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"NEWTON IS RIGHT, NEWTON IS WRONG. NO, NEWTON IS RIGHT AFTER ALL."

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Introduction

In this paper I would like to consider Leonard Euler's "oeuvres" in astronomy. Although he is particularly and usually noted for his work in mathematics, pure and applied, our focus will be on his success in astronomy and most notably in celestial mechanics. The approach I wish to take in this historical sketch will be to progress chronologically instead of imposing a particular structure on Euler's development. That is, I see a handful of "plots" which unfold over his lifetime. These plots interweave and to delineate them would render this sketch longer and more repetitive than need be. I will add, in this sketch, glimpses of the other "actors" who share the various stages on which Euler's script is drawn out. These actors, you will note, are not mere bit players but, rather, full participants in the history of astronomy and are each worthy of a similar sketch. This said, let us proceed to unravel the script.

The solar system, as it was known at the beginning of the eighteenth century, contained 17 recognized members: the sun, six planets, ten satellites (one belonging to the earth, four to Jupiter, and 5 to Saturn). Comets were known to have visited on occasion into the region occupied by the solar system and there were reasons to believe that one of them (Halley's Comet) was a regular visitor. But by and large, although there was a great interest in the comets, their action (if any) on the members of the solar system was ignored, a neglect which subsequent investigations has justified (FORBES, 1909). The number of fixed stars was known to be in the thousands[26] and their places on the celestial sphere determined (FORBES, 1909). They were known to be at very great though unknown distances from the solar system, and their influence on it was regarded as insensible.

The motions of the 17 members of the solar system were tolerably well known. Until nutation and aberration were properly understood, their actual distances from one another had been roughly estimated, while the proportions between most of the distances were known with considerable accuracy. Apart from the entirely anomalous ring of Saturn, most of the bodies of the system were known from observation to be nearly spherical in form. Newton, as we know, had shown that these bodies attract one another according to the law of universal gravitation. The astronomers of the eighteenth century inherited from Newton the following problem:

> *Given these 17 bodies, and their positions and motions at any time, to deduce from their mutual gravitation by a process of mathematical calculation their positions and motions at any other time*[27].

26 For instance, we know that Hevelius, catalogued 1500 stars, this after his observatory and records were burnt to the ground in 1679 and he being an old man. Flamsteed, as Astronomer Royal, left a catalogue of some 2900 stars.

27 This formulation of the project is certainly Laplacian in spirit. That astronomers of the day saw their task in this way is subject to debate. I want to argue that work done in the 1740s through to the 1760s, chiefly by Euler, is consistent with the formulation of this problem.

In the case of the solar system the problem is simplified not only by the consideration that one of the bodies can always be regarded as exercising only a small influence on the relative motion of the others, but also by the fact that the eccentricities and inclinations of the orbits of the planets and satellites are small quantities. In the case of the system of the sun, earth, and moon the characteristic feature is the great distance of the sun, which (in this case) is the disturbing body, from the other two bodies. In the case of the sun and two planets, the enormous mass of the sun as compared with the disturbing planet is the important factor. Both problems fall under the problem of three bodies but the cases differ. Consequently, two distinct branches of the subject evolved: **lunar theory** and **planetary theory.**

In the *Principia* the problem of two attracting bodies with an inverse square law of force is adequately solved (Propositions I-XVII and LVII-LXIV of Book I). There, Newton argued that an inverse square law is implied from circular, elliptical, parabolic, or hyperbolic orbits – the conic sections. Furthermore, all three of Kepler's laws are in full accord with Newton's theory – in fact they are derivable from the theory[28]. In Propositions LXV and LXVI Newton looks at the problem of three bodies but was not satisfied with his solution (and as we have already noted this is the problem over which Euler et al., puzzled).

It is important to note that Newton had solved the theoretical problem of the motion of two point masses under an inverse square law of attraction. For more than two point masses only approximations to the motion of the bodies could be found and this line of research led to a large effort by mathematicians to develop methods to attack this three body problem (WILSON, 1995). However, the problem of the actual motion of the planets

28 See Forster's (1988) critique of Duhem's problem. Forster also exploits this position to argue against the "anti-realist polemics of Cartwright." (FORSTER, 1988, p.86).

and moons in the solar system was highly complicated by other considerations.

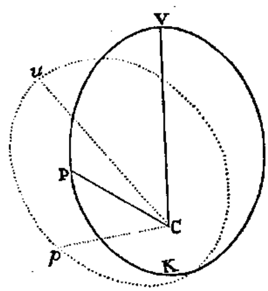
Even if the earth-moon system was considered as a two body problem, theoretically solved in the *Principia*, the orbits would not be simple ellipses. Neither the Earth nor the moon is a perfect sphere and so they do not behave like point masses. This was to lead to the development of mechanics of rigid bodies.

Newton and the Motion of the Lunar Apse: The Setting of the Problem

In Section IX, Book I of the *Principia*, entitled "The Motion of Bodies in Movable Orbits; and the Motion of Apsides," Newton eventually demonstrated that if the force exerted by a central body acted in a proportion other than inverse-square, the orbit would in effect rotate[29]. In the case of the ellipse this amounted to claiming that the apsides would move (see Newton's drawing below).

29 "The orbit would in effect rotate" simply means that the line of apsides, which corresponds to the major axis of an elliptical orbit, would rotate. The size and shape of an orbit are specified by (1) the semimajor axis (the mean distance of the smaller body from the larger body) and (2) the eccentricity (the distance of the larger body from the center of the orbit divided by the length of the orbit's semimajor axis). The position of the orbit in space is determined by three factors: (3) the inclination, or tilt, of the orbital plane to the reference plane (the ecliptic for sun-orbiting bodies; a planet's equator for natural and artificial satellites); (4) the longitude of the ascending node (measured from the vernal equinox to the point where the smaller body cuts the reference plane moving south to north); and (5) the argument of pericenter (measured from the ascending node in the direction of motion to the point at which the two bodies are closest). In the case of the Earth-Moon system this is the *perigee.* These five quantities, plus the time of pericenter passage, are called orbital elements. The gravitational attractions of bodies other than the larger body cause perturbations in the smaller body's motions that can make the orbit shift, or precess, in space or cause the smaller body to wobble slightly.

Figure 1 - Newton's diagram for the movement of the apsides in an elliptical orbit from Section IX, Book I (NEWTON, 1934).



In Proposition XLV Newton limits himself "to find the motion of the apsides in orbits approaching very near to circles" (NEWTON, 1934, p.141). That is, Newton is here limiting himself to an investigation of the motion of apsides in orbits of very small eccentricities. To this end the reader is offered three examples to show how the apsides' motion could be calculated when the centripetal force is assumed to be one of the following:

1. to be constant, or as $\frac{A^3}{A^3}$

2. to be as any power of the altitude, e.g., A^{n-3} or $\frac{A^n}{A^3}$; and

3. to be as the (multiple) sum of any m, n powers of the altitude, or as $\frac{(bA^m + cA^n)}{A^3}$, where b, c are constants.

