The Hill–Brown Theory of the Moon's Motion

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Curtis Wilson

The Hill–Brown Theory of the Moon's Motion

Its Coming-to-be and Short-lived Ascendancy (1877–1984)

Springer

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We must find a theory that will work; *and that means something extremely difficult; for our theory must mediate between all previous truths and certain new experiences. It must derange common sense and previous belief as little as possible, and it must lead to some sensible terminus or other that can be verified exactly.*

– William James, *Pragmatism*, 1907 edition, p. 216

Preface

The Hill–Brown theory of the Moon's motion was constructed in the years from 1877 to 1908, and adopted as the basis for the lunar ephemerides in the nautical almanacs of the US, UK, Germany, France, and Spain beginning in 1923. At that time and for some decades afterward, it was the most accurate lunar theory ever constructed. Its accuracy was due, first, to a novel choice of "intermediary orbit" or first approximation, more nearly closing in on the Moon's actual motion than any elliptical orbit ever could, and secondly to the care and discernment and stick-to-it-ive-ness with which the further approximations ("perturbations" to this initial orbit) had been computed and assembled so as yield a final theory approximating the Moon's path in real space with an accuracy of a hundredth of an arc-second or better. The method by which the Hill–Brown lunar theory was developed held the potentiality for still greater accuracy.

The intermediary orbit of the Hill–Brown theory may be described as a periodic solution of a simplified three-body problem, with numerical parameters carried to 15 decimal places. George William Hill, a young American mathematician working for the U.S. Nautical Almanac Office, had proposed it, and computed the numerical parameters to their 15 places. A self-effacing loner, he had in his privately pursued studies come to see that the contemporary attempts at predicting the Moon's motion were guaranteed to fail in achieving a lunar ephemeris of the accuracy desired.

Of the two lunar theories vying for preeminence in the 1870s, one was the work of Peter Andreas Hansen. Hansen's theory had been adopted as the basis for the lunar ephemerides in the national almanacs beginning with the year 1862, and it would continue in that role through 1922. It was numerical rather than algebraic. This meant that numerical constants were introduced at an early stage of the computation. A consequence was that, beyond this stage, the course of the calculation was not traceable; the algebraic structure of the theory was lost from sight. The only way to make responsible corrections to the theory was to start over again from the beginning – a daunting prospect, given that Hansen's construction of the theory had occupied 20 years. Already in the 1870s Hansen's theory was known to be seriously in need of correction. Further corrections would be required for the theory to keep pace with ongoing improvements in the precision of celestial observations.

The second theory, that of Charles Delaunay, which had also required about 20 years for its construction, was entirely algebraic; its calculative paths were therefore clearly traceable. Its method, deriving ultimately from Lagrange, was elegant, and Hill was initially charmed with it. But then came a disillusioning discovery. In the higher-order approximations, the convergence slowed to a snail's pace, and the complexity of the computations increased staggeringly. For perturbations of higher order than the 7th, Delaunay resorted to "complements," guesses as to what the $(n + 1)$th-order perturbation would be by extrapolation from already computed perturbations of the nth and $(n - 1)$th order. The complements were later found to be quite unreliable. Delaunay's resort to "complements," Hill concluded, was an admission that his method had failed.

In Part I of the following study, I tell of the new method that Hill now envisaged for developing the lunar theory, a method suggested by Euler's lunar theory of 1772. In the form in which E.W. Brown carried it to completion, it was semi-numerical: the initial orbit (Hill called it the "variation curve") was given by the dynamics of a simplified three-body problem. The numerical input for this three-body problem was a single number, the ratio of the mean motion of the Sun to the synodic motion of the Moon. This number was as exactly known as any of the constants of astronomy, and therefore unlikely to require revision. The remainder of the theory, consisting of the thousands of terms necessary to "correct" the simplified model taken as starting-point, was to be literal or algebraic throughout, and therefore straightforwardly correctable. Part II tells how Brown, recruited by George Howard Darwin of Christ's College Cambridge as Hill's continuator, skillfully organized the long series of computations required for the completion of the Hill–Brown theory.

