes) and ω (the inclination of the orbit) constant, and $d\zeta = d\phi$, one obtains:

$$dX = \frac{Xdv}{v} - vd\phi(\sin\zeta\cos\psi + \cos\zeta\cos\omega\sin\psi)$$

$$dY = \frac{Ydv}{v} - vd\phi(\sin\zeta\sin\psi - \cos\zeta\cos\omega\cos\psi)$$

$$dZ = \frac{Zdv}{v} + vd\phi\cos\zeta\sin\omega$$

by differentiation.[9]

Assuming, however, that ψ and ω are not constant, one gets in a similar fashion the equations:

$$dX = \frac{Xdv}{v} - vd\zeta(\sin\zeta\cos\psi + \cos\zeta\cos\omega\sin\psi)$$

$$-vd\psi(\cos\zeta\sin\psi + \sin\zeta\cos\omega\cos\psi) + vd\omega\sin\zeta\sin\omega\sin\psi$$

$$dY = \frac{Ydv}{v} - vd\zeta(\sin\zeta\sin\psi - \cos\zeta\cos\omega\cos\psi)$$

---

[8] From

$$QY^2 = AQ^2 + AY^2 - 2(AQ)(AY)\cos(QAY)$$

one obtains

$$QY^2 = u^2 + X^2 + Y^2 - 2u(X\cos\theta + Y\sin\theta)$$

since $AQ = u$ and $AY^2 = AX^2 + XY^2 = X^2 + Y^2$. Also, $AY\cos(QAY) = X\cos\theta + Y\sin\theta$.

Then one has

$$\tau^2 = QZ^2 = QY^2 + Z^2$$

which yields the desired result, after recognizing $X^2 + Y^2 + Z^2 = v^2$

[9] For example, from

$$Z = v\sin\zeta\sin\omega$$

one differentiates to obtain

$$dZ = v\cos\zeta d\zeta\sin\omega + dv\sin\zeta\sin\omega$$

the last term being equal to $\frac{Zdv}{v}$ and, assuming $d\zeta = d\phi$, one arrives at the desired equation.

$$-vd\psi(\cos\zeta\cos\psi - \sin\zeta\cos\omega\sin\psi) - vd\omega\sin\zeta\sin\omega\cos\psi$$

$$dZ = \frac{Zdv}{v} + vd\zeta\cos\zeta\sin\omega + vd\omega\sin\zeta\cos\omega$$

Since the two equations must be equal when ψ and ω are constant, Euler sets the two equations for dZ equal to each other to obtain:

$$d\phi = d\zeta + \frac{d\omega\sin\zeta\cos\omega}{\cos\zeta\sin\omega}$$

(C. Euler [1760], p. 12). From the equations where ψ, ω were assumed constant, Euler obtains:

$$dX\cos\psi + dY\sin\psi = \frac{dv}{v}(X\cos\psi + Y\sin\psi) - vd\phi\sin\zeta$$

$$dX\sin\psi - dY\cos\psi = \frac{dv}{v}(X\sin\psi - Y\cos\psi) - vv\phi\cos\zeta\cos\omega$$

(C. Euler [1760], 13) Since ψ, ω are in fact variable, the actual equations are:

$$dX\cos\psi + dY\sin\psi = \frac{dv}{v}(X\cos\psi + Y\sin\psi) - vd\zeta\sin\zeta - vd\zeta\sin\zeta - vd\psi\sin\zeta\cos\omega$$

$$dX\sin\psi - dY\cos\psi = \frac{dv}{v}(X\sin\psi - Y\cos\psi) - vd\zeta\cos\zeta\cos\omega - vd\psi\cos\zeta + vd\omega\sin\zeta\sin\omega$$

(C. Euler [1760], 13) By setting the two sets of equations equal, one obtains[10]

$$d\phi = d\zeta + d\psi\cos\omega$$

and

$$d\phi = d\zeta + \frac{d\psi}{\cos\omega} - \frac{d\omega\sin\zeta\sin\omega}{\cos\zeta\cos\omega}$$

By setting these two equations for $d\phi$ equal, he obtains:

$$\frac{d\psi(1-\cos^2\omega)}{\cos\omega} = \frac{d\omega\sin\zeta\sin\omega}{\cos\zeta\cos\omega}$$

which can be transformed into

$$d\psi = \frac{d\omega\sin\zeta}{\cos\zeta\sin\omega}$$

[10]From the two equations for $dX\cos\psi + dY\sin\psi$, one obtains

$$vd\phi\sin\zeta = vd\zeta\sin\zeta + vd\psi\sin\zeta\cos\omega$$

which gives $d\phi = d\zeta + d\psi\cos\omega$, as desired. Likewise, setting the two equations for $dX\sin\psi - dY\cos\psi$ equal, one obtains

$$vd\phi\cos\zeta\cos\omega = vd\zeta\cos\zeta\cos\omega + vd\psi\cos\zeta - vd\omega\sin\zeta\sin\omega$$

which yields the second equation.

a relationship between $d\psi$ and $d\omega$ (where, recall, ψ was the longitude of the nodes and ω the inclination of the orbit).[11]

Finally, by assuming ψ, ζ constant, Euler obtains:

$$YdX - XdY = -vd\phi((Y\cos\psi - X\sin\psi)\sin\zeta + (y\sin\psi + X\cos\psi)\cos\omega\cos\zeta)$$

However, $Y\cos\psi - X\sin\psi = v\sin\zeta\cos\omega$ and $Y\sin\psi + X\cos\psi = v\cos\zeta$. Substituting for these values one obtains

$$XdY - YdX = v^2 d\phi\cos\omega$$

Likewise,

$$YdZ - ZdY = v^2 d\phi\sin\omega\sin\psi$$

and

$$XdZ - ZdX = v^2 d\phi\sin\omega\cos\psi$$

can be obtained through a similar process (C. Euler [1760], 15)

Let the small change in position over time dt be represented by Zz, and the small change in angle ZAz to be $d\phi$. Then

$$Zz = \sqrt{dX^2 + dY^2 + dZ^2}$$

or, in polar coordinates,

$$Zz^2 = dv^2 + v^2 d\phi^2$$

Now, consider the action of the various bodies on Z, the body whose orbit is perturbed (the other orbit assumed immobile).[12] If A is the mass of the sun, B the mass of Z, and C the mass of the perturbing body, one obtains the following equations:

$$ddX = -\alpha dt^2\Big(\frac{(A+B)X}{v^3} - \frac{C(u\cos\theta - X)}{\tau^3} + \frac{C\cos\theta}{u^2}\Big)$$

$$ddY = -\alpha dt^2\Big(\frac{(A+B)Y}{v^3} - \frac{C(u\sin\theta - Y)}{\tau^3} + \frac{C\sin\theta}{u^2}\Big)$$

$$ddZ = -\alpha dt^2\Big(\frac{(A+B)Z}{v^3} + \frac{CZ}{\tau^3}\Big)$$

where, recall, v was the distance between the planet Z and the sun, u the distance between the perturbing planet and the sun, θ the longitude of the

[11] Euler is mistaken here; the relationship should be

$$\frac{d\psi(1-\cos^2\omega)}{\cos\omega} = \frac{d\omega\sin\zeta\sin\omega}{\cos\zeta}$$

and thus

$$d\psi = \frac{d\omega\sin\zeta\cos\omega}{\cos\zeta\sin^2\omega}$$

[12] This is equivalent to the restricted three body problem, where the motion of a third body, assumed not to affect the other two, is considered.

perturbing planet, and τ the distance between the two planets, and α is the gravitational constant for Euler's system of units (C. Euler [1760], 17).

By the appropriate manipulation, one can obtain the equations

$$XddY - YddX = C\alpha dt^2(X\sin\theta - Y\cos\theta)\left(\frac{u}{\tau^3} - \frac{1}{u^2}\right)$$

$$XddZ - ZddX = -\alpha CZdt^2\cos\theta\left(\frac{u}{\tau^3} - \frac{1}{u^2}\right)$$

$$YddZ - ZddY = -\alpha CZdt^2\sin\theta\left(\frac{u}{\tau^3} - \frac{1}{u^2}\right)$$

However, $XddY - YddX = d(XdY - YdX) = d(v^2 d\phi\cos\omega)$ and thus:

$$d(v^2d\phi\cos\omega) = \alpha Cvdt^2(\cos\zeta\sin(\theta-\psi) - \sin\zeta\cos\omega\cos(\theta-\psi))\left(\frac{u}{\tau^3} - \frac{1}{u^2}\right)$$

(by substituting $Z = v\sin\zeta\sin\omega$ and for $X\sin\theta - Y\cos\theta$ as above (C. Euler [1760], 19) Similar substitutions again yield

$$d(v^2d\phi\sin\omega\sin\psi) = -\alpha Cvdt^2\sin\theta\sin\zeta\sin\omega\left(\frac{u}{\tau^3} - \frac{1}{u^2}\right)$$

and

$$d(v^2d\phi\sin\omega\cos\psi) = -\alpha Cvdt^2\cos\theta\sin\zeta\sin\omega\left(\frac{u}{\tau^3} - \frac{1}{u^2}\right)$$

The first equation is of primary interest.

Euler notes that:

$$\cos\psi d(v^2d\phi\sin\omega) - v^2d\phi\sin\omega d\psi\sin\psi = -\alpha Cvdt^2\cos\theta\sin\zeta\sin\omega(\frac{u}{\tau^3} - \frac{1}{u^2})$$

(C. Euler [1760], 20)[13]

Likewise

$$\sin\psi d(v^2d\phi\sin\omega) + v^2d\phi\sin\omega d\psi\cos\psi = -\alpha Cvdt^2\sin\theta\sin\zeta\sin\omega\left(\frac{u}{\tau^3} - \frac{1}{u^2}\right)$$

Multiplying the first of these by $\sin\psi$ and the second by $-\cos\psi$ and adding yields

$$-v^2d\phi d\psi\sin\omega = \alpha Cvdt^2\sin\zeta\sin\omega\sin(\theta-\psi)\left(\frac{u}{\tau^3} - \frac{1}{u^2}\right)$$

and thus

$$d\psi = \frac{-\alpha Cdt^2\sin\zeta\sin(\theta-\psi)}{vd\phi}\left(\frac{u}{\tau^3} - \frac{1}{u^2}\right)$$

[13] He obtains this from

$$d(v^2d\phi\sin\omega\cos\psi) = \cos\psi d(v^2d\phi\sin\omega) - v^2d\phi\sin\omega\sin\psi d\psi$$

and making the above substitution for $d(v^2d\phi\sin\omega\cos\psi)$.

Multiplying the first by $\cos\psi$ and the second by $\sin\psi$ and adding, one obtains:

$$d(v^2 d\phi \sin\omega) = -\alpha C v dt^2 \sin\zeta \sin\omega \cos(\theta-\psi)\left(\frac{u}{\tau^3} - \frac{1}{u^2}\right)$$

On the other hand,

$$d(v^2 d\phi \sin\omega) = \sin\omega d(v^2 d\phi) + v^2 d\phi \cos\omega d\omega$$

From the equation for $d(v^2 d\phi \cos\omega)$, one obtains in a similar fashion:

$$\cos\omega d(v^2 d\phi) - v^2 d\phi \sin\omega d\omega = -\alpha C v dt^2 (\sin\zeta \cos\omega \cos(\theta-\psi)$$
$$- \cos\zeta \sin(\theta-\psi))\left(\frac{u}{\tau^3} - \frac{1}{u^2}\right)$$

From these two equations, Euler obtains:

$$d(v^2 d\phi) = -\alpha C v dt^2 (\sin\zeta \cos(\theta-\psi) - \cos\zeta \cos\omega \sin(\theta-\psi)\left(\frac{u}{\tau^3} - \frac{1}{u^2}\right)$$

by multiplying the first by $\sin\psi$ and the second by $-\cos\psi$ and adding (C. Euler [1760], 21). Multiplying by $2v^2 d\phi$ and integrating yields:

$$v^4 d\phi^2 = -2\alpha C dt^2 \int v^3 d\phi (\sin\zeta \cos(\theta-\psi) - \cos\zeta \cos\omega \sin(\theta-\psi))\left(\frac{u}{\tau^3} - \frac{1}{u^2}\right)$$

In addition:

$$2dXddX + 2dYddY + 2dZddZ = -2\alpha dt^2$$
$$\times\left(\frac{(A+B)dv}{v^2} + \frac{Cvdv}{\tau^3} - C(dX\cos\theta + dY\sin\theta)\left(\frac{u}{\tau^3} - \frac{1}{u^2}\right)\right)$$

By using the formulas for dX, dY and dZ one obtains

$$dX\cos\theta + dY\sin\theta = \frac{dv}{v}\Big(X\cos\theta + Y\sin\theta) - v d\phi(\sin\zeta\cos(\theta-\psi) - \cos\zeta\cos\omega\sin(\theta-\psi)\Big)$$

of which he designates

$$\sin\sigma = \sin\zeta\cos(\theta-\psi) - \cos\zeta\cos\omega\sin(\theta-\psi)$$

Similarly

$$X\cos\theta + Y\cos\theta = v\Big(\cos\zeta\cos(\theta-\psi) + \sin\zeta\cos\omega\sin(\theta-\psi)\Big) = v\cos\rho$$

where ρ is another new variable (C. Euler [1760], 22-23).[14]

[14]Euler states that $\rho = \angle QAZ$, which is true if one assumes that ω, the inclination of the orbit, is zero. Thus:

$$\cos\zeta\cos(\theta-\psi) + \sin\zeta\sin(\theta-\psi) = \cos(\zeta-\theta+\psi)$$

where $\zeta - \theta + \psi$ is the angle QAZ. Meanwhile, σ has no real world counterpart.

Thus

$$2dXddX + 2dYddY + 2dZddZ = -2\alpha dt^2\Big(\frac{(A+B)dv}{v^2} + \frac{Cvdv}{\tau^3}\Big)$$

$$+2\alpha Cdt^2(dv\cos\rho - vd\phi\sin\sigma)\left(\frac{u}{\tau^3} - \frac{1}{u^2}\right)$$

where

$$\tau = \sqrt{u^2 - 2uv\cos\rho + v^2}$$

However, the left hand side of this equation is $d(dX^2 + dY^2 + dZ^2)$ or $d(dv^2 + v^2d\phi^2)$ which means, upon integrating both sides, one obtains the equation:

$$dv^2 + v^2d\phi^2 = 2\alpha(A+B)dt^2\left(\frac{1}{v} - \frac{1}{f}\right) - 2\alpha Cdt^2\int\frac{vdv}{\tau^3}$$

$$+2\alpha Cdt^2\int(dv\cos\rho - vd\phi\sin\sigma)\left(\frac{u}{\tau^3} - \frac{1}{u^2}\right)$$

where f is some constant of integration (C. Euler [1760], 23).

Euler makes the following substitutions:

$$P = \int v^3d\phi\sin\sigma\left(\frac{u}{\tau^3} - \frac{1}{u^2}\right)$$

$$Q = \int\frac{vdv}{\tau^3}$$

$$R = \int(dv\cos\rho - vd\phi\sin\sigma)\left(\frac{u}{\tau^3} - \frac{1}{u^2}\right)$$

From these, the equations can be written:

$$v^4d\phi^2 = 2\alpha dt^2((A+B)G - CP)$$

where G is a constant of integration, and

$$dv^2 + v^2d\phi^2 = 2\alpha dt^2((A+B)\left(\frac{1}{v} - \frac{1}{f}\right) - CQ + CR)$$

by substitution into the preceding equation for $dv^2 + v^2d\phi^2$. Let $\frac{C}{A+B} = n$, assumed small. Then

$$v^4d\phi^2 = 2\alpha(A+B)dt^2(G - nP)$$

and

$$dv^2 + v^2d\phi^2 = 2\alpha(A+B)dt^2\left(\frac{1}{v} - \frac{1}{f} - nQ + nR\right)$$

which yields the equation

$$(G - nP)(dv^2 + v^2d\phi^2) = v^4d\phi^2\left(\frac{1}{v} - \frac{1}{f} + n(R - Q)\right)$$

or, rearranging terms:

$$\frac{dv}{v^2}\sqrt{G-nP} = d\phi\sqrt{\frac{1}{v}-\frac{1}{f}+n(R-Q)-\frac{(G-nP)}{v^2}}$$

Also, from the first equation, one obtains

$$v^2 d\phi = dt\sqrt{2\alpha(A+B)(G-nP)}$$

Up to this point, Euler's development does not differ in principle from earlier works. However, at this point it diverges, for an earlier worker would attempt only to determine the value of v from the differential equations, while for Euler, this is merely a starting point.

Assuming the planet moves always in some elliptical orbit (akin to the elder Euler's assumption in 1758), then v is given by $\frac{p}{1+q\cos x}$, where p is the semilatus rectum, q the eccentricity, and x the true anomaly. The semimajor axis (which Euler calls r) is given by $r = \frac{p}{1-q^2}$. This is an important change from the earlier work of his father and brother, who examined the change in the semilatus rectum (which does not directly determine the periodic times or secular variations): by examining the semimajor axis, Euler begins the process of eliminating sources of secular acceleration in the mean motions of the planets.

At perihelion or aphelion, $dv = 0$ and $\cos x = \pm 1$. Designating $\frac{1}{f} - n(R - Q) = M$ and $G - nP = N$, the above equation gives

$$\frac{dv}{v^2}\sqrt{N} = d\phi\sqrt{\frac{1}{v} - M - \frac{N}{v^2}}$$

where the latter expression is zero when $\cos x = \pm 1$ and consequently $v = \frac{p}{1\pm q}$. These two situations yield:

$$-M + \frac{1+q}{p} - \frac{N(1+q)^2}{p^2} = 0$$

and

$$-M + \frac{1-q}{p} - \frac{N(1-q)^2}{p} = 0$$

The difference of these two equations gives

$$\frac{2q}{p} = \frac{4Nq}{p^2}$$

from which one deduces $p = 2N$.

Substituting in the value $\frac{p}{2}$ for N in the first equation, Euler obtains

$$M = \frac{1+q}{p} - \frac{(1+q)^2}{2p}$$

Euler simplifies this as

$$M = \frac{1-q^2}{p} = \frac{1}{r}$$

(probably a typesetting error for $\frac{1}{2r}$; Euler uses the correct value later) (C. Euler [1760], 27).

Substituting these values for M and N one obtains:

$$\sqrt{-M+\frac{1}{v}-\frac{N}{v^2}}=\sqrt{-\frac{1-q^2}{2p}+\frac{1+q\cos x}{p}-\frac{(1+q\cos x)^2}{2p}}=\frac{q\sin x}{\sqrt{2p}}$$

and, replacing this into the above equation for $\frac{dv}{v^2}$ and rearranging:

$$\frac{dv}{v^2}=\frac{qd\phi\sin x}{p}$$

Once again, looking only at the variation of the semimajor axis, this last equation implies that

$$\frac{dr}{r^2}=-2dM=-2n(dQ-dR)$$

which gives the variation of $\frac{1}{r}$ (since $d\frac{1}{r}=-\frac{dr}{r^2}$) and, consequently, the variation of the major axis itself (C. Euler [1760], 27).[15]

In order to determine exactly the value of $\frac{dr}{r^2}$, Euler substitutes the value for dv into the equation for Q and R to get:

$$dQ=\frac{vdv}{\tau^3}=\frac{qv^3d\phi\sin x}{p\tau^3}$$

and

$$dR=\left(\frac{qv^2d\phi\sin x\cos\rho}{p}-vd\phi\sin\sigma\right)\left(\frac{u}{\tau^3}-\frac{1}{u^2}\right)$$

and thus

$$\frac{dr}{r^2}=\frac{-2nqv^3d\phi\sin x}{p\tau^3}+2nvd\phi\left(\frac{qv\sin x\cos\rho}{p}-\sin\sigma\right)\left(\frac{u}{\tau^3}-\frac{1}{u^2}\right)$$

Hence:

$$\frac{dr}{r^2}=-\frac{2nqv^3d\phi\sin x}{p\tau^3}-\frac{2nv^2d\phi}{p}(\sin(\phi-\theta)+q\sin(\phi-\theta-x))\left(\frac{u}{\tau^3}-\frac{1}{u^2}\right)$$

by using $\frac{p}{v}=1+q\cos x$ (C. Euler [1760], 39).[16]

---

[15]Differentiating $\frac{1}{2r}=M$ and rearranging gives the first part of the equation; recall that $M=\frac{1}{f}-n(R-Q)$ where f and n were constants; differentiating this gives the second half of the equation.

[16]From the second term, factor $\frac{p}{v}$, which yields:

$$\begin{aligned}&\frac{2nv^2d\phi}{p}(q\cos(\phi-\theta)\sin x-\frac{p}{v}\sin(\phi-\theta))\\&=\frac{2nv^2d\phi}{p}(q\cos(\phi-\theta)\sin x-q\cos x\sin(\phi-\theta)-\sin(\phi-\theta))\\&=-\frac{2nv^2d\phi}{p}(q\sin(\phi-\theta-x)+\sin(\phi-\theta))\end{aligned}$$

Since the mass of the sun is so much greater than that of any of the planets, $n \approx 0$. This allows Euler to use a first order approximation. Suppose $dx = d\phi$ (i.e., there is no motion of the apsides). Then

$$d\phi = \frac{adT\sqrt{ap}}{v^2}$$

(where a is some constant). Let the perturbing planet (whose orbit is assumed fixed) follow the equation

$$u = \frac{b}{1 + e\cos y}$$

and likewise, $d\theta = \frac{adT\sqrt{ab}}{u^2} = dy$.

The simplest case is when both orbits are circular. In that case, $v = p = r, q = 0, u = b$ and thus

$$dr = -2nr^3 d\phi \sin(\phi - \theta)\left(\frac{b}{\tau^3} - \frac{1}{b^2}\right)$$

the last portion of which can be expanded as a series in $\phi - \theta$, which Euler designates η:

$$\frac{b}{\tau^3} - \frac{1}{b^2} = A + B\cos\eta + C\cos 2\eta + \ldots$$

Now, $d\phi$ and dy have a constant ratio (if both orbits are circular), thus revealing the variations of the quantity r, "from which can be discerned no alteration in the mean motion." (C. Euler [1760], 41) By this, Euler probably means the following: in a circular orbit, $dy = d\theta$, and $d\phi$ and $d\theta$ have a constant ratio, and hence the right hand side of the above equation can be expressed solely in terms of $\sin\theta$ and its multiples. Hence, if the right hand side is integrated, one will obtain only periodic terms, and thus the variation of r is periodic, even though r itself might change considerably. As the mean motion depends on r, Euler feels confident in concluding:

> "Therefore when both orbits are without eccentricity, the mutual action of the planets produces no change in the mean motion: the few changes of the transverse axis in each revolution will combine, [and] they are accustomed to repeat [themselves] between the inequalities of motion." (C. Euler [1760], 41)

In other words, the variations are periodic, even if the transverse axes change from orbit to orbit.[17]

In the case when the planets have eccentric orbits, Euler advocates expressing $d\phi$ and $d\theta$ in terms of x and y, the true anomalies of the two planets. Then η is a linear function of x, y and the term $v^2(\frac{u}{\tau^3} - \frac{1}{u^2})$ in the equation for $\frac{dr}{rr}$ can

[17] Euler does not explicitly make the argument that the differentials are periodic, though it is implicit in his words. Thus, in the equations for dr, one has purely periodic terms on one side, which implies that the variations in r are periodic, and thus r is periodic.

then be expressed as a series in terms of the cosines of linear combinations of η, x, and y.

Rather than anticipate Lagrange, and reduce η, x, and y in terms of one of the variables, Euler retains all three, integrates the equations, and obtains terms whose divisors might be zero (as Lagrange will do, below). Unlike Lagrange, Euler makes no claims on whether these terms can, in fact, be zero: rather he notes that if they are zero, they will cause the semimajor axis to increase or decrease without end. (He gives no conditions on what could make this happen)

Though Euler answered the question to the satisfaction of the judges, the problem was not yet solved. However, the groundwork had been laid for the work of Lagrange who, 16 years after Euler, will prove the same thing, in a more rigorous and widely accepted fashion.

Chapter 7

Early Work of Laplace and Lagrange

Though Charles Euler won the 1760 prize contest, his essay remained obscure; Laplace mentions it disdainfully in only one place, and Lagrange had apparently never heard of it, duplicating many of the essential details sixteen years later.

In any case, Euler's work dealt only with one of the six elements of orbit. Of these six, non-periodic variations in three are essential to the question of stability: the major axis, the eccentricity, and the inclination of the orbits.[1]

7.1 The Early Lagrange

The earliest of Joseph Louis Lagrange's (1736-1813) work on the subject of celestial mechanics centered around the theory of Jupiter and Saturn, and the theory of the satellites of Jupiter. In both of these, Lagrange approaches the idea of examining the variation of the elements of orbit, but backs away from using it to directly determine the variation of the orbit.

7.1.1 Solution to Various Problems of Integral Calculus, 1765

The first of these, "Solutions to Various Problems of Integral Calculus" [1765], appeared in the 1762-5 edition of the *Miscellanea Taurinensia*. Here Lagrange examined methods of solving many types of differential equations, culminating in the attempted solution for "The movement of a body which describes a nearly circular orbit, by virtue of a central force proportional to any function of the distance" (Lagrange [1765], 566) The last and longest section deals with the inequalities in the motions of Jupiter and Saturn, and here Lagrange adopts, to

[1]The position of the nodes and aphelia can undergo secular variation without directly threatening the stability of the solar system.

some degree, Euler's method of examining the variation of the orbital elements. Lagrange also introduces the method of variation of parameters in the modern sense.

Let I, J, J' be the masses of the sun, Jupiter, and Saturn; r, r' be the distance (projected onto a fixed plane) between the sun and Jupiter and Saturn; q, q' be the tangent of the declination of the radius vector;[2] ϕ, ϕ' be their true anomalies (measured in the fixed plane); and $p = rq, p' = r'q'$ be the z coordinate. Also, u, u' are the actual distances between the sun and the planets (hence $u = \sqrt{r^2 + p^2} = r\sqrt{1 + q^2}$, and likewise for u'). Finally, ν is the distance between the two planets.

From the unperturbed equations, one determines:

$$q = \epsilon \sin(\phi - \alpha), s = \frac{I + J}{D}\sqrt{1 + q^2} + \eta \cos(\phi - \omega)$$

where $\epsilon, \alpha, \eta, \omega$ are constants of integration, and $D = C(1+\epsilon^2)$, C being another constant of integration, and $s = \frac{1}{r}$. These constants of integration are related to the orbital elements.[3]

Lagrange then assumes that the effect of the perturbation is to vary these orbital quantities:

> "71. Suppose now that the effect of the perturbing forces is to cause the quantities ϵ, α, η and ω to vary, so that the orbit is represented by an ellipse which continuously changes in orientation [*espace*] and position;" (Lagrange [1765], 615)

Thus from:

$$q = \epsilon \sin(\phi - \alpha)$$

Lagrange differentiates to obtain:

$$\frac{dq}{d\phi} = \epsilon \cos(\phi - \alpha) + \frac{d\epsilon}{d\phi} \sin(\phi - \alpha) - \frac{d\alpha}{d\phi}\epsilon \cos(\phi - \alpha)$$

Since ϵ and α are arbitrary constants which "can be made whatever one wants" (Lagrange [1765], 615).[4] Lagrange sets:

$$\sin(\phi - \alpha)d\epsilon = \epsilon \cos(\phi - \alpha)d\alpha$$

and hence

$$\frac{dq}{d\phi} = \epsilon \cos(\phi - \alpha)$$

[2] I.e., the angle between the radius vector and the fixed plane

[3] For example, ω is the position of perihelion, η is $\frac{e}{a\sqrt{1-e^2}}$, where a is the semimajor axis and e is the eccentricity; ϵ is the tangent of the inclination of the plane of the orbit, and α is the position of the line of the nodes.

[4] He is not entirely justified in saying this, especially for ϵ, which governs the size of the inclination of the orbit.

Here we see the origin of the modern method of variation of parameters.[5]

Differentiating again, and using the above to get $\frac{\epsilon\cos(\phi-\alpha)d\alpha}{\sin(\phi-\alpha)} = d\epsilon$ Lagrange obtains:

$$\frac{d^2q}{d\phi^2} = -\epsilon\sin(\phi-\alpha) + \frac{\epsilon d\alpha}{\sin(\phi-\alpha)d\phi}$$

From these, Lagrange obtains differential expressions relating α and ϵ, and relating ϕ and α (Lagrange [1765], 615). A similar procedure allows him to obtain equations for the variation of η, ω (Lagrange [1765], 617).

Thus, in [1765], Lagrange makes the closest approach yet to direct determination of the variation of the elements of orbit, actually obtaining differential equations for α, ϵ, η and ω. Lagrange however does not attempt to solve these differential equations; rather he returns to the Euler-Clairaut methods and attempts to determine the values of r, q and ϕ. Thus, rather than determine these quantities of position by solving for the orbital elements, he instead derives the orbital elements from the solutions to the radius vector.

Lagrange discovers an alteration in the mean motion of the planets, of $2.7402''n^2$ for Jupiter, and $-14.2218''n^2$ for Saturn, where n is the number of revolutions of the planet since 1750 (Lagrange [1765], 667).

7.1.2 Researches on the Inequalities of the Satellites of Jupiter, 1766

The other early work of Lagrange was his 1766 piece on the motion of the satellites of Jupiter, "Reseraches on the Inequalities of the Satellites of Jupiter", for the Academy Prize of that year. This, like the other work of this era, was grounded firmly in the Euler-Clairaut method of solving problems in celestial mechanics.[6] Once again, however, Lagrange approaches the method of variation of orbital elements.

Letting p be the tangent of the latitude and r be the radius vector projected to a fixed plane (hence, pr is the z coordinate), Lagrange arrives at the equation:

$$(c) \qquad \frac{d^2p}{d\phi^2} + p + \frac{r^3\left(P - pR + Q\frac{dp}{d\phi}\right)}{c^2 + 2\int Qr^3 d\phi} = 0$$

where P, Q, R the perturbations in the x, y, z directions, respectively, ϕ the true anomaly, and c a constant of integration (which, in the unperturbed equation, is given by $r^2 d\phi = cdt$) (Lagrange [1766], 70). In the unperturbed case, this

[5] In modern notation, one would set the quantity

$$\frac{d\epsilon}{d\phi}\sin(\phi-\alpha) - \frac{d\alpha}{d\phi}\epsilon\cos(\phi-\alpha)$$

equal to zero in the first derivative of q.

[6] Recall this method involved finding a solution, $\frac{1}{r}$ to the two-body problem, and assumed a perturbation $\frac{1}{r+\epsilon n}$ of the solution was the solution to the n-body problem.

simplifies to:

$$\frac{d^2p}{d\phi^2} + p = 0$$

which has solution

$$p = \lambda \sin(\phi - \epsilon)$$

where λ and ϵ are constants of integration. Treating these "constants" as variables determined by the perturbation, he obtains:

$$dp = d\lambda \sin(\phi - \epsilon) + \lambda \cos(\phi - \epsilon)(d\phi - d\epsilon)$$

Lagrange gives a justification for setting the $d\lambda$ and $d\epsilon$ terms equal to each other different from that in [1765]:

> "Now, so that the body is regarded as really moving in the plane determined by λ and ϵ, it is necessary that the value of dp be the same when these quantities remain constant, that is to say, that
>
> $$dp = \lambda \cos(\phi - \epsilon) d\phi;$$
>
> hence
>
> $$d\lambda \sin(\phi - \epsilon) = \lambda \cos(\phi - \epsilon) d\epsilon \text{(Lagrange [1766], 71)}$$

Next, as in [1765], Lagrange differentiates dp; Lagrange obtains:

$$\frac{d^2p}{d\phi^2} = -\lambda \sin(\phi - \epsilon) + \frac{\lambda d\epsilon}{\sin(\phi - \epsilon) d\phi}$$

and thus

$$\frac{d^2p}{d\phi^2} + p = \frac{\lambda d\epsilon}{\sin(\phi - \epsilon) d\phi}$$

which simplifies the problem:

> "Thus the equation (c) above is reduced to two equations of the first degree, which give λ and ϵ in terms of ϕ; from these [equations] one will determine the variation of the inclination of the orbit and the movement of the line of the nodes. This is [the method] most Geometers have used thus far in the research of the orbits of the planets;[7] but it appears to us faster to find the latitude p directly by a single equation, so that the quantities λ and ϵ are deduced more easily, because from:
>
> $$p = \lambda \sin(\phi - \epsilon), \frac{dp}{d\phi} = \lambda \cos(\phi - \epsilon)$$

[7] This is a peculiar statement for Lagrange to make, considering that it is untrue; as we have seen, Clairaut's methods were the favored means of solving problems in celestial mechanics, being used extensively by Bailly and especially Lalande, with Euler's multiple approximations a distant second. Did Lagrange mean to differentiate between the Geometers and the Astronomers?

one has:
$\lambda = \sqrt{p^2 + \frac{dp^2}{d\phi^2}}, \tan(\phi - \epsilon) = \frac{pd\phi}{dp}$" (Lagrange [1766], 71)

Lagrange is mindful of the work he did in [1765] as well, noting that a similar transformation of the equation relating the radius vector r (actually $u = \frac{1}{r}$) to ϕ leads to expressing the orbital path as an ellipse whose eccentricity and position of the line of apsides are variable, "as M. Newton had made use of in relation to [the problem of] the Moon." (Lagrange [1766], 72) Lagrange here must be referring to Newton's work in the *Principia* since, as noted earlier, Newton's *Theory of the Moon's Motion* contains results, not methods.