What is of importance for our purposes here are the two corollaries Newton developed based on Examples 2 and 3. These corollaries relate centripetal force to the motion of the apsides which they produce. Here is what Newton had to say:

> Cor. I. Hence if the centripetal force be as any power of the altitude, that power may be found from the motion of the apsides; and so contrariwise. That is, if the whole angular motion, with which the body returns to the same apsis, be to the angular motion of one revolution, or 360°, as any number as *m* to another as *n*, and the altitude called A; the force will be as the power $A^{\frac{nn}{mm}-3}$ of the altitude A; the index of which power is $\frac{nn}{mm}-3$. This appears by the second example (NEWTON, 1934, p.145).

Now, under the assumption that the motion of the apsides of any orbit of small eccentricity arises in the fashion alluded to in Proposition XLV, the first corollary indicates a method for determining the extent of deviation from the inverse-square proportion of the centripetal force by the given motion of an orbiting body! Newton illustrates this in concluding Corollary I to Proposition XLV:

> Lastly if the body in its progress from the upper apsis to the same upper apsis again, goes over one entire revolution and 3° more, and therefore the apsis in each revolution of the body moves 3° *in consequentia;* then *m* will be to *n* as 363° to 360° or as 121 to 120, and therefore $A^{\frac{nn}{mm}-3}$ will be equal to $A^{\frac{-29523}{14641}}$ and therefore the centripetal force will be reciprocally as $A^{\frac{29523}{14641}}$ or reciprocally as $A^{2\frac{4}{243}}$ very nearly. Therefore the centripetal force decreases in a ratio **something greater than the squared ratio** [emphasis added]; but approaching $59\frac{3}{4}$ times nearer to the squared than the cubed (NEWTON, 1934, p.146).

The virtually immobile aphelia of the planets implied, according to the formula of Corollary I of Proposition XLV, a nearly perfect inverse-square centripetal force acting between them respectively with the sun. The apses of the moon, whose motion amounts to roughly 3° per revolution, is obviously more sizeable.

The second corollary allowed the measurement of the effect on the motion of the apsides produced by a foreign force.

> Cor. II. Hence also if a body, urged by a centripetal force which is reciprocally as the square of the altitude, revolves in an ellipsis whose focus is in the centre of the forces; and a new and foreign force should be added to or subtracted from this centripetal force, the motion of the apsides arising from that foreign force may (by the third Example) be known; and so on the contrary. As if the force with which the body revolves in the ellipsis be as $\frac{1}{AA}$; and the foreign force subducted as cA, and therefore the remaining force as $\frac{A - cA^4}{A^3}$; then (by the third Example) A will be equal to 1, m equal to 1, and n equal to 4; and therefore the angle of revolution between the apsides is equal to $180^{\circ}\sqrt{\frac{1-c}{1-4c}}$. (NEWTON, 1934, p.146).

With respect to this Newton goes on to say:

> Suppose that foreign force to be 357.45 parts less than the other force with which the body revolves in the ellipsis; that is, c to be $\frac{100}{35745}$; A or T being equal to 1; and then $180^{\circ}\sqrt{\frac{1-c}{1-4c}}$ will be $180^{\circ}\sqrt{\frac{35645}{35345}}$ or 180.7623, that is

> 180°45'44". Therefore the body, parting from the upper apsis, will arrive at the lower apsis with an angular motion of 180°45'44", and this angular motion being repeated, will return to the upper apsis; and therefore the upper apsis in each revolution will go **forward 1°31'28". The apsis of the moon is about twice as swift** (NEWTON, 1934, p.147, emphasis added)

Corollary II of Proposition XLV related the apsidal motion to a mixture of inverse-square centripetal motion and a radial component of a perturbative force (foreign force). As we said, Newton was able only to account for roughly half the observed motion. But later in Proposition III of Book III Newton addresses the perturbative effect of the Sun on the moon's motion by saying:

> The action of the sun, attracting the moon from the earth, is nearly as the moon's distance from the earth; and therefore (by what we have shewed in Cor.2, Prop. XLV, Book I) is to the centripetal force of the moon as 2 to 357.45, or nearly so; that is, as 1 to $178\frac{29}{40}$. And if we neglect so inconsiderable a force of the sun, the remaining force, by which the moon is retained in its orb, will be reciprocally as D^2. This will yet more fully appear from comparing this force with the force of gravity, as is done in the next Proposition (NEWTON, 1934, p.407)[30].

Newton then adds a corollary to Proposition III that will be used later (in Cor. VII, Proposition XXXVII, Book III) in his calculation of the moon's centripetal acceleration.

30 This was added in the second edition.

> Cor. If we augment the mean centripetal force by which the moon is retained in its orb, first in the proportion of 177 29/40 to 178 29/40, and then in the duplicate proportion of the semi-diameter of the earth to the mean distance of the centres of the moon and earth, we shall have the centripetal force of the moon at the surface of the earth; supposing this force, in descending to the earth's surface, continually to increase in the reciprocal duplicate proportion of the height (NEWTON, 1934, p.407).

It would appear that Newton wished to have his readers believe that the ratio of the action of the sun (i.e., the mean "foreign" force) to the centripetal force on the moon would be 1 to $178\frac{29}{49}$. Recall the Cor. II of Proposition XLV, Book I formula for calculating the angle of revolution between apsides is equal to $180^{o}\sqrt{\frac{1-c}{1-4c}}$. Doubling this will yield the mean motion of the apsides in a complete revolution. Letting $c = 1/178\frac{29}{49}$ we get the mean angular motion to be 363°4'47". The moon's apsis, in one revolution, moves forward just over 3°. So, the substitution of this value of c yields a Cor. II value approximately equal to the observed mean motion of the lunar apogee per revolution.

Newton nowhere gives any direct explanation as to why he chose to use the ratio of 1 to 357.45 in illustrating Cor. II or Proposition XLV, Book I. Nor does he suggest that the motion of the moon's apogee might have a cause more complex than that described in the corollary and in the proof of Proposition III (WILSON, 1995, p.2).

Newton, in Propositions III and IV of Book III relies heavily on the result obtained from Corollaries I and II of Prop. XLV of Book I and Rules of Reasoning I and II. These two propositions are key in Newton's argument for universal gravitation.

If there was a weak spot in Newton's theory, the continental philosophers of nature reasoned that it would be in Newton's lunar theory. It is for this reason that we find Euler, Clairaut, and d'Alembert so heavily involved in its correction. In Proposition III Newton argued that the moon is maintained in its orbit by a centripetal inverse-square force directed toward the earth.

> That the force by which the moon is retained in its orbit tends to the earth; and is inversely as the square of the distance of its place from the earth's centre (NEWTON, 1934, p.406).

In order to show this Newton has to prove two things:

1. That at the centre of the centripetal force urging the moon into an orbit is the earth.
2. That this centripetal force is as the -2 power of the distance of the moon from the earth's centre.