Can our story appropriately be ended here? I say No. In the 1930s, J. Leslie Comrie of the British Nautical Almanac Office hazarded the opinion that the Hill–Brown theory would remain the basis of the lunar ephemerides to the year 2000. In fact, it would be replaced after some 50 years, and in the meantime the lunar problem would be transformed out of recognition. Brown lived long enough (he died in July, 1938) to have a role in early phases of the new development. I devote Part III of my study to describing this transformation, really three revolutions wrapped into one.

To begin with, even before Hill had conceived of the Hill–Brown theory, two anomalies had been discovered in the Moon's motion – variations in its motion which gravitational theory could not account for; they would still be unresolved when Brown completed his Tables in 1919. In 1853 John Couch Adams had shown that Laplace's theory of the Moon's secular acceleration (published in 1787) could account for only about half the observed secular acceleration, leaving the other half unexplained. Secondly, Simon Newcomb in the 1860s discovered that, besides its steady acceleration, the Moon's motion was subject to additional variations, involving accelerations both positive and negative, lasting sometimes for decades and sometimes for shorter times. In 1939 it was at last shown conclusively that the first of these anomalies was due to a deceleration in the Earth's rotation, and that the second was due to erratic variations in that same rotation. The assembling of the data leading to this conclusion was the result of a cooperative effort on the part of many astronomers, including Brown. The final proof was worked out by H. Spencer Jones, H.M. Astronomer at the Cape of Good Hope, and published in 1939.

Jones's proof meant that astronomy was in need of a new clock. Since Antiquity astronomers had depended on the diurnal motion of the stars to measure time. They now knew that this motion, a reflection of the Earth's rotation, was not strictly uniform, but was slowing gradually and also varying erratically. A new method of measuring time was necessary if astronomy was to be a self-consistent enterprise.

In an initial effort to restore logical consistency to their science, astronomers invented the notion of Ephemeris Time. This was intended to be the time presupposed in the ephemerides of the Moon, Sun, and planets, which time was in turn supposed to be the time presupposed in dynamical theory – still, in the 1950s, largely Newtonian. Unfortunately, the ephemerides were only approximately in accord with dynamical theory, and were subject to repeated revision to bring them more exactly in accord with the underlying dynamical theory. Moreover, intervals of Ephemeris Time could be measured only for the past – a considerable inconvenience. Observations made in the present had to be made in Universal Time, the varying time given by the apparent diurnal motion of the stars. Time intervals in Universal Time were then corrected later through comparisons with the ephemerides.

A more convenient option became available in 1955, with the invention of the atomic clock. Its possibility had been suggested in 1945 by Isidore Rabi, the inventor of the magnetic resonance method for studying the structure of atoms and molecules. Quartz clocks could be calibrated against an atomic frequency, and thus brought to new levels of precision and accuracy as timepieces. By 1970 atomic clocks had been so improved as to be accurate to about 5 ns per day. An experiment carried out in 1971 proved that these clocks obeyed the rules of relativity theory: their rate of running was dependent on the gravitational fields and accelerated frames of reference in which they were placed. Here were new complexities and newly available levels of precision which practical astronomy needed to take into account.

The second revolution came about through the development of the electronic computer and its application in the calculations of astronomy. During the 1920s, J. Leslie Comrie of the British Nautical Almanac Office initiated the application of available punched card technology to the computation of ephemerides. He demonstrated these processes to E.W. Brown and his graduate student, Walter J. Eckert, and Eckert took up with enthusiasm the project of adapting computer programs to the needs of astronomy. By the late 1930s Eckert had succeeded in computerizing the processes whereby Brown had originally computed the 3000 or so terms of the Hill–Brown theory; the computerized computations showed that, with but few exceptions, Brown's results were extremely accurate. In 1948, with the cooperative help of Thomas J. Watson of IBM, Eckert completed the design and construction of the Selective Sequence Electronic Calculator. One of the intended uses of this instrument was to compute an ephemeris of the Moon directly from Brown's trigonometric series, thus obviating use of Brown's *Tables*, which had been found to introduce systematic error.

Later, with further increments in computer speed and reliability, efforts were made to re-do the development of the lunar theory by Delaunay's method. The old difficulty of slow convergence re-appeared, and it was found better to start from Hill's "Variation Curve," computed numerically; the theory as a whole, like Brown's, would thus be semi-numerical.