As in [1765], Lagrange does not utilize the variation of parameters to obtain the orbital elements directly; rather, he attempts to solve for p (or actually r), and then utilize the above equations to determine the variations in the orbital elements (Lagrange [1766], 199, and Lagrange [1766], 219). Moreover, his method is very Eulerian, in that he first solves the differential equations of motion for the unperturbed case, then assumes the constants of integration are variable and solves for them in the perturbed case.

7.2 The Early Laplace

The mathematician usually credited with producing the classical proof of the stability of the solar system, Pierre Simon de Laplace (1749-1827), produced works on celestial mechanics in two periods separated by ten years. During the early 1770s, he produced three important articles. This early work is stylistically very different from his later work, and may be seen as the culmination of the Euler-I method: Laplace attempts to determine the actual distance r and true anomaly ϕ, rather than the elements of orbit. Those who credit Laplace with having derived the first proof of the stability of the solar system rest their claims on these early articles (cf. Moulton [1914], 432, Roy [1982], p. 248).

7.2.1 Researches on Integral Calculus and on the System of the World, 1772

First to be written (but second to appear, based on internal evidence) was "Researches on Integral Calculus and on the System of the World" ([1772]) which appeared in the second half of the *Memoirs* for 1772, published in 1776; the first portion of which (discussed below) was read to the Academy on August 31, 1774 (Dictionary of Scientific Biography, *Supplement*, 396).

In the introduction (written when the memoirs for that year were published, in 1776), the unnamed commentators credit Laplace with giving:

> "a complete theory of their [the planets'] inequalities, as much secular as periodic, and he demonstrates that, in the system of Newtonian attraction, the mean motion of the planets, and consequently

> their mean distance to the sun, is invariable. This result, however, is not new; M. de la Place had already come to it by another means, by another method printed, in the VIIth volume of the *Savans Etrangers*;[8] but that which he makes use of here is far more simple; moreover as these two different methods take one to the same result, it results in a degree of certainty which is difficult to deny, and which is all the more necessary, for all the Geometers who, before M. de la Place have occasioned themselves in this research, have found a secular variation in the mean motion of the planets."(*Histoires* 1772, Part II, 88)

In other words, the fact that Laplace arrived at his conclusion regarding the lack of secular variation in the mean motion implies that earlier works which yielded secular accelerations were at best incomplete.

In the article, Laplace shows that the secular terms in the solutions of the equations of orbit can be eliminated by an appropriate change of variables. However, the method is not as useful as he (or the commentator) claims it to be, for instead of actually resolving the problem of secular terms in (for example) the mean distance, it masks them by a change of variables.[9]

Laplace's third example involves the differential equation

$$0 = \frac{d^2y}{dt^2} + y - l + \alpha y^2$$

where l and α are constants (Laplace [1772], 378). To solve this, one begins by solving the equation

$$0 = \frac{d^2y}{dt^2} + y - l$$

which yields

$$y = l + p\sin t + q\cos t$$

p and q being constants. Let

$$y = l + p\sin t + q\cos t + \alpha z$$

and, substituting this into the equation and ignoring quantities of order α^2 one finds:

$$z = -\frac{2l^2 + p^2 + q^2}{2} + \frac{q^2 - p^2}{6}\cos 2t + \frac{pq}{3}\sin 2t + lpt\cos t - lqt\sin t$$

and thus

$$y = l - \alpha\frac{2l^2 + p^2 + q^2}{2} + (p - \alpha lqt)\sin t + (q + \alpha lpt)\cos t + \alpha\frac{q^2 - p^2}{6}\cos 2t + \frac{\alpha pq}{3}\sin 2t$$

[8] This is "On the Principles of Universal Gravitation...", discussed below; it appeared in the 1773 volume of *Savants etrangers*, in 1776

[9] Laplace's method is the modern method of "two-timing". See, for example, Kevorkian and Cole, *Perturbation Methods in Applied Mathematics*, p. 152ff.

Note that this solution contains secular terms (those of the form $t\sin t$).

The change of variables Laplace introduces is $t = T + t_1$, where t is the old time variable and t_1 is the new one, and T is a constant. In the differential equation:

$$0 = \frac{d^2y}{dt_1^2} + y - l + \alpha y^2$$

this gives the solutions (again, ignoring terms of order α^2):

$$y = l - \alpha\frac{2l^2 +^1 p^2 +^1 q^2}{2} + (^1p - \alpha l^1 qt_1)\sin(T + t_1) + (^1q + \alpha l^1 pt_1)\cos(T + t_1)$$

$$+\alpha\frac{^1q^2 -^1 p^2}{6}\cos(2T + 2t_1) + \frac{\alpha^1 p^1 q}{3}\sin(2t + 2t_1)$$

When $\alpha = 0$, $p =^1 p$ and $q =^1 q$ (since both of the above equations for y will be the solutions to the same differential equation), and hence p and ^1p, and q and ^1q are different by quantities of order α. This difference Laplace designates with a δ:

$$^1p = p + \delta p \qquad ^1q = q + \delta q$$

Laplace arrives at the equations

$$\delta p = -\alpha ltTq \qquad \delta q = \alpha lTp$$

by comparing the two solutions termwise when $t_1 = 0$ (Laplace [1772], p. 380).[10] Making $\alpha lT = x$, Laplace arrives at:

$$\tfrac{dp}{dx} = -q \qquad \tfrac{dq}{dx} = p$$

by means of a series expansion (Laplace [1772], 380).[11]

These equations have the solutions

$$p = e\cos(x + \omega) \qquad q = e\sin(x + \omega)$$

[10] Since the two equations are supposed to be identical when $t_1 = 0$, one has for the coefficient of the $\sin t$ term:

$$p - \alpha lqt =^1 p - \alpha l^1 qt_1$$

Then, since $^1p = p + \delta p$ and $^1q = q + \delta q$, and $t - t_1 = T$, this reduces to:

$$\delta p = -\alpha l(qT + \delta qt_1)$$

or, as $t_1 = 0$

$$\delta p = -\alpha lqT$$

Likewise, a comparison of the $\cos t$ coefficient yields the second equation.

[11] Since δp is zero when $\alpha = 0$, Laplace expresses ^1p and ^1q as:

$$^1p = p + \delta p = p + x\frac{dp}{dx} + \frac{x^2}{1\cdot 2}\frac{d^2p}{dx^2} + \dots$$

$$^1q = q + \delta q = q + x\frac{dq}{dx} + \frac{x^2}{1\cdot 2}\frac{d^2q}{Pdx^2} + \dots$$

Then, since x is of order α (being αlT), Laplace can then ignore higher order terms, to get the indicated equations.

When $x = 0$, $p =^1 p$ and $q =^1 q$ and thus:

$$^1p = e\cos(\alpha lT + \omega) \qquad q = e\sin(\alpha lT + \omega)$$

and thus, the solution, when $t_1 = 0$, is:

$$y = l - \frac{\alpha(2l^2 + e^2)}{2} + e\sin(T(1 + \alpha l) + \omega) - \frac{\alpha e^2}{6}\cos(2T(1 + \alpha l) + 2\omega)$$

where the secular terms are eliminated. According to Laplace, "This is the expression of y after any time T, neglecting quantities of order α^2." (Laplace [1772], 380)

Laplace then carries forward the approximation to include terms of order α^2, and shows how the higher order secular terms may be eliminated. Of his achievement, Laplace proudly says, "this is, as far as quantities of order α^3, the value of y after any time T; one may, in following this procedure, continue this approximation at least as far as one wants." (Laplace [1772], 382-3)

The implications of this statement are obvious: Laplace is saying that while one might find a solution with secular terms, these terms may be eliminated by an appropriate change of variables; moreover, the procedure may be repeated as often as desired, eliminating secular terms of order α^3, α^4, and so on.

Unfortunately, Laplace's method does not really allow one to answer questions about long term stability. For example, in the first approximation to order α, Laplace determined $\delta p = -\alpha lTq$ and $\delta q = \alpha lTp$, and allowing $\alpha lT = x$, derived $\frac{dp}{dx} = -q$ and $\frac{dq}{dx} = p$, which gives $p = e\cos(x + \omega)$ and $q = e\sin(x + \omega)$.

The problem with Laplace's claim (that this will rid the solution of the secular terms for all time, up to order α) is that the approximation relies on x being of order α. If x remained order α, this would not cause any problems, and Laplace's claim would be perfectly correct. However, as x depends linearly on T, as time goes by, x increases without limit, and the approximation is no longer valid. Thus, if Laplace can be said to have established anything in [1772], it is that the secular variations can be reduced to being of order α^n over short periods of time (small values of T).

Nevertheless, Laplace hopes to use the method to prove the invariability of the mean motion (among other things):

> "The most delicate part of this important theory is the determination of the secular inequalities of the movement of the planets, and, despite the ingenious researches of the best geometers of this century on this subject, it is admitted that there remains much to be desired [on this subject].
>
> "M. de Lagrange is the first to have examined this matter . . . in his excellent piece *On the Inequalities of the Satellites of Jupiter*[12] and in his *Theory of Jupiter and Saturn*[13]; the method which he

[12]Lagrange's 1766 piece which won the prize for the Academy's question of that year.

[13]Lagrange has no work with this title; Laplace is referring to Lagrange's "Solutions to Various Problems of Integral Calculus", above

> has employed for this is a masterpiece of analysis, and his excellent memoir *On the Variation of the Movements of the Nodes of the Planets and the Inclinations of Their Orbits*[14] contains the most general and most simple theory of these variations; but all the other secular inequalities, and especially those of the mean motion and the mean distance, have not yet been determined with the exactitude and the generality that one can desire, at least until the moment where I give, on this subject, my researches, in which I prove that the mean movements of the planets and their mean distances to the sun are invariable (see Volume VII of the *Savants etrangers*).[15] I propose here to consider all the inequalities, whether periodic or secular, of the movement of these bodies; one will see how the preceding method gives these inequalities, and I dare to flatter myself that this discussion interests geometers by its generality, and especially by the exactitude of its results." (Laplace [1772], 419)

In order to apply this method to the problem of the planets, Laplace begins with the differential equations of orbit, then assumes that the quantities ϕ (the true anomaly), r (the length of the radius vector) and s (the tangent of the inclination of the orbital plane) can be written as:

$$\phi = M + M'\delta\mu' + M''\delta\mu'' + \dots$$
$$r = N + N'\delta\mu' + N''\delta\mu'' + \dots$$
$$s = Q + Q'\delta\mu' + Q''\delta\mu'' + \dots$$

where $\mu', \mu'', \dots$ are the masses of the perturbing planets relative to the sun (and hence small quantities) (Laplace [1772], 431). Using the methods he established earlier, he eliminates the "secular" terms from the solutions to r and concludes:

> "that is to say that the secular equation of the mean motion of the planets is nothing, at least in powers of the approximation as far as quantities of the order of $\alpha^2\delta\mu'$. We will see after this that they are still nothing, [even] regarding terms of the order of $\alpha^3\delta\mu'$; and as the quantities of the orders $\alpha^2\delta\mu'$ and $\alpha^3\delta\mu'$ are already excessively small, one can conclude that *the reciprocal action of the planets, one on the other, can cause no sensible alteration in their mean motions, at least from the time which one has begun to cultivate astronomy, to our day*; I have found the analogous result to this by another method in the *Savants etrangers*, for the year 1773, page 218." (Laplace [1772], 454)

This second article is "On the Principle of Universal Gravitation and the Secular Inequalities of the Planets" ([1773]), which we discuss below.

[14]Laplace refers to Lagrange's "Researchs on the Secular Equations of the Movements of the Nodes and the Inclinations of teh Orbits of the Planets" [1774], discussed below. As this was written two years after the purported date on Laplace's essay, this comment was almost certainly added before its publication in 1776.

[15]Which contains Laplace's "On the Principal of Universal Gravitation and the Secular Inequalities of the Planets", discussed below.

7.2.2 On the Principle of Universal Gravitation and the Secular Inequalities of the Planets, 1773

This article was written around 1773 for the *Savants etrangers*, but not published until 1776; Laplace makes many references to it in his previous article (above), which indicates that it may in fact have been written first. It is valuable more for elucidating Laplace's thoughts on the subject of the various problems of universal gravitation than for its actual contribution to the stability problem, though this article is what gives Laplace some claim to demonstrating the stability of the solar system. However, as we shall see, the claim is fairly weak.

Of all the things that drove Laplace to begin the work that would culminate in his 1784 essay proving the stability of the solar system, the two most important were the as-yet unexplained secular acceleration of the Moon (as indicated by Dunthorne), and the remaining inequalities in the motions of Jupiter and Saturn, neither of which had a known viable explanation in universal gravitation alone. In his 1773 article, Laplace first lays down a few basic principles of his analysis, and then examines the status of Newtonian gravitation. His faith in the Newtonian system is clear:

> "There exists no point in physics whose truth is more incontestable, and better demonstrated in accordance with observation and calculation than this: *All celestial bodies gravitate towards one another*." (Laplace [1773], 212)

Specifically, Laplace means gravitation of the physical bodies of the planets by a simple inverse square law: the idea of altering the force law to account for other effects, such as non-sphericity, was unnecessary:

> "Some philosophers believe however that the law of gravity reciprocal to the square of the distance could not be true at small distances; but it seems to me that their assertion is devoid of basis; because this same law, [not only] holds for the great distances of planets to the sun, [but] is likewise true at the distance of the Moon, and likewise at the radius of the Earth, since it is proven that the gravity of a body on the surface of the Earth, is to its gravity at the distance of the Moon, as the square of this distance is to the [square of] radius of the Earth. It is impossible for us to pronounce with the same certitude, on [its validity for] smaller distances, but the analogy causes one to believe that this law need apply; moreover its simplicity makes it preferable to all others, until observations have force to [make] us abandon it." (Laplace [1773], 214)

Here Laplace sounds very much like Buffon: not only is there no reason yet to abandon the inverse square law, but its simplicity makes it more desirable in any event. The similarity with Buffon does not stop here; like Buffon, Laplace follows with a mathematical argument that the inverse square law must be that which prevails in the gravitation of the planets. Briefly, it is as follows:

suppose one decreases proportionally the distances, velocities, and sizes in the case involving the Earth, Sun, and Moon. In this situation, Laplace says:

> "It is obvious that the curve described by the Moon can remain similar to itself, at least when the force that acts upon it decreases in the same proportion." (Laplace [1773], 215)

Letting T be the mass of the Earth, h the distance to the Moon, and let m be a scaling factor, then the new mass of the Earth is $\frac{T}{m^3}$ and the new distance to the Moon is $\frac{h}{m}$. Let $\frac{T}{\phi(h)}$ be the force of the Earth on the Moon. The new force becomes $\frac{T}{m^3\phi(\frac{h}{m})}$, but since the curves are similar, the forces should be similar as well: thus

$$\frac{T}{m^3\phi(\frac{h}{m})} = \frac{T}{m\phi(h)}$$

and thus

$$m^2\phi(\frac{h}{m}) = \phi(h)$$

Let $\phi(h) = h^2 \cdot {}^1\phi(h)$, thus

$${}^1\phi(\frac{h}{m}) = {}^1\phi(h)$$

Since this must be true for all m, then ${}^1\phi(h)$ must be a constant, and thus $\phi(h) = Ah^2$. Hence, the gravitational force $\frac{T}{\phi(h)}$ must operate as an inverse square of the distance.

Laplace's example suggests he is thinking of the unresolved secular acceleration of the Moon (the explanation of which was the subject for the 1772 prize, discussed briefly below). He continues, examining the oblateness of the Earth, but concludes:

> "It does not appear that the figure of the Earth sensibly influences the movement of the Moon; the difference between the axes of the Earth are too small in comparison to the distance of that heavenly body for its effect to be perceived; but the oblateness of Jupiter is much greater than that of the Earth, [and] if the movements of its satellites and the inequalities caused by their mutual gravitation and that of the sun is well known, [then] one can determine the effect of the figure of Jupiter, and judge if it conforms to theory; but the observations are not sufficiently precise or abundant to establish any certainty on this matter." (Laplace [1773], 218)

The rest of the article consists mostly of Laplace's attempts to prove the sufficiency of the inverse square law, in and of itself, for causing the observed motions of the planets. He does this by suggesting alterations to the simple inverse square law, then knocking them down. In some cases, he does this by a simple argument that there is no basis for the suggestion:

> "It is not reasonable that the attractive virtue or, more generally, that any of the forces which exert themselves at *ad distans*, transmit themselves instantly from one body to another, because all that which transmits across space appears to us to successively affect its different points; but the ignorance that we have on the nature of the forces and the manner which they are transmitted need render us very reserved in our judgements, until experience enlightens us." (Laplace [1773], 219)

Laplace will elsewhere examine the effects of a finite speed of gravitation, but here he simply states that since there is no plausible reason to believe such an effect exists, it is best to ignore it for the time being.

Elsewhere, he examines the possibility that gravitation acts differently on bodies in motion than on bodies at rest. To Laplace, this is more plausible:

> "It remains for us to ask if gravity acts in the same manner on bodies at rest as those in motion; it is obvious that a body at rest, subject to gravity, experiences all its action, and falls following the vertical to the surface of the Earth; but if the body already moves towards the Earth in the direction of this vertical, it is natural to think that the velocity need subtract a part from the effect of gravity. This sentiment, very reasonable in itself, will be incontestable if the cause of gravity comes from the impulse of some fluid; but, as it is entirely unknown [whether this is true], I will submit to analysis the movement of celestial bodies under the supposition that gravity acts differently on the bodies based on their different movements; I compare the following calculation to observation, because there exist some phenomena unexplained thus far in the usual supposition [but] which can be derived from this one, [and] then one can not help but regard this [supposition] as indicated by nature, and consequently adopt it." (Laplace [1773], 221)

Under this supposition, Laplace determines that this results in a secular acceleration proportional to the square of time (Laplace [1773], 224). This leads directly into the problem of the secular acceleration of the Moon:

> "In comparing the eclipses of ages past with those of this century, astronomers have remarked that the tables of the Moon cannot be satisfactory, supposing that this heavenly body has a constant mean movement; they have consequently introduced an acceleration in this movement. M. Mayer, who appears to be one of those who are the most occupied with this subject, has determined the quantity of this acceleration; he finds it to be a degree in two thousand years, and sensibly proportional to the square of time counted since an epoch fixed as 1700; to be truthful, the proofs on which the acceleration of the mean motion of the Moon is based are skillfully discussed by M. de Lagrange, in the excellent piece which won the prize of the

> Academy for the year 1773; from his work it appears that [the secular acceleration] is unproven.[16] But, without examining the proofs [of the lack of a secular acceleration of the Moon] here, I note that they are rather reasonable." (Laplace [1773], 225)

In the end, Laplace seems hopeful but wary about the value of this hypothesis of gravity acting differently on moving bodies:

> "It results from the preceding article that the hypothesis of gravity acting differently on bodies at rest and in motion gives a very simple means to explain the secular equation of the Moon; meanwhile, however natural it seems to be, I am unwilling to regard this as certain, and I only propose it as a conjecture which to me appears to merit the attention of philosophers [i.e., scientists];" (Laplace [1773], 234)

One is reminded of Clairaut's suggestion that the motion of the apogee might be explained by the addition of an inverse quartic term to the inverse square law; perhaps mindful of Buffon's attack of Clairaut, Laplace takes pains to ensure that it is clear that he is not suggesting an alternative to universal gravitation in any but the most speculative manner.

Laplace is, in fact, skeptical of the need for any change to universal gravitation; in fact, his goal seems to be to to show that *none* of these suggested changes in the fundamental physical theory of the planets were plausible or necessary. By eliminating all other possible sources of the variation of the Moon's motion, Laplace wants to show that Newtonian gravitation, with the inverse square law, is necessary and sufficient to explain all the observed motions of the heavenly bodies, even if the mathematics has not yet been developed to demonstrate this.

Thus, Laplace discounts other possible sources of the inequalities:

> "One can agree, however, that by admitting in space an extremely rarefied fluid, one explains in a very satisfactory manner the secular equation of the Moon (see the piece of M. l'Abbe Bossut, who won the prize for the Academy for the year 1762). But the existence of such a fluid is very uncertain, unless one takes for the fluid the light of the sun." (Laplace [1773], 235)

He discounts Newton's hypothesis of the loss of mass from the sun, and seems to suggest that it is by emitting light that the sun loses mass:

> "In granting, with M. Mayer, a secular equation for the Moon of a degree in two thousand years, and supposing the parallax of the sun of $8''\frac{1}{2}$, I determine first of all the loss of the mass of the sun in this interval of time; then, to ascertain if this loss is real, I observe that it needs to cause a retardation of the mean movement of the Earth,

[16] Lagrange, as we shall see, concluded that the secular acceleration did not exist but as a sequence of observational errors.

> because the mass of the sun diminishes without end, [and] the orbit of the Earth needs to enlarge more and more: now, I find that, to admit in the mean movement of the Moon an acceleration of a degree in two thousand years, it is necessary to admit a retardation of many degrees in the place of the Earth, which is absolutely contrary to observations; from which I conclude that the impulse of the light of the sun can not produce the secular equation of the Moon." (Laplace [1773], 236-7)

He likewise discounts another possible explanation, the slowing down of the Earth's day by the effects of the winds — which would make the secular acceleration of the Moon more apparent than real (Laplace [1773], 236).

Laplace even mentions the work of Charles Euler (ignored by or unknown to Lagrange) though he is not content with it:

> "The Academy proposed for the subject of the prize of the year 1760, to determine the alteration of the mean movement of the Earth, produced by the action of the celestial bodies. The winning piece of M. Charles Euler, however worthy it might be otherwise, has added nothing, it seems to me, to that which one knew already of the effect of the attraction of the planets." (Laplace [1773], 240)

This is a peculiar statement. Euler's work might be criticized for incompleteness or for incomprehensibility, but Laplace criticizes it for adding "nothing" to the known effects of the planets. This criticism is justified, perhaps, because Euler did not attempt to carry the calculation of the change in the major axis any further than simply arriving at the differential equation for it, and hence his work "added nothing" beyond that which his father and brother accomplished, but the criticism seems unnecessarily severe.

The 1773 article is unusual in that Laplace uses the Euler-Lagrange method to determine the variation of the elements of orbit; it is the earliest appearance of this method in Laplace's work. However, it appears only in a brief subsection during his calculation of the effects of the supposition that gravity acts differently on bodies at rest than on bodies in motion, where he extends the method for bodies with highly eccentric orbits (Laplace [1773], 231-2). Laplace does not follow the method throughout, and even here, uses it merely to obtain a better value for r, the radius vector, and ϕ, the true anomaly.

Elsewhere, Laplace's method is as follows. Letting ψ', ψ'', ψ''' be the forces along the three coordinate axes x, y, z, where

$$x = r\cos\phi, y = r\sin\phi, z = rs$$

and M the mass of the body whose orbit is to be determined, S the mass of the sun, one has the equations of motion:

$$\frac{d\phi}{dt} = \frac{1}{r^2}\left(C + \int \frac{\psi' r dt}{M}\right) \tag{1}$$

$$\frac{d^2r}{dt^2} - \frac{1}{r^3}\left(C + \int \frac{\psi' r dt}{M}\right)^2 - \frac{\psi''}{M} = 0 \tag{2}$$

$$0 = \frac{d^2s}{dt^2} + \frac{2dsdr}{rdt^2} + \frac{s}{r^4}\left(C + \int \frac{\psi' r dt}{M}\right)^2 + \frac{S\psi'' - \psi}{Mr} \tag{3}$$

In the two planet case, where ν is the distance between the two planets, and r', ϕ', s' determines x', y', z' as above, one has:

$$-\frac{\psi''}{M} = \frac{S+p}{r^2(1+s^2)^{\frac{3}{2}}} + p'\frac{\cos(\phi'-\phi)}{r'^2(1+s'^2)^{\frac{3}{2}}} + \frac{p'}{\nu^3}[r - r'\cos(\phi'-\phi)]$$

$$\frac{\psi' r}{M} = p' r \sin(\phi'-\phi)\Big[\frac{1}{r'^2(1+s'^2)^{\frac{3}{2}}} - \frac{r'}{\nu^3}\Big]$$

$$-\frac{\psi}{M} = \frac{(S+p)s}{r^2(1+s^2)^{\frac{3}{2}}} + \frac{p's'}{r'^2(1+s'^2)^{\frac{3}{2}}} + \frac{p'}{\nu^3}(rs - r's')$$

If these are substituted into equations (1), (2), (3) above, one has the equations for the perturbed orbit of the planet (Laplace [1773], 242). These equations are:

$$\begin{aligned}
&(A) \quad \frac{d\phi}{dt} = \frac{1}{r^2}\left(C + \int p' r dt \sin(\phi'-\phi)\Big[\frac{1}{r'^2(1+s'^2)^{\frac{3}{2}}} - \frac{r'}{\nu^3}\Big]\right) \\
&(B) \quad 0 = \frac{d^2r}{dt^2} - \frac{1}{r^3}\left(C + \int p' r dt \sin(\phi'-\phi)\Big[\frac{1}{r'^2(1+s'^2)^{\frac{3}{2}}} - \frac{r'}{\nu^3}\Big]\right)^2 \\
&\qquad + \frac{S+p}{r^2(1+s^2)^{\frac{3}{2}}} + \frac{p'r}{\nu^3} + p'\cos(\phi'-\phi)\Big[\frac{1}{r'^2(1+s'^2)^{\frac{3}{2}}} - \frac{r'}{\nu^3}\Big] \\
&(C) \quad 0 = \frac{d^2s}{dt^2} + \frac{2dsdr}{rdt^2} + \frac{s}{r^4}\left(C - \int p' r dt \sin(\phi'-\phi)\Big[\frac{1}{r'^2(1+s'^2)^{\frac{3}{2}}} - \frac{r'}{\nu^3}\Big]\right) \\
&\qquad + \frac{p'}{r}[s' - s\cos(\phi'-\phi)]\Big[\frac{1}{r'^2(1+s'^2)^{\frac{3}{2}}} - \frac{r'}{\nu^3}\Big]
\end{aligned}$$

Since the masses of the planets are very small compared with that of the sun, Laplace designates $p = \delta m, p' = \delta m'$. In the case of the unperturbed planetary orbits, one has the solutions:

$$\phi = nt + A' - 2\alpha e \sin(nt+\epsilon) + \frac{5}{4}\alpha^2 e^2 \sin 2(nt+\epsilon) + \dots$$

$$r = a\Big[1 + \frac{\alpha^2 e^2}{2} + \alpha e \cos(nt+\epsilon) - \frac{\alpha^2 e^2}{2}\cos 2(nt+\epsilon) + \dots\Big]$$

where ϵ is some constant, and when the eccentricity is αe, i.e., it is of order α (Laplace [1773], p. 242. In these equations, a is the semimajor axis, $\alpha e a$ the excentricity; A' the "mean distance to a fixed line".

In order to determine the variation caused by a perturbation, Laplace differentiates both sides of (A) with respect to $\delta m'$, and designates the differential with δ. Hence, (A) becomes:

$$\frac{d\delta\phi}{dt} = -\frac{2c}{r^3}\delta r - \frac{\delta m'}{r^2}\int r dt \sin(\phi' - \phi)\Big[\frac{1}{r'^2(1+s'^2)^{\frac{3}{2}}} - \frac{r'}{\nu^3}\Big] \tag{6}$$

and (B) becomes

$$0 = \frac{d^2\delta r}{dt^2} + \frac{3c^2}{r^4}\delta r - \frac{2(S+\delta m')}{r^3}\delta r \tag{7}$$

$$+\frac{2c\delta m'}{r^3}\int r dt \sin(\phi' - \phi)\Big[\frac{1}{r'^2(1+s'^2)^{\frac{3}{2}}} - \frac{r'}{\nu^3}\Big]\frac{\delta m' r}{\nu^3} + \delta m' \cos(\phi' - \phi)\Big[\frac{1}{r'^2(1+s'^2)^{\frac{3}{2}}} - \frac{r'}{\nu^3}\Big]$$

To solve this problem, Laplace notes that all of the terms of the same order, specifically constants, terms proportional to time t, terms proportional to $\sin(nt+\epsilon)$, and terms proportional to $\cos(nt+\epsilon)$ must have coefficients equal to zero (Laplace [1773], 243). Considering these terms alone, he writes:

$$\frac{2c\delta m'}{r^3}\int r dt \sin(\phi' - \phi)\Big[\frac{1}{r'^2(1+s'^2)^{\frac{3}{2}}} - \frac{r'}{\nu^3}\Big]$$

$$\frac{\delta m' r}{\nu^3} + \delta m' \cos(\phi' - \phi)\Big[\frac{1}{r'^2(1+s'^2)^{\frac{3}{2}}} - \frac{r'}{\nu^3}\Big]$$

$$= a\frac{\delta m'}{a^3}A + \alpha^2 a\frac{\delta m'}{a^3}Bnt + \alpha a\frac{\delta m'}{a^3}C\cos(nt+\epsilon) + \alpha a\frac{\delta m'}{a^3}D\sin(nt+\epsilon)$$

Moreover, ignoring terms of order $\alpha^2\delta m'$ and ignoring all terms but those of the order of the time t:

$$-\frac{\delta m'}{r^2}\int r dt \sin(\phi' - \phi)\Big[\frac{1}{r'^2(1+s'^2)^{\frac{3}{2}}} - \frac{r'}{\nu^3}\Big] = -\frac{\alpha^2\delta m'}{a^3}\frac{a^2}{2c}Bnt$$

Finally, writing

$$\frac{S+\delta m}{a^3} = n^2$$

and

$$\frac{c}{a^2} = n$$

and thus

$$\frac{\delta m'}{a^3} = n^2\delta\mu'$$

where $\delta\mu'$ is the ratio of the mass of the planet p to that of the sun. Ignoring terms of order α^2, this allows Laplace to write:

$$\frac{d\delta\phi}{dt} = -\frac{2c}{r^3}\delta r - \frac{\alpha^2\delta\mu'}{2}Bn^2t \tag{8}$$

$$0 = \frac{d^2\delta r}{dt^2} + \frac{3c^2}{r^4}\delta r - \frac{2(S+\delta m}{r^3}\delta r + a\delta\mu' n^2 A + \alpha a\delta\mu' n^2 C\cos(nt+\epsilon)$$

$$(9) \qquad +\alpha a\delta\mu' n^2 D\sin(nt+\epsilon) + \alpha^2 aB\delta\mu' n^3 t$$

From these equations, Laplace derives his determinations of the secular variation of the elements. First, he supposes:

$$\delta\phi = \delta\mu' gnt + \alpha^2\delta\mu' hn^2t^2$$

and

$$\delta r = a\delta\mu'[l + \alpha \cdot^1 pnt\cos(nt+\epsilon) + \alpha qnt\sin(nt+\epsilon) + \alpha^2 Knt]$$

In other words, the perturbation causes an acceleration in the true anomaly and a linear change in the radius vector (Laplace [1773], 244). Laplace then substitutes these into equations (8) and (9), above, to determine:

$$g = 2A, l = -A,^1 p = \frac{1}{2}D$$

$$q = -3eA - \frac{1}{2}C, K = \frac{3}{2}eD - B, h = \frac{3}{4}(B - eD)$$

by comparing coefficients (Laplace [1773], 244). From these, he then determines:

$$\phi = nt + A' + 2A\delta\mu' nt + \alpha^2\delta\mu'\frac{3}{4}(B-eD)n^2t^2 + \dots$$

$$r = a\Big(1 + \alpha\Big(e + \frac{1}{2}\delta\mu' Dnt\Big)\cos\Big[nt\big(1 + 3A\delta\mu' + \frac{1}{2e}C\delta\mu'\big) + \epsilon\Big] + \dots\Big)$$

From these, one can read directly quantities of interest.

Laplace then examines the changing of the position of the orbit of the planet. His procedure is essentially the same: beginning with the equations of motion, he allows $s = \alpha\lambda, s' = \alpha\lambda'$, and determines the variation in the equations of motion caused by a perturbation $\delta m'$. After simplifying and resorting to the use of series, Laplace finally determines that in the case of two planets, one perturbed by the other, the change in the apogee is given by:

$$\delta\mu' i \cdot 360^\circ\left(\frac{1}{4}zb_1 + \frac{\frac{1}{2}\alpha e'}{\alpha e}\cos(L'-L)[b_1(1+z^2) - 3bz]\right)r$$

the increase of the equation of center (the eccentricity) is:

$$\alpha e'\delta\mu'\sin(L'-L)i\cdot 360^\circ[b_1(1+z^2) - 3bz]$$

the diminution of the inclination of the orbit is given by:

$$\frac{1}{4}zb_1\alpha\gamma'\sin(\Gamma'-\Gamma)\delta\mu' i\cdot 360^\circ$$

and the retrograde movement of the node is:

$$\frac{zb_1}{4}\delta\mu'\left[1-\frac{\alpha\gamma'}{\alpha\gamma}\cos(\Gamma'-\Gamma)\right]i\cdot 360^\circ$$

where z is the ratio ${}^{a'}/_{a}$, Γ, Γ' are the positions of the nodes, $\alpha\gamma, \alpha\gamma'$ are the inclinations (assumed small), and:

$$\frac{1}{\nu^3}=\frac{1}{(1-2z\cos\theta z^2)^{\frac{3}{2}}}=b+b_1\cos\theta+\ldots$$

is the expression that defines b_1 (Laplace [1773], 263). In the same manner, Laplace determines the acceleration of its mean motion is zero:

> "Thus it appears certain that the reciprocal action of the planets can not cause noticeable variation in their mean movements, at least, as far as quantities of the order of $\alpha^2\delta\mu'$; it might be however that in higher powers of the approximation, one will find a secular equation; but there is reason to believe that it will be insensible, because it can only be, as I have observed (art. LV), the order of $\alpha^4\delta\mu'$, which is to say the same order as the product of the fourth powers of the eccentricities of the perturbed planets by the ratio of its mass to that of the sun. Now, the quantities of the order of $\alpha^2\delta\mu'$ are already very small, [and] it is very probable that those of the order of $\alpha^4\delta\mu'$ are absolutely insensible." (Laplace [1773], 263)

Because of this, Laplace says, the observed alteration of the mean motions of the planets must be due to some cause other than their mutual attraction.