The first part is proven by Phenomenon 6 - that the moon describes areas proportional to the times of description around the earth - and Propositions II and III of Book I[31]. Newton moves on to claim the

> latter [i.e., 2 above] from the very slow motion of the moon's apogee; which in every single revolution amounting to 3˚3¢ forwards, may be neglected. For (by Cor. I, Prop. XLV, Book I) it appears, that, if the distance of the moon from the earth's centre is to the semidiameter of the earth as D to 1, the force, from which such a motion will result, is reciprocally as $D^{2\frac{4}{243}}$, i.e., inversely as the power of D, whose

31 I will not recall the proof here as it is not substantive for our aim. Since we wish to look forward to Euler, Clairaut, and d'Alembert's discussion of the lunar theory then it is Newton's use of Prop. XLV of Book I that is germane.

> exponent is $2\frac{4}{243}$; that is to say, in the proportion of the distance something greater than reciprocally duplicate, but which comes 59 ¾ times nearer to the square than to the cube (NEWTON, 1934, p.406).

The 3°3¢ forward precession is accounted for, via Cor. I Prop XLV Book I, as measuring a centripetal force of -2.0167. Recall in that Corollary we are instructed to use $\frac{n^2}{m^2}-3$ in calculating the index of the altitude. Since we have a forward precession, m=363°3¢ and n=360°. Therefore

$$f \alpha \frac{1}{r^{2.0167}}$$

$$f \alpha \frac{1}{r^{2\frac{4}{243}}},$$

which is the same result Newton's calculations yield. Newton limits himself *to find the motion of the apsides in orbits approaching very near circles*[32]. Some of the planetary orbits have appreciable eccentricity. Mercury, for instance, has an orbital eccentricity of 0.2056.

32 Ronald Laymon (LAYMON, 1983) has claimed that "Newton's descriptions of the phenomena were typically incompatible with the then accepted observational data". A stable elliptical orbit, i.e., an elliptical orbit without precession, would correspond exactly to a centripetal force varying as the inverse square of the distance. Since our moon precesses forward it would seem that this example is compatible with Laymon's conjecture. Bill Harper (HARPER, 1989) has conclusively shown that Laymon's conjecture is wrong with respect to what Newton explicitly cited as phenomena. For example, consider the evidence for the Harmonic Law for Jupiter's moons (i.e., $R^3/T^2=K$) in Phenomenon I. In short, what Newton cited as Phenomenon I falls within what we would normally accept as reasonable limits of error. The largest error in Newton's table is less than a half standard deviation from the mean. Likewise, the same can be said for the evidence for the Harmonic Law ratio for the five primary planets as stated in Phenomenon IV.

Mercury, one of the primary planets, is obviously included in Proposition II, Book III and where he cites Proposition XLV. Valluriet al., (VALLURI et al., 1997, p.25) show how Newton is able to trust his result from Proposition XLV even where the orbital eccentricity is not close to zero, i.e., even where the orbits are not approaching very near circles[33].

In order to show the acceptability of Proposition III we have claimed that Newton needed to show that this centripetal force is as the -2 power of the distance of the moon from the earth's center. In the case of the moon there is a small departure from the inverse square variation. Newton further claims in Proposition III:

> But in regard that this motion is owing to the action of the sun (as we shall afterwards show), it is here to be neglected. The action of the sun, attracting the moon from the earth, is nearly as the moon's distance from the earth; and therefore (by what we have shewed in Cor. 2, Prop. XLV, Book I) is to the centripetal force of the moon as 2 to 357,45, or nearly so; that is, as 1 to $178\frac{29}{40}$. And if we neglect so inconsiderable a force of the sun, the remaining force, by which the moon is retained in its orb, will be reciprocally as D^2 (NEWTON, 1934, p.407).

33 The authors point out that: „Newton himself, viewing the systematic dependencies between force law and orbital shape that he had discovered, concluded that the relation between force law and orbital figure was one of mutual implication". They go on to say that Newton had found that for orbits differing from circles, very special force laws give rise to orbital closure and that "it must have seemed unlikely that such closure could arise under other laws for isolated values of the eccentricity–a violation of the systematic order he had discovered" (VALLURI et al., 1997, p.25).

At this point we have no reason to think that the sun would have any effect on the moon. After all this is what universal gravitation would imply and we have not proven that yet. Nonetheless, we can take Newton to be claiming that the motion of the lunar apsides is due to the influence of the sun and this perturbative effect ought to be subtracted in order to isolate the centripetal force drawing the moon toward the earth. In Cor. II of Prop. XLV of Book I, Newton gave the formula for calculating precession from the centripetal component of a foreign force as a fraction of the main central force. We recall that in this corollary (quoted above) Newton came up with the figure of forward precession of 1°31'28". Doubling this value we get 3°2'56" which is 4 minutes shy of the value Newton cites as the lunar precession in Proposition III, Book III.

Euler, Lunar Theory, and the Vindication of Universal Gravitation

It has been maintained in the literature[34] that on careful reading of Newton's analysis in the *Principia* (Prop. XLV, Book I, Props., III, IV of Book III) shows that he was able to come up with only half the moon's apsidal motion. After the publication of the *Principia* (all editions) there was some debate as to what Newton had really accomplished. It is with respect to this problem that we find, in the 1740s, three of Europe's foremost mathematicians engaged.

We notice, first, that it took fifty or so years after the publication of the *Principia* to achieve serious progress in analytical celestial mechanics. As early as 1714 the British Parliament offered to award a handsome prize for a procedure that would

34 See, for instance, Craig Waff's lucid account, "Clairaut and the motion of the lunar apse: the inverse-square law undergoes a test," in Wilson (1995).

give accurate longitudes at sea[35]. A prize of £30,000 would be awarded for a procedure that would give longitude to within ¼º, £20,000 to within ½º, and £10,000 to within 1º (HANKINS, 1970, p.30). Even with such a lucrative incentive, no lunar theory of this accuracy and precision was developed within the first half of the eighteenth century. Curtis Wilson has pointed out, rightly, that the lack of lunar theory with the requisite accuracy was due more to ignorance of how the mathematics were to be constructed than to anything else (WILSON, 1995, p.89). In fact, the various papers presented to the Paris Academy for the 1748 prize for a theory of Saturn and Jupiter aptly show the energies and time spent just figuring out how theories of perturbation, like the Sun-Jupiter-Saturn system or the Earth-Moon-Sun system, were to be carried out and constructed (WILSON, 1995, p.89).

In the 1740s, Euler, Clairaut, and d'Alembert undertook new analytical derivations of the Moon's motions from the inverse-square law. Initially, all three found a similar anomalous result: like Newton in Proposition XLV of Book I they were able only to recover half the observed motion. Much of the ensuing debate centered on Clairaut but an account of the debate and the other actors as well will prove to be illuminating.

35 We know that lunar theory was especially inviting because it offered a possible method for finding longitude at sea. Mariners had to depend on dead reckoning to find their longitude. The first reliable and accurate sea-going chronometers were made by Harrison and Berthoud in the 1760s and until that time it seemed that a table of the moon's position would be the best way to determine longitude. The Paris Academy offered yearly prizes on this and related topics. These alternated yearly between celestial mechanics and the theory of navigation. (HANKINS, 1970, p.29): "Lunar Theory was also inviting because it offered a possible method for finding longitude at sea. Ever since the Age of Exploration, mariners had had to depend on dead reckoning to find their longitude. The first accurate sea-going chronometers were made by Harrison and Berthoud during the 1760s and until that time it seemed that a table of the moon's position would be the best way to determine longitude".