The third revolution concerned new types of data, above all, data giving the *distances* of celestial bodies. These types of data were introduced by radar-ranging, space-craft ranging, and after 1969 in the case of the Moon, laser-ranging. Earlier, the more accurate data had been angular, measuring the positions of celestial bodies laterally with respect to the line of sight. The new astronomical data, measuring the distances of celestial bodies, was more accurate by about four orders of magnitude. These types of data were the work of Jet Propulsion Laboratory (JPL), which had the task of sending spacecraft aloft and then astronauts to the Moon. The newer data types required the development of numerical integration techniques and more comprehensive (and relativistic) physical models. Laser light, and spacecraft sent aloft, achieved new wonders in determining the Moon's position, increasing the precision of the measurement by four orders of magnitude. The transformation completed itself in 1984, when responsibility for producing lunar ephemerides, and planetary ephemerides as well, passed from the Nautical Almanac Office in Washington, DC to Jet Propulsion Laboratory in Pasadena.

Without doubt, it was the end of an era.

But the mathematical and philosophical interest of an analytic solution to the lunar problem, in the Hill–Brown-Eckert manner, remains high. Such a solution reveals something of the nature and limitations of our knowledge of similar problems.

Annapolis, MD
January, 2010

Curtis Wilson

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Part I

Hill Lays the Foundation (1877–1878)

1

George William Hill, Mathematician

George William Hill (1838–1914), a mathematician with the U.S. Nautical Almanac Office from 1861 to 1892, in two papers of 1877 and 1878 laid the foundations of a new lunar theory, departing from a basic pattern that had characterized earlier algebraic theories of the Moon's motions with one exception, to be mentioned below. The first of Hill's papers was printed privately, but very quickly a copy (probably sent by Hill) reached the lunar theorist John Couch Adams of Cambridge University, and Adams called attention to its seminal importance in the Royal Astronomical Society's *Monthly Notices* for November of that year.[1] Wider recognition of its innovative character came during the course of the next decade. In 1887 Hill was awarded the Gold Medal of the Royal Astronomical Society. His sponsors for the award included Adams, George Howard Darwin, and the new president of the society, J.W.L. Glaisher, who devoted his presidential address to a précis and evaluation of Hill's two papers.

The starting-point for Hill's theory was a particular solution of two second-order differential equations expressing what Henri Poincaré would later call 'the restricted problem of three bodies' (*le problème restreint de trois corps*). These equations idealized the lunar problem, treating the Moon as of infinitesimal mass and as moving in the ecliptic plane, the Sun as having zero parallax, and the Earth as moving uniformly in a circle about the Sun. Hence, before this theory could yield the Moon's *actual* motions, it would need to be modified so as to allow for the inclusion of further "inequalities." In his paper of 1878 Hill proposed to treat the inequalities that are proportional to the sine of the lunar inclination, the solar eccentricity, and the solar parallax; but the memoir as published contains no further mention of these inequalities. In the 1880s and 1890s Hill published a number of papers on lunar inequalities; but by the 1890s, we learn, he had bequeathed the project of systematically developing his lunar theory to a younger man.

[1] J.C. Adams, "On the Motion of the Moon's Node in the Case When the Orbits of the Sun and Moon are Supposed to Have No Eccentricities, and When Their Mutual Inclination is Supposed Indefinitely Small," *Monthly Notices of the Royal Astronomical Society*, (hereinafter *MNRAS*) *38* (Nov., 1877), 43–49.

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This transfer was brought about by George Howard Darwin (1845–1912), son of Charles Darwin and an applied mathematician of Christ's College, Cambridge. Hill, writing on 10 December 1889 to Darwin in reply to Darwin's note of 22 November (no longer extant, apparently), explained what had kept him from further developing his lunar theory:

> My energies at present are devoted to the evolving a theory and tables of Jupiter and Saturn, and other projects have to be laid aside for this time. Thus it has happened that I have done scarcely anything beyond what you have seen in print, in reference to the Lunar Theory. It is very problematical whether I ever have an opportunity of continuing these researches. I should be glad to see Mr. Brown or any one else enter upon that field of labor....[2]

The Mr. Brown here mentioned was Ernest W. Brown (1866–1938), a student and protégé of Darwin's at Christ's College during the 1880s. In 1892 he was to migrate to the United States, take a position at Haverford College, and set himself to work on the elaboration of Hill's theory.