In spirit, Laplace's procedure is very similar to Clairaut's: begin with the unperturbed equations of motion, assume a form of the perturbed equations of motion (in Laplace's case, the radius vector becomes $r+\delta r$ and the true anomaly $\phi+\delta\phi$), determine the constants involved, and read off the orbital elements from the form of equations for the radius vector and true anomaly. Like Clairaut's method, however, it is limited by the assumption of the form of the quantities $\delta r, \delta\phi$.

Thus, one sees no great change in Laplace from the Euler I/Clairaut period: Laplace is still attempting to compute the radius vector r and longitude ϕ from the equations of motion, and compute the acceleration of the mean motion from its equation.

7.2.3 On Particular Solutions to Differential Equations and the Secular Inequalities of the Planets, 1772A

It is only with the third (and probably the last written) of Laplace's articles, "Onn Particular Solutions to Differential Equations and the Secular Inequalities of the Planets" ([1772A]), that Laplace adopts the Euler-Lagrange method, and tries to determine the variation of the orbital elements. [1772A] appeared in the first part of the memoirs for 1772, published in 1775. The first part deals with methods of solving the differential equations relating to the equations of motion for the planets, and differs little from Laplace's earlier work on the subject. The second part, though, shows Laplace using the Euler-Lagrange method to determine the variation of the eccentricity and perihelion position of the planets.

Though Laplace uses the Euler-Lagrange method, he does not contribute to its development; he draws his inspiration from Lagrange's [1774], as he himself admits. Hence, Laplace's investigations in [1772A] shall be discussed alongside Lagrange's [1774], in the next chapter. However, Laplace's final notes of [1772A] are of interest, for they indicate his plans for future work:

> "This would be the place to apply the preceding researches to different bodies of our planetary system, but I propose to give in an upcoming volume a general theory of the movement of the planets, in which I will resume this matter.
>
> "I will make use of a new method of approximation by which the preceding researches can be conducted, and which is general and above all very simple ..." (Laplace [1772A], 361)

The "new method" Laplace cites is that of [1772]. Despite his hopes for this method, it will not play an important role in Laplace's later proof of the stability of the solar system; overall, the value of Laplace's early work was to show, up to an approximation, that there were no secular terms. Lagrange (and later, Laplace) took this as incentive enough to try and establish the lack of secular terms *a priori*, and would ultimately develop what they called "rigorous" proofs of the lack of secular variations in the mean motion.

Chapter 8

The First Stability Proof

Lagrange's truly productive decade in the field of celestial mechanics was during the 1770s, where he provided several important papers, including what would become the primary proof of the constancy of the mean motions of the planets — and hence, the semimajor axes — an essential requirement to the classical proof of the stability of the solar system. Moreover, he demonstrated the periodic nature of the variations of the product of the tangent of the inclination and the sine (or cosine) of the position of the nodes; thus, of the three important parameters of orbit, two were shown by Lagrange to be either invariable or periodic. Thus, by 1776, Lagrange will have provided proofs that two of the three undergo only small, periodic variations, leaving only one — eccentricity — for Laplace to deal with. For this accomplishment, Lagrange deserves — and received from his contemporaries — a great deal of credit for the first demonstration of the long term stability of the solar system.

It seems that no small part of the motivation for Lagrange examining the elements of orbit — especially the mean motion — had to do with his belief of the non-existence of secular accelerations in the mean motion of the planets. In particular, the Moon did not (in Lagrange's view) undergo *any* secular acceleration, despite all observational evidence. Thus, in Lagrange, we see the antithesis of the physical school: rather than modify theory if it does not fit observation, Lagrange prefers to discard observations when they conflict with theory.

8.1 On the Secular Acceleration of the Moon, 1774

In 1772, the Academy, dissatisfied with the current state of lunar theory, offered a prize for new tables of the Moon; Euler and Lagrange both won for their contributions, and Euler produced a newer, more accurate theory of the Moon, complete with tables. However, neither Euler nor Lagrange were able to account for the Moon's secular acceleration. To Euler, this was clear evidence that universal gravitation alone was insufficient to completely determine the motion of the planets — and hence the planets must certainly move through a resisting medium.

Because of the lack of correspondence between the theories of the Moon, as presented by Euler and Lagrange, and the observations indicating a secular acceleration, made by Halley and Dunthorne, the Academy's next question, two years later, involved the secular acceleration of the Moon; Lagrange produced "On the Secular Acceleration of the Moon" ([1773]), which won the prize for that year.

Lagrange's involvement with the problem began early; correspondence with d'Alembert indicates interest in lunar theory dating back to 1764. For this particular prize, unlike previous ones, d'Alembert felt the focus ought to be on the secular equation in particular, rather than a general theory of the Moon's motion. He states his views in a letter to Lagrange, dated March 25, 1772:

> "There is a chance (because this is not yet absolutely decided) that we will propose for the subject of the year 1774 the two questions of the unknown equations and the secular equation, without asking again for tables of the Moon; thus you want all the time to plumb these two points." (Lagrange [1772A], 231)

Lagrange worked on the problem and by the end of 1772, seemed to have arrived at a remarkable conclusion, which he hints at in a letter to Condorcet on December 1, 1772:

> "I am now after the question of the acceleration of the Moon; I found singular results which merit the attention of geometers and astronomers. I will send the account of my researches for the contest, if nothing opposes it." (Lagrange [1772B], 7)

Could this "singular result" be Lagrange's determination that the secular acceleration does not exist in reality? It seems probable.

Lagrange examines the two parts of the question:

> "In the first, one asks by what means one can be assured that no sensible error results from the quantities that one has neglected in the calculation of the movement of the Moon." (Lagrange [1773], 335)

In other words, how does one know when the approximate solution to the differential equations of the Moon's motion is sufficiently accurate. The secular acceleration might be the result of an actual, physical phenomenon; or, as in the earlier anomaly in the motion of the Moon's apogee, it might be the result of an insufficiently accurate approximation. The second part of the question:

> "asks if, after regarding not only the action of the sun and of the Earth on the Moon, but moreover, if it is necessary, the action of the

> other planets on this satellite, and likewise the nonspherical figure of the Moon and Earth, one can explain, by only the theory of gravitation, why the Moon appears to have a secular equation, without the Earth having a sensible one." (Lagrange [1773], 335-6)

It is this second question that Lagrange intends to address, and in answering it, he will ultimately deny that planetary perturbations can cause a secular acceleration in the mean motion of the Moon, and provide an argument that embodies the fundamental reasoning behind [1776].

Lagrange simply does not believe that the Moon has a secular acceleration, and the aim of his article seems to be eliminating all possible sources of the such an acceleration *except* for observational error, to which he finally attributes the acceleration.

After preliminary comments on the nature of the inequalities, empirically determined by Mayer, he notes that as one obtains different results depending on whether one assumes the acceleration is constant (yielding an error that varies as the square of time) or some other assumption, then:

> "Thus it is only from theory that one can determine the secular equation of the planets and of the Moon in particular; and the question is solved if, among all the inequalities which result from the mutual attraction of celestial bodies, one has [an inequality] such as one supposed thus far in the movement of the Moon, and whose effect is only sensible after many ages; ...." (Lagrange [1773], 345)

In other words, one should attempt to find the secular variation from theory first, rather than the other way around. Lagrange continues:

> "now, regarding the Moon, although it is demonstrated that its periodic inequalities are entirely and uniquely due to the action of the sun combined with that of the Earth, it appears very difficult and nearly impossible to deduce from the same cause the secular inequalities of this planet; at least none of those who have worked up to the present on the solution of the problem of three bodies have been able to find in the formula of the position of the Moon terms capable of producing a true or even apparent alteration in its mean motion..." (Lagrange [1773], 345)

Lagrange will return to the three (and n-body problem) later. Here, he offers a possibility for the secular acceleration of the Moon:

> "But there is a circumstance which no one has paid attention to before now in this calculation of the movements of the Moon: the non-sphericity of the Earth, which produces a small alteration in the force which draws the Moon towards the Earth, and from which there results a new perturbing force of the orbit of the Moon, which, being combined with that which comes from the action of the sun, could possibly produce the terms which give the secular equation of the Moon." (Lagrange [1773], 345)

One sees in Lagrange's statement an answer to Euler's constant claim that the non-sphericity of the planets should affect their overall motion; though Euler often claimed this must certainly be the case, his actual work on the problem was limited to finding general solutions to the motion of bodies under a variant of a simple, inverse-square law. Here, Lagrange actually determines the effects of non-sphericity of a specific body (the Earth) on another (the Moon), and most of the balance of the article is Lagrange's attempt to determine if the non-sphericity of the Earth could be the source of the variation.

The method used by Lagrange is the old, Euler-I method: if the motion of the Moon in the unperturbed two-body problem is given by

$$\frac{1}{r} = u = \frac{1 + e\cos v}{p}$$

where r is the radius vector, e the eccentricity, v the true anomaly, and p the semilatus rectum, then the perturbed equation has solution:

$$\frac{1}{r} = u = \frac{1 + e\cos v + s}{p}$$

Lagrange then attempts to determine the value of s from the differential equations.

However, despite his best efforts, Lagrange cannot obtain a value for the secular acceleration of the Moon this way, and he concludes that the non-sphericity of the Earth cannot be the cause of the secular acceleration.

As for the remaining inequalities in the motion of the Moon, Lagrange briefly examines the effects of the perturbations of the sun (and other planets); once more he concludes the other planets cannot possibly produce a secular variation in the Moon's mean motion, and provides the earliest proof (albeit not very mathematical) of the lack of a secular acceleration in the Moon's orbit:

> "In effect, it is easy to see that the expressions of the perturbing forces of the Moon, produced by the action of any other planet, can depend only on the angles s, η, ζ relative to the Moon, and the analogous angles s', η', ζ' relative to the planet (s' being the anomaly of the planet, η' its elongation to the Earth, and ζ' its distance to the node); [however] these expressions contain only sines and cosines of angles formed by the combination of those angles and their multiples; and one could prove easily that the quantity $\frac{\Pi}{u^3}$ can likewise only be formed of sines and cosines; and if one wants to regard at the same time the action of the sun, it will add again to those six angles that of the anomaly of the sun, which one can name ξ. All reduces thus to examining if one can find a combination of these seven angles s, η, ζ, s', η', ζ', ξ and their multiples, which gives an angle quite, or at least nearly, constant;[1] now, after the known values of the ratios

[1] This would give a constant term in the integration, which would give rise to a secular term in the solution to the differential equations of motion.

> of these angles, one can be assured easily that it is hardly possible to form such a combination, without employing very large multiples; from which one can conclude that the terms which can produce a secular equation are only present after many corrections of the orbit, and are in consequence of an order too small to be able to give a sensible equation and conforming to the observations." (Lagrange [1773], 385)

In short, since the perturbations are periodic, their effects must be periodic as well. In a nutshell, this is the basis for his 1776 proof of the constancy of the mean motions.[2]

Since there appears to be no gravitational source of perturbation, the source must be non-gravitational, and here Lagrange raises the question of a resisting ether.

> "Therefore since the secular equation of the Moon, as given by the tables of Mayer, can be neither the effect of the non-sphericity of the Earth, nor that of the Moon, nor that of the action of the other planets on the Moon, and in consequence can not be explained by the means of gravitation alone, it follows that, if this equation is real, it comes from some other cause, such as the resistance the Moon feels from some very rare fluid, in which it moves; however, on the other hand, the hypothesis of a very subtle fluid, whose resistance sensibly alters the movement of the celestial bodies, is not very well confirmed by the observations of the other planets, [and] at the same time appears to be contradicted by the case of Saturn, whose movement slows in place of accelerating, which it would need to by virtue of the resistance of the ether, it seems to me that one can not admit this hypothesis only to explain the question of the secular equation." (Lagrange [1773], 385-6)

Lagrange's rejection of the resisting ether is important, for it shows the remarkable shift in opinion: from the essays of Bossut and Johann Euler in 1762, which evidenced certainty in the existence of a resisting medium, to Lagrange's opinion a dozen years later, about the certain lack of existence of the same. In fact, neither Lagrange nor Laplace feel the need to introduce a resisting medium to explain the remaining inequalities in the motions of the planets, and both deny its plausibility and existence, at various times and places.

In Lagrange's case, one has the remarkable (and extreme) view that *because* theory cannot account for a phenomenon, *then* the phenomenon must not exist, or at least, it is an artifact of imperfect observation:

[2]It is interesting to note that while Lagrange raises the issue (mentioned by Mayer and Flamsteed) of a very long period variation (his "very large multiples"), he does not seem to make a connection between such a variation which, on historic time, would appear to be a secular variation, and the apparent secular acceleration of the Moon; ultimately, Laplace will explain that such a long period variation is what is responsible for the Moon's secular acceleration.

> "I say: *if this equation is real*; because it appears to me that the current proofs are not quite definitive, since they are based only on some observations made in centuries long past, the exactitude of which one may hardly depend." (Lagrange [1773], 386; italics are Lagrange's.)

Lagrange then attacks the validity of the observations.

> "Thus in this manner the errors[3] of the Table of Dunthorn are nearly enough, in the same interval of time:
>
> $$-54', -20', -4', +8', +10';$$
>
> and, if one supposes that these errors are due to an equation which increases as the square of time, and which need moreover change the epoch and the mean movement of the Tables, it is clear that the second differences must be constant..." (Lagrange [1773], 389)

However, they are not constant, and thus he concludes that they are not caused by a secular acceleration whose value is proportional to the square of the time: the apparent change in the mean motion must be caused by some other effect, most likely observational error.

8.2 Researches on the Secular Equation of the Nodes and Inclinations of the Orbits of the Planets, 1774

Shortly after finishing work on the Moon's secular variation, Lagrange began to examine the variation of the elements of the planets. He expressed his interest to Laplace, in one of the earliest letters between the two, March 15, 1773. Here Lagrange cites his intention of returning to the theory of Jupiter and Saturn, and especially their secular equations:

> "Regarding my theory of Jupiter and Saturn, as this is only an attempt, it could be that the secular equations that I have deduced are not exact for want of not having pushed the approximation far enough; this is also one of the matters that I propose to discuss anew when I have completed some other works; I will be happy to have been forewarned by you if your researches leave me nothing more to say on this subject." (Lagrange [1773A], 57)

In other words, though Lagrange intends to begin working anew on the problem of the secular equations, he would not be disappointed if Laplace were to complete his work first. This Laplace does not do, however; in fact, Laplace will produce no more works on celestial mechanics until the mid-1780s.

[3] Lagrange means the deviation between observation and calculation.

The first of Lagrange's works to appear was "Reseraches on the Secular Equation of the Nodes and the Inclinations of the Orbits of the Plants" [1774], presented to the French Academy in 1774. This work is a marked departure from Laplace's work on the subject: rather than attempt to derive, from the differential equations of orbit, the radius vector r and from it determine the secular variation in this quantity, Lagrange examines the variations of the elements of orbit directly; in this particular case, it is the nodes and inclinations which are to be examined. As we shall see, this has an immediate effect on the work of Laplace, who will then add a section to a work of his included in the upcoming 1776 volume of the Memoirs for the Academy, using Lagrange's methods to determine the variation of the eccentricities and aphelion positions of the planets.

[1774] sets the stage for Lagrange's 1776 article, and his 1781 extension. More importantly, [1774] established many general *methods* that Lagrange would use in 1781, and is more clearly linked to that work than to [1776]. The similarities suggest that after Lagrange finished his 1774 piece, he attempted to apply the methods to the mean motion, but was unable to arrive at a satisfactory solution, and instead proceeded in a different direction. Later, he re-examined the problem and produced his second proof of stability, published in 1781.

The first mention of [1774] appears in a letter to d'Alembert, dated June 6, 1774. Lagrange had been occupying himself with a problem relating to the line of the nodes of several planes, whose inclinations maintained a constant ratio.

> "When there are only two mobile planes, I can give a complete solution to the problem; but, if I suppose a larger number, I find the formulas absolutely intractable. However I have found a particular method for treating the case of as many mobile planes as one wants, but only in the hypothesis that the mutual inclinations are all very small, as well as the movements of the nodes, which is the case of the planetary orbits. If you find this matter interesting, I can compose a Memoir for the Academy ..." (Lagrange [1774A], 287)

Just under six weeks later, Lagrange wrote a letter to Condorcet, where he speaks of his progress on the work. On July 18, 1774, Lagrange writes:

> "The memoir on the movement of the nodes and the variation of the inclinations of the orbits of the planets of which I have already spoken to you, and which I have set aside for your Academy, if [the Academy] will permit me to present it to [them], will soon be finished. It contains a new theory on this matter, and the numerical application to each of the primary planets, as well as to the satellites of Jupiter; but I want to send it to you only on the condition that you and M. d'Alembert would judge it in advance and suppress it in the case that you find it not worthy of the Academy." (Lagrange [1774B], 27)

Lagrange delivered the paper, and Condorcet and d'Alembert must have given it their recommendation, for [1774] was read before the Academy on December 15, 1774.

Lagrange begins with the differential equations of orbit:

$$\tfrac{d^2x}{dt^2} = -X \qquad \tfrac{d^2y}{dt^2} = -Y \qquad \tfrac{d^2z}{dt^2} = -Z$$

where X, Y, and Z are the forces in the corresponding directions. By appropriate manipulation, these yield:

$$\frac{xd^2y - yd^2x}{dt^2} = Xy - Yz$$

$$\frac{yd^2z - zd^2x}{dt^2} = Xz - Zx$$

$$\frac{yd^2z - zd^2y}{dt^2} = Yz - Zy$$

of which the left hand sides are exact; hence, denoting

$$P = \int (Yz - Zy)dt \qquad Q = \int (Xz - Zx)dt \qquad R = \int (Xy - Yx)dt$$

one has

$$\tfrac{xdy-ydx}{dt} = R \qquad \tfrac{xdz-zdx}{dt} = Q \qquad \tfrac{ydz-zdy}{dt} = P$$

and consequently

$$Px - Qy + Rz = 0$$

by multiplying by the appropriate variable and adding (Lagrange [1774], 636-7).

If P, Q and R are constant, this is the equation for a fixed plane passing through the origin of the coordinate axes and the line of the nodes (relative to the x axis) has a tangent of $\frac{P}{Q}$, and the inclination of the plane has a tangent of $\frac{\sqrt{P^2+Q^2}}{R}$. From this, Lagrange will derive simple equations relating the tangent of the inclination and the position of the nodes, which he will subsequently use to determine their variations. This is the important innovation of Lagrange in [1774]: unlike the work of the Eulers, and his own [1765] and [1766], Lagrange needs at no point the actual solution to the equations of motion, but can work directly from the differential equations.

Lagrange then begins a brief discussion of what would cause P, Q, R to be constant (i.e., the path would always remain in the same plane), and then what would cause the ratios between P, Q, R to be the same (i.e., the line of the nodes and the angle of inclination would always remain the same) (Lagrange [1774], 637-8).[4]

[4] This second case is of greater interest; for example, in the plane $atx + bty + ctz = 0$, where t is a parameter, the values of the coefficients of x, y, z change as t changes, but the plane remains the same. It is probably for this reason, or one very similar, that Lagrange considers the second case.

Lagrange calls the angle of the line of the nodes ω, and θ the tangent of the inclination; hence:

$$\theta \sin \omega = \tfrac{P}{R} \quad \theta \cos \omega = \tfrac{Q}{R}$$

which has the advantage of eliminating the radical (Lagrange [1774], 639). By introducing these "conjugated" quantities in place of the traditional elements of orbit, Lagrange greatly simplifies the resulting equations of motion.[5] Lagrange's treatment of these conjugated quantities is different from all his predecessors: he sees them as a goal in and of themselves.

Changing into polar coordinates, where r is the projection of the radius vector onto the xy plane, q is the longitude, and p the tangent of its latitude, i.e.:

$$x = r \cos q, y = r \sin q, z = rp$$

and substituting into the equation $Px - Qy + Rz = 0$, one gets

$$P \cos q - Q \sin q + Rp = 0$$

which "serves to determine p." (Lagrange [1774], 640). This is because one may substitute in for P and Q their values $R\theta \sin \omega$ and $R\theta \cos \omega$, respectively, and derive:

$$p = \theta \sin(q - \omega)$$

(which, Lagrange notes, may also be derived from spherical trigonometry) (Lagrange [1774], 640).

For simplicity, Lagrange introduces

$$s = \theta \sin \omega, u = \theta \cos \omega$$

and thus

$$p = u \sin q - s \cos q$$

Likewise, from

$$Rs = P, Ru = Q$$

one obtains the differential equations

$$R\frac{ds}{dt} + \frac{dR}{dt}s = Yz - Zy$$

$$R\frac{du}{dt} + \frac{dR}{dt}u = Xz - Zx$$

which will be used to determine s and u (Lagrange [1774], 640).[6]

To apply this to the case of the solar system (and of a planet T being affected by the other planets), Lagrange must introduce the function which determines

[5] Conjugated quantities like this are not new to Lagrange; the semilatus rectum, whose value appears often in the work of the Eulers, is the product of the semimajor axis with $1-e^2$. The issue is very similar to the idea of "natural coordinates" for differential equations, and some variation or other of these "natural coordinates" will form the heart of theoretical celestial mechanics for the century following Lagrange.

[6] Recall that $P = \int (Yz - Zy)dt$, and hence $dP = Yz - Zy$.

the forces X, Y, Z. First, he introduces the parameters $(TS), (T_1S), \ldots$, the distance between each planet $T, T_1, T_2, \ldots$ and the sun (assumed to be located at the center of the coordinate system). The quantities $(TT_1), (TT_2), \ldots, (T_1T_2), \ldots$ are the distances between the planets and each other. Hence the planet T is attracted to the sun, and planets $T_1, T_2, \ldots$ by the forces

$$\frac{S}{(TS)^2}, \frac{T_1}{(TT_1)^2}, \frac{T_2}{(TT_2)^2}, \ldots$$

which can be decomposed along the coordinate axes. Thus the force on T is:

$$\frac{Sx}{(TS)^3} + \frac{T_1(x - x_1)}{(TT_1)^3} + \frac{T_2(x - x_2)}{(TT_2)^3} + \ldots$$

$$\frac{Sy}{(TS)^3} + \frac{T_1(y - y_1)}{(TT_1)^3} + \frac{T_2(y - y_2)}{(TT_2)^3} + \ldots$$

$$\frac{Sz}{(TS)^3} + \frac{T_1(z - z_1)}{(TT_1)^3} + \frac{T_2(z - z_2)}{(TT_2)^3} + \ldots$$

Meanwhile, however, the sun is likewise attracted by all the planets with the forces

$$\frac{T}{(TS)^2}, \frac{T_1}{(T_1S)^2}, \frac{T_2}{(T_2S)^2}, \ldots$$

which are similarly decomposed into forces along the coordinate axis to yield, for X, Y and Z:

$$X = \Big[\frac{S+T}{(TS)^3} + \frac{T_1}{(TT_1)^3} + \frac{T_2}{(TT_2)^3} + \ldots\Big]x + T_1\Big[\frac{1}{(T_1S)^3} - \frac{1}{(TT_1)^3}\Big]x_1 + T_2\Big[\frac{1}{(T_2S)^3} - \ldots$$

$$Y = \Big[\frac{S+T}{(TS)^3} + \frac{T_1}{(TT_1)^3} + \frac{T_2}{(TT_2)^3} + \ldots\Big]y + T_1\Big[\frac{1}{(T_1S)^3} - \frac{1}{(TT_1)^3}\Big]y_1 + T_2\Big[\frac{1}{(T_2S)^3} - \ldots$$

$$Z = \Big[\frac{S+T}{(TS)^3} + \frac{T_1}{(TT_1)^3} + \frac{T_2}{(TT_2)^3} + \ldots\Big]z + T_1\Big[\frac{1}{(T_1S)^3} - \frac{1}{(TT_1)^3}\Big]z_1 + T_2\Big[\frac{1}{(T_2S)^3} - \ldots$$

respectively (Lagrange [1774], 643).

From here, he obtains the expressions for $Yz - Zy$ and $Xz - Zx$, which appear in the differential equations for $\frac{ds}{dt}$ and $\frac{du}{dt}$.

In order to simplify these expressions, Lagrange expresses $x, y, z, x_1, y_1, z_1, \ldots$ in terms of $r, q, s, u, r_1, q_1, s_1, u_1, \ldots$. Hence x, y, z are:

$$r \cos q, r \sin q, r(u \sin q - s \cos q)$$

respectively (Lagrange [1774], 644). Likewise:

$$y_1 z - y z_1 = \frac{rr_1}{2}(u - u_1)[\cos(q - q_1) - \cos(q + q_1)]$$

$$+ \frac{rr_1}{2}(s + s_1) \sin(q - q_1) - \frac{rr_1}{2}(s - s_1) \sin(q + q_1)$$

and

$$x_1z - xz_1 = \frac{rr_1}{2}(s - s_1)[\cos(q - q_1) + \cos(q + q_1)]$$
$$+\frac{rr_1}{2}(u + u_1)\sin(q - q_1) + \frac{rr_1}{2}(u - u_1)\sin(q + q_1)$$

and likewise for similar quantities; finally:

$$(TS) = r\sqrt{1 + (u \sin q - s \cos q)}$$

$$(T_1S) = r_1\sqrt{1 + (u_1 \sin q_1 - s_1 \cos q_1)}$$

$$\vdots$$

$$(TT_1) = \sqrt{r^2 - 2rr_1\cos(q - q_1) + r_1^2 + (ru \sin q - rs \cos q - r_1u_1 \sin q_1 + r_1s_1 \cos q_1)^2}$$

$$(TT_2) = \sqrt{r^2 - 2rr_2\cos(q - q_2) + r_2^2 + (ru \sin q - rs \cos q - r_2u_2 \sin q_2 + r_2s_2 \cos q_2)^2}$$

$$\vdots$$

Now, Lagrange remarks, as the orbits of the planets are very little inclined to the ecliptic, the quantities $\theta, \theta_1, \ldots$ and hence $u, s, u_1, s_1, \ldots$ are very small and thus, "at least in the first calculation", they may be neglected in the quantities $(TS), (T_1S), \ldots$ (Lagrange [1774], 644). Thus, after substituting these quantities into the differential equations above, one obtains:

$$R\frac{ds}{dt} + \frac{dR}{dt}s = \frac{T_1rr_1}{2}\left(\frac{1}{r_1^3} - \frac{1}{[r^2 - 2rr_1\cos(q - q_1) + r_1^2]^{\frac{3}{2}}}\right)$$

$$\times\left[(u-u_1)\cos(q-q_1)+(s+s_1)\sin(q-q_1)-(u-u_1)\cos(q+q_1)-(s-s_1)\sin(q+q_1)\right]$$

$$+\frac{T_2rr_2}{2}\left(\frac{1}{r_2^3} - \frac{1}{[r^2 - 2rr_2\cos(q - q_2) + r_2^2]^{\frac{3}{2}}}\right)$$

$$\times[(u-u_2)\cos(q-q_2)+(s+s_2)\sin(q-q_2)-(u-u_2)\cos(q+q_2)-(s-s_2)\sin(q+q_2)]$$

$$+\ldots$$

$$R\frac{du}{dt} + \frac{dR}{dt}u = \frac{T_1rr_1}{2}(\frac{1}{r_1^3} - \frac{1}{[r^2 - 2rr_1\cos(q - q_1) + r_1^2]^{\frac{3}{2}}})$$

$$\times[-(s-s_1)\cos(q-q_1)+(u+u_1)\sin(q-q_1)-(s-s_1)\cos(q+q_1)+(u-u_1)\sin(q+q_1)]$$

$$+\frac{T_2rr_2}{2}(\frac{1}{r_2^3} - \frac{1}{[r^2 - 2rr_2\cos(q - q_2) + r_2^2]^{\frac{3}{2}}})$$

$$\times[-(s-s_2)\cos(q-q_2)+(u+u_2)\sin(q-q_2)-(s-s_2)\cos(q+q_2)+(u-u_2)\sin(q+q_2)]$$

$$+\ldots$$

Furthermore, if one regards the orbits as nearly circular, then $r, r_1, r_2, \ldots$ can be assumed to be constant, and thus $q, q_1, q_2, \ldots$ are proportional to the time and hence:

$$q = \mu t, q_1 = \mu_1 t, q_2 = \mu_2 t, \ldots$$

where $\mu, \mu_1, \mu_2, \ldots$ are constants (Lagrange [1774], 645). Since $R = \frac{r^2 dq}{dt}$, then $R = \mu r^2$; likewise, $R_1 = \mu_1 r_1^2, \ldots$, which are all constant quantities (Lagrange [1774], 645).

Finally, Lagrange expands the term $[r^2 - 2rr_1 \cos(q - q_1) + r_1^2]^{-\frac{3}{2}}$ as:

$$(r, r_1) + (r, r_1)_1 \cos(q - q_1) + (r, r_1)_2 \cos 2(q - q_1) + (r, r_1)_3 \cos 3(q - q_1) + \ldots$$

where $(r, r_1), (r, r_1)_1, \ldots$ are functions of r, r_1, found by "known methods" (Lagrange [1774], 646).[7] Likewise, other expressions of the same type can be expanded.

After these substitutions are made, the equations may be solved for $\frac{ds}{dt}$ and $\frac{du}{dt}$:

$$\frac{ds}{dt} + \frac{T_1 r_1 (r, r_1)_1}{4\mu r}(u - u_1) + \frac{T_2 r_2 (r, r_2)_1}{4\mu r}(u - u_2) + \ldots + \Pi = 0$$

$$\frac{du}{dt} + \frac{T_1 r_1 (r, r_1)_1}{4\mu r}(s - s_1) + \frac{T_2 r_2 (r, r_2)_1}{4\mu r}(s - s_2) + \ldots + \Psi = 0$$

where Π and Ψ "denote the totality of terms which contain the variables u and s combined with sines or cosines." (Lagrange [1774], 646) (In other words, Π and Ψ represent the non-linear portions of the differential equations)

What Lagrange has done is "decoupled" the differential equations, into a periodic portion and a non-periodic portion, and he will make use of this separation in the solution to the problem. First, he ignores the terms Π and Ψ:

> "now, as there is no term in the quantities Π and Ψ which is not multiplied by the sine or cosine of the angles $q, q_1, q \pm q_1, \ldots$, it is clear that these quantities can produce in the values of s and u inequalities dependent only on the place of the planets in their orbit, from which, as one wants to make abstraction of these sorts of inequalities and find only the movement of the nodes and the variation of the inclination insofar as they are independent of the movement of the planets in their orbit, one can first of all reject the quantities which act on them, which renders the differential equations in $s, u, s_1, u_1, \ldots$ very simple and very easy to integrate." (Lagrange [1774], 646-7)

Though he never mentions the words "secular inequalities", it is clear that these are the inequalities he is concerned with: they are the quantities that do not depend on the positions of the planets in their orbit.[8]

[7] Presumably Lagrange means one of the various series expansion techniques.

[8] These quantities can, to first order, be expected to repeat themselves, as the positions of the planets repeat themselves, and are of no concern to Lagrange here.

Lagrange's method is simple: he decouples the equations, first solving for the non-periodic terms, and leaving the periodic terms. For brevity:

$$\begin{array}{lll}(0,1)=\frac{T_1 r_1 (r,r_1)_1}{4\mu r} & (0,2)=\frac{T_2 r_2 (r,r_2)_1}{4\mu r} & \ldots \\ (1,0)=\frac{T r (r_1,r)_1}{4\mu_1 r_1} & (1,2)=\frac{T_2 r_2 (r_1,r_2)_1}{4\mu_1 r_1} & \ldots \\ (2,0)=\frac{T r (r_2,r)_1}{4\mu_2 r_2} & (2,1)=\frac{T_1 r_1 (r_2,r_1)_1}{4\mu_2 r_2} & \ldots\end{array}$$

and thus (ignoring the purely periodic quantities Π and Ψ):

$$\frac{ds}{dt}+(0,1)(u-u_1)+(0,2)(u-u_2)+\ldots=0$$

$$\frac{du}{dt}-(0,1)(s-s_1)-(0,2)(s-s_2)+\ldots=0$$

$$\frac{ds_1}{dt}+(1,0)(u_1-u)+(1,2)(u_1-u_2)+\ldots=0$$

$$\frac{du_1}{dt}-(1,0)(s_1-s)-(1,2)(s_1-s_2)+\ldots=0$$

$$\vdots$$

Of these, Lagrange has high expectations:

> "It is by the integration of these equations that one can reach a single exact solution of the problem of the movement of the nodes and the variation of the inclination of the orbits of the various planets $T, T_1, T_2, \ldots$ by virtue of their mutual attractions." (Lagrange [1774], 648)

Lagrange then analyzes the two-planet case, with the second planet's orbit T_1 fixed (i.e., s_1 and u_1 are constant); in this case, the differential equations become simply:

$$\frac{ds}{dt}+(0,1)u=0, \frac{du}{dt}-(0,1)s=0$$

which can be reduced to

$$\frac{d^2s}{dt^2}+(0,1)^2 s=0$$

which yields the solutions

$$s=A\sin[\alpha-(0,1)t]$$

$$u=A\cos[\alpha-(0,1)t]$$

where A, α are constants of integration.