Euler developed methods of integrating linear differential equations in 1739, fifty or so years after the publication of the first edition of the *Principia.* Euler drew up lunar tables in 1744 and reflected his study of the gravitational attractions in the earth, moon, and sun system. Clairaut and d'Alembert were also studying perturbations of the moon and, in 1747, Clairaut proposed adding a 1/r4 term to the gravitational law to explain the observed motion of the Moon's perihelion. However, by the end of 1748 Clairaut had discovered that a more accurate application of the inverse square law came close to explaining the precession. He published this version in 1752, and two years later, d'Alembert published his calculations going to more terms in his approximation than Clairaut. This work of the mathematicians of the eighteenth century is of importance for its role in getting Newton's inverse square law of force accepted in Continental Europe.

In a paper entitled "Recherches sur le mouvement des corps célestesengénéral" presented to the Berlin Academy of Sciences on 8 June, 1747, Euler called into question the accuracy of the inverse-square law[36]. For our present purposes we need to understand what Euler in fact says about Lunar Theory. With respect to the mutual forces among planets Euler expressed some doubts that they followed the inverse-square law exactly (EULER, 1747, p.4-5)[37]. His doubts were strengthened by empirical evidence.

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36 Euler had been in correspondence with Clairaut since 1740 regarding the theoretical determination of the lunar nodes. A summary of the paper he read to the Berlin Academy in October 1744 was entitled „Sur le mouvement des noeuds de la Lune, et sur la variation de son inclinaison à l'Ecliptique" („On the motion of the lunar nodes and the variation of the lunar inclination). Euler further carried out an investigation of the moon's motion in May 1745 as evidenced by his correspondence with a former colleague at the St. Petersburg Academy, Jean-Nicolas Delisle.

37 For instance Euler claims in paragraph 8: "Voilà donc une raisson assez forte, pourquoi il sera permis de croire que la force, dont les Planetes sont poussées

There were the anomalies in the motions of Jupiter and Saturn and, more to our point, there was a careful study started in 1744 in which he found a number of disagreements between observation and the results derived from the inverse-square law. Euler pointed out that other commentators claimed that a perfect agreement existed between Newton's theory and the observations (EULER, 1747, p.4-5). Aside from the lunar inequalities, Newton's theory accorded fairly well with observations. As observational accuracy increased, perturbations in orbital paths were made easier to observe. For Euler, and anyone sensitive to Newton's confusing comments regarding lunar theory, the motion of the lunar apse was reason enough to call into question universal gravitation and the inverse-square ratio. (Recall that this agreement between observation and Newton's theory was immensely important in encouraging acceptance of the inverse-square law). The empirical evidence upon which Euler carefully studied and consequently doubted the accuracy of the inverse-square law included observations of the lunar nodes. In paragraph 11 of this essay he claims:

> Because at first having supposed, that the forces as much from the Earth as from the Sun, which act on the Moon, are perfectly proportional reciprocally to the squares of the distances, I have always found the motion of the apogee almost two times slower than the observations dictate; and although several small terms, which I have been obliged to neglect in the calculation, may be able to accelerate the motion of the apogee, I have

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vers le Soleil, ne se régle pas parfaitement sur la raison renversée des quarrés des distances: et cette loi sera encore moins certaine, quand il s'agit des forces, dont les Planetes s'atirent mutuellement". ("Here then is a strong reason to maintain that the force directing the planets towards the sun is not in perfect agreement with the inverse-square law: and this law becomes even less certain when one considers the mutual attraction of the planets").

> ascertained after several researches, that they would be unable by far to make up for this lack, and that it is absolutely necessary, that the forces by which the Moon is at present time solicited, are a little different from the ones, which I have supposed; because the least difference in the soliciting forces produces a very considerable one in the motion of the apogee. I have noticed, as well, a small difference in the motion of the line of nodes from that expected from calculations, which undoubtedly arises form the same source (EULER, 1747, p.5)[38].

In the next paragraph, 12, Euler describes another unsuccessful test of the theory:

> Knowing the weight at the surface of the earth, I have concluded from it the absolute force, which ought to act on the Moon, by supposing that it decreased in doubled ratio of the distances. From this force compared to the periodic time of the Moon, I have deduced the mean distance of the Moon to the Earth, and

38 "Car d'abord ayant supposé, que les forces tant de la Terre que du Soleil, qui agissent sur la Lune, sont parfaitement proportionelles réciproquement aux quarrés des distances, j'ai trouvé toujours le mouvement de l'apogée presque deux fois plus lent, que les observations le marquent; et quoique plusieurs petits termes, que j'ai été obligé de negliger dans le calcul, puissent accélérer le mouvement de l'apogée, j'ai pourtant bien vu après plusieurs recherches, qu'ils ne sauroient de beaucoup près suppléer à ce deffaut, et qu'il faut absolument, que les forces, dont la Lune est actuellement sollicitée, soient un peu différentes de celles, que j'avois supposées; car la moindre différence dans les forces sollicitantes en produit une très considérable dans le mouvement de l'apogée. J'ai remarqué aussi une petite différence entre le mouvement de la ligne des noeuds, que le calcul donne, et celui que les observations ont donné à connoitre, qui vient sans doute de la même source." Euler, "Recherches sur le mouvement des Corps célestes en général", (p.5). The original is in French and translated here by the author.

> then its horizontal parallax to this same distance. But this parallax has been found a little too small, with the result that the Moon is less distant from us, than according to the theory, and leaving the force, by which the Moon is impelled toward the Earth, smaller, than I have supposed (EULER, 1747, p.5) [39].

Euler goes on to describe that his attempt involved separating the component of the total force acting on the moon corresponding to the Earth from that corresponding to the Sun. The force of the Earth on the Moon ought, then, to result in an action of the Moon in perfect agreement with Kepler's laws (paragraph 13). But his attempt couldn't be accomplished according to commonly accepted rules. He, therefore, concludes:

> All these reasons joined together appear therefore to prove invincibly, that the centripetal forces which one conceives to apply in the Heavens, do not follow exactly the law established by Newton[40].

Added to this are his researches on the perturbation of Saturn's motion by Jupiter. Euler surmised that there would likely

39 "Connoissant la quantité de la pesanteur à la surface de la Terre, j'en ai conclu la force absolue, qui doit agir sur la Lune, en supposant qu'elle décroit en raison doublée des distance. De cette force comparée au tems périodique de la Lune, j'ai déduit la distance moyenne de la Lune à la Terre, et ensuite sa parallaxe horizontale à cette même distance. Mais cette parallaxe s'est trouvée un peut trop petite, de sorte que la Lune est moins éloignée de nous, qui suivant la théorie, et partant la force, dont la Lune est poussée vers la Terre, plus petite, que j'avois supposé" (EULER, 1747, p.5).

40 "Toutes ces raisons jointesensemble paroissent donc prouver invinciblement, que les forces centripetes qu'on conçoit dans le Ciel, ne suivent pas exactement la loi établie par Neuton" (EULER, 1747, p.6).

be found further discrepancies between the calculated and the observed motions of the other planets of the solar system.