Meanwhile, at the urging of Simon Newcomb, Hill had committed himself to constructing a new theory of Jupiter and Saturn. In 1877, when Newcomb became director of the Nautical Almanac Office, he had envisaged two ambitious projects for his staff: the development of a set of planetary tables consistent in their assignment of masses to the planets (the planetary tables recently published by Le Verrier in Paris lacked such consistency), and the development of lunar tables more accurate than those currently available. The theory of Jupiter and Saturn was the most difficult of the planetary problems, and Newcomb asked Hill – whom he would later characterize as "easily ... the greatest master of mathematical astronomy during the last quarter of the nineteenth century"[3] – to take it on. This theory absorbed most of Hill's efforts from 1882 to 1892. He insisted on carrying out all the calculations himself, relying on an assistant only for verifications.

F.R. Moulton on Hill's death in 1914 wrote an appreciation of the man and his achievement. Hill, he says, was "retiring and modest to the verge of timidity.... He was absorbed in his own work but never inflicted it on others. In fact, he would hardly discuss it when others desired him to do so."[4] Moulton reports a conversation he had with Hill "after one of the meetings of the National Academy in Washington a few years ago" (Moulton does not specify the year, which was presumably in the twentieth century). It was a fine spring day; Hill had asked Moulton to join him in a walk, and was unusually forthcoming about his own earlier work:

> Hill told me that he thought the greatest piece of astronomical calculation ever carried out by one man was Delaunay's lunar theory, and that his work on Jupiter and Saturn came second. Now the greater part of this work was straight computation by methods which were largely due to Hansen, and

[2] Hill to G.H. Darwin, 10 Dec. 1889, University of Cambridge Library manuscript collection (hereinafter UCL.MS), DAR.251:3533; quoted with permission.

[3] S. Newcomb, *The Reminiscenses of an Astronomer* (Houghton: Mifflin, 1903), 218.

[4] F.R. Moulton, "George William Hill," *Popular Astronomy, 22* (1914), 391–400, 391.

> which could have been carried out under Hill's direction by men who did not have his great ability for original work. It seems probable that science lost much because Newcomb caused Hill to spend about eight years of the prime of life on this work. At any rate, this was the direct cause of his laying aside, as he thought for a time only, his researches on the lunar theory.[5]

Hill recognized that the working out of his lunar theory would involve much tedious calculation; he estimated it would require about 10 years, assuming a number of assistants to do the routine calculations. From a letter of Hill to Darwin of July 1886, we gain some sense of the strain that Hill felt when engaged in "that field of labor." Darwin had written to invite Hill to contribute a paper to a certain journal (unspecified in Hill's letter); but Hill is begging off:

> ...I have made arrangements for going off in a few days to the wilds of Canada to pass the vacation. The relaxation I get during the summer vacation is a matter of great importance to me, as by it I gain sufficient strength to keep in working trim for the following nine or ten months; and it is all the more effective, if, during the time, I can be absolutely free from the worry of scientific investigations.[6]

In 1892, at age 54, Hill retired from the Nautical Almanac Office, and returned to the family farm in West Nyack, New York, where he had always preferred to be. He was an amateur botanist, with considerable expertise in identifying wild plants, and he loved taking solitary walks and botanizing. From Washington he brought with him the still unfinished tables for Jupiter and Saturn, and completed them in West Nyack.

In tackling the problem of Jupiter and Saturn, Hill considered the possibility of using Delaunay's method – the method Delaunay had applied to the Moon[7]; it had not previously been applied to planetary perturbations. He abandoned this idea, however, and adopted instead a modification of the method of Hansen's *Auseinandersetzung*.[8] Hansen had already applied an early version of his method to Jupiter and Saturn, thus providing a model.[9] Hill apparently judged that Hansen's processes would lead more swiftly to the result aimed at than the extensive transformations required by Delaunay's method.

In 1895 Hill was chosen president of the American Mathematical Society for the 1895–1896 term. His presidential address, delivered on 27 December 1895, concerned "the Progress of Celestial Mechanics since the Middle of the Century."[10]

[5] Ibid., 398.

[6] CUL. MS. DAR.251: 2614, Hill to Darwin, 12 July 1886.