Recall that $s=\theta\sin\omega, u=\theta\cos\omega$, where ω was the line of the nodes and θ was the tangent of the inclinations; hence:

$$\tan\omega=\frac{s}{u}=\tan[\alpha-(0,1)t]$$

and thus

$$\omega = \alpha - (0,1)t$$

and the line of the nodes regresses at the constant rate $(0,1)$. Since the rate of regression depends on $(0,1)$, which is $\frac{T_1 r_1 (r,r_1)_1}{4\mu r}$, this rate is known completely.

Moreover:

$$\theta = \sqrt{s^2 + u^2} = A$$

and hence the tangent of the inclination is a constant (Lagrange [1774], 648-9). This must be the "complete solution" of the problem mentioned to d'Alembert in the June 6, 1774 letter.[9]

Lagrange then extends the method, applying it to the orbits "two by two", first assuming one then the other is motionless. The result is:

$$\begin{array}{ll} s = A\sin(at+\alpha) & u = A\cos(at+\alpha) \\ s_1 = A_1\sin(at+\alpha) & u_1 = A_1\cos(at+\alpha) \\ s_2 = A_2\sin(at+\alpha) & u_2 = A_2\cos(at+\alpha) \end{array}$$

(As the section immediately following will show, Lagrange is actually setting $s = A\sin(at+\alpha) + B\sin(bt+\beta) + \ldots$, and u to the corresponding cosine term.) (Lagrange [1774], 655).

From this, Lagrange derives:

$$aA + (0,1)(A - A_1) + (0,2)(A - A_2) + \ldots = 0$$

$$aA_1 + (1,0)(A_1 - A) + (1,2)(A_1 - A_2) + \ldots = 0$$

$$aA_2 + (2,0)(A_2 - A) + (2,1)(A_2 - A_1) + \ldots = 0$$

derived by substituting the above values for $s, u, s_1, u_1, \ldots$ into the differential equations for $\frac{ds}{dt}$, and then comparing coefficients (Lagrange [1774], 656).

This gives a system of $n+1$ equations in the $n+1$ unknowns $A, A_1, \ldots$.[10]. By eliminating these constants, one ends with an equation of degree n in a. By determining the solutions of a, which Lagrange designates as $a, b, c, \ldots$, one obtains the solution:

$$s = A\sin(at+\alpha) + B\sin(bt+\beta) + \ldots$$

$$s_1 = A_1\sin(at+\alpha) + B_1\sin(bt+\beta) + \ldots$$

$$\vdots$$

$$u = A\cos(at+\alpha) + B\cos(bt+\beta) + \ldots$$

$$u_1 = A_1\cos(at+\alpha) + B_1\cos(bt+\beta) + \ldots$$

$$\vdots$$

[9] See above.

[10] Actually, it is two sets of $n+1$ equations, the second involving the unknowns $B, B_1, \ldots$.

which serve to determine the quantities s and u.[11]

This assumes that all the roots are real and distinct. If they are not, then the quantities $s, s_1, s_2, \ldots, u, u_1, u_2, \ldots$ contain arcs of circles (linear terms) or exponentials. At this point, Lagrange uses observational evidence to bolster his conclusion:

> "Before ending this Article, we need again to remark that, while we have supposed that all of the roots $a, b, c, \ldots$ of the equation in x are real and unequal, it can nonetheless turn out that some of them are equal or imaginary; but it is easy to resolve this case by known methods: we observe only that, in the case of equal roots, the values of $s, s_1, s_2, \ldots, u, u_1, u_2, \ldots$ contain arcs of the circle, and that in the case of imaginary roots these values contain ordinary exponentials; from which, in one and the other case, the quantities which they represent increase in measure as t increases; in consequence the preceding solutions cease to be exact after a certain time (23); but fortunately, this case does not appear to have place in the system of the world." (Lagrange [1774], 665-6)

The assumption of real, distinct roots is an important one, and not, in general, justified, though later in the article Lagrange will calculate these quantities and show that for the solar system, in any case, this assumption is legitimate.

In the multiple planet case, it should be pointed out, the inclination of the orbit is not necessarily constant. Unfortunately, while the two planet case allows a simple determination of the inclination and the progression of the nodes, the multiple planet case has the rather more complicated expressions:

$$\tan\omega = \frac{A\sin\alpha + B\sin(bt+\beta) + C\sin(ct+\gamma) + \ldots}{A\cos\alpha + B\cos(bt+\beta) + C\cos(ct+\gamma) + \ldots}$$

$$\theta = \sqrt{\begin{array}{c} A^2 + B^2 + C^2 + \ldots + 2AB\cos(bt+\beta-\alpha) \\ +2AC\cos(ct+\gamma-\alpha) + \ldots + 2BC\cos[(b-c)t+\beta-\gamma] + \ldots \end{array}}$$

The examination of these equations makes up the next sizable portion of [1774]. In particular, the position of the nodes, ω is constant in the two-planet case, but is clearly variable in the multiple planet case, and might, conceivably, be unbounded, though as far as stability of the orbits of the planets is concerned, the position of the nodes is not important

[11] It can be seen that Lagrange's assumption of all the s and u changing at the same rate is thus not necessarily a damaging assumption. The same would be accomplished by assuming that:

$$s = A\sin(at+\alpha) + B\sin(bt+\beta) + \ldots$$
$$u = A\cos(at+\alpha) + B\cos(bt+\beta) + \ldots$$
$$s_1 = A_1\sin(at+\alpha) + B_1\sin(bt+\beta) + \ldots$$
$$u_1 = A_1\cos(at+\alpha) + B_1\cos(bt+\beta) + \ldots$$

By substituting these into the equation for $\frac{ds}{dt}$, $\frac{du}{dt}$ one obtains, for the coefficient of $\sin(at+\alpha)$ and $\cos(at+\alpha)$ the above equations.

More important, as far as stability is concerned, is θ, the tangent of the inclinations:

> "38. Regarding the tangent of the inclination of the orbit θ, it is clear that it will necessarily always be contained in certain limits, at least when the roots $b, c, \ldots$ are never equal or imaginary (31,32).
>
> If there are only two mobile orbits, one has
>
> $$\theta = \sqrt{A^2 + B^2 + 2AB\cos(bt + \beta - \alpha)};$$
>
> and it is obvious that the two limits of θ are $A + B$ and $A - B$.
>
> In general, it is easy to see that the value of θ will necessarily always be contained between the greatest and the least values of the quantity:
>
> $$\pm A \pm B \pm C \pm \ldots$$
>
> in assigning the signs as desired; but if one wants to determine exactly the maximum and minimum of θ, he needs to resolve the equation
>
> $$bAB\sin(bt + \beta - \alpha) + cAC\sin(ct + \gamma - \alpha) + \ldots$$
> $$+(b - c)BC\sin[(b - c)t + \beta - \gamma] + \ldots = 0$$
>
> which is not easy when there is more than one term." (Lagrange [1774], 673-4)

Lagrange does not even attempt to solve this problem in general; this is almost certainly what Lagrange meant by the problem of more than two bodies being "absolutely intractable".[12]

The remainder of the article consists of Lagrange using the formulas to determine the variations of the nodes and inclinations of the five principal planets.

Lagrange was in the process of applying the same methods to the eccentricities and the position of the aphelion of the orbit when Laplace's [1772A] appeared, anticipating Lagrange's work (see below).

On December 17, 1774, Laplace read the second part of [1772A] to the Academy. Laplace clearly indicates his debt to Lagrange:

> "...I have shown that the mean motion of the planets and, in consequence, their mean distance is not subject to any secular equation, by virtue of their action on one another;[13] but the forms which I have reached can only hold for a limited time, after which they become inexact, and, although they appear to me sufficient for all the

[12]Again, in his letter to d'Alembert. One is reminded of the difference between Lagrange, the pure mathematician, satisfied that the problem *has* a solution of the desired form — i.e., one that is bounded — and Laplace, the applied mathematician, who would almost certainly have determined the actual extreme values of the inclination. Lagrange keeps the two activities well compartmentalized.

[13]This was in [1773]

> time during which astronomy has been cultivated, especially with respect to quantities proportional to the square of the time (*M. E. art. 56*), however, it would be very interesting from the standpoint of analysis, to have the exact expression of these inequalities. That which I have given is only the differentials; I have proposed to myself for a long time to integrate them; but the small utility of this calculation for the need of astronomy, combined with the difficulties which they present, caused me to abandon this idea, and I avow that I would not have returned [to it], without the excellent Memoir *On the Secular Inequalities of the Movement of the Nodes and the Inclination of the Planetary Orbits* that M. de Lagrange sent to the Academy, and which appears in the following volume." (Laplace [1772A], 354-5)

Laplace's work follows Lagrange's developments very closely, dealing with the eccentricities and aphelia in much the same manner that Lagrange dealt with the inclinations and nodes.[14]

Laplace lets $a, a', a'', \ldots$ be the mean distances of the planets $p, p', \ldots$; $ea, e'a', e''a'', \ldots$ be their excentricities (thus e is the eccentricity as we call it today); $L, L', L'', \ldots$ the longitude of the aphelion;, $\gamma, \gamma', \gamma'', \ldots$ the tangents of the inclination of their orbits; $\Gamma, \Gamma', \Gamma'', \ldots$ the longitude of the nodes, and $\delta\mu, \delta\mu', \delta\mu'', \ldots$ their masses relative to the sun, $i, i', i'', \ldots$ the number of revolutions since a given epoch (since the start of their motions). Supposing the inclinations and excentricities are very small, and

$$\tfrac{a'}{a} = z \quad \text{and} \quad \tfrac{1}{(1-2z\cos\theta+z^2)^{\frac{3}{2}}} = b + b_1\cos\theta + \ldots$$

Laplace sets $di \cdot 360°$ as the mean angle described by p around the sun while the Earth describes the angle dt. Finally, he defines:

$$(0,1) = \frac{1}{4} z b_1 \delta\mu' \frac{di \cdot 360°}{dt}$$

$$\overline{(0,1)}dt = \frac{1}{2}[b_1(1+z^2) - 3bz]\delta\mu' di \cdot 360°$$

Recall these quantities appeared in the equations for the change in the eccentricity and apogee in [1773], hence Laplace's choice of these quantities. This is the difference between Lagrange's [1774] and Laplace's [1772A]: in [1774], Lagrange derives the equations for s and u directly from the equations of motion; Laplace uses his earlier derivation of the elements of orbit from the truncated series solutions to the equations.

[14] The date that the paper was registered was only two days after Lagrange's paper was read, which suggests either that Laplace had worked out much of the development and put the finishing touches on the work after hearing Lagrange's paper (which, given the large departure in method from Laplace's earlier work, seems unlikely); or that Laplace developed his equations in two days (which is possible); or that Laplace had access to Lagrange's work earlier (recall that [1774] was completed sometime after mid-July) and delayed publication to give Lagrange due priority.

With the quantities $(0,1)$ and $\overline{(0,1)}$ established, Laplace may now write:

$$dL = (0,1)dt - \overline{(0,1)}\frac{e'}{e}\cos(L'-L)dt + (0,2)dt + \dots$$

$$de = \overline{(0,1)}e'dt\sin(L'-L) + \overline{(0,2)}e''dt\sin(L''-L) + \dots$$

$$-d\Gamma = (0,1)\Big[1 - \frac{\gamma'}{\gamma}\cos(\Gamma'-\Gamma)\Big]dt + \dots$$

$$-d\gamma = (0,1)\gamma'\sin(\Gamma'-\Gamma)dt + \dots$$

Note that here, unlike in [1773], Laplace is considering more than two planets. Moreover, he assumes that the effects are additive: to obtain the total change in the eccentricity, one need determine the change of the eccentricity caused by each of the perturbing planets, "two by two", just as Lagrange does.

Akin to Lagrange in [1774], Laplace writes:

$$\begin{array}{cc} x = e\sin L & z = \gamma\sin\Gamma \\ y = e\cos L & s = \gamma\cos\Gamma \\ x' = e'\sin L' & z' = \gamma'\sin\Gamma' \\ y' = e'\cos L' & s' = \gamma'\cos\Gamma' \end{array}$$

(thus, Laplace's x and y play a role similar to Lagrange's s and u. Laplace forgets that he had introduced z in another context earlier; from this point forward, his previous definition of z is forgotten. This is yet another piece of evidence that indicates that this portion of [1772] was actually written much later than the first portion (Laplace [1772], p. 357).)

Laplace obtains the differential equations:

$$de = \frac{xdx + ydy}{e}$$

and

$$\cos L dL = \frac{edx - xde}{e^2}$$

from $e = \sqrt{x^2+y^2}$ and $\sin L = \frac{x}{e}$, respectively (Laplace [1772], 357). Hence

$$dL = \frac{ydx - xdy}{e^2}$$

Moreover:

$$\sin(L'-L) = \sin L'\cos L - \sin L\cos L' = \frac{yx' - xy'}{ee'}$$

$$\cos(L'-L) = \sin L'\sin L + \cos L'\cos L = \frac{xx' + yy'}{ee'}$$

From this, Laplace obtains

$$ydx - xdy = (x^2+y^2)(0,1)dt - \overline{(0,1)}(xx' + yy')dt + \dots$$

$$xdx + ydy = \overline{(0,1)}(yx' - xy')dt$$

by substituting in the appropriate values for x, y, dx, dy, dL, de (Laplace [1772], 357-8).

By multiplying the first by y and the second by x and dividing the result by $(x^2 + y^2)$, he obtains the differential equations for dx, dy, dx', dy':

$$dx = dx\Big((0,1)y - \overline{(0,1)}y' + (0,2)y - \overline{(0,2)}y'' + (0,3)y - \overline{(0,3)}y''' + \dots\Big)$$

$$dy = dy\Big(-(0,1)x + \overline{(0,1)}x' - (0,2)x + \overline{(0,2)}x'' - (0,3)x - \overline{(0,3)}x''' + \dots\Big)$$

and likewise for dx', dy' (Laplace [1772A], 358).

Following Lagrange, Laplace makes:[15]

$$x = A\sin(ht + \alpha), y = A\cos(ht + \alpha)$$

$$x' = A'\sin(ht + \alpha), y' = A'\cos(ht + \alpha)$$

and obtains for x, y:

$$x = A\sin(ht + \alpha) + A'\sin(h't + \alpha') + A''\sin(h''t + \alpha'') + \dots$$

$$y = A\cos(ht + \alpha) + A'\cos(h't +' \alpha) + A''\cos(h''t + \alpha'') + \dots$$

where, by using the $2n$ equations for $x, y, x', y', \dots$ when $t = 0$ one can determine the unknown constants.[16] Laplace stops here, directing the reader to Lagrange's "very beautiful method" of determining the constants (Laplace [1772A], 361)

Laplace's method follows the Euler-Lagrange system, of determining the actual variation of the orbital quantities. However, as we have already seen, Laplace did not consider it to be the best direction for further research, and instead hoped to develop his method of "eliminating" secular terms to its best advantage. However, while he still introduces this method in his *Mécanique céléste*, Laplace will not utilize it any further in his work on the stability of the solar system, and in fact, it will be the Euler-Lagrange method that will provide the first proof of the stability of the solar system, and not any improved method of approximation, such as Laplace's [1773].

---

[15] As with Lagrange, Laplace uses

$$x = A\sin(ht + \alpha), y = A\cos(ht + \alpha)$$

to mean

$$x = A\sin(ht + \alpha) +' A\sin('ht +' \alpha) +'' A\sin(''ht +'' \alpha) + \dots$$

As before, this is a notational convenience that does not affect the results.

[16] As in Lagrange's method, these seemingly contradictory expressions of x and y stem from a method of mathematical shorthand.

8.3 On the Alteration of the Mean Motion of the Planets, 1776

A question that remained unanswered (unless one accepts the 1760 proof of Charles Euler) was the variation in the mean motion of the planets. Such a variation might exist, though Laplace's [1773] suggested it did not, and Lagrange's own [1773] contains hints that the mutual gravitational interaction of the planets could not cause a variation in the mean motion of the Moon. Could Lagrange extend the result to the mean motion of the planets? It may have been Lagrange's intention of doing so, though at one point he abandoned the field to his younger contemporary. On April 10, 1775, he writes to Laplace:

> "Sir and very illustrious colleague, I have received your Memoirs,[17] and I am obliged to you to have anticipated the pleasure of reading them. I hasten myself to thank you for them, and to note the satisfaction that your lecture has given me. The ones in which I have the most interest, are your researches on the secular inequalities. I have been proposing to myself for a long time to resume my old work on the theory of Jupiter and Saturn, to push it further and to apply it to the other planets; I had likewise planned to send to the Academy a second Memoir on the secular inequalities of the movement of the aphelia and excentricities of the planets, in which this matter will be treated in a manner analogous to that which I have determined the inequalities of the movement of the nodes and inclinations, (Lagrange's [1774]) and I have already prepared this material; but, as I see that you have undertaken this research yourself, I renounce it willingly, and I am very happy that you relieve me of this work, [and am] persuaded that science can only gain much from it." (Lagrange [1775A], 60)

Lagrange's statements suggest that he had not yet arrived at a satisfactory derivation of the variations in the eccentricities and the aphelion positions.

As for abandoning the field, however, Lagrange quickly changes his mind, and on May 29, 1775, he writes to d'Alembert:

> "I am now after a complete theory of the variations of the elements of the planets by virtue of their mutual attraction. That which M. de la Place has made on this matter has pleased me greatly, and I flatter myself that he will not be displeased with me for not holding the type of promise that I have made to him to abandon it entirely; I am not able to resist the desire to occupy myself anew with it, but I am not less delighted that he also on his side; I am even very eager to read his latest researches on this subject, but I ask him to send nothing in manuscript and to only send me the printed works; I ask

[17] Probably [1772A], according to Wilson [1985].

> you to tell him this, and give him at the [same] time a thousand compliments on my part." (Lagrange [1775B], 299)

Thus, Lagrange's "renouncement" lasted only a little over a month. D'Alembert reports back, on July 10, 1775, with high praise for Lagrange's return to the fray:

> "My dear and illustrious friend, I have communicated to M. de la Place the content of your letter which regards him, and I am charmed that you will occupy yourself again to enrich the theory of the planets by your work." (Lagrange [1775C], 302)

D'Alembert does not indicate Laplace's response to Lagrange's resumption of his analysis of the secular variation of the planets.

The only remaining important orbital quantity whose variations had not yet been determined after the work of Laplace and Lagrange was the semimajor axis (or the mean motion, as the two were directly related to each other, and no other orbital elements). Lagrange may have tried the method of [1774] to include the mean motion. However, he would not announce the discovery of the necessary conjugated quantity (the semilatus rectum, $\frac{a}{1-e^2}$), until 1781, when he publishes the intellectual descendant of [1774] (see below). Instead, he returns to the method of the Eulers: first writing the solution to the equations of motion, and from that solution, obtaining a differential expression for the desired quantity. He published his results in 1776, sixteen years after Charles Euler won an award of merit for his proof of the constancy of the mean motions of the planets. This was Lagrange's "On the Alteration of the Mean Motion of the Planets" ([1776]) read to the Academy at Berlin on October 24, 1776.

Lagrange begins by pointing to the importance of the problem:

> "One of the most important and at the same time most difficult quantities in the theory of the planets is that of their mean motions or the duration of their revolutions. The astronomers, comparing modern observations with the most ancient memoirs which we have saved, have remarked that the mean motions of Saturn, Jupiter, and the Moon are not uniform, but that Saturn appears to slow from age to age, and those of Jupiter and the Moon appear on the contrary to be subject to a continuous acceleration; consequently they introduce in the Tables of these planets secular equations which need to be applied to their supposedly uniform mean motionsx." (Lagrange [1776], 255)

Lagrange notes also that some astronomers have discerned an acceleration in the Earth's mean motion, but the acceleration is so small that it is generally neglected (Lagrange [1776], 256). He seems to be skeptical of its existence or its importance. Here Lagrange seems to be a little less certain about the non-existence of the lunar acceleration.

In any case, Lagrange does not question the value of universal gravitation (this might be considered indicative of celestial mechanics moving to a "mature" phase):

> "As the system of universal gravitation suffices to explain the periodic inequalities of the planets, it is also natural to regard this same gravitation as the cause of their secular inequality; but it is infinitely more difficult to deduce these last inequalities than the first, as much by their smallness, [as] because the most thorny and difficult calculations are necessary to assign and distinguish in the differential equations all the different terms which can produce them." (Lagrange [1776], 256)

Here we see Lagrange making a far-reaching claim on the value of universal gravitation: it has not yet explained all the periodic inequalities of the planets, as Lagrange claims, though it is well on its way to doing so. At no point in this work does Lagrange suggest that other factors might be at work, producing variations (secular or otherwise) in the orbits of the planets.

What are these secular variations? Euler found one; however:

> "M. Euler, in his first piece on the irregularities of Jupiter and Saturn, did not find any secular equation; but in his second piece on the same subject, he found a secular equation equal for both planets of $2'24''$ for the first century, counted from 1700; this is hardly in accord with the observations." (Lagrange [1776], 256)

Lagrange's own work (above) found a different value for the secular variation of Jupiter and Saturn, namely $2'', 740$ and $-14'', 221$, respectively (Lagrange [1776], 256; cf. Lagrange [1765], 667). However:

> "But M. de Laplace had extended the approximation further than I have, and has calculated more exactly the different terms which can produce the inequalities increasing as the square of the time in the mean movement of Jupiter and Saturn, and has been the first to determine that these terms compensate and cancel [each other] either entirely or nearly entirely, and consequently leave only a null or very small result that one needs to regard. And as the compensation is independent of the particular values of the orbits of Jupiter and Saturn, one can conclude, in general, that the reciprocal action of the planets cannot sensibly alter their mean movements, at least as long as their orbits are supposed to be nearly circular, and their masses very small compared with that of the sun, this being the case of all the planets of our system." (Lagrange [1776], 257)

Lagrange also comments about the Moon, the other infamous example of a secular acceleration in the solar system:

> "As for the Moon in particular, the geometers who have worked on the theory of this planet have never encountered in the differential equation of its orbit terms which give rise to a secular equation in its mean movement, however far they have carried the precision of their calculations; there remains only to examine if the nonspherical figure of the Earth and the Moon could have any influence in the mean movement of the Moon; I have done this in my piece on this question, and I have found that the terms which can produce an acceleration in the Moon's mean movement also very nearly cancel each other; [a] result analogous to that which M. Laplace [found] for Jupiter and Saturn. From this one can also conclude, in general, that the non-sphericity of the celestial bodies can not produce a sensible alteration in their mean motions." (Lagrange [1776], 257)

The implication is that since no secular acceleration can be shown to exist for the Moon, even assuming the Earth and Moon are non-spherical, nor for Jupiter and Saturn, then a similar proof ought to be possible for all the remaining bodies in the solar system, and hence none should undergo a secular acceleration. This would seem to confirm the popular belief that it was Laplace who first proved the stability of the solar system.

However, Lagrange's subsequent remarks indicate that he is dissatisfied (at least intellectually) with Laplace's achievement, and that Laplace's early work served more as a spur to development, than an end by itself. It would not be wrong to say that it was the discovery of the lack of secular variation in the *approximations* to the motion of Jupiter and Saturn that suggested to Lagrange that it might be possible to prove *theoretically* that no such variations were possible:

> "It is true that as one only reached these results by the method of approximation, one need not regard them as being rigorous; however, as the terms which give a secular equation cancel each other in the first approximation, one is led to think that it will be the same of the terms coming from successive approximations; but the calculation necessary for assured knowledge is painful by its length, [and] no one will never be tempted by the enterprise; moreover, one never can reach [this conclusion], by this means, which has approximate conclusions, and it always remains doubtful if the proposition is true in all rigor." (Lagrange [1776], 257-8)

In other words, Laplace's conclusion, derived from the *approximate* solutions to the differential equations of orbit, cannot be regarded as a *theoretical* proof of stability, since regardless of the degree of the approximation, it remains an approximation. Lagrange's assessment of Laplace's conclusion is very different from that of the Academy's comments in 1773! Lagrange continues:

> "Happily I have found a means to demonstrate it *a priori* and without supposing that the orbits of the planets are nearly circular; it

> is this that I want to develop in this memoir with all the detail due to the importance and to the difficulty of the matter." (Lagrange [1776], 258)

Lagrange uses the method of the Euler's, and examines the differential equations for the *elements* of the orbit. Lagrange's development is similar to that of Charles Euler's work of 1760, but Lagrange gives no indication that he is aware of this earlier treatise. Lagrange intends to examine the effects of the perturbing forces on the elements of orbit:

> "In this manner the derangements produced by the perturbing forces are contained in the variation of the six elements of the elliptical orbit, which are the major axis of the ellipse, the eccentricity, the position of the major axis or of the line of apsides, the inclination of the plane of the ellipse to another given plane, the position of the line of intersection of the two planes or the line of the nodes, and the epoch of the mean movement, that is to say the value of the mean longitude for a given time; and the question is reduced to determining the law of these variations, that is to say the differential values of the elements [of orbit] regarded as variable." (Lagrange [1776], 258-9)

With this statement, Lagrange dissociates himself from the previous work of Laplace, for unlike Laplace, Lagrange is not interested in the particular value for the position vector, but in the overall variation of the elements of orbit.[18] Moreover, Lagrange points out, of all the elements of orbit, only the major axis is important for determining the periodic times:

> "But for our purposes we do not need to know the variations of all these elements; because as in the invariable orbit the duration of the revolution depends only on the size of the major axis of the ellipse, it is natural thus to conclude that in the variable orbit it is likewise only the variations of the major axis which can influence the duration of the periodic times; in effect, when the variations of the elements are very small, one can without sensible error suppose that these elements remain the same during each revolution, and that they change only from one revolution to the other; and in this hypothesis it is obvious that the variations in periodic time can only come from those [variations] in the major axis." (lagrange [1776], 259)

To analyze the problem, Lagrange lets F be the centripetal force at unit distance; r the distance of the body to the center, and thus $r = \sqrt{x^2 + y^2 + z^2}$,

[18] As we have already seen, Laplace's interest to this point focuses almost entirely on the determination of the position vector, and arguments based on *its* variation, rather than determination of the overall variations. Of Laplace's work during the 1770s, only [1772A], inspired by Lagrange's [1774], deals with the variation of the elements of orbit.

$\frac{F}{r^2}$ is the value of the centripetal force, with components $\frac{Fx}{r^3}, \frac{Fy}{r^3}, \frac{Fz}{r^3}$ and hence the differential equations of orbit are:

$$\frac{d^2x}{dt^2} + \frac{Fx}{r^3} + X = 0$$

$$\frac{d^2y}{dt^2} + \frac{Fy}{r^3} + Y = 0$$

$$\frac{d^2z}{dt^2} + \frac{Fz}{r^3} + Z = 0$$

where X, Y and Z are perturbing forces (Lagrange [1776], 260) In the unperturbed case (where X, Y and Z equal zero), one has three second order differential equations, and thus obtains six constants of integration.

Since it is Lagrange's intention to examine the variation of these constants of integration (from which one obtains the six elements of orbit), Lagrange supposes that each constant k of integration can be written as some function V of the seven variables x, y, z, t, $\frac{dx}{dt}$, $\frac{dy}{dt}$, $\frac{dz}{dt}$, where $V = k$. This can be differentiated to

$$dV = 0$$

as k is a constant (Lagrange [1776], 261).[19] Also, since dV contains terms of the form $d\frac{dx}{dt}$, these may be replaced (from the original differential equations) by $-\frac{Fx}{r^3}dt$, and likewise for $d\frac{dy}{dt}$ and $d\frac{dz}{dt}$. In this way, no new quantities are introduced.

In the perturbed case, the quantities

$$d\frac{dx}{dt}, d\frac{dy}{dt}, d\frac{dz}{dt}$$

become

$$-\frac{Fx}{r^3}dt - Xdt, -\frac{Fy}{r^3}dt - Ydt, -\frac{Fz}{r^3}dt - Zdt$$

which are substituted into the equation of dV; however, the terms corresponding to the unperturbed equation cancel, leaving:

$$dV = \frac{dV}{d\frac{dx}{dt}}(-Xdt) + \frac{dV}{d\frac{dy}{dt}}(-Ydt) + \frac{dV}{d\frac{dz}{dt}}(-Zdt)$$

which, as $dV = dk$, is a differential equation for dk (Lagrange [1776], 262).[20] This method works perfectly well regardless of which parameter k was (hence,

[19] Lagrange means to take the differentiation with respect to t.

[20] In modern notation, we would have:

$$\frac{dV}{dt} = \frac{dk}{dt}$$

and

$$\frac{dV}{dt} = \frac{\partial V}{\partial x}\frac{dx}{dt} + \frac{\partial V}{\partial y}\frac{dy}{dt} + \frac{\partial V}{\partial z}\frac{dz}{dt} + \frac{\partial V}{\partial \frac{dx}{dt}}\frac{d\frac{dx}{dt}}{dt} + \frac{\partial V}{\partial \frac{dy}{dt}}\frac{d\frac{dy}{dt}}{dt} + \frac{\partial V}{\partial \frac{dz}{dt}}\frac{d\frac{dz}{dt}}{dt}$$

Lagrange refers many times to its generality, here and in later works: he is, perhaps, contrasting it to [1774], whose method worked primarily because of the ability to expression the nodes and inclinations as a conjugated quantity with useful properties). Thus, the remaining problem is determining the form of the function V.

Lagrange applies this to the major axis of the elliptical orbit. If p is the semilatus rectum, e the eccentricity of the ellipse, r the radius vector and ϕ the longitude from a fixed line and α the angle of the major axis from the same line, then $\phi - \alpha$ is the true longitude and

$$r = \frac{p}{1 + e\cos(\phi - \alpha)}$$

This is the important difference between [1774] and [1776]: in [1774], Lagrange obtains the differential equations for the elements of orbit directly, but in [1776], Lagrange begins with a solution to the unperturbed case and derives the differential equations from it. The former is a new innovation with Lagrange; the latter is similar to the methods employed by the Eulers.

From the properties of elliptical motion

$$r^2 d\phi = dt\sqrt{Fp}$$

and

$$p = a(1 - e^2)$$

Beginning with

$$\frac{1}{r} = \frac{1 + e\cos(\phi - \alpha)}{a(1 - e^2)}$$

and differentiating, Lagrange obtains:

$$\frac{d\left(\frac{1}{r}\right)}{d\phi} = -\frac{e\sin(\phi - \alpha)}{a(1 - e^2)}$$

Squaring the second equation, and replacing p one obtains

$$\frac{r^4 d\phi^2}{dt^2} = Fa(1 - e^2)$$

Note that $\frac{d\frac{dx}{dt}}{dt}$ and similar terms are simply $\frac{d^2x}{dt^2}$, and hence:

$$\frac{d^2x}{dt^2} = -\frac{Fx}{r^3} - X$$
$$\frac{d^2x}{dt^2} = -\frac{Fy}{r^3} - Y$$
$$\frac{d^2x}{dt^2} = -\frac{Fz}{r^3} - Z$$

Eliminating the terms corresponding to the unperturbed solution leaves one with the different expression for $\frac{dk}{dt}$, the desired element of orbit.

The first equation gives:

$$\left(\frac{a(1-e^2)}{r}-1\right)^2 = e^2\cos^2(\phi-\alpha)$$

The second gives

$$\left(a(1-e^2)\frac{d\left(\frac{1}{r}\right)}{d\phi}\right)^2 = e^2\sin^2(\phi-\alpha)$$

which gives:

$$\left(\frac{a(1-e^2)}{r}-1\right)^2 + a^2(1-e^2)^2\left(\frac{d\left(\frac{1}{r}\right)}{d\phi}\right)^2 = e^2$$

when added (Lagrange [1776], 264). This equation becomes:

$$a^2(1-e^2)^2\left(\frac{1}{r^2}+\left(\frac{d\left(\frac{1}{r}\right)}{d\phi}\right)^2\right) - \frac{2a(1-e^2)}{r} + 1 - e^2 = 0$$

or

$$a^2(1-e^2)\left(\frac{1}{r^2}+\frac{dr^2}{r^4d\phi^2}\right) - \frac{2a}{r} + 1 = 0$$

by dividing by $1-e^2$ (Lagrange [1776], 264).[21]

Now the third equation from above gives

$$1-e^2 = \frac{r^4d\phi^2}{Fadt^2}$$

which, if substituted into the above equation, gives:

$$a\left(\frac{r^2d\phi^2+dr^2}{Fdt^2}-\frac{2}{r}\right)+1=0$$

and, finally, this gives

$$\frac{1}{2a} = \frac{1}{r} - \frac{r^2d\phi^2+dr^2}{2Fdt^2}$$

which is a differential equation that determines the variation of the semimajor axis a (Lagrange [1776], 264).

[21] Also, $d(\frac{1}{r}) = -\frac{1}{r^2}dr$ and thus

$$\left(\frac{d(\frac{1}{r})}{d\phi}\right)^2 = \left(-\frac{dr}{r^2d\phi}\right)^2 = \frac{dr^2}{r^4d\phi^2}$$

To simplify this equation, Lagrange notes that $r = \sqrt{x^2+y^2+z^2}$, and also that $r^2 d\phi^2 + dr^2$ is simply the same as $dx^2 + dy^2$ and hence this simply becomes

$$\frac{1}{2a} = \frac{1}{\sqrt{x^2+y^2+z^2}} - \frac{dx^2+dy^2+dz^2}{2Fdt^2}$$

and, if $k = \frac{1}{2a}$, then this is the equation V (Lagrange [1776], 265).