We present a discussion of these more general problems here to raise a particular point, viz., that Euler maintained that Newton's inverse-square law could no longer be taken as a given in describing the forces between celestial bodies. For he goes on to say in the fourteenth paragraph:

> The theory of Astronomy is therefore still much further removed from the degree of perfection, to which it has been thought to be already carried. Because if the forces, by which the Sun acts upon the Planets, and the latter upon each other, were exactly in inverse ratio of the squares of the distances, they would be known, and consequently the perfection of the theory would depend on the solution of this problem: *That the forces, by which a Planet is solicited, being known, the motion of this Planet is determined*. This problem quite difficult as it can be, appertains nevertheless to pure Mechanics, and it can be hoped, that with the assistance of some new discoveries in Analysis, its solution can finally be attained. But as the law itself of the forces, by which the Planets are solicited, is not yet perfectly known, it is no longer an affair of Analysis alone: and much more than it is necessary in order to work for the perfection of theoretical Astronomy (EULER, 1747, p.6)[41].

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41 "La théorie de l'Astronomie est donc encore beaucoup plus éloignée du degré de perfection, auquel on pourroit penser, qu'elle soit déjà portée. Car si les forces dont le Soleil agit sur les Planetes, et celles-cy les unes sur les autres, étoient axactement en raison renversée des quarrés des distances, elle seroient connues, et par conséquent la perfection de la théorie dependroit de la solution de ce probleme: *Que les forces, dont une Planete est sollicitée, étant connues, on determine le mouvement de cette Planete.* Ce probleme tout difficile qu'il puisse être,

In short, for meaningful progress to be made with the help of applications of mechanics and analysis, the true motions of the planets are to be determined not with a known attraction law, i.e., inverse-square, but rather the law is itself to be scrutinised.

In another paper completed after «Recherches sur le mouvement des corps célestes en général» Euler made a similar conclusion. In his "Recherches sur la question des inégalités du mouvement de Saturne et de Jupiter", which was his successful entry in the 1748 prize contest of the Paris Academy of Sciences, Euler draws a slightly different conclusion in his discussion of perturbative effects observed in the motions of Jupiter and Saturn.

In 1746 the Paris Academy announced that it was seeking, for the 1748 contest, "a theory of Saturn and of Jupiter by which the inequalities which these planets appear to cause mutually, principally near the time of their conjunction, can be explained". We do need to examine it briefly here as it does inform an understanding of the Eulerian Lunar theory.

We find Euler, in the introduction to his essay, reasoning that it was overwhelmingly likely that the academy had in mind Newton's theory of universal gravitation "which has been found up to now so admirably well in agreement with all the celestial motions, that whatever will be the inequalities which are found in the motions of the Planets, it can always be boldly claimed that the mutual attraction of the Planets is the cause of them". Whatever doubts Euler was harbouring against inverse-square variations and the nature of gravity - whether it was attraction or impulsion in some medium -we see Euler here proclaiming the acceptability of universal gravitation by the community of

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appartient néantmoins à la Mécanique pure, et on pourroit espérer, qu'à l'aide de quelques nouvelles découvertes dans l'Analyse, on sauroit enfin parvenir à sa solution. Mais comme la loi même des forces, dont les Planetes sont sollicitées, n'est pas encore parfaitement connue, ce n'est plus une affaire de l'Analyse seule: et il en faut bien davantage pout travailler à la perfection de l'Astronomie théorique" (EULER, 1747, p.6).

practicing astronomers. In order to solve this problem, claims Euler, one needs to solve a purely mechanical problem.

The inequalities observed by astronomers in the motion of Jupiter and Saturn are readily perceived; to answer satisfactorily the prize question, it is necessary only to determine the motion of three mutually attracting bodies in proportion to their masses and inversely as the squares of their distances and then to put the Sun for one of these bodies and Saturn and Jupiter for the other two (EULER, 1748, p.46)[42].

The proposed prize question is reduced, then, for Euler, to a purely mechanical problem. Euler cautions the reader, though, that although the problem is a problem of mechanics, it was one of the most difficult in mechanics to solve. A perfect or exact solution could only be achieved once much further progress is made in mathematical analysis. Nonetheless, since the Sun's mass is considerably larger than Saturn's and Jupiter's and since the latter two planets have near-circular orbits, a solution to the problem by approximation is feasible. It is in this fashion that Euler attacked the problem. Interestingly at the end of §2 Euler proclaims that this project, Saturn-Jupiter, is more difficult than the Lunar problem (which to that point had been judged to be the more difficult).

He shows, in §7, some concern that he has not been able to reconcile theory and observation, i.e., that he has not been able to perfectly account for all of Saturn's irregularities. Nevertheless, he still claimed that the prize ought to be awarded to him for

42 "Recherches sur la question des inégalités du mouvement de Saturne et de Jupiter": "[L]a cause des inégalités que les Astronomes on remarqués dans le mouvement de Saturne et Jupiter, est manifeste; et pour satisfaire à la question proposée, on n'aura qu'à déterminer le mouvement de trois corps qui s'attirent mutuellement en raison composée de celle de leurs masses, et de la raison inverse des quarrés de leurs distances, et mettre ensuite à la place de l'un de ces trois corps le Soleil, et les corps de Saturne et Jupiter au lieu des deux autres" (EULER, 1748, §, p.46).

his work on the Saturn-Jupiter problem, and confirmed by his work in Lunar theory which, showed that a general correction to Newton's theory was in order.[43] After having carefully compared lunar observations with the theoretical predictions, Euler noticed that the Earth-Moon distance is not as large as required by the theory and this in turn implied that the Moon gravitates toward the Earth in a fashion less than inverse-square of the distances. Newton's theory was in need of correction for the above reasons and for the fact that there are certain small irregularities in the Moon's motion[44].

After comparing lunar observations with theory and comparing this work to his work on the motion of Saturn, it seemed that

> the Newtonian ratios according to the square of the distances held for small distances and deviated from truth with large distances (EULER, 1748, §7, p.49)[45].

Now with respect to Saturn, which is itself at an immense distance from the Sun, its irregularities could perhaps be explained by its large distance from the Sun. Euler even suggests that perhaps Jupiter's action on Saturn might diverge from inverse-square, as the distance between them is immense. Valentin Boss argues that

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43 He noted that this correction would require a greater number of more accurate observations and more time to examine them in order to make necessary corrections.

44 It is curious that Euler does not identify the "small irregularities" (quelques petities irrégularités). We again note that this paper won the prize contest of 1748 even though the solution of the prize problem wasn't forthcoming in the paper *per se*.

45 "Il me semble donc que la proportion Newtonienne selon les quarrés des distances, n'est vraie qu'à peu près dans le sforces des corps célestes, et que peut-être elle s'écarte d'autant plus la vérité que les distances sont grandes" (EULER, 1748, §7, p.49).

this aspect of Euler's theory reflects his adherence to an aether theory and makes him anti-Newtonian (BOSS, 1972, p.215). We will see that although Euler accepted the aether theory, the issue surrounding his alleged anti-Newtoniasm is not resolved by his metaphysical belief in aether.