[7] See Hill's article, "Notes on the Theories of Jupiter and Saturn," *The Analyst*, VIII (1881), 33–40, 89–93; *The Collected Mathematical Works of George William Hill*, I, 351–363.

[8] P.A. Hansen, *Auseinandersetzung einer zweckmässigen Methode zur Berechnung der absoluten Störungen der kleinen Planeten*, in *Abhandlungen der Königlich Sächsischen Gesellschaft der Wissenschaften, 5* (1859): 43–218; 6(1859), 3–147.

[9] *Untersuchung über die gegenseitigen Störungen des Jupiters und Saturns*, Berlin, 1831.

[10] *Bulletin of the American Mathematical Society*, second series, *II* (1896), 125–136; *The Collected Mathematical Works of George William Hill*, IV, 99–110.

Nowhere in it does he mention his own lunar theory; he deals solely with the work of Delaunay, Gyldén, and Poincaré. Brown, having gone to New York to hear it, reported to Darwin that "it wasn't particularly interesting."[11] Hill had mastered an enormous amount of the detail of celestial mechanics, including the crucial details that had led him to his new lunar theory. But he was not particularly successful at transmitting to others a larger view. Frank Schlesinger's account of Hill's lecturing on his specialty at Columbia University for a semester in 1899 tells us that the lecturer was tense and that the three graduate students who constituted his audience were awed and uncomprehending.[12] As Newcomb will remark later, Hill lacked the teaching faculty.[13]

The archives of the Naval Observatory Library contain an undated, typed memorandum of three pages, giving Hill's assessment of the status of the lunar problem and his estimate as to what the development of the new lunar theory he had laid the foundations of would require (for the text, see the Appendix). A reference there to a memoir by Radau – it dealt with the planetary perturbations of the Moon and had appeared in the *Bulletin astronomique* in April and May, 1892 – is consonant with the memorandum's having been drawn up around the time of Hill's retirement. The addressee of the memorandum is not specified, but in it Hill refers three times to "Prof. Newcomb," in particular mentioning Newcomb's corrections of Hansen's lunar tables. Hansen's lunar tables had been adopted as the basis for the lunar ephemerides in the British *Nautical Almanac* and the French *Connaissance des Temps* beginning in 1862; with corrections introduced by Newcomb they were adopted for the American lunar ephemerides beginning in 1883. Hill, while respectful of Newcomb's endeavors, is in effect criticizing Newcomb's attempt to "make do" with Hansen's theory.

Hill allows that, from a purely practical point of view, Hansen's tables, with minor corrections, might be used for an indefinite time without serious error. But the comparison Newcomb has made (in *Astronomical Papers prepared for the use of the American Ephemeris and Nautical Almanac*, I, 1882, 57–107) between the terms in Hansen's theory and those in Delaunay's shows discrepancies in the values of the coefficients amounting in some cases to $0''.5$; some of these were probably due to numerical mistakes made by Hansen. "It is not creditable to the advanced science of the present day," Hill remarks, "that we should be in any uncertainty in this respect." He goes on to urge that, "in treating this subject, we should start from a foundation reasonably certain in its details, all known forces being taken correctly into account." Hansen's theory, in his opinion, could not furnish such a foundation. "To pass from Hansen to a theory absolutely unencumbered with empiricism is a matter of difficulty. It is not even certain that the figures in [Hansen's *Tables de la lune*, 1857] are actually founded on the formulas of the introduction [to those tables]."

[11] Brown to Darwin, 12 January 1896, CUL. MS. DAR.251: 477.

[12] F. Schlesinger, "Recollections of George William Hill", *Publications of the Astronomical Society of the Pacific*, 49, 5–12.

[13] S. Newcomb, *The Reminiscenses of an Astronomer*, 218.

Hill is endorsing the further development of his own lunar theory, urging that Ernest W. Brown be encouraged in the computations he has commenced.

> Aid should be given in order that we may have the results sooner.... I estimate that on this plan new tables could be prepared and ready for use in ten years. Of course, sufficient computing force must be given to the undertaker of this project, perhaps three persons might suffice.

Hill's confidence in his theory was not misplaced. Brown in the course of his work demonstrated the superior accuracy of the new theory compared to earlier theories, including Hansen's and Delaunay's.

Hill does not imagine that the new tables will resolve all difficulties. Unknown causes are acting, producing unsolved puzzles that are unlikely to be cleared up in a mere decade.