Since the values of $\frac{dV}{d\frac{dx}{dt}}$ and similar partial derivatives are of interest, Lagrange calculates them. The virtue of Lagrange's form is that these partials are trivial:

$$\frac{dV}{d\frac{dx}{dt}} = -\frac{dx}{Fdt}$$
$$\frac{dV}{d\frac{dy}{dt}} = -\frac{dy}{Fdt}$$
$$\frac{dV}{d\frac{dz}{dt}} = -\frac{dz}{Fdt}$$

and thus he arrives at:

$$d\frac{1}{2a} = \frac{Xdx + Ydy + Zdz}{F}$$

(again, this is the derivative with respect to t). Lagrange says:

> "*Voilá*, as desired, a very simple formula for determining the alterations of the major axis $2a$ of the elliptical orbit of a body drawn by a central force $\frac{F}{r^2}$ and deranged by any perturbing forces X, Y, Z.
>
> "To apply this formula to answer the question which is the object of this memoir, it is clear that one needs to begin by determining the forces which act on each planet, both by virtue of the attraction of the sun and the other planets." (Lagrange [1776], 265-6)

In order to do this, Lagrange introduces what amounts to a potential function that he calls Ω. While the introduction of Ω does not make the problem any easier to solve, it has the virtue of making the differential equations involved easier to write and thus more "elegant", one of the primary differences between the methods of Laplace and Lagrange.

Letting the positions of the other planets with masses of $T', T'', \ldots$ with coordinates $x', y', z', x'', y'', z'', \ldots$ and radius vectors $r', r'', \ldots$, and $\delta', \delta'', \ldots$ be the distance between the planet of interest and the corresponding perturbing body, Lagrange defines the quantity Ω to be:

$$T'\left(\frac{xx'+yy'+zz'}{r'^3} - \frac{1}{\delta'}\right) + T''\left(\frac{xx''+yy''+zz''}{r''^3} - \frac{1}{\delta''}\right) + T'''\left(\frac{xx'''+yy'''+zz'''}{r'''^3} - \frac{1}{\delta'''}\right) + \ldots$$

and, using ∂ to indicate the total differential with regard to x, y and z, then one has, simply:

$$Xdx + Ydy + Zdz = \partial\Omega$$

and hence

$$d\frac{1}{2a} = \frac{\partial\Omega}{S+T}$$

is the new form of the differential equation of the major axis (Lagrange [1776], 267).

Suppose the mean motions of the planets $T, T', T'', \ldots$ are given by $\theta, \theta', \theta'', \ldots$ then one may obtain a series expressing r, x, y, z in terms of the sines and cosines of θ and its multiples, and similarly for $r', x', y', z', \ldots$. Hence the quantity $\frac{\Omega}{S+T}$ can be written as

$$M \times^{\sin}_{\cos} (m\theta + n\theta' + p\theta'' + \ldots)$$

M being a quantity that depends on the elements of orbit of the planets, and $m, n, p, \ldots$ being integers.

Since θ alone depends on x, y, z, then $\frac{\partial\Omega}{S+T}$ is:

$$\pm mM \times^{\sin}_{\cos} (m\theta + n\theta' + p\theta'' + \ldots)d\theta$$

Up to this point, Lagrange has kept his promise of making no compromising approximations; not only has the demonstration been exact to this point (thus removing the doubt that the proposition is true "in all rigor"), but furthermore he has not made any supposition on the form of the orbits: in particular, he has avoided the "near circularity" requirement of all previous works on the subject. However, at this point he breaks both promises.

First, he discounts the square and products of the perturbing force. This allows him to assume that the quantity M is a constant.[22] Next, by "known theorems" (Kepler's Laws), one has:

$$\theta : \theta' : \theta'' : \ldots = \sqrt{\frac{S+T}{a^3}} : \sqrt{\frac{S+T'}{a'^3}} : \sqrt{\frac{S+T''}{a''^3}} : \ldots = \frac{1}{\sqrt{a^3}} : \frac{1}{\sqrt{a'^3}} : \frac{1}{\sqrt{a''^3}} : \ldots$$

since one may neglect $T, T', T'' \ldots$ compared with the mass of the sun S.[23] Thus:

$$\theta' = \theta\sqrt{\frac{a^3}{a'^3}}, \theta'' = \theta\sqrt{\frac{a^3}{a'^3}}, \ldots$$

where Lagrange regards $a, a', a'', \ldots$ as constant; this is equivalent to a first order approximation in the semimajor axes. Since the whole purpose of this exercise is to determine the value of the variation of a, this represents another approximation, leaving Lagrange's work open to similar criticism.

If the values of $\theta', \theta'', \ldots$ are replaced with the above, then one has for $d\frac{1}{2a}$ the quantity

$$\pm mM \times^{\sin}_{\cos} \left[(m + n\sqrt{\frac{a^3}{a'^3}} + p\sqrt{\frac{a^3}{a'^3}} + \ldots)\theta\right] d\theta$$

[22] Actually, he has already assumed M is a constant in determining the differential $\frac{\partial\Omega}{S+T}$.

[23] I.e., a first order approximation in the masses of the planets.

and hence, upon integration, the value $\frac{1}{2a}$ is

$$\frac{mM \times_{\cos}^{\sin} \left[(m + n\sqrt{\frac{a^3}{a'^3}} + p\sqrt{\frac{a^3}{a'^3}} + \ldots)\theta \right]}{m + n\sqrt{\frac{a^3}{a'^3}} + p\sqrt{\frac{a^3}{a'^3}} + \ldots}$$

By this, Lagrange claims, "one knows all the inequalities which can vary the major axis $2a$ of the elliptical orbit of a planet." (Lagrange [1776], 269).

Furthermore, Lagrange notes:

> "One sees by this that the inequalities are always proportional to the sine or cosine of the angles, and consequently are necessarily periodic." (Lagrange [1776], 269)

Aside from the mathematical detail, this is precisely what Lagrange argued in the case of the Moon, in his 1773 work on the secular acceleration of the Moon: since the perturbations are periodic, the effects must be as well.

Even in this simplified case, there remains the following problem: the coefficient of θ,

$$m + n\sqrt{\frac{a^3}{a'^3}} + p\sqrt{\frac{a^3}{a'^3}} + \ldots$$

might be zero, in which case $\frac{1}{2a}$ would contain a secular term and thus the periodicity of the variations of the major axis would be threatened.[24] However, Lagrange dismisses this problem:

> "But it is easy to convince ourselves that this case can never have place in our system, where the values of $\sqrt{a^3}, \sqrt{a'^3}, \sqrt{a''^3}, \ldots$ are incommensurable with one another." (Lagrange [1776], 270)

Lagrange gives no further reason why he thinks this is the case. Certainly his statement (which amounts to having the periodic times of the planets likewise incommensurable) can neither be proven nor disproven.

Though Lagrange intends his method to be applicable to all n-body problems, he concludes with an application to the Lunar theory. He uses his method to find a new way of demonstrating the impossibility of the non-sphericity of the Earth causing the secular acceleration of the Moon:

> "Finally one can also demonstrate by this that the nonspherical figure of the Earth can not alter the Moon's mean motion; because one may imagine, as Newton did in his *Theory of the Precession of the Equinoxes*, that the particles of the Earth which form the excess sphere on the Earth are an infinitude of small moons adhering to each other, and which turn in a day around the center of the Earth,

[24]The secular term would arise from the $M \cos 0 d\theta$ term in the expansion of $d\frac{1}{2a}$, which would product a $M\theta$ term.

> it is easy to see that the action of all these particles on the Moon can produce in the major axis of its elliptical orbit only a periodic variation, as the duration of the revolution of the Moon is incommensurable with the daily rotation of the Earth." (Lagrange [1776], 270-1)

(If Lagrange thought this was the final answer to the secular acceleration of the Moon, he would be proven wrong by Laplace, who finally determined, in 1786, that they were long period variations in the mean motion, caused by a very slow —but periodic — change in the eccentricity of the Earth's orbit.)

Lagrange wrote to his mentor d'Alembert, hoping to get his impression of the work. On December 12, 1778 Lagrange asks:

> "Have you received our Volume of 1775? That of 1776 is nearly ready to appear; I will send it to you at the first occasion which presents itself; since it has some things of mine, but nothing which demands your attention, [including] whether there is perhaps a direct and general demonstration of the impossibility of the alteration of the mean movements by virtue of their reciprocal action, [a] demonstration on which I greatly desire to know your opinions." (Lagrange [1778], 343)

If d'Alembert told Lagrange what he thought of the article, the letter has not been preserved. Laplace's reaction has been, however. Condorcet had, apparently, conveyed copies of several pieces of Lagrange to Laplace (possibly the Memoirs for Berlin of 1776, which was published in 1779), and on June 9, 1779, Laplace writes enthusiastically:

> ". . . but that which has the most interest, are your researches on the alteration of the mean movement of the planets. The happy [*heureuse*] application of the beautiful method that you have revealed, at the beginning of your Memoir, on the finite partial differences, the extremely simple formula by which you reach the variation of the major axis, the very fine remark that this formula is integrable in regarding only the variation of the coordinates of the perturbed planet, and the consequence which results from it, anytime that the mean motions of the planets are incommensurable with each other, [that] the variations of their major axis are necessarily periodic; all this, joined to the elegance and simplicity of your analysis, has caused me pleasure that I can not express." (Lagrange XIV, 84)

It is clear from this that Laplace considers Lagrange's results to be a major accomplishment, and not just some intermediate step in the proof of the constancy of the mean motions. (This is further evidenced by Laplace's lavishly lauding Lagrange's achievements in other articles; see below)

In fact, Laplace even includes this method, essentially unchanged, as a second proof of the constancy (up to periodic terms) of the semimajor axes, in

Mécanique Céléste; it is the "Second Method of Approximation" (Chapter VIII, Book I).

Hence, by 1776, Lagrange had proven the invariability of the semimajor axes of the planets (up to periodic terms), and the periodic nature of the variation of the tangent of the inclination, two of three orbital elements that affect the stability of the system of the planets. More importantly, as these were obtained by using approximations that might compromise their utility, Lagrange had also supplied the method of approach that would form the basis of Laplace's later proof (again, to an approximation) of the stability of the solar system: the method of determining the variation of the orbital parameters from the differential equations of motion. Using Lagrange's methods, Laplace supplied a proof of the periodic variability of the eccentricity and a method of determining the variation of the aphelion.

What remained was for Laplace to link them all together, to produce his "celebrated equations" (as Moulton calls them), linking their masses, semimajor axes, eccentricities, and inclinations into a single equation set equal to a constant — and hence, even the maximum variations of the periodic sort had to be limited.

Chapter 9

Later Works of Lagrange and Laplace

During the 1770s, Lagrange proved in [1774] that the inclination and the nodes underwent limited variations, and using a variation of Lagrange's method, Laplace demonstrated [1772] that the perihelion position and the eccentricity were likewise constrained. Lagrange further demonstrated in [1776] that the major axes of the orbits of the planets underwent only periodic variation, and hence that the mutual gravitation of the planets caused no change in their mean motions.

What remained was to link the results into a coherent whole, and provide a general method of analyzing all the variations in the elements of orbit. Lagrange would do this in [1781], [1782], and [1783], and Laplace would link all the results of the past decade into his [1784], providing his "celebrated equation", linking the variations in the major axes, eccentricities, and inclinations.

9.1 Theory of Secular Variations, 1781 and 1782

In 1781, the Berlin Academy of the Sciences published Lagrange's "First Part of the Theory of the Secular Variations of the Elements of the Planets", ([1781]). Lagrange describes it:

> "This is as complete a theory of these [secular] sorts of variations as I intend to give; a subject which interests equally astronomers by its utility for the perfection of tables, and the geometers by the new resources of analysis by which it gives rise . . . I believe however the need to re-examine this matter in its entirety, [and] to treat it thoroughly and in a manner that is more direct and more rigorous than I have yet made." (Lagrange [1781], 126)

This would seem to imply doubt on Lagrange's part of the validity and rigor of his earlier work. However, Lagrange does not, in fact, question the rigorousness of his previous memoir; somewhat later in the memoir he writes:

> "The method that I have followed for finding the different forms of the variations of the planets is, it seems to me, the most direct and most natural that is possible, being deduced from the same principles; but I could have reached it more simply by the general method which I used to determine the variation of the mean distance, in the Memoir of 1776, [a] method which has the advantage to be applicable to all the equations of the same type." (Lagrange [1781], 147-8)

Thus, while [1781] may be more natural or direct, and offer a unified method of examining the variation of the orbital elements, Lagrange still felt that [1776] contained its essential results.

[1781] is a clear descendant of his work of [1774]: many of the techniques in the earlier piece make an appearance in this work as well. It is possible that, after the publication of [1774], Lagrange attempted to use the same method to show stability of the major axes, but ran into difficulties. As a result, he turned to the method appearing in [1776]. Only later did he return to the methods of [1774], and work it into one of the proofs of the constancy of the major axes that will appear in [1781]. Of particular interest in this article is that Lagrange focuses on what might be called "invariant quantities", something that will form the heart of Laplace's work of 1784.

Lagrange begins with the force g, the gravitational force at unit distance, and $\rho = \sqrt{x^2 + y^2 + z^2}$, giving the differential equations:

$$\frac{d^2x}{dt^2} + \frac{gx}{\rho^3} + X = 0$$

$$\frac{d^2y}{dt^2} + \frac{gy}{\rho^3} + Y = 0$$

$$\frac{d^2z}{dt^2} + \frac{gz}{\rho^3} + Z = 0$$

where X, Y, Z are the perturbing forces. As before, he writes:

$$\frac{xdy - ydx}{dt} = R$$

$$\frac{xdz - zdx}{dt} = Q$$

$$\frac{ydz - zdy}{dt} = P$$

and hence arrives at the equation

$$Px - Qy + Rz = 0 \qquad (C)$$

just as in [1774] (Lagrange [1781], 126-130). As before, he introduces θ, the tangent of the inclination, and ω, the location of the nodes, which are:

$$\theta = \frac{\sqrt{P^2 + Q^2}}{R}, \tan\omega = \frac{P}{Q}$$

and hence

$$\theta \sin \omega = \frac{P}{R}, \theta \cos \omega = \frac{Q}{R}$$

again, as given in [1774] (Lagrange [1781], 130).

At this point, Lagrange notes, one may determine the variation of the nodes and inclinations, as he has already shown. To determine the other elements, he introduces some new differentials, namely those of $\frac{x}{\rho}, \frac{y}{\rho}, \frac{z}{\rho}$:

$$d\frac{x}{\rho} = \frac{y(ydx - xdy) + z(zdx - xdz)}{\rho^3}$$

$$d\frac{y}{\rho} = \frac{x(xdy - ydx) + z(zdy - ydz)}{\rho^3}$$

$$d\frac{z}{\rho} = \frac{x(xdz - zdx) + y(ydz - zdy)}{\rho^3}$$

which can be written as:

$$d\frac{x}{\rho} = -\frac{Ry + Qz}{\rho^3}dt$$

$$d\frac{y}{\rho} = \frac{Rx - Pz}{\rho^3}dt$$

$$d\frac{z}{\rho} = \frac{Qx + Py}{\rho^3}dt$$

by substituting in the values for R, Q, P (Lagrange [1781], 131). If these are multiplied by g, and the values of $\frac{gx}{\rho^3}, \frac{gy}{\rho^3}, \frac{gz}{\rho^3}$ are substituted from the differential equations, where they are:

$$-\frac{d^2x}{dt^2} - X, -\frac{d^2y}{dt^2} - Y, -\frac{d^2z}{dt^2} - Z$$

respectively, the equations

$$gd\frac{x}{\rho} = \frac{Rd^2y + Qd^2z}{dt} + (RY + QZ)dt$$

$$gd\frac{y}{\rho} = \frac{Pd^2z - Rd^2x}{dt} + (PZ - RX)dt$$

$$gd\frac{z}{\rho} = -\frac{Qd^2x + Pd^2y}{dt} - (QX + PY)dt$$

are derived (Lagrange [1781], 131-2).

In order to simplify these equations, Lagrange introduces a new set of variables and their differentials:

$$(RY + QZ)dt - \frac{dRdy + dQdz}{dt} = dN$$

$$(PZ - RX)dt - \frac{dPdz - dRdx}{dt} = dM$$
$$-(QX + PY)dt + \frac{dQdx + dPdy}{dt} = dL$$

which allows him to write the above equations as:

$$\frac{gx}{\rho} = \frac{Rdy + Qdz}{dt} + N$$
$$\frac{gy}{\rho} = \frac{Pdz - Rdx}{dt} + M \qquad (E)$$
$$\frac{gz}{\rho} = -\frac{Qdx + Pdy}{dt} + L$$

(Lagrange [1781], 132). If these are multiplied by x, y, z respectively, and added, and recalling the definitions of P, Q, R, one obtains:

$$g\rho = Nx + My + Lz + P^2 + Q^2 + R^2 \qquad (F)$$

and, recalling that $\rho = \sqrt{x^2 + y^2 + z^2}$, one can recover the equation for a conic (Lagrange [1781], 133).

If the equations of (E) are multiplied by $P, -Q, R$, respectively, and added, one obtains:

$$0 = NP - MQ + LR$$

which expresses a relationship that always holds between L, M, N, P, Q, R (Lagrange [1781], 133). For brevity, Lagrange introduces five new expressions:

$$\Pi^2 = P^2 + Q^2 + R^2$$

$$\lambda^2 = L^2 + M^2 + N^2$$

$$A = PM + QN, B = RN - PL, C = -RM - QL$$

and then introduces new variables, ζ, ξ, ψ such that

$$x = \frac{P}{\Pi}\zeta + \frac{N}{\lambda}\xi + \frac{C}{\Pi\lambda}\psi$$
$$y = -\frac{Q}{\Pi}\zeta + \frac{M}{\lambda}\xi + \frac{B}{\Pi\lambda}\psi$$
$$z = \frac{R}{\Pi}\zeta + \frac{L}{\lambda}\xi + \frac{A}{\Pi\lambda}\psi$$

and hence

$$Px - Qy + Rz = \Pi\zeta$$
$$Nx + My + Lz = \lambda\xi$$

Thus, (C) and (F) above may be written in the simplified form:

$$\zeta = 0, g\rho = \lambda\xi + \Pi^2$$

Moreover, as $\rho^2 = x^2+y^2+z^2$ and $A^2+B^2+C^2 = \Pi^2\lambda^2$ (obtained by expanding out the expression for A, B, C) one determines that

$$\rho^2 = \zeta^2 + \xi^2 + \psi^2$$

which indicates that the coordinates ζ, ξ, ψ are just rectangular coordinates, with the same origin as the coordinates x, y, z.[1] Moreover, $\zeta = 0$, and thus the path of the planet is a curve in the $\xi - \psi$ plane, whose path is given by:

$$g\rho = \lambda\xi + \Pi^2$$

which is an ellipse whose major axis is $\frac{\Pi^2}{g}$ and whose excentricity is $\frac{\lambda}{g}$.[2] This is the key to Lagrange's subsequent work:

> "From which one can conclude that the bodies will be changed at each instant as if they really describe the ellipse determined by these equations: but this ellipse varies continuously in position and size, and one knows the variation of its elements by means of the differential formulas (A) and (D) of Nos. 3 and 6." (Lagrange [1781], 136).[3]

Of particular interest is Π^2, which, as indicated before, is $P^2 + Q^2 + R^2$, since $\frac{\Pi^2}{g}$ is the length of the major axis and thus determines whether or not any secular variation of the mean motion exists.

Lagrange once again introduces the function Ω as before, and

$$X = \frac{d\Omega}{dx}, Y = \frac{d\Omega}{dy}, Z = \frac{d\Omega}{dz}$$

(where Lagrange uses d to indicate a partial derivative); hence

$$dP = \left(\frac{d\Omega}{dy}z - \frac{d\Omega}{dz}y\right)dt$$

$$dQ = \left(\frac{d\Omega}{dx}z - \frac{d\Omega}{dz}x\right)dt$$

$$dR = \left(\frac{d\Omega}{dx}y - \frac{d\Omega}{dy}x\right)dt$$

From

$$\Pi^2 = P^2 + Q^2 + R^2$$

[1] ζ, ξ, and ψ are simply the new coordinates when the $x - y - z$ axes are rotated at some angle, preserving the angle between the axes.

[2] The excentricity is the distance between the center of coordinates and one focus; if this quantity is divided by the semimajor axis, the result is the eccentricity. To distinguish between the two, which were often used interchangeably, I shall spell the former as above.

[3] (A) and (D) are the formulas for dP, dQ, dR and dL, dM, dN, respectively.

Lagrange obtains

$$\Pi d\Pi = \frac{d\Omega}{dx}(Qz+Ry)dt + \frac{d\Omega}{dy}(Pz-Rx)dt - \frac{d\Omega}{dz}(Py+Qx)dt$$

by differentiating and substituting in the values for dP, dQ, dR (Lagrange [1781], 143). However, the values for P, Q, R are known in terms of x, y, z and their differentials, so these can be substituted to obtain:

$$\begin{aligned}
(Qz+Ry)dt &= (xdz-zdx)z+(xdy-ydx)y \\
&= x(ydy+zdz)-dx(y^2+z^2) \\
&= x\rho d\rho - \rho^2 dx \\
(Pz-Rx)dt &= (ydz-zdy)z-(xdy-ydx)x \\
&= y(xdx+zdz)-dy(x^2+z^2) \\
&= y\rho d\rho - \rho^2 dy \\
(Py+Qx)dt &= (ydz-zdy)y+(xdz-zdx)x \\
&= -z(xdx+ydy)+dz(x^2+y^2) \\
&= -z\rho d\rho + \rho^2 dz
\end{aligned}$$

To simplify this, Lagrange introduces:

$$\frac{d\Omega}{dx}x + \frac{d\Omega}{dy}y + \frac{d\Omega}{dz}z = \Phi$$

$$\frac{d\Omega}{dx}dx + \frac{d\Omega}{dy}dy + \frac{d\Omega}{dz}dz = (d\Omega)$$

and thus

$$\Pi d\Pi = \Phi\rho d\rho - \rho^2(d\Omega)$$

Lagrange will use these equations to determine the variation of the major axis.

Likewise, a similar group of substitutions yields:

$$dN = 2x(d\Omega) - \Phi dx - \frac{d\Omega}{dx}\rho d\rho$$

$$dM = 2y(d\Omega) - \Phi dy - \frac{d\Omega}{dy}\rho d\rho$$

$$dL = 2z(d\Omega) - \Phi dz - \frac{d\Omega}{dz}\rho d\rho$$

which will yield the variation of the excentricity $\frac{\lambda}{g}$, the longitude of the aphelion ϕ, and the latitude η of the aphelion with respect to the plane of projection (Lagrange [1781], 145).

From

$$\lambda^2 = L^2 + M^2 + N^2$$

and the above, Lagrange derives:

$$\lambda d\lambda = 2(Nx + My + Lz)(d\Omega)$$

$$-\Phi(Ndx + Mdy + Ldz)$$
$$-\left(N\frac{d\Omega}{dx} + M\frac{d\Omega}{dy} + L\frac{d\Omega}{dz}\right)\rho d\rho$$

and as

$$Nx + My + Lz = g\rho - \Pi^2, Ndx + Mdy + Ldz = gd\rho$$

and, substituting into the equations for N, M, L the values of P, Q, R, as well as

$$\frac{dx^2 + dy^2 + dz^2}{dt^2} = \frac{2g}{\rho} - \Delta$$

(where $\Delta = \frac{g^2-\lambda^2}{\Pi^2}$, a quantity introduced by Lagrange earlier) this becomes

$$\lambda d\lambda = \left(2g\rho - 2\Pi^2 - \frac{\rho^2 d\rho^2}{dt^2}\right)(d\Omega) - \Delta\Phi\rho d\rho$$

and, moreover

$$\frac{\rho^2 d\rho^2}{dt^2} = -\Delta\rho^2 + 2g\rho - \Pi^2$$

which yields

$$\lambda d\lambda = (\Delta\rho^2 - \Pi^2)(d\Omega) - \Delta\Phi\rho d\rho$$

This yields directly the variation of the excentricity, $\frac{\lambda}{g}$, the one remaining orbital parameter whose variation affects the overall stability of the planets.

Since

$$\lambda^2 = g^2 - \Delta\Pi^2$$

this can be differentiated, and substituting for $\Pi d\Pi$ from above, one obtains:

$$\lambda d\lambda = -\Delta\Pi d\Pi - \Pi^2\frac{d\Delta}{2} = -\Delta\Phi\rho d\rho + \Delta\rho^2(d\Omega) - \Pi^2\frac{d\Delta}{2}$$

On the other hand, comparison with the above value for $\lambda d\lambda$ yields the simple equation

$$\frac{d\Delta}{2} = (d\Omega)$$

"which serves to determine the variation of the mean distance $\frac{g}{\Delta}$." (Lagrange [1781], 147).

Compare this with the formula obtained in 1776:

$$d\frac{1}{2a} = \frac{d\Omega}{S+T}$$

which is the variation of the major axis (which Lagrange still feels to be valid, including an essentially unchanged sketch of the derivation of this formula after obtaining the above). The difference between the two is that a change in the mean distance incorporates *both* a change in the eccentricity (which does not affect the mean motion of the planets) and the major axis (which does); the two are related by:

$$\frac{g}{\Delta} = \frac{a(1-e^2)}{2}$$

To make use of these formulas, Lagrange returns to the method he used in 1774, to determine the change of the inclination and nodes. First he uses

$$x = r\cos q, y = r\sin q$$

where r is the projection of the radius vector and q the true longitude. In the equation

$$Px - Qy + Rz = 0$$

this substitution gives:

$$z = \frac{r}{R}(Q\sin q - P\cos q)$$

and from

$$\rho = \sqrt{x^2+y^2+z^2} = \frac{r}{R}\sqrt{R^2+(Q\sin q - P\cos q)^2}$$

and

$$g\rho = Nx + My + Lz + \Pi^2$$

and

$$B = RN - PL, C = -RM - QL$$

he obtains

$$r = \frac{R\Pi^2}{g\sqrt{R^2+(Q\sin q - P\cos q)^2} + C\sin q - B\cos q}$$

From

$$xdy - ydx = Rdt$$

he obtains

$$r^2 dq = Rdt$$

and this, combined with the above, gives

$$dt = \frac{R\Pi^4 dq}{[g\sqrt{R^2+(Q\sin q - P\cos q)^2} + C\sin q - B\cos q]^2}$$

which serves to determine q as a function of t (Lagrange [1781], 155).

The right hand side may be expanded as:

$$dq + \beta\sin q dq + \gamma\cos q dq + \delta\sin 2q dq + \epsilon\cos 2q dq + \ldots = \frac{dt}{R\Pi^4\alpha}$$

where α is a finite quantity and, because of the smallness of P, Q relative to R (because of the smallness of the excentricities and inclinations), is a rapidly convergent series (Lagrange [1781], 156).[4]

[4] Specifically

$$\frac{1}{[g\sqrt{R^2+(Q\sin q - P\cos q)^2} + C\sin q - B\cos q]^2} = \alpha(1+\beta\sin q + \gamma\cos q + \ldots)$$

For simplicity, he lets

$$dp = \frac{dt}{R\Pi^4\alpha}$$

which allows him to write

$$q = p + (B)\cos p - (C)\sin p + (D)\cos 2p - (E)\sin 2p + \dots$$

where $(B), (C), (D), \dots$ are functions of the series coefficients $\alpha, \beta, \dots$.

In the unperturbed orbit, p is the mean longitude, q the true longitude, and p is linear. In the perturbed orbit, however, R, Π, α are all variables, and the "mean longitude" is actually a function of these quantities. What Lagrange desires to demonstrate is that even in this perturbed case, the differential dp of the mean longitude always remains within certain bounds.

Specifically:

> "It is therefore necessary to rigorously determine the law of the variation of the quantity in question [i.e., $\frac{t}{R\Pi^4\alpha}$], and for this it is necessary to know the value of the quantity α which represents the constant term in the fraction
>
> $$\frac{1}{[g\sqrt{R^2 + (Q\sin q - P\cos q)^2} + C\sin q - B\cos q]^2}$$
>
> developed in terms of the sines and cosines of multiples of q: it is this which we will now occupy ourselves." (Lagrange [1781], 159)

Lagrange expands this fraction as a trigonometric series, and determines that:

$$\alpha = \frac{g}{R\Pi^4\Delta^{\frac{2}{3}}}$$

after much mathematical detail (Lagrange [1781], 159-1650. Hence:

> "It follows from this that the quantity $\frac{1}{R\Pi^4\alpha}$ becomes $\frac{\Delta^{\frac{3}{2}}}{g}$; this is the value of $\frac{dp}{dt}$ (30), that is to say the speed of the movement of the mean longitude. Now, since $\frac{g}{\Delta}$ is the mean distance of the ellipse (13), one sees that this velocity will be inversely proportional to the square root of the cube of the mean distance, as one knows in the case of invariable ellipses. One may suppose this to be a consequence of the instantaneous invariability of the elements of orbit; but we believe that, seeing its great importance, it would be better to demonstrate it directly and rigorously, to not leave any doubt [*scrupule*] on the consequences that we have deduced, relative to the mean motion of the Planets." (Lagrange [1781], 165; numbers in parentheses refer to equation numbers in [1781].)

In other words, Lagrange is saying that even under perturbation, the mean motion of the planets is given by their mean distances. However, Lagrange feels that this point is so important that it would be better to establish it in a direct manner.

Thus, it is significant that he then establishes the invariability of Δ by precisely the same method used in [1776] (Lagrange [1781], 165-6) Recalling that

$$d\Delta = 2(d\Omega)$$

and expressing Ω in terms of the sines and cosines of $p, p', p'', \ldots$, he notes that $(d\Omega)$ consists of nothing but terms involving sines and cosines, and hence the mean motion of the planets is invariable, as far as variation in the mean distance is concerned.

> "It follows from this very simple analysis that the variation of the quantity Δ can only be periodic; consequently neither the mean distance, which is expressed by $\frac{g}{\Delta}$, nor the velocity of the mean motion, which is given by $\frac{\Delta^{\frac{3}{2}}}{g}$ (34) is subject to any type of secular variation. Thus, as long as one regards only these types of variations, one is justified in regarding these elements as constant and inalterable by the mutual action of the planets." (Lagrange [1781], 166)

Lagrange's focus here on the mean distance, as opposed to his focus in 1776 on the major axes of the planets, is important. The 1776 proof relied on the eccentricities of the orbits being small; however, there is no *a priori* reason why they should remain small, and if they should grow, then some doubt can arise about the validity of the methods used to show that the major axes are invariable. However, if the mean distance itself is proven to undergo only periodic variations, then the eccentricity cannot grow without limit, since the two are interrelated.

The theoretical results are, however, at odds with the observational evidence of a secular acceleration in the motions of Jupiter and Saturn; Lagrange must reconcile the two. As he did with the secular acceleration of the Moon in [1773], Lagrange calls the observations themselves into question:

> "If thus the movement of Saturn slows itself from age to age, and that of Jupiter accelerates, as the observations seem to prove, it is due to causes other than that of mutual action, but all the same one needs to regard these phenomena as very doubtful, and resolve only to admit them when they are sufficiently established by a long series of observations." (Lagrange [1781], 166-7)

It is surprising that Lagrange entirely dismisses universal gravitation as a cause in the secular variations in the mean motion, especially since he had earlier (in [1776]) established the necessity of maintaining incommensurability in the mean motions. As Laplace will show later, it is the near commensurability in the mean motions of Jupiter and Saurn (in a ratio of nearly 5 to 2) that makes for a long period variation in their mean motions (cf. Wilson [1985]).

The second half of [1781], published in 1782, did little to extend the work, though in it Lagrange computed the actual secular variations in the elements of the orbit of the six principal planets. Lagrange seems freer to make grand, sweeping statements about his results, perhaps due to the more computational nature of the work.

After determining the variation in the excentricity and inclination of Saturn and Jupiter (and with the understanding that, by [1781], Part I, the mean motions are invariable), he writes:

> "Thus one sees, by the results we have found, that the excentricities and the inclinations of the orbits of Saturn and of Jupiter need always remain very small, and that their variations consist only of oscillations by which these elements become alternatively greater and smaller than their mean values, but without ever deviating from very small quantities. One sees also by our formulas that the coefficients of t without signs of sine and cosine are necessarily always real, whatever values that one gives to the masses of the two planets, because in augmenting or diminishing these masses one only increases or diminishes proportionally the coefficients marked by the brackets round or square[5], without changing the signs. From which it follows that the system of Saturn and Jupiter, insofar as one regards it as independent of the other planets, which is always permitted, as we have shown above, is in itself in a stable and permanent state, at least in making abstraction of the action of all other causes, such as that of a Comet, or of a resisting medium in which the planets move, or . . . " (Lagrange [1782], 303)

(Lagrange's ellipsis). Lagrange is thus the first to make a clear claim on the long-term stability of the solar system. According to Lagrange, the system — or at least Jupiter and Saturn — will never "want a reformation".

Lagrange concludes his work with comments that anticipate Laplace's words on the same subject:

> "Such are the general formulas of the secular variation for the six principal planets. These forms are at no point limited to a certain span of time, but hold for an indefinite time, because, no term has been found which is susceptible to infinite augmentation, the variations supposed very small in the differential equation, such that the excentricity and the inclination of the orbit, effectively always remain very small; and as this supposition is the only one that we permit ourselves in these equations to simplify them, thus it follows that their integration is entirely rigorous, [and] there can not be any doubt on the legitimacy and the generality of our solutions applied to the solar system." (Lagrange [1782], 343)

[5] Coefficients in one of the trigonometric expansions used in this, and previous, works.