Two of the judges, Clairaut and d'Alembert, had a particular interest in the contest as they themselves were hard at work on the Lunar Theory and the general three-body problem.

Clairaut and d'Alembert

In 1745 Clairaut was working on the problem of the Moon's orbit and on the three-body problem in general. D'Alembert attacked the same problem the following year so that by 1747 both were actively involved in solving it. Even though both were in Paris, they worked in secret giving parts of their theories to the Academy and depositing uncompleted portions with the Secretary of the Academy in the form of sealed *pliscachetés* (envelopes)[46].

Clairaut was undoubtedly pleased in receiving Euler's paper (for by this time Euler had already gained a considerable reputation). Euler's discomfort with the inverse-square law resonated with Clairaut. Instead of looking at the discrepancy between the observed lunar distance and its predicted distance according to the inverse-square law, Clairaut maintained that the more

46 D'Alembert, it seems, played in this competition much more energetically than Clairaut. A few times throughout his career, d'Alembert would go so far as publishing incomplete works, mistakes and all, in order to circumvent Clairaut from claiming primacy. (Clairaut was elected to the Academy in July 1731 at the age of 18; d'Alembert was elected to the Academy in 1741 at the age of 23.) With respect to the Lunar Theory, d'Alembert even had the Secretary initialise every page of one memoir to guarantee primacy (HANKINS, 1970).

illuminating discrepancy was the one surrounding the motion of the lunar apogee[47].

Even before reading Euler's essay, Clairaut (as one of the judges of the prize contest) maintained that his discovery of the motion of the lunar apogee was sufficiently important to be publicly discussed at the mid-November meeting of the Paris Academy in 1747.[48] The greatest possible surprise came at this mid-November meeting when Clairaut announced, in rather pompous phrases, that ***the Newtonian theory of gravity is false!*** Although Cartesianism was beginning to dissipate, even in Paris by this time, to have Clairaut, whose work on the shape of the earth with Maupertuis had helped fortify Newtonianism on the continent, publicly call into question the inverse-square law surely delighted the Cartesians at the Paris Academy. He added that after careful calculations on the motion of apsides of the moon, he had found that the observed motion differed by a factor of two from the results predicted by Newton's law[49]d'Alembert was by this time in agreement. So we have three of the foremost mathematicians in Europe in agreement regarding the shortcomings of the inverse-square law-that there was a discrepancy between observation and theory for the motion of the apsides. Euler, in fact, wrote Clairaut 30 September, 1747 claiming:

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47 Euler did later communicate to Clairaut that he came to consider the motion of the Moon's apogee to be pivotal in the proof that the forces, which act on the Moon, do not adhere exactly to Newton's law.

48 For a careful reading of these events see Waff, "Clairaut and the motion of the lunar apse," Wilson (1995); Hankins' (1970) book, *Jean d'Alembert* offers a lively and useful discussion of these events.

49 His controversial pronouncements at this meeting had been previously deposited in a sealed envelope with the Secretary in September, two months prior to this meeting. We should note that two months after Clairaut deposited his results but prior to this meeting, d'Alembert deposited a *plis cacheté* on the same subject and his results agreed with those of Clairaut (HANKINS, 1970; WAFF, 1995).

> I am able to give several proofs that the forces which act on the moon do not exactly follow the rule of Newton, and the one you draw from the movement of the apogee is the most striking, and I have clearly pointed this out in my lunar theory, … since the errors cannot be attributed to the observations, I do not doubt that a certain derangement of the forces supposed in the theory is the cause. This circumstance makes me think that the vortices or some other material cause of these forces ought to be altered when they are transmitted by some other vortex (HANKINS, 1970, p.32).

It would appear that with Euler's backing, Clairaut would be less hesitant to deny Newton's theory. This passage is further illuminating in that we see Euler, who believed in an aether, returning to vortices in light of a failure in Newton's theory[50].

The academicians who still retained allegiances to Descartes were, obviously, excited that the Newtonians were causing the destruction of their own doctrine. History offers us many twists. Clairaut eventually solved the lunar problem and this placed him firmly in the Newtonian camp. Another Newtonian, the naturalist Buffon, attacked Clairaut's previous findings on metaphysical grounds. Clairaut had suggested that Newton's law ought to be modified to contain two terms so that force might vary in the following way:

$$F \propto \frac{1}{r^2} + \frac{1}{r^4}$$

where k is some constant to be determined. The second term, claimed Clairaut, might account for not only the perturbative effect but might also account for the phenomena of surface

50 Euler in a letter to Clairaut later ceded priority to Clairaut. D'Alembert would do the same thing in a letter to Cramer.

tension. In his reading at the 20 January, 1748 session of his *Reflexions sur la loi de l'attraction* Buffon insisted that the law of gravitation must have only one term, otherwise it would not be a simple function of the distance and would represent several forces rather than a single force. For Buffon, this suggested that the discrepancy might be due to a magnetic force-that is, gravitation might not be the only operative force. If gravitation is not the only operative force then it was conceivable, Buffon argued, that different types of matter may attract according to different laws (HANKINS, 1970, Chapter 3; WAFF, 1995, p.174). In the four or five public exchanges between Clairaut and Buffon, it becomes apparent that Clairaut's Newtonianism is quite different from Buffon's. Buffon was defending inverse-square variation largely on metaphysical grounds whereas Clairaut was offering a multi-term modification of inverse-square in order to aid in explaining a wide variety of phenomena-both celestial and terrestrial - without the use of any other physical principle besides gravitation.

In the meanwhiled'Alembert had begun speculating about Buffon's suggestion and seems to have had a vested interest in the Buffon-Clairaut controversy or debate. d'Alembert suggested at one point that it may be worthwhile to investigate a possible correlation between the movements of the moon with the variations of a compass needle, for this would make his conjecture that the force acting on the moon does not simply depend on its distance from the earth but is a function of this distance and some other unknown variable (HANKINS, 1970, p.33), d'Alembert points out that this would be a considerable undertaking.

It is interesting to note d'Alembert's movement with respect to the lunar theory. In later developments d'Alembert would admit to having misread lunar tables but, even with the necessary corrections, his results accorded well with Euler's. Perhaps d'Alembert, at the end of August 1748, best exemplifies the spirit (as Ernst Cassirer would have us believe) of the scientific developments:

> Although this proves that there is another force besides that of gravitation which acts on the moon, it seems to me that the theory of the moon, such as it is, is the most victorious proof of the Newtonian system of attraction (HANKINS, 1970, p.34).

d'Alembert's increased precision in his lunar theory had narrowed the gap between inverse-square and experience. Nonetheless, he still maintained that the motion of the apsides was probably due to an extra force such as magnetism. By December 1748 d'Alembert had completed his book on the theory of the moon. But with unusual restraint he opted to wait for Clairaut to finish in order to see if he and Clairaut were in agreement.

Just as he had earlier stood and pompously proclaimed the fallibility of Newton's theory, Clairaut, on 17 May 1749, publicly declared in the Academy that ***Newton's law had been correct all along!*** A mere five days prior to Clairaut's retraction, d'Alembert wrote Cramer that he was embarrassed to have even entertained the notion of overthrowing Newton.