> The comparison of [the new] theory with observation will give residuals which are the combined effects of the necessary changes in the values of the arbitrary constants and the action of the unknown causes. The latter undoubtedly exist, and I am afraid the period of observation is too short to show their real law.

Here Hill may have in mind Newcomb's earlier discovery that Hansen's tables were well fitted to lunar observations from 1750 to 1850, but deviated from observations made before and after that period. As Newcomb discovered, Hansen had altered numerically the theoretical value of the perturbations of the Moon due to Venus, attempting in this way to accommodate these earlier and later observations, while claiming that this was the sole piece of sheer empiricism in his tables. Newcomb at the date Hill writes is still tinkering with this term – a mistaken effort in Hill's view. Hill's own guess is that the discrepancies are due to the attractions of meteors, a guess that will prove equally illusory.

The Moon's motion, it was found, departed from Hansen's tables in two ways that Newtonian theory could not account for. First, the Moon was accelerating over the centuries. Part of this acceleration was derivable from planetary perturbation of the Earth, but the rest was not. Delaunay suggested that the excess acceleration might be due to a deceleration in the Earth's rotation caused by tidal friction. The second effect was a fluctuation in the Moon's motion; its speed, besides accelerating, was altering in seemingly random ways. Like the excess acceleration, the fluctuations might be attributable to alterations in the Earth's rotation. But demonstrating these conjectures would take some doing, and would not be accomplished till 1939. The introduction of the atomic clock in 1955 will put the assignment of these effects to changes in the Earth's rotation beyond possible doubt. Both the tidal deceleration and the fluctuations remain subjects of ongoing research today. In Part III of our study we shall enter into more detail concerning this topic, insofar as it is relevant to lunar astronomy.

Hill's innovations in the lunar theory led to two later developments in mathematics that we shall touch on in passing. In computing the motion of the Moon's perigee he found himself confronted with an infinite determinant, which he succeeded in solving. This feat sparked the interest and admiration of Henri Poincaré, and Poincaré's ensuing investigation of infinite determinants then led to a considerable mathematical development in later decades.[14] Secondly, Hill's detailed working out of a periodic solution of the three-body problem brought such periodic solutions to the attention of mathematicians, including, again, Poincaré. Such periodic solutions became for Poincaré the point of departure for explorations of the phase space of the three-body problem – researches which cast new light on the theory of differential equations as well as on the nature of classical mechanics.[15] In the present study we focus on the lunar theory itself: Hill's promising beginnings, and Brown's elaboration of them into a complete lunar theory.

[14] See M. Bernkopf, "A History of Infinite Matrices," *Archive for History of Exact Sciences, 4* (1967–1968), 308–358, especially 313ff.

[15] See J. Barrow-Green, *Poincaré and the Three Body Problem* (Providence, RI: American Mathematical Society; London: London Mathematical Society, 1997).

2

Lunar Theory from the 1740s to the 1870s – A Sketch

The attempt to cope with Newton's three-body problem not geometrically as Newton had done but algebraically, using the calculus in the form elaborated by Leibniz, got under way in the 1740s. That this attempt had not been made earlier appears to have been due to lack of an appreciation, among Continental mathematicians, of the importance of trigonometric functions for the solution of certain differential equations; they failed to develop systematically the differential and integral calculus of these functions. Newton had used derivatives and anti-derivatives of sines and cosines, but had not explained these operations to his readers. Roger Cotes, in his posthumous *Harmonia mensurarum* of 1722, articulated some of the rules of this application of the calculus. But Euler, in 1739, was the first to provide a systematic account of it. In the process he introduced the modern notation for the trigonometric functions, and made evident their role qua functions. Thus sines and cosines having as argument a linear function of the time, t, could now be differentiated and integrated by means of the chain rule. Differential equations giving the gravitational forces acting on a body could be formulated and solved – though only by approximation.

Euler was the first to exploit these possibilities in computing the perturbations of the Moon. The tables resulting from his calculation were published in 1746, without explanation of the procedures whereby they had been derived.