Again, Lagrange believes that his demonstration is rigorous (as will most astronomers of the last half of the eighteenth century, including Laplace who, as we shall see, did nothing to extend Lagrange's work in this category). Lagrange concludes:

> "This constancy of the mean motions, and that of the mean movements which is the consequence, is the most interesting result of our analysis, and the most remarkable point of the system of the world. The planets, by virtue of their mutual attraction, change insensibly the form and the position of their orbits, but without coming out of certain limits; their major axes alone remain inalterable; at least the Theory of gravitation makes only for periodic alterations [which are] dependent on the respective positions of the Planets, and does not indicate any of a secular type, either constantly increasing, or simply periodic, but of a period very long and independent of the situation of the planets, as that which the same Theory gives for the other elements of orbit, and that we determined. We have already demonstrated this property of the major axes in the *Memoirs* of 1776; but the demonstration that we have given in the first part of these researches is in some manner more general and more complete because we have considered all the elements of the orbit as variable at this time, and that we have regarded the effects of this variability with all the care necessary in a matter so delicate." (Lagrange [1782], 344)

This last sentence might be taken to indicate Lagrange's realization of an insufficiency in his 1776 proof; however, it seems clear, given that he repeats the 1776 proof in the first part of [1781], that his "more general and more complete" theory of 1781 is so only because he derives not just the variation in the major axis (as [1776] did), but also the variations in the inclination and excentricities.

Thereafter Lagrange sketches the technique of the 1776 article, though not in detail. This indicates that Lagrange felt he had arrived at a better proof — in the sense of being more comprehensible and more "natural" (or elegant) — than that given earlier, but not one more thorough or more rigorous.

9.2 Secular Variation of the Mean Motion, 1783

Lagrange published in 1783 another article which might seem to have a bearing on the stability question: "On the Secular Variation of the Mean Motion of the Planets", ([1783]), which appeared in the memoirs for the Royal Academy of Sciences at Berlin. Yet despite the previous connection between the determination of the mean motion of the planets and their semimajor axes, which would in turn determine the stability of the solar system, this article has little bearing on the matter. Lagrange was probably inspired to write this by the continued inability to explain the apparent secular variations in the motion of the Moon, Jupiter, and Saturn.

The variations in the mean motion that Lagrange speaks of are those caused by the variation of the other elements of the orbit, aside from any change in the semimajor axis, and are of more interest to the observational astronomer in computing the places of the planets, than the theoretical astronomer for determining the long term behavior of the planets. Lagrange had earlier made the distinction between the "apparent" mean motion and the actual mean motion (cf. Lagrange [1765], 652). Still, the article indicates Lagrange's thoughts on the matter:

> "M. de Laplace has calculated in detail the terms proportional to the square of time that the perturbing forces of a planet could introduce in the expression of its longitude, [and] found that these terms mutually destroy each other, at least in the first approximation.[6] This result has given me occasion to find rigorously, and by a direct method, the law of variations of the major axis of the orbit of a planet disturbed by the action of some others; and I have demonstrated that these variations could only be periodic, and relative to the configuration of the planets among themselves; from which it follows that the mean motion of a planet can not be subject to any secular variation, insofar as this variation depends on the major axis of its orbit ..." (Lagrange [1783], 381)

Lagrange does not indicate which article he is referring to (1776 or 1781), when he states that he "proved rigorously" the law of variations of the major axis of the orbit. However, once again, Lagrange indicates that Laplace's work is more important for what it inspired than what it produced and, once again, Lagrange contends that his work demonstrates the periodic nature of the variations in the major axis.

As for [1783] itself:

> "It is therefore only a problem, to answer the question of the secular variation of the mean motions of the planets, of executing the analysis that we have indicated and to apply the following results to each planet; this is the object of the present Memoir, which should be regarded as a supplement to the *General Theory of Secular Variations*." (Lagrange [1783], 382)

In a sense, Lagrange's title is misleading: he is not concerned with a secular variation in the *mean motion*, which depends only upon the major axis; rather, he is concerned with a secular variation of the true longitude, caused by changes other than a change in the mean motion. In practical terms, a change such as that would *appear* to be a change in the mean motion, since this quantity is not measured directly, but from the angular motion and the elapsed time. (It might be in this sense that Lagrange calls it the "mean motion")

[6] Probably Laplace's [1773]

Lagrange's article begins with the differential expression relating the mean longitude p to the true longitude q:

$$dp = f(q)dq$$

where $f(q)$ is some algebraic function of sines and cosines (Lagrange [1783], 383). This can be integrated to obtain:

$$p = F(q)$$

and thence inverted to obtain

$$q = \Phi(p)$$

which expresses the true longitude as a function of the mean longitude (essentially giving the position of the planet as a function of time). However, the above is true only in an unperturbed orbit. In the variable orbit:

$$dF(q) = f(q)dq + \delta F(q)$$

where δ is the variation in F caused by changes in parameters other than q (e.g., eccentricity, inclination, major axis), and thus:

$$f(q)dq = dp = dF(q) - \delta F(q)$$

and thus

$$dp + \delta F(q) = dF(q)$$

which, upon integration, yields;

$$p + \int \delta F(q) = F(q)$$

and thus, applying Φ to both sides:

$$q = \Phi\left(p + \int \delta F(q)\right)$$

Any change in the "mean motion" must be accounted for by $\int \delta F(q)$, which Lagrange (in the previous memoir) designates at $d\Sigma$. Lagrange reiterates that p contains no secular variations, because the semimajor axes of the planets are invariable, and hence any secular variation in q must be caused by Σ.

To simplify the following analysis, Lagrange first ignores the inclinations of the orbits, and considers an approximation as far as the third order in the eccentricities. From [1781], he has:

$$F(q) = q - 2M\cos q + 2N\sin q - \frac{3MN}{2}\cos 2q$$

$$+\frac{3(N^2 - M^2)}{4}\sin 2q - \frac{3MN^2 - M^3}{3}\cos 3q + \frac{N^3 - 3NM^2}{3}\sin 3q$$

where M, N are as they were in [1781]. From this, he obtains:

$$d\Sigma = -2dM\cos q + 2dN\sin q - \frac{3M}{2}\left(\sin 2qdM + \cos 2qdN\right)$$
$$+\frac{3N}{2}\left(\cos 2qdM - \sin 2qdN\right) - 2MN\left(\sin 3qdM + \cos 3qdN\right)$$
$$+(M^2 - N^2)\left(\cos 3qdM - \sin 3qdN\right)$$

where dM, dn can be obtained from [1781], above, and since the inclinations are assumed to be zero, terms containing z vanish (Lagrange [1783], 385).

After many substitutions in the two planet case, Lagrange arrives at:

$$\frac{d\Sigma}{dp} = (0) + (1)(x^2 + y^2) + (2)(x'^2 + y'^2) + (3)(xx' + yy')$$

where $(0), (1), (2), (3)$ have values that depend on the mean distances r, r', the mass of the perturbing planet T', and the derivatives with respect to r of the coefficients of the expansion:

$$\left[r^2 - 2rr'\cos(p-p') + r'^2\right]^{-\frac{1}{2}} = [r, r'] + [r, r']_1\cos(p-p') + [r, r']_2\cos 2(p-p') + \dots$$

From [1781], however, Lagrange knows he can express $x, y, x', y', \dots$ as:

$$A\sin(at+\alpha) + B\sin(bt+\beta) + C\sin(ct+\gamma) + \dots$$
$$A\cos(at+\alpha) + B\cos(bt+\beta) + C\cos(ct+\gamma) + \dots$$
$$A'\sin(at+\alpha) + B'\sin(bt+\beta) + C'\sin(ct+\gamma) + \dots$$
$$A'\cos(at+\alpha) + B'\cos(bt+\beta) + C'\cos(ct+\gamma) + \dots$$

respectively (Lagrange [1783], 393). Substituting these into the equation for $\frac{d\Sigma}{dp}$ one obtains two types of terms: those that do not contain sines or cosines (and are, in fact, constants), and those proportional to the cosines of the angles such as

$$(a-b)t + \alpha - \beta, (a-c)t + \alpha - \gamma, (b-c)t + \beta - \gamma, \dots$$

which are integrable (Lagrange [1783], 394). Hence:

> "It follows from this that the value of Σ is composed of two sorts of terms, ones which are rigorously proportional to p, and consequently affect the mean and uniform movement of the planet; [and] others proportional to the sine of the angles $(a-b)t+\alpha-\beta, (a-c)t+\alpha-\gamma, \dots$, and whose coefficients are much greater than those of the corresponding cosines in the differential equation, since the integration has augmented them in the ratio of $a-b, a-c, \dots$ to 1. These terms therefore give the true secular equations in the mean movement of the planet; and it is only a question to see if they sensible enough to be perceived by observation." (Lagrange [1783], 394)

It is not truly correct anymore to call these variations “secular”: they are actually long period variations which, because of the historic time scale of the observations of the planets, *appear* to be secular. They are “secular” only in the sense that astronomers have been using the term since Flamsteed.

Lagrange then takes up the problem of including the inclinations, and his words suggest he will attempt a “bootstrap” method, similar to that used by Euler in his 1748 and 1752 work on the motion of Saturn and Jupiter (which he will also examine in this essay). Yet Lagrange decides not to:

> “One could for this use the complete value of $F(q)$ given in the preceding memoir, and regarding in the calculation the quantities $\frac{P}{R}$, $\frac{Q}{R}$ and by the variations determined in the *Theory of Secular Variations*; but it will be much simpler to consider directly the real orbit of the planet, and to find the variation of the mean motion according to those [variations] of the elements of this orbit.” (Lagrange [1783], 394)

What follows begins little differently from Lagrange’s work in [1776]: the only difference is in the depth and degree of the approximations, and not in the basic technique.

Lagrange closes [1783] with an examination of the as-yet unexplained secular variation in the motion of Jupiter and Saturn. (The secular acceleration of the Moon does not even enter into his discussion; perhaps Lagrange feels it was finally disposed of in his 1774 essay) When he does so, however, he finds that even this theory yields no perceptible secular variation in their mean motions, and he concludes with:

> “Thus the secular equations of the mean movements of Saturn and Jupiter are equally insensible, and could be reputed to be absolutely nothing.
>
> “[From] This result we now dispense with examining the secular equations of the other planets, as we proposed to do; because it is easy to prove that the values of these equations are even less than those which we have found [for Jupiter and Saturn]. Thus one can henceforth regard [this] as a truly rigorous demonstration that the mutual attraction of the principal planets can not produce in their mean movements any sensible alteration.” (Lagrange [1783], 414)

To what, then, could the observed accelerations in the motions of Jupiter and Saturn be attributed? A few pages earlier, Lagrange writes:

> “From this one can conclude that the mean movement of Saturn is inalterable by the action of Jupiter; and that therefore, if this movement is subject to variations, one needs to find a cause other than that of the mutual gravitation of the planets.” (Lagrange [1783], 411)

"*If* this movement is subject to variations": again Lagrange suggests, this time in a more veiled manner, that there is *no* secular variation in their mean motions: that all the observations that indicate there is one are in error. While Lagrange allows that some hitherto unnoticed cause might produce a secular variation, his predilection is obvious: the observations must be in error or, at least, the evidence is weak and need to be strengthened.

Aside from extending his work into the computational realm and examining the periodic variations in the elements of orbit (in 1783 and 1784), Lagrange does no more significant work on this topic.

9.3 Laplace's Memoir on Secular Inequalities, 1784

Shortly after Lagrange published his last important work on celestial mechanics [1783], Laplace returned to the subject with "Memoir on the Secular Inequalities of the Planets and Satellites", read to the Academy on November 23, 1785 (Dictionary of Scientific Biography, *Supplement*, p. 397). Laplace's later work has a very different flavor from his earlier pieces: for the first time (barring his extension of Lagrange's [1774] to the eccentricities and aphelion positions of the planets) Laplace is concerned with the orbital elements, and not with the position vectors of the planets.

However, neither this nor its successor ([1787], see below) provide new results or theorems. In particular, Laplace was not interested in reproving or proving more rigorously Lagrange's results; rather, he accepts Lagrange's prior work without reservation. Laplace gives an indication of his place in the process of proving the lack of secular variations:

> "Of all the inequalities, the most interesting is that which can alter the mean motion of the planets. Many astronomers have admitted a secular equation proportional to the square of time in the mean motion of Jupiter and Saturn. The geometers who have achieved the best success in the theory of these planets, MM. Euler and de la Grange, are believed to have found the cause in the mutual action of these two bodies; but their results differ so much from each other, that there is reason to suspect some error: it is that which decided me to return to this matter and to treat it with all the care which its importance merits. In taking the precision as far as the third powers inclusively of the excentricities and the inclination of the orbits, I found that the theory gives no secular inequalities in the mean movements and in the mean distances of the planets from the sun; from which I conclude that these inequalities are null or at least insensible since the epoch of the most ancient observations to our day." (Laplace [1784], 50)

Here Laplace is referring to his work in [1772] or [1773], where he eliminated secular terms by a change of variables. Laplace continues, giving high praise to Lagrange:

> "M. de la Grange has since extended [this result] to an unlimited time, and shown by an ingenious and simple analysis that the mean distances of the planets to the sun are unchanging and their mean motions uniform, which is equally true for the satellites, since they form around their primary planets a system similar to that of the planets around the sun. Thus the planets and the satellites always maintain the same mean distance to the focus of the principal force which moves them, at least while one regards only their mutual action and while one supposes their movements are incommensurable with each other, as is the case for the planets of our system." (Laplace [1784], 50)

As Lagrange did before him, Laplace dismisses the one crucial assumption in Lagrange's proof (that the mean motions are incommensurable), by claiming that this is the case in our solar system.

However Laplace, the astronomer, does note one particular case known to Lagrange where this incommensurability is not well-founded: the inner three of the four satellites of Jupiter known at the time.

> "I now return to the general law of the uniformity of the celestial mean motions. Those of the three first satellites of Jupiter have a remarkable ratio and which can give rise to the belief that this law is not observed in their regard. The discussion of this ratio, and of the case which it produces and of its influence on the movement of the satellites appears to me to merit the attention of geometers and astronomers." (Laplace [1784], 56)

Even in this case, however, Laplace suggests (but does not conclusively demonstrate) that the law of constancy of the mean motions (and hence the mean distances) is not threatened.

First, Laplace introduces a rather unusual "rule", almost Pythagorean in its attempt to reconcile pure number and physical reality:

> "Now one can establish as a general rule that, if the result of a long series of precise observations [on the mean motions] approaches a simple ratio in a manner such that the difference is indistinguishable by observation and could be attributed to the errors which they [observations] are susceptible, this ratio is probably of that nature. Thus [since] observations have not perceived any difference between the mean motion of the revolution of the Moon around itself and around the Earth, one is justified in supposing that these two movements are rigorously the same. In applying this rule to the movements of the first three satellites of Jupiter, we can conclude,

> with a great likelihood, that the difference between the mean motions of the first and second [satellites] is exactly equal to twice the difference between the mean motions of the second and third. This equality is not the effect of chance, and it is against all reason to suppose that these three bodies were originally placed at these exact distances; it is thus natural to think that their mutual attraction is the true cause." (Laplace [1784], 57)

Designating the mean motions of the satellites in time t by $nt, n't, n''t$, and the quantity $n - 3n' + 2n''$ as s (which, according to Laplace's statement, above, ought to be identically equal to zero), and V the mean longitude of the first satellite, Laplace states (without proof) that perturbations of the second order with respect to the masses of the satellites introduce into s and V quantities proportional to the time. Then:

> "In making these disappear by the method that I have already given for this end, I arrive at two differential equations of the first order in s, V, and t. Their integrals give a complete explanation of the phenomenon which is under discussion, and present at the same time many interesting consequences." (Laplace [1784], 58)

Laplace will return to the question of the satellites later in the article; he concludes this section with:

> "One sees, by what we have said, that the mutual action of the satellites of Jupiter produce in their mean movements only periodic inequalities; and we can generally conclude that, if one regards only the law of universal gravitation, the mean distances of celestial bodies to the focus of their principal forces are unchanging." (Laplace [1784], 61)

We shall see how Laplace demonstrates this.

Unfortunately, this lack of gravitational perturbation of the mean motion leaves open the question of the variation of the mean motion of Jupiter and Saturn. Lagrange might demand more evidence of secular variations in the mean motion of Jupiter and Saturn, since they could not be accounted for mathematically, but Laplace would not dispose of the observational evidence so casually, and attempted to find an explanation for it. At first, he looked to the comets, but upon further reflection, Laplace decided that the cause of the variation must somehow be related to mutual gravitation:

> "A general property[7] of the action of the planets on each other is that, if one regards only those quantities which have a very long period, the sum of the masses of each planet, divided respectively by the major axes of their orbit, remain nearly constant, from which

[7]This "general property" is roughly equivalent to the conservation of potential energy, though Laplace will derive it the article.

> it follows that the squares of the mean movements are reciprocal to the cubes of these axis, if the movement of Saturn slows itself by the action of Jupiter, that of Jupiter must accelerate by the action of Saturn, which conforms to that which we observe." (Laplace [1784], 51)

By this, Laplace notes, the ratio of deceleration of Saturn to the acceleration of Jupiter should be as 7 to 3, which is what is found observationally.

> "It is thus very probable that the observed variations in the movements of Jupiter and Saturn are an effect of their mutual action, and since it is established that this action can not produce any inequality, whether constantly increasing, or periodic, and of a period very long and independent of the situation of these planets, and that it [universal gravitation] causes only inequalities dependent on their configuration [with respect] to each other, it is natural to think that there exists in their theory a considerable inequality of this type, whose period is very long and from which these variations result." (Laplace [1784], 52)

In other words, the apparent secular acceleration of Jupiter and deceleration of Saturn must be the result of a periodic inequality of long period, specifically caused by their mutual attraction. This is at odds with Lagrange's declaration of [1781], and Laplace will ultimately show that it is precisely such a long period variation is responsible for the changes in the mean motion.[8]

Laplace makes very clear what he feels remains to be done:

> "But are the excentricities and the inclinations contained within narrow limits? This is an important point in the system of the world which still remains to be illuminated, and the discussion of which is the single thing that remains to be desired in the theory of secular inequalities. I have proven, in the second Part of our *Memoirs* for the year 1772, that, if one considers only the action of two planets, the excentricities and the inclinations of their orbits are always very small; and M. de la Grange has shown, in the *Memoires de Berlin* for the year 1782, that this is equally true for the orbits of the planets of our system, beginning with the most reasonable assumption on their masses." (Laplace [1784], 61-2)

(The article Laplace refers to is the second part of [1781], where Lagrange computes the actual variations, and not where he establishes the theory for their computation.) Laplace continues:

> "However the uncertainty which one has regarding many of these masses can lay down some doubt on this result, and it is necessary

[8] In "Théorie de Jupiter et de Saturne". Laplace read two parts of a first draft on May 10, 1786 and July 15, 1786. See Wilson [1985] for full details.

> to be assured by a method independent of all hypotheses, that, by virtue of the mutual action of the planets, the excentricities and the inclinations of their orbits are always inconsiderable. I propose again to fulfill this objective in this Memoir, and establish in a general manner that the secular inequalities of the excentricities and the inclination of the orbits of the planets contain neither arcs of circles, nor exponentials; from which it follows that, by virtue of the action of these bodies, their orbits flatten more or less, but never vary far from the circular form and always conserve the same major axes." (Laplace [1784], 62)

Laplace's criticism regarding the uncertainty of the masses is peculiar. The point Laplace should make clear is that the uncertainty in the masses makes the quantities of the variation uncertain; the point he appears to make is that the *existence* of the variation is questionable, and needs to be established in some other way.

Laplace introduces the function λ (which serves much the same function as Lagrange's Ω); if $m, m', m'', \ldots$ are the masses of the bodies, M the mass of the central body (and $M = 1$), and $x, y, z, x', y', z', \ldots$ the rectangular coordinates of the bodies, and $r, r', \ldots$ the distance between M and the bodies $m, m', \ldots$ respectively, then λ is:

$$\begin{aligned} \frac{mm'}{\sqrt{(x'-x)^2+(y'-y)^2+(z'-z)^2}} &+ \frac{mm''}{\sqrt{(x''-x)^2+(y''-y)^2+(z''-z)^2}} \\ &+ \frac{m'm''}{\sqrt{(x''-x')^2+(y''-y')^2+(z''-z')^2}} \\ &+ \ldots \end{aligned}$$

This allows the differential equations for x, y, z to be written as:

$$0 = \frac{d^2x}{dt^2} + \frac{(1+m)x}{r^3} + \frac{m'x'}{r'^3} + \ldots - \frac{1}{m}\frac{\partial\lambda}{\partial x} \tag{1}$$

$$0 = \frac{d^2y}{dt^2} + \frac{(1+m)y}{r^3} + \frac{m'y'}{r'^3} + \ldots - \frac{1}{m}\frac{\partial\lambda}{\partial y} \tag{2}$$

$$0 = \frac{d^2z}{dt^2} + \frac{(1+m)z}{r^3} + \frac{m'z'}{r'^3} + \ldots - \frac{1}{m}\frac{\partial\lambda}{\partial z} \tag{3}$$

These are transformed, first by multiplying (1) by

$$2mdx - \frac{2m(mdx + m'dx' + \ldots)}{1 + m + m' + \ldots}$$

and (2) by

$$2mdy - \frac{2m(mdy + m'dy' + \ldots)}{1 + m + m' + \ldots}$$

and (3) by

$$2mdz - \frac{2m(mdz + m'dz' + \ldots)}{1 + m + m' + \ldots}$$

and, likewise, the same equations for $m', m'', \ldots$ by the analogous factors, and noting that:

$$0 = \frac{\partial \lambda}{\partial x} + \frac{\partial \lambda}{\partial x'} + \ldots$$
$$0 = \frac{\partial \lambda}{\partial y} + \frac{\partial \lambda}{\partial y'} + \ldots$$
$$0 = \frac{\partial \lambda}{\partial z} + \frac{\partial \lambda}{\partial z'} + \ldots$$

Laplace arrives at:

$$\begin{aligned} 0 = 2m\frac{dxd^2x + dyd^2y + dzd^2z}{dt^2} + 2m'\frac{dx'd^2x' + dy'd^2y' + dz'd^2z'}{dt^2} + \ldots \\ -2\frac{mdx + m'dx' + \ldots}{1 + m + m' + \ldots} \cdot \frac{md^2x + m'd^2x' + \ldots}{dt^2} \\ -2\frac{mdy + m'dy' + \ldots}{1 + m + m' + \ldots} \cdot \frac{md^2y + m'd^2y' + \ldots}{dt^2} \\ -2\frac{mdz + m'dz' + \ldots}{1 + m + m' + \ldots} \cdot \frac{md^2z + m'd^2z' + \ldots}{dt^2} \\ +2\left(\frac{mdr}{r^2} + \frac{m'dr'}{r'^2} + \ldots\right) - 2d\lambda \end{aligned}$$

These equations can be integrated easily to yield:

$$\begin{aligned} 0 = f + m\frac{dx^2 + dy^2 + dz^2}{dt^2} + m'\frac{dx'^2 + dy'^2 + dz'^2}{dt^2} + \ldots \\ -\frac{(mdx + m'dx' + \ldots)^2}{(1 + m + m' + \ldots)dt^2} - \frac{(mdy + m'dy' + \ldots)^2}{(1 + m + m' + \ldots)dt^2} \\ -\frac{(mdz + m'dz' + \ldots)^2}{(1 + m + m' + \ldots)dt^2} - 2\left(\frac{m}{r} + \frac{m'}{r'} + \ldots\right) - 2\lambda \end{aligned}$$

where f is an arbitrary constant (Laplace [1784], 65).

In a similar manner, Laplace obtains three equations, only the first of which is of real interest namely:

$$\begin{aligned} c = m\frac{xdy - ydx}{dt} + m'\frac{x'dy' - y'dx'}{dt} + \ldots \\ -\frac{mx + m'x' + \ldots}{1 + m + m' + \ldots} \cdot \frac{mdy + m'dy' + \ldots}{dt} \\ +\frac{my + m'y' + \ldots}{1 + m + m' + \ldots} \cdot \frac{mdx + m'dx' + \ldots}{dt} \end{aligned} \tag{5}$$

where c is an arbitrary constant (Laplace [1784], 66).

In the case of unperturbed, elliptical motion:

$$\frac{dx^2 + dy^2 + dz^2}{dt^2} = \frac{2(1+m)}{r} - \frac{1+m}{a}$$

where a is the semimajor axis of the ellipse that m describes around M (Laplace [1784], 67). However, this is not really true; Laplace adds the perturbing functions $\Psi, \Psi', \ldots$, which are all of order $m, m', \ldots$, to obtain:

$$\frac{dx^2 + dy^2 + dz^2}{dt^2} = \frac{2(1+m)}{r} - \frac{1+m}{a} + \Psi$$

and likewise for the equations involving $x', y', z', \ldots$. Substituting these into (4) Laplace obtains:

$$\begin{aligned}
f = {} & \frac{m}{a} + \frac{m'}{a'} + \ldots \\
& - \frac{m^2(m' + m'' + \ldots)}{1 + m + m' + \ldots}\left(\frac{2}{r} - \frac{1}{a}\right) \\
& - \frac{m'^2(m + m'' + \ldots)}{1 + m + m' + \ldots}\left(\frac{2}{r'} - \frac{1}{a'}\right) \\
& - \ldots \\
& + \frac{2mm'dxdx' + 2mm''dxdx'' + 2m'm''dx'dx'' + \ldots}{(1 + m + m' + \ldots)dt^2} \\
& + \frac{2mm'dydy' + 2mm''dydy'' + 2m'm''dy'dy'' + \ldots}{(1 + m + m' + \ldots)dt^2} \\
& + \frac{2mm'dzdz' + 2mm''dzdz'' + 2m'm''dz'dz'' + \ldots}{(1 + m + m' + \ldots)dt^2} \\
& + 2\lambda - \frac{m(1 + m' + m'' + \ldots)}{1 + m + m' + m'' + \ldots}\Psi - \frac{m'(1 + m' + m'' + \ldots)}{1 + m + m' + m'' + \ldots}\Psi' + \ldots
\end{aligned}$$

In order to arrive at his conclusion, Laplace introduces the strong assumption that the quantities:

$$\frac{mm'dxdx'}{dt^2}, \frac{mm''dxdx''}{dt^2}, \frac{mm'dydy'}{dt^2}, \ldots$$

are periodic. Also, since $\Psi, \Psi', \ldots$ are of order m, the terms that these perturbations introduce into the above equation are of order m^2 (Laplace claims m^3). Thus, to order m^2 and ignoring periodic terms one has:

$$f = \frac{m}{a} + \frac{m'}{a'} + \frac{m''}{a''} + \ldots \tag{8}$$

> "Thus, in supposing that the sequence of centuries leads to remarkable changes in the semimajor axes $a, a', \ldots$ of the orbits, they need always satisfy the preceding equation in which f is invariable." (Laplace [1784], 68)

Note, however, that Laplace does not consider that $a, a', \ldots$ are invariable; merely that they must always satisfy the above equation. This is what Laplace referred to at the beginning of his article as a "general property of the planets".

In arriving at this conclusion, Laplace has, on the one hand, gone further than Lagrange, and considered the *simultaneous* changes in $a, a', \ldots$ (which Lagrange did not consider, assuming that $a', a'', \ldots$ remained constant while a changed). On the other hand, he has made a much stronger set of assumptions on the periodicity of functions of $x, x', \ldots$ and the products of these functions, and an equally strong assumption on the size of the perturbations $\Psi, \Psi', \ldots$. This would seem to defeat Laplace's intention of demonstrating the periodic nature of the changes in excentricity and inclination "independent of all hypotheses".

Returning to equation (5), above, Laplace notes that, in an elliptical orbit, one has the equality

$$\frac{1}{2}dt\sqrt{\frac{a(1-e^2)}{1+\theta^2}} = \frac{1}{2}(xdy - ydx)$$

where a is the semimajor axis as above, e the eccentricity, and θ the tangent of the inclination (Laplace [1784], 69). One sees in this a great change from Laplace's previous methods. Rather than try and solve the differential equations for x, y, z in terms of t, Laplace *replaces* quantities involving x, y, z with quantities expressed in terms of the orbital elements themselves. This was the innovation of Lagrange.

Likewise for $x', y', x'', y'', \ldots$. Hence equation (5) becomes:

$$c = m\sqrt{\frac{a(1-e^2)}{1+\theta^2}} + m'\sqrt{\frac{a'(1-e'^2)}{1+\theta'^2}} + \ldots \qquad \text{([1784] 9)}$$

> "in supposing thus that, after a considerable time, the excentricities and the inclinations of the orbits undergo remarkable changes, they need always satisfy the preceding equation in which the constant c is invariable." (Laplace [1784], 69)

If e and θ are very small, and one may neglect the quantities $e^4, e^2\theta^2, \theta^4$, then (9) gives:

$$c = m\sqrt{a} + m'\sqrt{a'} + \ldots - \frac{1}{2}m(e^2+\theta^2)\sqrt{a} - \frac{1}{2}m'(e'^2+\theta'^2)\sqrt{a'} - \ldots$$

(by expanding $\sqrt{\frac{1-e^2}{1+\theta^2}}$ by the binomial theorem, and ignoring higher order terms) which Laplace transforms into:

$$m(e^2+\theta^2)\sqrt{a} + m'(e'^2+\theta'^2)\sqrt{a'} + \ldots = \text{const.} \qquad \text{([1784] XIV)}$$

since "the mean distances of the planets to the sun are at no point perturbed by their mutual action" (Laplace [1784], 90). This is the first appearance of

Laplace's "celebrated theorem". Note that Laplace assumed that the mean distances $a, a', \ldots$ are constant, something that was established by Lagrange in [1776] and [1781].[9]

Laplace expresses this equation in other terms. He introduces the variables, p, q:

$$\theta \sin I = p, \theta \cos I = q$$

where θ is the tangent of the inclination of the orbit, and I the position of the line of the nodes (Laplace [1784], 70). These variables actually correspond to Lagrange's s and u, respectively, and make their first appearance in Laplace's work here.[10]

This substitution allows him to write two more additional equations:

$$\text{const.} = m\sqrt{a}(p^2 + q^2) + m'\sqrt{a'}(p'^2 + q'^2) + \ldots$$

$$\text{const.} = mq\sqrt{a} + m'q'\sqrt{a'} + \ldots$$
$$\text{const.} = mp\sqrt{a} + m'p'\sqrt{a'} + \ldots$$

derived from work elsewhere in his article (Laplace [1784], 92).

The theorems Laplace does prove, however, demonstrate in a simple fashion that the orbital parameters do not contain any exponential or secular terms: as all the terms are positive and non-zero, none can increase without limit.

> "From this we can generally conclude that the expressions of the excentricities and of the inclinations of the orbits of the planet contain neither arcs of circles nor exponentials, and that thus the system of the planets is contained within invariable limits, at least as long as one regards only their mutual action." (Laplace [1784], 92)

The theorem relies heavily on the semimajor axes of the planets being invariable (or, at least, subject to no secular variation), demonstrated by Lagrange in [1781], which Laplace assumes is a valid demonstration.

Recall, however, that Lagrange's demonstration required that the mean motions of the planets be incommensurable. Perhaps to derive a "complete" theory, Laplace then examines the case of the motion of the inner three satellites of Jupiter; in this case, the mean motions *are* commensurable. Laplace's motivation for including this section on the satellites of Jupiter is unclear; it certainly adds nothing to the basic theorems on the stability of the planets of the solar system, whose mean motions Laplace is assuming are incommensurable. If

---

[9]Laplace is certainly very familiar with Lagrange's work; in his derivation of an equation that limits the eccentricities in the planets in the same way he limits the changes in the major axes, Laplace refers to his results in the second part of [1772], which, recall, was the application of Lagrange's method of [1774] to the eccentricities and the aphelion positions; as well as Lagrange's [1782]. It seems clear that Laplace is referring to Lagrange's result on the invariability of the mean distances derived by Lagrange in [1781].

[10]In 1772, Laplace made use of quantities s and u, which were

$$\alpha e \sin A = p, \alpha e \cos A = q$$

but did not interpret these quantities (Laplace [1772], p. 461).

Laplace wished to dispose of the case of commensurable mean motions, this example does not better the situation, for in the end, Laplace relies on observational evidence and not mathematical reasoning to establish the insignifiance of the changes in the mean motion.

In order to deal with this problem, Laplace first decouples the sources of the variation, and considers only the variations caused by the other satellites:

> "We now consider the movements of the first three satellites of Jupiter. First of all we observe that, the movement of the fourth does not offer any commensurable ratio with those of the three others, [and] we can here neglect its action. One can for the same reason neglect the action of the sun; finally, one can ignore the figure of Jupiter, as the influence on the variations of the major axis is nothing." (Laplace [1784], 70-1)

This last statement seems to be a clear reference to Lagrange's work of 1776, where he demonstrated that the oblateness of the Earth's figure could, in no way, account for a secular acceleration in the motion of the Moon, since the equatorial bulge could be treated as a collection of small satellites whose period was incommensurable with that of the Moon.