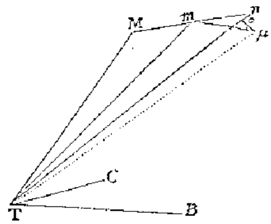
In the next part, I will provide a sketch of Clairaut's general solution to the problem. Although the solution itself is extremely important it will be in our interest to say something about the developments outlined here and how they fit into the general thesis that these developments exemplify what we have been calling the entrenchment of Newton's ideal of empirical success.

Sketch of Clairaut's Solution

Clairaut's retraction came at the end of 1748 and beginning of 1749. It wasn't until 1752, however, that his memoire outlining his solution was printed. I will refer to the first memoire, "Concerning the Orbit of the Moon," as Memoire (a) and to "Demonstration of the Fundamental Proposition of My Lunar Theory," as Memoire (b).

The goal is to develop a general equation for the curve which would be described by a body affected by the action of two forces, Σ directed toward a centre, T, and Π drawing the body away perpendicularly from the centrally directed force.

Figure 2 - Clairaut, Alexis-Claude (CLAIRAUT, 1749) Memoire (b) drawing for a 3-body problem.



He let *Mm* be that part of the curve described in an infinitely small time, *dx*. Letting *Mm*=*mn*, *n* is the point where the body would be after another infinitely small time, *dx*, if the accelerating force ceased to act at the end of the first instant. The composition of forces Π and Σ gives us *m*μ as the part of the curve described in the second instant (*Mm* is the part of the curve described in the first instant.) At *m* the body is under the influence of an accelerating force. In the absence of such a force the body would "fly off" tangentially toward *n*. The composition of the radial and transverse forces will result in the body being held in orbit about *T* by being deflected toward μ .

Clairaut makes the assumption that this body moves in an orbital plane. The equations Clairaut derives at the end of this first problem are:

$$rddv + 2drdv = \Pi dxx^2 \quad (1)$$
$$rdv^2 - ddr = \Sigma dx^2 \quad (2)$$

where r is the radius vector Tm, v the longitude, Π the sum of the transverse forces, Σ the sum of the radial forces, and dx the infinitely small time[51]. Clairaut's solution is to show that if Σ, the sum of the radial forces, is composed of an inverse square variation plus some other term, then if the other term approaches zero, the radial force then is an inverse-square variation and any perturbative effect is due to the transverse force. For the earth-moon system this means that Newton was right (as we will see) in Proposition III, Book III to claim that the small forward precession of the lunar apsides was due to the effect of the sun on the moon and subtracting this effect left the centripetal force drawing the moon toward the earth as proportional to the inverse-square of the distance. Equation (2) now becomes

$$rdv^2 - ddr = \left(\frac{\mathrm{M}}{\mathrm{rr}} + \phi\right) dx^2 (2a)$$

By two successive integrations of (1), Clairaut arrives at a differential of time:

$$dx = \frac{rrdv}{\sqrt{(f^2 + 2\Pi r^3 dv)}}$$

Substituting this expression in (2a) and integrating twice Clairaut deduces

$$\frac{\varsigma}{Mr} = 1 - g \sin v - g \cos v + \sin v \int \Omega\, dv \cos v - \cos v \int \Omega dv \sin v \ (3)$$

which is the sought after equation, and where f and g are constants of integration and W is a function of r and the perturbing force P[52]. The left side of (3) along with the first three terms on the right side define r with respect to a fixed elliptical orbit. The second part then carries the perturbative effect and this can be

51 I have kept with Clairaut's notation although it would simplify matters a bit to label time as dt instead of dx.

52 Inspection of the translation provided in the appendix will confirm this.

explained through approximation. The other two terms of equation (3),

$$\sin v \int \Omega \, dv \cos v - \cos v \int \Omega dv \sin v$$

represent the correction to the orbit due to the perturbative effect of forces F and P.

With his result Clairaut was able to claim that Newton's inverse-square law could be used to account for the deviation of the moon's motion from an idealised Keplerian orbit.

Euler on the Motion of the Lunar Apse

The problem of the apsidal motion admitted an algebraic solution. Newton did not have the analytical tools necessary to develop the algorithmic solution required. He, as has been so well documented, employed a geometric technique which was inadequate to capture properly the motion of the lunar apse. There was not any significant development with respect to the problem surrounding the motion of the lunar apse until the 1740s when Euler and other continental philosophers applied their analytical techniques to the solution of this problem. Recently, Victor Katz and Curtis Wilson, among others, have shown that differentiation and integration of trigonometric functions had not been part of the standard procedures in the solution of differential equations prior to 1739 when it was Euler who incorporated them[53].

Euler, we have seen, had grave doubts regarding the exactitude of the inverse-square law. In both "Recherches sur le mouvement des corps célestes en générale" and "Recherches sur la question des inégalités du mouvement de Saturne et de Jupiter,"

53 This was some twenty-five years after the British Parliament established a handsome prize for determining longitude at sea which required an adequate lunar theory.

Euler expressed his reluctance in accepting the inverse-square law. Like Clairaut and d'Alembert, Euler was able to recover only half of the motion of the lunar apse. Euler determined that by calculation the Moon is closer than it is in fact. This discrepancy between theory and observation preoccupied Euler, Clairaut, and d'Alembert until the problem was solved. In this light the British Parliament prize, as handsome as it was, can be claimed not to have been the motivation it was meant to have been. Euler, it appears, was driven by an ideal to have theory in agreement with observation. Euler admitted that Newton's theory enjoyed a considerable amount of success. Nonetheless, the motion of the lunar apse, as well as the perturbative effect of Jupiter on Saturn, drew into question Newton's success.

It can be said that a number of natural philosophers also questioned Newton's discussion of the motion of the lunar apse. This problem was one of many suggested in the *Principia.* Does not the whole intellectual climate formed in large part by Euler, Clairaut, and d'Alembert admit to a "Kuhnian crisis in science?" Euler, I believe, offers an interesting test case for what is the entrenchment of Newton's ideal of empirical success.

By 1748 Euler had publicly called into question the inverse-square law on the evidence that it yielded only half of the motion of the lunar apse. What is interesting about Euler is his metaphysical conviction in aether theory. He maintained that the transmission of all forces had to be carried out in some medium.

Even in September 1760, much after the debate surrounding the motion of the lunar apse had ceased, we find Euler still holding on to aether theory. In his fifty-fourth letter to a young German princess, the Princess of Anhalt-Dessau, Euler remarks:

> It is established, then, by reasons which cannot be controverted, that a universal gravitation pervades all heavenly bodies, by which they are attracted to each other; and that this power is greater in proportion to their proximity.