In March of 1746 the prize commission of the Paris Academy of Sciences, meeting to select a prize problem for the Academy's contest of 1748, chose the mutual perturbations of Jupiter and Saturn. Since Kepler's time, Jupiter had been accelerating and Saturn slowing down, and in other ways deviating from the Keplerian rules. Newton assumed the deviations to be due to the mutual attraction of the two planets, and proposed coping with the deviations in Saturn by referring Saturn's motion to the center of gravity of Jupiter and the Sun, and assuming an oscillation in Saturn's apsidal line. These proposals do not appear to have led to helpful results. The contest of 1748 was the first academic contest of the eighteenth century in which a case of the three-body problem was posed for solution.

The winning essay was Euler's; it was published in 1749. It was not successful in accounting for the anomalies in the motions of Saturn and Jupiter, but its

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technical innovations proved to be crucially important in later celestial mechanics. One of them was the invention of trigonometric series – a series in which the arguments of the successive sinusoidal terms are successive integral multiples of an angular variable. Euler's angle in the case of Jupiter and Saturn was the difference in mean heliocentric longitude between the two planets, which runs through 360° in the course of about 20 years. As it does this, the distance between the two planets varies by a factor of about 3.4, and hence the forces they exert on each other vary by a factor of about $(3.4)^2 = 11.6$. The expression of the perturbing force by means of a trigonometric series enabled Euler to solve the differential equations of motion to a first-order approximation. Trigonometric series later found other applications in celestial mechanics, for instance in expressing the coordinates of the Moon in terms of the mean anomaly, and the relations between mean anomaly, eccentric anomaly, and true anomaly.

A second seminal innovation in Euler's essay was his use of multiple observations in refining the values of certain coefficients. It was the first explicit appeal in mathematical astronomy to a statistical procedure. The method of least squares had not yet been invented. Euler's procedure involved forming the differential corrections for the coefficients in question, then selecting observations in which a given coefficient could be expected to be large, and solving the resulting equations approximately by neglecting terms that were relatively small. Tobias Mayer soon put this procedure to use in the lunar theory.

The lunar problem differs significantly from the planetary problem. The distance from the Moon of the chief perturbing body, the Sun, changes by only about $1/390$th of its value during the course of a month, and the resulting perturbation is so minimal that it can be ignored in the first approximation. What primarily causes the lunar perturbations is the *difference* between the forces that the Sun exerts on the Moon and on the Earth. Were the Moon entirely unperturbed by the Sun, it would move about the Earth in an ellipse, one focus of which would be occupied by the Earth's center of mass; a limiting case being a circle concentric to the Earth. But as Newton showed in Corollaries 2–5 of Proposition I.66 of his *Principia*, if the Moon's pristine orbit about the Earth were a concentric circle, the effect of the Sun's extra force, over and above the force it exerts on the Earth, would be to flatten the circle in the direction of the line connecting the Earth with the Sun (the line of syzygies), decreasing its curvature there, while increasing it in the quadratures (where the angle between the Sun and Moon is 90°). Also, the Moon's angular speed about the Earth would be greater in the line of syzygies than in the quadratures. The variation in angular speed had been discovered by Tycho in the 1590s, and was named by him the "Variation." Newton derived a quantitative measure of the Variation in Propositions III.26–29 of the *Principia*, showing (on the assumption again of the Moon's having pristinely a circular orbit) that the Moon's displacement from its mean place would reach a maximum of $35'10''$ in the octants of the syzygies, and the oval into which the circle is stretched would have its major axis about one-seventieth longer than its minor axis.

Astronomers had found the eccentricity of the Moon's orbit to be, on average, about one-twentieth of the semi-major axis; were the Sun not perturbing the Moon, such an eccentricity would imply an elliptical orbit with the major axis exceeding the minor by only about 1/800th. Thus eccentricity by itself distorts the shape of the Moon's orbit less than solar perturbation. On the other hand, it causes a greater departure of the Moon from its mean motion, rising to a maximum displacement of nearly 6° approximately midway between perigee and apogee. (This departure from the mean motion is what led astronomers to assume an eccentric lunar orbit in the first place.) The true orbit of the Moon, Newton implies, is a kind of blend of the Variation oval and the eccentric ellipse – "an oval of another kind."[16]

When Newton undertook to derive a quantitative measure of the Moon's apsidal motion, probably in 1686, he attempted to meld the effects of these two orbits; his procedure was bold but unjustifiable. From this leap in the dark he later retreated, apparently recognizing its illegitimacy.[17]