Letting m, m', m'' and a, a', a'' be the masses and the semimajor axes of the three inner satellites, respectively, he supposes that, after some time, the latter are perturbed into:

$$a + \delta a, a' + \delta a', a'' + \delta a''$$

Neglecting the squares of the excentricities and inclinations of the orbit as well as those of δa, $\delta a'$, $\delta a''$, equations ([1784] 8) and ([1784] 9) from above give:

$$0 = \frac{m\delta a}{a^2} + \frac{m'\delta a'}{a'^2} + \frac{m''\delta a''}{a''^2}$$

$$0 = \frac{m\delta a}{\sqrt{a}} + \frac{m'\delta a'}{\sqrt{a'}} + \frac{m'' + \delta a''}{\sqrt{a''}}$$

by differentiation (Laplace [1784], 71).[11]

From these two equations $\delta a'$ and $\delta a''$ can be solved for in terms of δa, yielding:

$$\delta a' = \frac{-m\delta a a'^2}{m' a^2} \frac{a^{\frac{3}{2}} - a''^{\frac{3}{2}}}{a'^{\frac{3}{2}} - a''^{\frac{3}{2}}}$$

$$\delta a'' = \frac{m\delta a a''^2}{m'' a^2} \frac{a^{\frac{3}{2}} - a'^{\frac{3}{2}}}{a'^{\frac{3}{2}} - a''^{\frac{3}{2}}}$$

If $nt, n't, n''t$ are the mean motions of the satellites, then one may use

$$n^2 = \frac{1}{a^3}, n'^2 = \frac{1}{a'^3}, n''^2 = \frac{1}{a''^3}$$

[11] Actually, it is a first order linear approximation.

and, differentiating

$$\delta n = -\frac{3}{2} n \frac{\delta a}{a}$$

$$\delta n' = -\frac{m}{m'} \delta n \frac{n'^{\frac{4}{3}}(n - n'')}{n^{\frac{4}{3}}(n' - n'')}$$

$$\delta n'' = \frac{m}{m''} \delta n \frac{n''^{\frac{4}{3}}(n - n')}{n^{\frac{4}{3}}(n' - n'')}$$

In this way, determining the variations in the mean motions of all three satellites reduces to finding those of just the first (Laplace [1784], 72)

From ([1784] 1), ([1784] 2), and ([1784] 3), above, multiplying these by dx, dy, and dz, respectively (where x, y, z and the respective primes and double primes are the coordinates of the satellites), and further designating

$$R = \frac{m'(xx' + yy' + zz')}{r'^3} + \frac{m''(xx'' + yy'' + zz'')}{r''^3} - \frac{\lambda}{m}$$

and letting d represent the differential with respect only to the coordinates of the first satellite, the equations become:

$$0 = \frac{dxd^2x + dyd^2y + dzd^2z}{dt^2} + (1+m)\frac{dr}{r^2} + dR$$

which can then be integrated to

$$0 = \frac{dx^2 + dy^2 + dz^2}{dt^2} - \frac{2(1+m)}{r} + \frac{1+m}{a} + 2\int dR$$

Supposing the term $2dR$ contains a constant term kdt; then, ignoring the periodic terms one obtains:

$$0 = \frac{dx^2 + dy^2 + dz^2}{dt^2} - \frac{2(1+m)}{r} + \frac{1+m}{a} + kt$$

Or, making this secular component kt due entirely to the variation of the semimajor axis a, Laplace obtains

$$\frac{1+m}{a+\delta a} = \frac{1+m}{a} + kt$$

which gives (neglecting quantities of order m and the square of δa)

$$\delta a = -a^2 kt$$

and thus, the question of the variation of δa reduces to determining the constant k of dR.

Laplace points out that dR does not contain any constant terms, insofar as quantities of the order of the perturbing masses (Laplace [1784], 73. The work cited is Lagrange [1776]).

Introducing ν, ν', and ν'' as the angles formed by the projection of the radius vectors r, r', r'' onto Jupiter's orbital plane, and $nt+\epsilon$, $n't+\epsilon'$, and $n''t+\epsilon''$ the mean longitudes, one has

$$(2n''-3n'+n)t+2\epsilon''-3\epsilon'+\epsilon$$

"very nearly constant, according to observations" (Laplace [1783], 73). Laplace designates this angle as V.

Assuming the eccentricities and inclinations to be negligible, one also has, in rectangular coordinates:

$$\begin{array}{ccc} x = r\cos\nu & y = r\sin\nu & z=0 \\ x' = r'\cos\nu' & y' = r'\sin\nu' & z'=0 \\ x'' = r''\cos\nu'' & y'' = r''\sin\nu'' & z''=0 \end{array}$$

and hence

$$R = \frac{m'r\cos(\nu'-\nu)}{r'^2} + \frac{m''r\cos(\nu''-\nu)}{r''^2}$$
$$-\frac{m'}{\sqrt{r^2-2rr'\cos(\nu'-\nu)+r'^2}} - \frac{m''}{\sqrt{r^2-2rr''\cos(\nu'-\nu)+r''^2}}$$

which Laplace supposes can be written as a series in terms of cosines of $\nu'-\nu$, $\nu''-\nu$ and its multiples:

$$\begin{aligned} R &= m'\left[A^{(0)}+A^{(1)}\cos(\nu'-\nu)+A^{(2)}\cos 2(\nu'-\nu)+\ldots\right] \\ &\quad +m''\left[B^{(0)}+B^{(1)}\cos(\nu''-\nu)+B^{(2)}\cos 2(\nu''-\nu)+\ldots\right] \end{aligned}$$

Then dR is determined, by differentiating with respect to r and ν.

To continue, Laplace refers to Lagrange's 1766 work on the satellites of Jupiter, and uses Lagrange's lengthy expressions for r, r', and r'', and ν, ν', and ν''. After a tedious amount of work, Laplace arrives at:

$$k = m'm''n\sin V\left(l\frac{\partial A^{(1)}}{\partial r'} - hA^{(1)}\right)$$

and hence

$$\delta a = -m'm''a^2nt\sin V\left(l\frac{\partial A^{(1)}}{\partial r'} - hA^{(1)}\right)$$

and thus

$$\delta n = \frac{3}{2}m'm''an^2t\sin V\left(l\frac{\partial A^{(1)}}{\partial r'} - hA^{(1)}\right)$$

From this, Laplace concludes:

$$\begin{aligned} 2\delta n''-3\delta n'+\delta n &= \tfrac{3}{2}an^2t\sin V\left(l\tfrac{\partial A^{(1)}}{\partial r'} - hA^{(1)}\right) \\ &\quad \times\left[m'm''+3mm''\frac{n'^{\frac{4}{3}}(n-n'')}{n^{\frac{4}{3}}(n-n'')}+2mm'\frac{n''^{\frac{4}{3}}(n-n')}{n^{\frac{4}{3}}(n'-n'')}\right] \end{aligned}$$

Letting the coefficient of $n^2 t \sin V$ be designated α, and the quantity $2n'' - 3n' + n$ be designated s, Laplace concludes:

$$\delta s = \alpha n^2 t \sin V$$

Using the method of [1772], Laplace first expresses s as the series:

$$s + \alpha t \frac{ds}{\alpha dt} + \frac{\alpha^2 t^2}{1 \cdot 2} \frac{d^2 s}{\alpha^2 dt^2} + \ldots$$

(in other words, assuming $s = s(\alpha t)$; here s is the initial value), and thus

$$\frac{ds}{dt} = \alpha n^2 \sin V$$

Recall that

$$V = 2n''t - 3n't + nt + 2\epsilon'' - 3\epsilon' + \epsilon$$

and thus

$$dV = dt(2n'' - 3n' + n) = sdt$$

one then has an equation relating s, V, t. Differentiating this last with respect to t, one obtains

$$\frac{d^2 V}{dt^2} = \alpha n^2 \sin V$$

(using the above expression for $\frac{ds}{dt}$) (Laplace [1783], 84). This can then be solved:

$$\frac{\pm dV}{\sqrt{\lambda - 2\alpha n^2 \cos V}} = dt$$

where λ is an arbitrary constant.

Laplace, ever the applied mathematician, then breaks the solution down into three cases and compares the behavior of V (as determined by λ) to the observed behavior of the Jovian Moons, to determine which of the three apply: in this case, it is the situation where α is positive and λ is less than $2\alpha n^2$, which yields for V a libration around $V = 180°$.

Letting $V = 180° + \omega$ (and hence, $dV = d\omega$), Laplace obtains:

$$d\omega = dt\sqrt{\lambda + 2\alpha n^2 \cos\omega}$$

Since ω is very small, Laplace uses the substitution

$$\cos\omega = 1 - \frac{\omega^2}{2}$$

and

$$\frac{\lambda + 2\alpha n^2}{\alpha n^2} = \varrho$$

the equation becomes

$$\frac{d\omega}{\sqrt{\varrho^2 - \omega^2}} = ndt\sqrt{\alpha}$$

which has the solution

$$\omega = \varrho \sin(nt\sqrt{\alpha} + \gamma)$$

where ϱ and λ need to be determined by "observation alone" (Laplace [1783], 82-3). Moreover

$$s = \pm n\varrho\sqrt{\alpha}\cos(nt\sqrt{\alpha} + \gamma)$$

since $\frac{dV}{dt} = s$ (Laplace [1783], p. 83).[12]

This implies, then, that s is periodic, and so neglecting periodic terms, one may conclude that

$$\epsilon - 3\epsilon' + 2\epsilon'' = 0 \qquad n - 3n' + 2n'' = 0$$

In other words, the inner satellites preserve the commensurable relationship between their mean motions.

> "These equations subsist even in the case where, by unknown causes, such as the resistance of the ether, the mean movements of the satellites of Jupiter are subject to a secular equation." (Laplace [1783], 84)

Supposing these secular equations give rise, in the expression of δn, $\delta n'$, and $\delta n''$ terms of the form it, $i't$, and $i''t$, and after much work (including another change of variables, of the type characteristic of his work in [1772]), Laplace arrives at an important connection between these quantities i, i' and i'', and V:

> "The mean value of V is $180° + \frac{i-3i'+2i''}{\alpha n^2}$; thus, in supposing $i-3i'+2i''$ positive, V will be greater than $180°$, and one can explain by this why all the tables of the satellites of Jupiter give $V > 180°$; but one must observe that the quantities i, i', i'' need to be insensible relative to αn^2, since, otherwise, they would produce, in the mean movements of the satellites, secular equations that the would be rendered quite sensible in the interval of time that has passed since their discovery to our day." (Laplace [1784], 87)

In other words, in this particular case where the mean motions are commensurable, the observational evidence suggests that the secular equations, although existing in theory, are vanishingly small in practice.

9.4 Memoir on Secular Variations of the Orbits of the Planets, 1787

Laplace reformulated this work in [1787], a short (only 12 pages) article that contains the important theorem relating the permissible changes in the elements of orbit to each other. Again, he praises Lagrange's work lavishly, clearly differentiating his "approximate" work from that of Lagrange's "more rigorous" formulation:

[12]The manuscript reads $\frac{dV}{dt} = ds$, which is clearly a misprint

> "Therein are two very interesting [results] on the secular variations of the orbits which are independent of the masses of the planets: one is the uniformity of the mean celestial motions, [and] the other is the stability of the planetary system. I have reached elsewhere, by approximation, the first of these results that M. de la Grange has since demonstrated rigorously." (Laplace [1787], 296)

As for the second of these results, Laplace writes:

> "As for the stability of the planetary system, I have proven in our *Memoirs* for the year 1784 that as long as the planets always move in the same sense and their orbits are nearly circular and little inclined to one another, the excentricities and inclinations of these orbits are always contained within narrow limits, and thus the system of the world makes only oscillations around a mean state from which it never strays more than a small quantity." (Laplace [1787], 296)

The requirement that the planets move around the sun "in the same sense" (i.e., in the same direction) is implicit in the 1784 proof, though not explicitly stated by Laplace.[13] The new proof of Laplace combines elements of many of his previous articles to provide a very simple derivation of ([1784] XIV). Laplace begins with the masses $m_0, m_1, m_2, \ldots$ of the planets, with that of the sun being 1; the mean movements $n_0 t, n_1 t, n_2 t, \ldots$, the mean distances $a_0, a_1, a_2, \ldots$, the excentricities $e_0, e_1, e_2, \ldots$, the longitude of their aphelia $\omega_0, \omega_1, \omega_2, \ldots$, the tangents of their inclinations $\theta_0, \theta_1, \theta_2, \ldots$, and the longitudes of their ascending nodes $I_0, I_1, I_2, \ldots$. Then he introduces the quantities:

$$\begin{array}{cc} e_0 \sin \omega_0 = p_0 & e_0 cos \omega_0 = q_0 \\ e_1 \sin \omega_1 = p_1 & e_1 cos \omega_1 = q_1 \\ \cdots & \cdots \\ \theta_0 \sin I_0 = h_0 & \theta_0 \cos I_0 = l_0 \\ \theta_1 \sin I_1 = h_1 & \theta_1 \cos I_1 = l_1 \\ \cdots & \cdots \end{array}$$

(h and l were also introduced in 1784, along with p and q) (Laplace [1787], 297). Then Laplace expands the function

$$(a_i^2 - 2a_i a_r \cos V + a_r^2)^{-\frac{3}{2}}$$

as

$$(a_i, a_r) + (a_i, a_r)_1 \cos V$$

[13]The reason for this is that the values of $\sqrt{a}$ upon which his equation depends is determined from the mean motions: if a planet moves in an opposite direction, its mean motion has a negative value, and hence one should take the negative of $\sqrt{a}$. This would permit the values of some of the terms of ([1784] XIV) to be negative, and one could increase without limit, provided a corresponding term of the opposite sign likewise increased.

to first degree (Laplace [1787], 297).[14] Note that Laplace's (a_i, a_r) and $(a_i, a_r)_1$ are equivalent to Lagrange's (r, r_1) and $(r, r_1)_1$.[15] Laplace then designates:

$$\frac{m_r n_i}{4} a_i^2 a_r (a_i, a_r)_1 = (i, r)$$

$$\frac{m_r n_i}{2} a_i [(a_i^2 + a_r^2)(a_i, a_r)_1 - 3a_i a_r (a_i, a_r)] = [i, r]$$

which makes:

$$m_i \sqrt{a_i}(i, r) = m_r \sqrt{a_r}(r, i)$$

$$m_i \sqrt{a_i}[i, r] = m_r \sqrt{a_r}[r, i]$$

Moreover:

$$(a_i, a_r) = (a_r, a_i)$$

$$(a_i, a_r)_1 = (a_r, a_i)_1$$

due to the symmetry of the series expansions, and hence:

$$\frac{m_i(i, r)}{a_i n_i} = \frac{m_r(r, i)}{a_r n_r}, \frac{m_i[i, r]}{a_i n_i} = \frac{m_r[r, i]}{a_r n_r}$$

because the planets, provided they all move in the same direction, follow the relationship:

$$n_i = \frac{1}{\sqrt{a_i^3}}, n_r = \frac{1}{\sqrt{a_r^3}}$$

which can be substituted into the preceding (Laplace [1787], 298).

From this, he obtains the differential equations:

$$\frac{dp_i}{dt} = q_i \sum (i, r) - \sum q_r [i, r]$$

$$\frac{dq_i}{dt} = -p_i \sum (i, r) - \sum p_r [i, r]$$

where the sums are done over r. Laplace had previously derived this result in [1772], using his method of eliminating secular terms by means of a change of variables (Laplace [1772], 462).[16]

[14] While there is some justification is considering an approximation as far as the first degree in the eccentricity and tangent of the inclinations, there is considerably less for allowing a first degree approximation in terms of the mean distances.

[15] Strictly speaking, Laplace's (a_0, a_r) is equivalent to (r, r_1); Lagrange does not include the corresponding coefficients for the other planets $T_1, T_2, \ldots$.

[16] To first order in the masses and eccentricities, Laplace derives the following equation for r, the radius vector, in the two planet case:

$$\begin{aligned} r \quad = \quad & a\Big(1 + \tfrac{1}{2}(p^2 + q^2) - \tfrac{1}{4}(h^2 + l^2) \\ & + \sin(nt + B_1)[p + (0,1)qu - \overline{(0,1)}q'u] \\ & \cos(nt + B_1)[q - (0,1)pu + \overline{(0,1)}p'u]\Big) \\ & + \overline{(0,1)}u(p'q - pq') + \tfrac{1}{2}(0,1)u(h'l - hl') \\ & Q + p\delta\mu' R + q\delta\mu' R_1 + p'\delta\mu' R_2 + q'\delta\mu' R_3\Big) \end{aligned}$$

If one multiplies the first of these by p_i and the second by q_i, and adds them, one obtains:

$$p_i \frac{dp_i}{dt} + q_i \frac{dq_i}{dt} = \sum (q_i p_r - p_i q_r)[i, r]$$

Then multiplying this by $m_i\sqrt{a_i}$, and summing over i one arrives at:

$$\sum' m_i\sqrt{a_i}(p_i \frac{dp_i}{dt} + q_i \frac{dq_i}{dt}) = \sum' \sum \Big(q_i p_r - p_i q_r \Big) m_i\sqrt{a_i}[i, r]$$

where $\sum'$ is the summation with respect to i. However, all terms on the right hand side of this equation cancel,[17] and one obtains:

$$\sum' m_i\sqrt{a_i}\left(p_i \frac{dp_i}{dt} + q_i \frac{dq_i}{dt} \right) = 0$$

Which, upon integration, yields:

$$\sum' m_i\sqrt{a_i}(p_i^2 + q_i^2) = \text{const.}$$

or, as $p_i^2 + q_i^2 = e_i^2$

$$\sum' m_i\sqrt{a_i}e_i^2 = \text{const.}$$

provided that the semimajor axes of the planets a are constants, something which has not yet been established (recall that Lagrange proved only that the major axes underwent no secular variations).

A similar set of procedures on the equations involving h and l yields:

$$\sum' m_i\sqrt{a_i}\theta_i^2 = \text{const.}$$

where a is the mean distance, $\delta\mu, \delta\mu'$ are the masses of the planets, $p = \alpha e \sin A$, $p' = \alpha e' \sin A'$, $q = \alpha e \cos A$, and $q' = \alpha e' \cos A'$, $h = \alpha\gamma \sin C_1$, $h' = \alpha\gamma' \sin C_1'$, $l = \alpha\gamma \cos C_1$ and $l' = \alpha\gamma' \cos C_1'$ (where α is a small quantity, e is the eccentricity, γ the tangent of the inclination of the orbit; A the aphelion position, and C, C_1 is the position of the nodes. Note that this is an approximation not to even less than the first order in the eccentricities and inclinations). B, B_1 are constants of integration, and $R, R_1, \ldots$ and Q are periodic quantities that do not contain terms including $e, e', \ldots, A, A', \ldots$ or $C, C', \ldots$. u and t are related by:

$$\tfrac{1}{4}(b_1)\delta\mu' nt = (0,1)u \qquad \tfrac{1}{2}[(b_1)(1+i^2) - 3i(b)]\delta\mu' = \overline{(0,1)}u$$

where n is the mean motion of the planet in the unperturbed orbit, and b, b_1 the coefficients of the first two terms that appear in the series expansion for the cube of the distance between the two planets.

From this, Laplace uses his method of "eliminating" secular terms, by making the quantities $p + (0,1)qu - \overline{(0,1)}q'u$ equal to a linear approximation to p, and thus

$$\frac{dp}{du} = (0,1)q - \overline{(0,1)}q'$$

Likewise:

$$\frac{dq}{du} = -(0,1)p + \overline{(0,1)}p'$$

The extension to the n body case yields Laplace's equations, above.

[17]Since $m_i\sqrt{a_i}[i, r] = m_r\sqrt{a_r}[r, i]$, as per the above.

and similar equations for h and l (Laplace [1787], 299-300) Once again, since no term can grow without limit, this is proof enough that the excentricities and the inclinations contain neither exponentials nor the "arcs of circles" (secular terms). (Laplace [1787], 304)

> "The preceding analysis can only be applied to a system of planets which all move in the same sense, as is the case in our planetary system; in this case, one sees that the system is stable and never moves more than a little from a mean state around which it oscillates with extreme slowness. But does this remarkable property apply equally to a system of planets which move in a different sense? This is very difficult to determine. As this research has no utility in astronomy, we refrain from occupying ourselves with it." (Laplace [1787], 306)

Thus, so far as Laplace is concerned, the stability of *this* solar system has been assured, and the *general* problem of a system of planets operating under the inverse square law is of no concern.

Chapter 10

Reactions to the Theorems

The work of Lagrange and Laplace gave the first answer to Newton's Query number 31: no such intervention by a Deity was necessary. (One may speculate that Newton would have been ambivalent about this discovery: on the one hand, it would be a triumph of the power of universal gravitation; on the other hand, the necessity of Divine intervention in maintaining the solar system could be used as strong proof that the Divine existed.)

The contemporaries of Laplace and Lagrange heralded it as a remarkable achievement of physical science — *Lagrange's* remarkable achievement. It was Lagrange who first established rigorously (at least by the standards of the time) that the major axes of the planets were subject only to periodic variation. It was Lagrange who first derived and solved the set of differential equations determining the product of the inclinations and line of the nodes. Laplace did the same thing for the position of perihelion and the eccentricity, of course, but in doing so he was merely following the path that Lagrange had charted.

As for Laplace, the astronomer, the applied mathematician, his achievement was one of showing to an approximation that which Lagrange demonstrated rigorously; while Laplace's methods might be of interest to the practical astronomer who wished to compute the places of the planets in the not too distant future, they were hardly relevant to the solution of the classical stability question.

10.1 Lalande

For example, in the third edition of Lalande's *Astronomy*, published in 1792, the author states (numbers in parentheses refer to sections in Lalande's work):

> "One has reason to believe an acceleration in [the mean motion of] Jupiter (1169) and one for the Moon (1483): one could attribute the acceleration of Jupiter to the resistance of the ether; but the retardation that one finds in Saturn indicates however that one and the other need be an effect of their reciprocal attraction: this was already the opinion of Cassini (Mem. 1746). The geometers have

> occupied themselves for a long time in researching, by the calculus of attraction, the explanation of these secular equations without being able to find it, as one can see in the pieces of M. Euler on the Moon, prize of 1770, pag. 94, and 1772, pag. 15; and in the work of M. de la Grange, likewise in his Memoirs, which start with those of Berlin, 1776, 1781.[1] M. de la Place read a memoir in 1785, for establishing the same thing for the secular equation of the planets: he has shown that the attractions of the planets, as far as an approximation of the third degree, have no effect on their mean movements (*Mem. presentes*, 1778)[2]. M. de la Grange has demonstrated rigorously that the major axes of the orbit at no point change by their mutual attractions." (Lalande [1792], III, 478)

Thus, Laplace has shown *approximately* what Lagrange has demonstrated *rigorously*. Laplace's contribution is demonstrating that the variation of the other elements is limited:

> "It appears that in general all is periodic; the planets move all in the same sense, in nearly circular orbits, little inclined to one another, it follows, by theory, that these same excentricities and inclinations are always contained in narrow limits, and that therefore the system of the world makes only oscillations about a mean state that never exceed a small quantity (M. de la Place, *Mem. de l'acad.* 1787)." (Lalande [1792], III 479)

As for the resisting ether, an issue that Laplace examined briefly and Lagrange not at all:

> "3680. Thus the resistance of the material ether is not proven; attraction explains all the observed effects; and one need recognize that, if the celestial bodies are not in an absolute vacuum, they are at least in a material whose effect is insensible, and which is to us as a vacuum: this alone suffices to overthrow [*dissiper*] the system of vortices that we have already refuted (3529)." (Lalande [1792], III, 479)

Thus, the resisting material ether drops out of scientific thought.

10.2 Laplace

The major work on celestial mechanics was, of course, Laplace's *Mécanique Céléste*, a massive compilation of the results and methods of Laplace and his

[1]Lalande here is most likely referring to Lagrange's [1776] and [1781]

[2]This may be a misprint for 1787, in which case the article referred to *might* be [1784], read in 1784 and published in 1787

contemporaries and predecessors. Its utility as a compilation, historically speaking, is limited by Laplace's total lack of referencing who was responsible for what innovation.

Laplace provides not one but two proofs of the constancy of the semimajor axis. The first is his own, and consists essentially of determining, in an Euler-I fashion, the variation of the radius vector r from the perturbations; as the perturbations are periodic, so must the variations in the radius vector. The second method, however, is Lagrange's, and differs in no significant detail from the work of 1776. Like Lagrange, Laplace will argue that this quantity cannot have non-periodic terms, unless the mean motions of the planets are commensurable — which cannot take place in the solar system. Laplace also includes, in the first volume of *Mécanique Céléste* the work of 1784 and 1787, linking the elements of orbit into one great equation.

10.3 Playfair

The January, 1808 edition of the *Edinburgh Review* contains a review of Laplace's *Traité de Méchanique Céléte* [sic] written by John Playfair (1748-1819). He writes:

> "He [Lagrange] found, by a method peculiar to himself, and independent of any approximation, that the inequalities produced by the mutual action of the planets, must, in effect, be all periodical: that amid all the changes which arise from their mutual action, two things remain perpetually the same; *viz.* the length of the greater axis of the ellipse which the planet describes, and its periodical time round the sun, or, which is the same thing, the mean distance of each planet from the sun, and its mean motion remain constant. The plane of the orbit varies, the species of the ellipse and its eccentricity change; but never, by any means whatever, the greater axis of the ellipse, or the time of the entire revolution of the planet. The discovery of this great principle, which we may consider as the bulwark that secures the stability of our system, and excludes all access to confusion and disorder, must render the name of La Grange for ever memorable in science, and ever revered by those who delight in the contemplation of whatever is excellent and sublime." (Playfair [1808], 263-4)

Of course, Laplace's accomplishments were important too: he reconciled the observational evidence of the acceleration of Jupiter and Saturn to the theoretical discovery of Lagrange (Playfair [1808], 264). Moreover, Laplace was responsible for discovering:

> "In all the planetary orbits, this eccentricity is small,and the ellipse approaches nearly to a circle. These eccentricities, however, continually change, though very slowly, in the progress of time, but in such a manner, that none of them can ever become very great. They

> may vanish, or become nothing, when the orbit will be exactly circular; in which state, however, it will not continue, but change in the course of time, into an ellipsis, of an eccentricity that will vary as before, so as never to exceed a certain limit. What this limit is for each individual planet would be difficult to determine, the expression of the variable eccentricities being necessarily very complex. But, notwithstanding of this, a general theorem, which shows that none of them can ever become great, is the result of one of La Place's investigations." (Playfair [1808], 265)

In what appears to be, except for Euler, a particularly English concern, Playfair raises the connection between this "beautiful system" and whether or not it could proceed without "the counsel and dominion of an intelligent and powerful Being":

> "Now, the investigations of La Place enable us to give a very satisfactory reply to these questions; *viz.* that the conditions essential to the stability of a system of bodies gravitating mutually to one another, are by no means necessary, insomuch that systems can easily be supposed in which no such stability exists. The conditions essential to it, are the movements of the bodies all in one direction,[3] their having orbits of small eccentricity, or not far different from circles, and having periods of revolution not commensurable with one another. Now, these conditions are not necessary; they may easily be supposed different; any of them might be changed, while the others remained the same. The appointment of such conditions therefore as would necessarily give a stable and permanent character to the system, is not the work of necessity; and no one will be so absurd as to argue, that it is the work of chance: it is therefore the work of design, or of intention, conducted by wisdom and foresight of the most perfect kind." (Playfair [1808], 278-9)

Playfair then chides Laplace for not pointing this out: "This is not taken notice of by La Place; and that it is not, is the only blemish we have to remark in his admirable work." (Playfair [1808], 279) One is reminded of the (probably apocryphal) remark Laplace made when Napoleon made a similar comment about the lack of God in his work: "I have no need of that hypothesis."

10.4 Lagrange

Lagrange later published another version of his work, "Memoire sur la Theorie des Variations des Elements des Planetes," read to the Institute on August 22, 1808. Here Lagrange seems to show a distinct preference for his [1781] as opposed to [1776]:

[3]Laplace's requirement in 1787

> "Thus the elements of the elliptical movement, which by the action of the sun alone are constants, become subject to small variations; while, rigorously, the movement is no more elliptical, one can nonetheless regard it as such at each instant; the variable ellipse need then osculate as I have shown elsewhere, and which is based on the variation of constants. It is in this manner that I have considered and calculated the variation of the elements of the planets in the Theory of these variations, which I have given in the *Memoirs of the Berlin Academy*, [in the] years 1781, 1782, and in the following." (Lagrange [1808], 715)

One should keep in mind, however, that while [1781] was meant to extend [1776], Lagrange did not intend to replace the one with the other, for the technique of [1776] to prove the invariability of the semimajor axes formed an important part of [1781].

10.5 Poisson

Poisson, who will ultimately carry out the approximation of Laplace to an even higher degree, read to the Institute (formerly the French Academy of the Sciences), on October 16, 1809, a paper "Sur la Variation des Constantes arbitraires dans les questions Mécanique". His opening remarks praise Lagrange lavishly, and make little mention of Laplace:

> "Since Euler, who gave the first differential expression of the variation of the longitude of the nodes and of the inclination of the orbit[4], one has found, by different means, the differentials of the other elements, but in the *Mémoires de Berlin*, for the years 1781 and 1782, M. Lagrange has given on this subject a general and complete theory, in which the differentials of the elliptical elements are expressed by means of the partial differences of a similar function, composed of the coordinates of the perturbing planets, and multiplied by the functions of these same coordinates; except, however, the the differential of the longitude of a planet to a determined epoch, that M. Lagrange has expressed in series, and which I have since put in a more simple form." (Poisson [1809], 267)

Thus, according to Poisson, it is Lagrange's work, particularly [1781] and [1782], that contains the essential results.

10.6 Somerville

In addition to the 1829 English edition, translated by Bowditch, other commentators attempted to bring the work of Laplace to more general circulation. One

[4]Either in Euler's [1758] or the earlier [1749], where Euler produced the differential equations without their derivations.

of them was Mary Somerville, who in 1831 published *Mechanism of the Heavens*; it is a somewhat less mathematical overview of celestial mechanics than Bowditch's translation of *Mécanique Céléste*.

Somerville notes:

> "The consequences [of a secular variation in the elements of orbit] being so dreadful, it is natural to inquire, what proof exists that creation will be preserved from such a catastrophe? for nothing can be known from observation, since the existence of the human race has occupied but a point in duration, while these vicissitudes embrace myriads of ages. The proof is simple and convincing ..." (Somerville [1831], xiv)

Somerville notes the discovery was made by Lagrange, but even had she not said so explicitly, it is implicit in her remarks on the proof of stability, for the argument in the introduction is very similar to that given by Lagrange in [1773]:

> "Three circumstances have generally been supposed necessary to prove the stability of the system: the small eccentricities of the planetary orbits, their small inclinations, and the revolution of all the bodies, as well planets as satellites, in the same direction. These, however, are not necessary conditions: the periodicity of the terms in which the inequalities are expressed is sufficient to assure us, that though we do not know the extent of the limits, nor the period of that grand cycle which probably embraces millions of years, yet they never will exceed what is requisite for the stability and harmony of the whole, for the preservation of which every circumstance is so beautifully and wonderfully adapted." (Somerville [1831], xv)

Thus, she credits the "periodicity of the terms" as the sufficient condition for the stability of the solar system. While Laplace did rely on periodicity, the proof of stability *from* periodicity is Lagrange's.

10.7 Recent Assessments

As time blurs the accomplishments of the mathematicians of the past, the relative achievements of Laplace and Lagrange undergo a switch, probably due to Laplace's achievements being more recent and, with *Mécanique Céléste*, more visible. Indeed, time has not been kind to Lagrange's contributions. For example, Moulton will credit Laplace's [1773] with demonstrating that the eccentricities and major axes of the planets undergo only periodic variations, in 1773 (Moulton [1914], 432). Szebehely, in his brief discussion of the history of celestial mechanics, mentions none of Lagrange's achievements in the area of stability, citing instead Laplace's [1773] and [1784] as important landmarks in the stability question (Szebehely [1989], 18; Roy [1982] 248). A fairly representative sentence, from the Dictionary of Scientific Biography, reads:

> "Laplace himself had long since shown ([1776c]) that the eccentricities and inclinations of the orbits are bound to remain small under the operation of gravity, provided that only two planets are considered; and in 1782 Lagrange had further shown ("Oeuvres de Lagrange, V, 211-345) that the same is true generally of the location of nodes and aphelia, given plausible hypotheses about the masses...That was the final object of [Laplace's] current memoir." (Dictionary of Scientific Biography, Volume XV, 327)

Thus, both Lagrange's [1774], the basis for the cited article (Laplace [1773]), as well as Lagrange's [1776] are ignored; moreover, the work of [1774] is attributed to [1781].

Chapter 11

Development Since 1784

Even after Lagrange and Laplace, questions still remained. The approximations could be carried out to higher order. Moreover, even if Laplace's "celebrated equation" held after higher degrees were taken into account, the great disparity of masses in the solar system did not preclude, for example, Jupiter undergoing a tiny variation of eccentricity that caused Mercury's orbital eccentricity to increase tremendously, and thereby ruin the assumption of near circularity.

In 1809, Poisson discovered, upon considering approximations to the second degree in the masses, terms of the form $t \sin at$, which are unbounded, but yet periodically return to zero; this called for a new definition of stability, to become known as "Poisson stability" (Goroff [1993], I64). For short periods of time, these secular terms might be eliminated through use of Laplace's method in [1773], though the existence of such terms raised doubts about the overall stability of the solar system.

LeVerrier and Cellerier carried the approximations to the mean motion to still higher orders; Leverrier's work was published in the Volume II of the *Annales de l'Observatoire de Paris* (1856), and took up many pages of dense calculations. After much work, Leverrier demonstrated that the variations were, indeed, small. He did, however, discover that there were certain initial conditions that would cause a body to attain a large inclination relative to the other planets, due to the influence of Jupiter and Saturn.