> This fact is incontestable; but it has been made a question, whether we ought to give it the name of impulsion or attraction. The name undoubtedly is a matter of indifference, as the effect is the same. The astronomer, accordingly, attentive only to the effect of this power, gives himself little trouble to determine whether the heavenly bodies are impelled towards each other, or whether they mutually attract one another; and the person who examines the phenomena only is unconcerned whether the earth attracts bodies, or whether they are impelled towards it by some invisible cause.
>
> But in attempting to dive into the mysteries of nature, it is of importance to know if the heavenly bodies act upon each other by impulsion, or by attraction; if a certain subtile invisible matter impels them towards each other; or if they are endowed with a secret or occult quality by which they are mutually attracted. On this question philosophers are divided. Some are of the opinion, that this phenomenon is analogous to an impulsion; others maintain, with Newton, and the English in general, that it consists of attraction (EULER, 1833, p.191)[54].

So by 1760 it would appear that Euler was resolutely a supporter of universal gravitation and the inverse-square law. Although he claims in this letter that it matter not whether gravitation is properly conceived as an impulse or as an attraction as the deciding factor is the bulk of effects observed as a result of gravitation. Nonetheless he still finds it necessary to discuss either conception noting, in a fashion reminiscent of Leibniz, that Newton and the attractionists held that the heavenly bodies

54 Letter LIV, dated 7th September, 1760.

act on one another while being endowed with a *secret* or *occult* quality. Euler, himself, remained an impulsionist, as evidenced by this letter to the young princess.

Euler maintained that gravity had a physical cause, not yet known in detail but certainly arising from the action of the fluid matter filling space. In these letters to the young princess, Euler contained himself to a discussion of the attractionists and the impulsionists. Given his position, by this time, as an impulsionist, it is a curious fact that Euler does not discuss rivals to Newton's theory although he accepted the role of an aether in gravitation. While one may pick one or the other of aether or void, Euler, by 1751, and expressed through these letters of 1760, thought that observations corresponded quite well to the theoretical expectations of Newton's theory. We see Euler not attempting to prove Newton's theory is correct piecemeal but, rather, by measuring phenomena which are backed up by open ended sets of observations. He was cashing in on a method, propounded by Newton, which the vortical theory (and mechanism generally) could not match. For this reason, the Newtonian theory appears without rival, not even Descartes' vortices, in Euler's letters to the Princess.

In 1753 Euler published his own derivation of the lunar inequalities in his *Theoria motus lunae exhibens omnes eius inaequalitates.* We are told that the aim of this work was to test Clairaut's retraction of May 1749. That is, Euler was publicly verifying, for himself, that Clairaut was right in asserting that Newton's inverse-square law sufficed to recover not half but the full motion of the lunar apse. At Euler's suggestion, the St. Petersburg Academy chose for its first prize contest for 1751 the question of whether the motions of the Moon accorded with the inverse-square law of Newton. Euler was one of the judges. As in 1747 where Clairaut and d'Alembert anticipated Euler entry in the Paris Academy's contest, we now find Euler anxiously waiting for Clairaut's paper on a similar topic.

By late winter or early spring of 1751 Euler communicated with Clairaut telling the latter that he was in receipt of four essays, one of which is obviously the latter's with the remaining three poor not only in relation to Clairaut's, but in and of themselves. He went on to claim:

> It is with infinite satisfaction that I have read your piece, which I have waited for with such impatience. It is a magnificent piece of legerdemain, by which you have reduced all the angles entering the calculation to multiples of your angle v, which renders all the terms at once integrable. In my opinion this is the principal merit of your solution, seeing that by this means you arrive immediately at the true motion of the apogee; and I must confess that in this respect your method is far preferable to the one I have used. However I see clearly that your method cannot give a different result of the motion of the apogee than mine; in which I have recently made some change, for having previously reduced all angles to the eccentric anomaly of the Moon, I have now found a way to introduce the true anomaly in its place. Thus while your final equation has its two principal variables the distance of the Moon from the Earth and the true longitude, I have directed my analysis to the derivation of an equation between the longitude of the Moon and its true anomaly, which seems to be more suitable for the usage of astronomy[55].

Like Clairaut, Euler satisfied himself that Newton's inverse-square law could account for the motion of the lunar apse. He says in a letter dated 27th July, 1751 to Tobias Mayer:

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55 Translated by Curtis Wilson from G. Bigourdan, "Lettres inédites d'Euler à Clairaut".

> At least, when I repeated my researches on this subject, I took into account the true anomalies of the Sun and Moon as well as the true distance of the Moon from the Sun, which seems to me to be far more convenient. My actual intention was to investigate thoroughly how accurate the theory of Newton concerning the motion of the Moon's apogee agrees with the observations. For not only I, but others too who worked on this, have hitherto always found that according to theory the motion of the apogee should be only half as large as it has, in fact, found to be. Now, however, I have found to my great pleasure that this part of Newton's theory is exactly in accordance with the observations: therefore it is much less to be doubted that the agreement should not be complete: there are, however, still nearly insurmountable difficulties connected with the calculation for determining accurately all the lunar inequalities from the theory (FORBES, 1971, p.38).

The agreement is there, the refinements are to come. That is, the approximations needed to carry out the calculations of the lunar inequalities were difficult. The image one gets here is that as assumptions are stripped away the parameters involved in the calculations increase. As these increases, the calculations become more and more difficult[56]. Euler, in a letter to Mayer nearly a

56 Again in the letters to the princess Euler relates this position. For instance, he claims: "The motion of the moon has accordingly in all ages greatly embarrassed philosophers; and never have they been able to ascertain, for any future given time, the exact place of the moon the heavens. … Now in calculating eclipses formerly, there was frequently a mistake of an hour or more, the eclipse actually taking place an hour earlier or later than the calculation. Whatever pains the ancient astronomers took to determine the moon's motion, they were always very wide of the truth. It was not until the great *Newton* discovered the

half-year later mentions the difficulty in carrying out such calculations. In this letter, Euler notes that the lunar inequalities are complicated by the shape of the earth and whether the Moon has a declination. Even with all these complications, Euler is confident in proclaiming the exactitude of inverse-square. The influence Euler exerted with such a pronouncement is remarkable. By 1751, the tide of informed opinion was clearly favoring the agreement of inverse-square with the motion of the lunar apogee. Mayer went on to publish the most accurate lunar tables with Euler's help.

The reader will recall that Newton placed a great deal of weight behind his argument for universal gravitation on the proposition that the Moon's motions can be accounted for with an inverse-square variation. Propositions III and IV (the Moon Test) of Book III are monumental in this respect. That Newton left lunar theory in a state of confusion allowed for non-Newtonians to attack Universal Gravitation. We have seen that Euler, Clairaut, and d'Alembert saw lunar theory as the "jugular" so to speak. Their efforts went from trying to account for the motion of the lunar apogee along Newton's theory to showing that since latter was not successful the theory was in need of correction. In the process they discovered that the theory actually did yield the motion of the apogee. In the case of Euler, his metaphysical commitment to aether theory did not detract him from accepting inverse-square variation. His letters to a young princess showed him accepting inverse-square while still claiming he was an impulsionist. He had no scientific grounds for such a belief and he was ready to admit so. He thought conceptually that it made more sense than attraction at a distance but it had no bearing on the veracity of inverse-square.

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real powers which act upon the moon, that we began to approach nearer and nearer to truth, after having surmounted many obstacles which retarded our progress"Letter LXI, 23rd September, 1760 (EULER, 1833, p.211).

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