Most significantly, in the 1870s, Newcomb and Lindstedt were able to show that the series for the elements of orbit (or rather, Lagrange and Laplce's conjugated quantities s, u, h, l) could be written formally as a trigonometric series.[1] As Poincaré writes:

> "This result might have been considered by Laplace or Lagrange as completely establishing the stability of the solar system. We are somewhat more painstaking today, since the convergence of the

[1] Newcomb's publication occurred exactly 100 years after Lagrange's "Sur l'Alteration des Moyens Mouvements des Planetes"

> expansion has not been proved; the result is no less important for that." (Poincaré [1892], II, 354)

Poincaré further established that it was impossible to produce exact solutions to the equations of motion in the n-body problem, where n is bigger than 2: approximate solutions by means of infinite series are the only viable solutions. Poincaré also established that these series generally diverge, making them useless for prediction over infinite time. Poincaré differentiates between the notion of *numerical* stability over a short period of time (which might be hundreds of millions of years) and mathematical stability over infinite time, reminiscent of Johann Albrecht Euler's suggestion that the effects of the ether were insignificant even after thousands of years.

Since Poincaré, much work has been done with computer simulations; these digital orreries (to use a phrase coined by Ivars Peterson) have shown the outer planets to show fairly regular behavior (with the exception of Pluto) over the next 875 million years (Sussman and Wisdom, [1988], 434); J. Laskar demonstrated similar results using different methods for the inner planets (Laskar [1989], 23-4). However, these methods run up against a "wall of chaos", making useful predictions very problematic beyond 100 million years, as small uncertainties in initial positions are multiplied into vast uncertainties in trajectories over extended periods of time.

Important theoretical progress was made with the KAM Theory, which demonstrated that if the perturbations were small, a large set of initial conditions led to quasi-periodic solutions; if it could be shown that the solar system was in one of those initial conditions, then the stability question could be answered — though, unfortunately, proof that the solar system is in one of these initial states is lacking (Laskar [1989], 23). A conjecture aired by Zhihong Xia and others suggests that arbitrarily near every set of initial conditions there are points that escape to infinity, suggesting it would be impossible to establish the stability of the solar system (Kaplan [19??]).

Hence, as to whether or not the real stability problem should ever be solved, a quote from Euler will be our last word on the subject. When the problem of the lunar theory was still unsolved, and the first proofs of the stability of the solar system years in the future, Euler wrote to Tobias Mayer on February 26, 1754, and said:

> "You will certainly also have noticed that if the Moon were 4 or 5 or more times farther away from us than it actually is, if its eccentricity were several times larger, and if its inclination to the ecliptic were also far greater than it actually is; then, I maintain, its motion could no longer be determined through such tables. I would be very curious to see tables for such a Moon, and in spite of most people considering such a work to be superfluous and almost laughable, out of such a theory the greatest advantages could nevertheless be drawn. I must confess that I do not at all foresee the possibility of the same [kind of] tables being completed, through which one could also determine

only in the crudest way the position of such a Moon. Perhaps such an investigation quite exceeds our energies, and perhaps this is the cause of there being actually no such body in existence in the world, whose motion would always remain quite unknown to us. Hence one can infer a particular reason for wondering at the wisdom of the Creator, for it could have perhaps happened otherwise that such a heavenly body were actually in existence, that one could regard in all respects neither as a primary planet nor as a satellite. The motion of the Moon and of Saturn therefore appear to determine the limits of human understanding . . . ” (Forbes [1971], 79)

Chapter 12

The Lagrange Points: Geometric Determination

Newton's *Principia* was the first and last important work of celestial mechanics to use purely geometrical methods. Although Newton did not do so, it is clear from the following that a purely geometrical solution to the circular three body problem may be found using his techniques.

First, we quote one of Newton's propositions. Book I, Section II, Proposition IV, Theorem IV of the *Principia* states:

> "The centripetal forces of bodies, which by equable motions describe different circles, tend to the centres of the same circles; and are to each other as the squares of the arcs described in equal times divided respectively by the radii of the circles." (Newton [1726], 47)

In particular, Corollary III states:

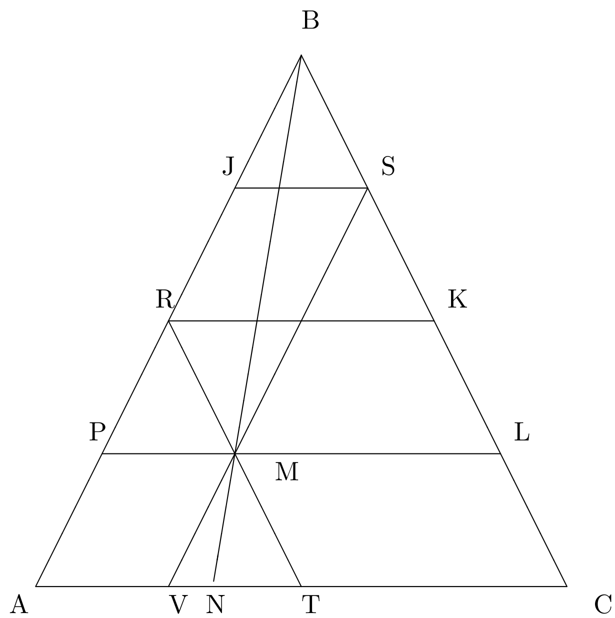
> "Whence if the periodic times are equal, and the velocities therefore as the radii, the centripetal forces will be also as the radii; and the contrary." (Newton [1726], 48)

First we prove that the force vector points to the center of mass if and only if the distances of the third body from the other two are equal. Then we prove that the centripetal force is proportional to the distance from the center of mass if and only if the three bodies are placed at the vertices of an equilateral triangle. This last, combined with Newton's proposition, means that their periods about the center of mass will be equal.

Proposition I: let three bodies be placed at A, B, and C, with bodies A and C attracting others to themselves in a ratio proportional to their masses M and m, and inversely proportional to some power of the distance between them. Let $SB : RB = \frac{m}{BC^n} : \frac{M}{AB^n}$. Draw SJ, RK parallel to AC; draw SV parallel to AB and RT parallel to BC so they intersect at M; draw PL through M parallel to

AC. Extend BM down to F. Then if $AB = BC$, then $AN : NC = m : M$, and conversely.

Modern translation: the vector sum of the centripetal forces from A and C points at the center of mass if and only if the distances AB and BC are equal.



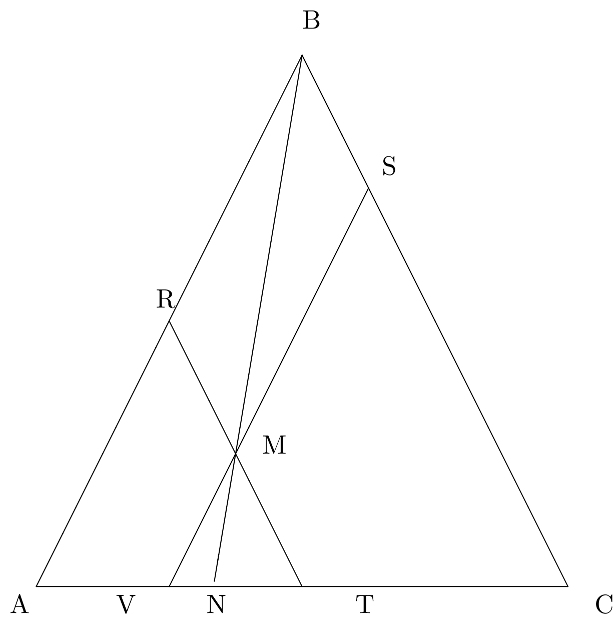
Proof: Note that $JSMP, KLMR$ are parallelograms.

Suppose $AB = BC$. Then $SB : RB = \frac{m}{BC^n} : \frac{M}{AB^n} = m : M$. $\triangle JBS \sim \triangle ABC$ and thus $JB = SB$. $\triangle RBK \sim \triangle JBS$ so $JS : RK = JB : RB = SB : RB$, and thus $JS : RK = m : M$. Since $KLMR, JSMP$ parallelograms, then $RK = ML$ and $JS = MP$. Hence $MP : ML = m : M$. Now, since $PL \parallel AC$, then $MP : ML = AN : NC$ and thus $AN : NC = m : M$, which was to be proven.

Conversely, suppose $AN : NC = m : M$. Then $MP : ML = m : M$, and thus $JS : RK = m : M$. This gives $JB : RB = m : M$. Suppose $SB = aJB$. Then $SB : RB = am : M$. $\triangle ABC \sim \triangle JBS$, so likewise $BC = aAB$, so $SB : RB = \frac{m}{BC^n} : \frac{M}{AB^n} = m : a^n M$. Hence $am : M = m : a^n M$ or $a : 1 = 1 : a^n$, and thus $a = 1$ and $AB = BC$.

Proposition II: Given three bodies placed at A, B, and C, where $AB = BC$. Let $SB : RB = \frac{m}{BC^n} : \frac{M}{AB^n}, AV : SB = \frac{m}{AC^n} : \frac{m}{BC^n}, CT : RB = \frac{M}{AC^n} : \frac{M}{AB^n}$. Let $RM \parallel BC$ and $SM \parallel AB$. Then $\triangle ABC$ is equilateral if $BM : BN = CT : CN = AV : AN$, and conversely.

Modern translation: if $\triangle ABC$ is equilateral, with A and C having bodies attracting others to themselves proportional to their masses and inversely proportional to some power of their distance, then the centripetal forces at C is proportional to the distance from C to the center of mass, and conversely.



Proof: Suppose $\triangle ABC$ is equilateral. Then $AC = AB$, and $CT : RB = 1 : 1$ (since $CT : RB = \frac{M}{AC^n} : \frac{M}{AB^n}$). Hence $CT : AC = RB : AC$, the last also being equal to $RB : AB$. This implies RT is parallel to BC. However, since $RM \parallel BC$, then M is on RT. Hence $\triangle NMT \sim \triangle NBC$. Hence, $CT : CN = BM : BN$. Likewise, $AV : SB = 1 : 1$, and thus $AV : AC = SB : AC = SB : BC$, and thus $SV \parallel AB$, hence M is on SV. Thus $\triangle NMV \sim \triangle NBA$, and thus $AV : AN = BM : BN$. Thus $CT : CN = BM : BN = AV : AN$, which was to be proved.

Conversely, suppose $BM : BN = CT : CN = AV : AN$. Since $AB = BC$ and thus $\triangle ABC$ is isoceles, then by Proposition I, above, we have $AN : NC = m : M$. Draw SJ, RK parallel to AC, and PL through M parallel to AC. Then one has the parallelograms $JSMP, RKLM$. Then $PM : ML = AN : NC = m : M$ by similar triangles. Also, $JS = PM$ and $RK = ML$, thus $JS : RK = m : M$. Suppose $AB : AC = a : 1$. Then, since $\triangle RBK \sim \triangle ABC$, then $RB : RK = a : 1$. Also, $SB : JS = JB : JS = a : 1$ (for $JB = SB$, since $\triangle JBS \sim \triangle ABC$). Thus, $SB : JS = RB : RK$. Hence, $SB : RB = JS : RK$, which is as $m : M$. However, $SB : RB = \frac{m}{BC^n} : \frac{M}{AB^n} = \frac{m}{a^n} : M$. Thus, $a^n : 1 = 1 : 1$, and $a = 1$. Thus, $AB : AC = 1 : 1$, and $\triangle ABC$ is equilateral.

Theorem: if two bodies are placed at A and C, attracting others to themselves as some power of the distance, and a third of negligible mass at B, moving under the influence of the other two, then the three describe equal circles around the center of mass if $\triangle ABC$ forms an equilateral triangle, and conversely.

Chapter 13

Bibliography

d'Alembert (1754): Jean le Rond d'Alembert. *Recherches sur differens points importans du System du Monde, tome I*, 1754. Bruxelles, Culture et civilisation, 1966.

Alexander (1956): H. G. Alexander, ed. *The Leibniz-Clarke Correspondence*. 1956. Philosophical Library, 1956.

Armitage (1966): Angus Armitage. *Edmond Halley*, 1966. Thomas Nelson and Sons, Ltd., 1966.

Arnold (1985): Arnold, V. I., ed. *Dynamical Systems III*, 1985. Springer-Verlag, 1988.

Bailly (1763): Jean Sylvain Bailly. "Premier Mémoire sur la Théorie des Satellites de Jupiter". Read March 27, 1762. *Histoire de l'Academie 1763*, p. 121-136; p. 172-189; p. 377-384.

Baily (1966): Francis Baily, ed. *An Account of the Revd. John Flamsteed, the first Astronomer-Royal*. Dawsons, 1966.

Bentley (1836): Richard Bentley. *The works of richard Bentley, D.D., Volume I*, ed. by Rev. Alexander Dyce. F. Macpherson, 1836.

Bernoulli (1730): Bernoulli, Jean. "Nouvelles Pensées sur le Systême de M. Descartes, Et la maniere d'en déduire les Orbites & les Aphélies des Planètes." *Recueil des pieces qui ont remporte les prix de l'Académie royale des sciences*. Académie des Sciences, 1752-77, Volume II.

Berrnoulli (1734): Bernoulli, Daniel. "Recherches Physique et Astronomiques sur le Problem Proposé pour la Seconde Fois par l'Academie Royale des Sciences de Paris." *Recueil des pieces qui ont remporte les prix de l'Académie royale des sciences*. Académie des Sciences, 1752-77, Volume III.

Bernoulli (1734): Bernoulli, Jean. "Essai d'une Nouvelle Physique Celeste, servant à expliquer les principaus Phenomenes du Ciel, & en particulier la cause physique de l'inclinaison des Orbites des Planetes par rapport au plan de l'Équateur du Soleil." *Recueil des pieces qui ont remporte les prix de l'Académie royale des sciences*. Académie des Sciences, 1752-77, Volume III.

Bigourdan (1928): Bigourdan, G. "Lettres Inédites d'Euler à Clairaut, tirées des archives de l'Académie des sciences". *Comptes rendus du Congrés des*

Sociétés de savantes de Paris, 1928.

Bossut (1762): Bossut, Abbé. "Recherches sur les altérations que la résistance de l'Ether peut produire dans le mouvement moyen des Planetes." *Recueil des pieces qui ont remporte les prix de l'Académie royale des sciences*. Académie des Sciences, 1752-77, vol. VIII.

Bouguer (1748): Bouguer, Pierre. "Entretiens sur la Cause de l'Inclinaison des Orbites des Planetes." *Recueil des pieces qui ont remporte les prix de l'Académie royale des sciences*. Académie des Sciences, 1752-77, vol. II.

Brewster (1855): Brewster, David. *Memoirs of the Life, Writings, and Discoveries of Sir Isaac Newton*, 1855. Johnson Reprint Corp., 1965.

Brown (1896): Brown, Ernest W. *An Introductory Treatise on the Lunar Theory*, 1896. Dover.

Buffon (1747A): Buffon, Georges Louis Leclerc, Comte de. "Reflexions sur la loi de l'Attraction". *Histoire de l'Academie Royale des Sciences, 1745*.

Buffon (1747B): Buffon, Georges Louis Leclerc, Comte de. "Addition au Memoire qui a pour titre: Reflexions sur la Loi de l'Attraction". *Histoire de l'Academie Royale des Sciences, 1745*.

Cassini (1734): Cassini, Jacques. "De L'inclinaison du Plan de l'Écliptique et de l'Orbite des Planetes Par rapport à l'Équateur de la Révolution du Soleil autour de son Axe." *Histoire de l'Academie 1734*, p. 107-122.

Cassini (1735): Cassini, Jacques. "De la Révolution du Soleil et des Planetes autour de leur Axe; et de la maniére que l'on peut concilier, dans le Sisteme des Tourbillons, la vîtesse avec laquelle les Planetes se meuvent à leur surface, avec celle que l'Éther ou le Fluide qui les environne, doit avoir suivant la regle de Képler." *Histoire de l'Academie 1735*.

Cassini (1740): Cassini, Jacques. *Elemens d'Astronomie*, 1740. De l'Imprimerie royale, 1740.

Cassini (1746): Cassini, Jacques. "Des Deux Conjonctions de mars avec Saturne, qui sont arrivées en 1745, avec quelques conjectures sur la cause des inégalités que l'on a remarquées dans les mouvemens de Saturne & de Jupiter." *Histoire de l'Academie*, 1746.

Clairaut (1747A): Clairaut, Alexis C. "Du Systeme du Monde, dans les principes de la gravitation universelle". *Histoire de l'Academie Royale des Sciences 1745*.

Clairaut (1747B): Clairaut, Alexis C. "Response au Reflexions de Mr. de Buffon, sur la loi de l'Attraction & sur le mouvement des Apsides". *Histoire de l'Academie Royale des Sciences 1745*.

Clairaut (1749): Clairaut, Alexis C. "de l'Orbite de la Lune en ne negligent pas les quarrés des quantités de même ordre que les forces perturbatrices". *Premiere Suite des Memoires de Mathematique et de Physique 1748*.

Clairaut (1765): Clairaut, Alexis Claude. *Théorie de la Lune, déduite du seul Principe de l'Attraction Réciproquement proportionelle aux Quarrés des Distances* 2nd Edition, 1765. Chez Dessaint & Saillant, 1765.

Cohen (1972): Cohen, I. B. and Alexandre Koyre, *Philosophiae naturalis princiia mathematica*. Harvard University Press, 1972.

Cohen (1975): Cohen, I. B. *Isaac Newton's Theory of the Moon's Motion*. Dawson, 1975.

Cudworth (1889): Cudworth, William. *Life and Correspondence of Abraham Sharp, the Yorkshire mathematician and astronomer, and assistant of Flamsteed*, 1889. S. Low, Marston, Searle, & Rivington, Ltd. 1889.

Descartes (1644): Descartes, Rene. *Principles of Philosophy*, 1644. Translation by Valentine Rodger Miller and Reese P. Miller. Kluwer Boston, 1983.

Dunthorne (1746): Dunthorne, Richard. "A Letter from Mr. Richard Dunthorne, to the Rev. Mr. Cha. Mason, F.R.S. and Woodwardian Professor of Nat. Hist. at Cambridge, concerning the Moon's Motion." *Philosophical Transactions* 44 (1746), pp. 412-420.

Dunthorne (1749): Dunthorne, Richard. "A Letter from the Rev. Mr. Richard Dunthorne to the Reverend Mr. Richard Mason F.R.S., and Keeper of the Woodwardian Museum at Cambridge, concerning the Acceleration of the Moon." *Philosophical Transactions* 46 (1749), pp. 162-171.

C. Euler (1760): Euler, Charles. "Meditationes in Quaestionem Utrum motus medius Planetarum semper maneaet aeque velox, an successu temporis quampiam mutationem patiatur? & quoenam sit ejus causa?" *Recueil des pieces qui ont remporte les prix de l'Académie royale des sciences*. Académie des Sciences, 1752-77, Volume VIII.

J. Euler (1759): Euler, Johann Albrecht. "Recherches sur vle Derangement du Mouvement d'une Planete par l'Action d'une autre Planete ou d'une Comete". *Opera Omnia*. B. G. Teubner, 1911-1956. Series 2, Volume 25, p. 258-280.

J. Euler (1762): Euler, Johann Albrecht. "Mémoire dans lequel on examine Si les Planetes se meuvement dans un milieu dont la résistance produise quelque effect sensible sur leur mouvement?" *Recueil des pieces qui ont remporte les prix de l'Académie royale des sciences*. Académie des Sciences, 1752-77, Volume VIII.

Euler (1747): Euler, Leonhard. "Recherches sur le mouvements des Corps Celestes en General". *Opera Omnia*. B. G. Teubner, 1911-1956. Series 2, Volume 25, p. 1-44.

Euler (1749): Euler, Leonhard. "Recherches sur la Question des Inegalites du Mouvement de Saturne et de Jupiter, Sujet propose pour le Prix de l'Annee 1748, par l'Academie royale des Sciences de Paris." *Opera Omnia*. B. G. Teubner, 1911-1956. Series 2, Volume 25, p. 45-157.

Euler (1749A): Euler, Leonhard. "Part of a Letter from Leonhard Euler, Prof. Math. at Berlin, and F.R.S., to the Rev. Mr. Caspar Wetstein, Chaplain to his Royal Highness the Prince of Wales, concerning the gradual Approach of the Earth to the Sun." *Philosophical Transactions*, 46 (1749-50), pp. 203-205.

Euler (1749B): Euler, Leonhard. "Part of a Letter from Mr. Professor Euler to the Reverend Mr. Wetstein, Chaplain to his Royal Highness the Prince, concerning the Contraction of the Orbits of the Planets." *Philosophical Transactions* 46 (1749-50), pp. 356-359.

Euler (1752): Euler, Leonhard. "Recherches sur les Inegalités de Jupiter et de Saturne." *Recueil des pieces qui ont remporte les prix de l'Académie royale* *des sciences*. Académie des Sciences, 1752-77, Volume VII.

Euler (1753): Euler, Leonhard. "De Peturbatione Motus Planetarum ab Eorum Figura Non Sphaerica Oriunda". *Opera Omnia*. B. G. Teubner, 1911-1956. Series 2, Volume 25, p. 158-174.

Euler (1753B): Euler, Leonhard. "Theoria Motus Lunae". *Opera Omnia*. B. G. Teubner, 1911-1956. Series 2, Volume 23.

Euler (1758): Euler, Leonhard. "De Motu Corporum Coelestium a Viribus Quibuscunque Perturbato". *Opera Omnia*. B. G. Teubner, 1911-1956. Series 2, Volume 25, p. 175-209.

Forbes (1971): Forbes, Eric G., ed. *The Euler-Mayer Correspondence (1751-1755)*, 1971. American Elsevier Press, 1971.

Gaythorpe (1925): Gaythorpe, S. B. "On Horrocks's Treatment of the Evection and the Equation of the Centre, with a Note on the Elliptic Hypothesis of Albert Curtz and its Correction by Boulliau and Newton." *Monthly Notices of the Royal Astronomical Society*, (85), 1925, p. 858-865.

Gregory (1726A): Gregory, David. *The Elements of Physical and Geometrical Astronomy, Volume I*, 1726. Johnson Reprint Corp. 1972.

Gregory (1726B): Gregory, David. *The Elements of Physical and Geometrical Astronomy, Volume II*, 1726. Johnson Reprint Corp. 1972.

Grant (1852): Grant, Robert. *History of Physical Astronomy, from the Earliest Ages to the Middle of the Nineteenth Century*. Henry G. Bohn, 1852.

Halley (1693): Halley, Edmond. "Emendationes ac Notae vertustas *Albatenii* Observationes Astronomicas, cum restitutione Tabularum Lunisolarium eiusdem Authoris." *Philosophical Transactions* 17 (1693), pp. 913-921.

Halley (1695): Halley, Edmond. "Some Accounts of the Ancient State of the City of Palmyra, with Short Remarks upon the Inscriptions found there." *Philosophical Transactions* 19 (1695), pp. 160-175.

Huygens (1690): Huygens, Christiaan. *Traite de la Lumiere; avec un Discours de la Cause de la Pesanteur*, 1690. Bruxelles, Culture et Civilisation, 1967.

Kaplan (19??): Kaplan, Samuel R., ed. "Directions of Hamiltonian Dynamics and Celestial Mechanics". Preprint, 1996.

Katz (1987): Katz, Victor J. "The Calculus of the Trigonometric Functions." *Historia Mathematics* (14) 1987, No. 4 (Novemer 1987), p. 311-324.

Kopelevich (1966): Kopelevich, Yu. Kh. "The Petersburg Astronomy Contest in 1751." Soviet Astronomy-AJ, Vol. 9, No. 4 (January-February 1966), p. 653-660.

Lalande (1747): Lalande, J. J. "Mémoire sur les Équations Séculaires et sur les moyens mouvements du Soleil, de la Lune, de Saturne, de Jupiter, & de Mars, Avec les observations de Tycho-brahé, faites sur Mars en 1593, tirées des manuscrits de cet Auteur." *Histoire de l'Academie*, 1757, p. 411-476.

Lalande (1758): Lalande, J. J. "Mémoire sur les inégalités de Mars Produites Par lÁction de Jupiter, en Raison inverse du carré de la distance." *Histoire de l'Academie 1758*, p. 12-28.

Lalande (1760): Lalande, J. J. "Calcul des Inégalités de Vénus, par l'attraction de La Terre." *Histoire de l'Academie 1760*, p. 309-333.

Lalande (1792): Lalande, J. J. *Astronomie*. 1792. Johnson Reprint Corp. 1966.

Lagrange (1765): Lagrange, Joseph Louis. "Solutions de Différents Problémes dee Calcul Intégral." *Oeuvres de Lagrange*. Gauthier-Villars, 1867-92. Volume I, p. 469-668.

Lagrange (1766): Lagrange, Joseph Louis. "Recherches sur les inegalities des satellites de Jupiter." *Oeuvres de Lagrange*. Gauthier-Villars, 1867-92. Volume VI, p. 65-225.

Lagrange (1772A): Lagrange, Joseph Louis. Letter to d'Alembert, March 25, 1772. *Oeuvres de Lagrange*. Gauthier-Villars, 1867-92. Volume XIII.

Lagrange (1772B): Lagrange, Joseph Louis. Letter to Condorcet, December 1, 1772. *Oeuvres de Lagrange*. Gauthier-Villars, 1867-92. Volume XIV.

Lagrange (1773): Lagrange, Joseph Louis. "Sur l'equation seculaire de la Lune." *Oeuvres de Lagrange*. Gauthier-Villars, 1867-92. Volume VI, p. 335-399.

Lagrange (1773A): Lagrange, Joseph Louis. Letter to laplace, March 15, 1773. *Oeuvres de Lagrange*. Gauthier-Villars, 1867-92. Volume XIV.

Lagrange (1774): Lagrange, Joseph Louis. "Sur le mouvement des noeuds des orbites Planetaires." *Oeuvres de Lagrange*. Gauthier-Villars, 1867-92. Volume IV, p. 111-147.

Lagrange (1774A): Lagrange, Joseph Louis. Letter to d'Alembert, June 6, 1774. *Oeuvres de Lagrange*. Gauthier-Villars, 1867-92. , Volume XIII.

Lagrange (1774B): Lagrange, Joseph Louis. Letter to Condorcet, July 18, 1774. *Oeuvres de Lagrange*. Gauthier-Villars, 1867-92. Volume XIV.

Lagrange (1775A): Lagrange, Joseph Louis. Letter to Laplace, April 10, 1775. *Oeuvres de Lagrange*. Gauthier-Villars, 1867-92. Volume XIV.

Lagrange (1775B): Lagrange, Joseph Louis. Letter to d'Alembert, May 29, 1775. *Oeuvres de Lagrange*. Gauthier-Villars, 1867-92. Volume XIII.

Lagrange (1775C): Lagrange, Joseph Louis. Letter to d'Alembert, July 10, 1775. *Oeuvres de Lagrange*. Gauthier-Villars, 1867-92. Volume XIII.

Lagrange (1776): Lagrange, Joseph Louis. "Sur l'alteration des moyens mouvements des Planetes." *Oeuvres de Lagrange*. Gauthier-Villars, 1867-92. Volume IV, p. 255-271.

Lagrange (1778): Lagrange, Joseph Louis. Letter to d'Alembert, December 12, 1778. *Oeuvres de Lagrange*. Gauthier-Villars, 1867-92. Volume XIII.

Lagrange (1781): Lagrange, Joseph Louis. "Théorie des variations séculaires des éléments des Planètes, Preière Partie." *Oeuvres de Lagrange*. Gauthier-Villars, 1867-92. Volume V, p. 125-207

Lagrange (1782): Lagrange, Joseph Louis. "Théorie des variations séculaires des éléments des Planètes, Seconde Partie." *Oeuvres de Lagrange*. Gauthier-Villars, 1867-92. Volume V, p. 208-344.

Lagrange (1783): Lagrange, Joseph Louis. "Theories des variations periodiques des mouvements des Planetes." *Oeuvres de Lagrange*. Gauthier-Villars, 1867-92. Volume V, p. 347-377.

Lagrange (1784): Lagrange, Joseph Louis. "Theories geometrique du mouvements des aphelies des Planetes pour servir d'addition aux principes de Newton." *Oeuvres de Lagrange*. Gauthier-Villars, 1867-92. Volume V, p. 565-586.

Lagrange (1808): Lagrange, Joseph Louis. "Mémoire sur la Théorie des Variations des Éléments des Planètes et en particulier des Variations des Grands Axes de Leur Orbites." *Oeuvres de Lagrange*. Gauthier-Villars, 1867-92. Volume VI.

Laplace (1772): Laplace, Pierre Simon. "Recherches sur le Calculs Inégral et sur le Système du monde." *Oeuvres Completes de Laplace*. Gauthier-villars, 1878-1912. Volume VIII.

Laplace (1772A): Laplace, Pierre Simon. "Memoire sur les Solutions particulieres des Equations differentielles et sur les Inegalites seculaires des Planetes." *Oeuvres Completes de Laplace*. Gauthier-villars, 1878-1912. Volume VIII, p. 325-366.

Laplace (1773): Laplace, Pierre Simon. "Sur le principe de la Gravitation Universelle et sur les inegalities seculaires des Planetes qui en Dependent." *Oeuvres Completes de Laplace*. Gauthier-villars, 1878-1912. Volume VIII, p. 201-275.

Laplace (1784): Laplace, Pierre Simon. "Memoire sur les Inegalites seculaires des Planetes et des Satellites." *Oeuvres Completes de Laplace*. Gauthier-villars, 1878-1912. Volume XI, p. 49-92.

Laplace (1787): Laplace, Pierre Simon. "Memoire sur les Variations seculaires des Orbites des Planetes." *Oeuvres Completes de Laplace*. Gauthier-villars, 1878-1912. Volume XI, p. 295-306.

Laskar (1989): Laskar, J. "A numerical experiment on the chaotic behaviour of the Solar System", *Nature* 338 (16 March 1989).

Le Monnier (1757): Le Monnier, Pierre Charles. "Variations Apparentes dans l'Inclinaison observée de l'Orbite du Cinquiéme Satellite de Saturne, Avec des Réflexions sur les limites des atmosphères du Soleil & des Planètes, & sur quelques usages particuliers tant des télescopes, que du catalogue général du Zodiaque." *Histoire de l'Academie 1757*, p. 88-96.

Maclaurin (1748): Maclaurin, Colin. *An Account of Sir Isaac Newton's Philosophical Discoveries*, 1748. Johnson Reprint Corp. 1968.

MacPike (1937): MacPike, Eugene Fairfield, *Correspondence and Papers of Edmond Halley*, 1937. Taylor and Francis, 1937.

Maraldi (1718): Maraldi, Giacomo Felipe. "Observations du Passage de Jupiter proche de l'Etoile appellée Propus". (Read December 14, 1718) *Histoire de l'Academie 1718*

Moulton (1914): Moulton, F. R. *An Introduction to Celestial Mechanics*, MacMillan, 1914.

Newton (1726): Newton, Isaac. *The Principles of Natural Philosophy*, English edition, 1726. Encyclopedia Britannica, 1955.

Newton (1726b): Newton, Isaac. *Isaac Newton's Philosophiae Naturalis Principia Mathematica*, Third Edition (1726) with variant readings. Ed. Alexander Koyré and I. Bernard Cohen. 1972.

Newton (1730): Newton, Isaac. *Opticks*. Dover, 1952.

Playfair (1808): Playfair, John. Review of *Traité Méchaniique Céleste*. *The Edinburgh Review*, XXII (January 1808), p. 249-284.

Poincaré (1993): Poincaré, Henri. *New Methods of celestial mechanics*, introductionby Daniel Goroff. American Institute of Physics, 1993.

Poisson (1809): Poisson, Simon Denis. "Memoire sur la variation des constantes arbitraires dans les questions de Mecanique", *Journal de l'Ecole Polytechnique*, tome viii, p. 266-344.

Roy (1982): Roy, A. E. *Orbital Motion*. Adam Hilger Ltd., 1982.

Scott (1959): Scott, J. F., editor. *The Correspondence of Isaac Newton*. Royal Society at the University Press, 1959.

Simpson (1740): Simpson, Thomas. *Essays on Several Curious and Useful Subjects in Speculative and Mix'd Mathematicks*, 1740.

Simpson (1757): Simpson, Thomas. *Miscellaneous Tracts on Some curious, and very interesting, Subjects in Mechanics, Physical Astronomy, and Speculative Mathematics*, 1757.

Sussman (1988): Sussman, Gerald Jay, and Jack Wisdom. "Numerical Evidence that the Motion of Pluto is Chaotic", *Science* 241 (July 22, 1988).

Suzuki (1996): Suzuki, Jeff. "Geometric Determination of the Lagrange Points". Appendix to doctoral thesis *A History of the Stability Problem From Newton to Laplace*, 1996.

Turnbull (1959): Turnbull, H. W. *Correspondence*. Royal Society at the University Press, 1959.

Somerville (1831): Somerville, Mary. *Mechanism of the Heavens*. 1831.

Szebehely (1989): Szebehely, Victor G. *Adventures in Celestial Mechanics*, 1989. University of Texas Press.

Voltaire (1738): Voltaire. *The Elements of Newton's Philosophy*, translated by John Hanna. Cass, 1967 (reprint of 1738 edition).

Whiteside (1974): Whiteside, D. T. *The Mathematical Papers of Isaac Newton*. Cambridge University Press, 1974.

Wilson (1972): Wilson, Curtis. "How Did Kepler Discover His First Two Laws?" *Scientific American*, 226 No. 3 (March 1972).

Wilson (1980): Wilson, Curtis. "Perturbations and Solar Tables from Lacaille to Delambre: the Rapprochment of Observation and Theory" *Archive for the History of the Exact Sciences*, 22 (1980), p. 42-304.

Wilson (1985): Wilson, Curtis. "The Great Inequality of Jupiter and Saturn: from Kepler to Laplace" *Archive for the History of the Exact Sciences*, 33 (1985) p. 15-290.