The first published lunar theory giving explicit derivation of the inequalities by means of the Leibnizian calculus was Alexis-Claude Clairaut's *Théorie de la lune* (1752). Clairaut and Jean le Rond d'Alembert, both members of the prize commission for the Paris Academy's contest of 1748, had been occupied with the lunar theory since the commission met in the spring of 1746. Both of them discovered, early on, that their calculations yielded in the first approximation only about half the motion of the Moon's apse. With respect to the other known inequalities of the Moon, their calculations had yielded reasonably good approximations. Neither Clairaut nor d'Alembert supposed that the second-order approximation would be able to remove the large discrepancy in the apsidal motion. In September 1747 Clairaut learned that Euler in his lunar calculations had found the same discrepancy. The three mathematicians were calculating along rather different routes; hence the apsidal discrepancy did not appear to be an artifact of a particular procedure. Clairaut presented this discovery to the Paris Academy in November 1747, proposing that a term be added to Newton's inverse-square gravitational law, with the additional force varying inversely as the fourth power of the distance; the coefficient of this second term was to be adjusted so as to yield the missing apsidal motion. The proposal met with vigorous protest from Buffon, who regarded a two-term law as metaphysically repugnant.

Clairaut's proposal to modify the gravitational law was in accord with an idea suggested earlier by John Keill – that the inverse-square law holding for interplanetary distances might take on a modified form at smaller distances, so as to account for the forces involved in, for instance, capillary and chemical actions. Euler, by contrast, thought the gravitational law would fail at very large distances, for he attributed all forces to the impact of bodies, and gravitational force to the pressure of an aether; but the aether responsible for the "attraction" toward a particular celestial body would presumably extend only a finite distance from the body. D'Alembert, differing from both Clairaut and Euler, regarded the inverse-square law of gravitation

[16] See D.T. Whiteside, *The Mathematical Papers of Isaac Newton*, VI, (Cambridge: Cambridge University Press, 1974) 519.

[17] See my "Newton on the Moon's Variation and Apsidal Motion," in *Isaac Newton's Natural Philosophy* (eds. Jed Z. Buchwald and I. Bernard Cohen: Cambridge, MA: The MIT Press, 2001), 155–168.

as sufficiently confirmed by the empirical evidence Newton had supplied; the cause of the discrepancy in apsidal motion, he advised, should be sought in the action of a separate force, such as magnetism, reaching from the Earth to the Moon.

The issue was resolved in the spring of 1749, when Clairaut proceeded to a second-order approximation. In the new calculation, certain terms deriving from the transverse component of the perturbing force proved after integration to have very small divisors; the re-calculated coefficients were thus extremely large. These revisions led in turn to a value for the apsidal motion nearly equal to the observed value. The inverse-square law, it appeared, required no alteration.[18] On the other hand, the slow convergence revealed in the initial analytic assault on the lunar theory was to prove a persistent difficulty.

Euler published a detailed lunar theory in 1753. Its primary purpose was to confirm or disconfirm Clairaut's new result by an entirely different route. Euler eliminated the radius vector from his calculations, since it did not admit of precise measurement by the means then available (namely, micrometer measurements of the Moon's diameter). He took his value for the apsidal motion from observation, but in his equations assumed that the inverse-square law required modification by the addition of a term which he symbolized by μ. The end-result of his calculation was that μ was negligible and could be set equal to zero.

D'Alembert had registered his early writings on the lunar theory with the Paris Academy's secretary, but learning of Clairaut's new result, stipulated that they should not be published. In 1754 he published a lunar theory re-worked from the earlier versions, but now incorporating a multi-stage derivation of the apsidal motion. He gave four successive approximations, with algebraic formulas for the first two. Whether further approximations would continue to converge toward the observational value, he pointed out, remained a question. Neither he nor Clairaut searched for the deeper cause of the slow convergence they had encountered.

The predictive accuracy achieved in the lunar theories of our three mathematicians was between 3 and 5 arc-minutes – not particularly better than the accuracy of a Newtonian-style lunar theory, such as Le Monnier published in his *Institutions astronomiques* of 1746.

The first lunar tables accurate enough to give the position of the Moon to two arc-minutes, and hence to give navigators the geographical longitude to 1°, were those of Tobias Mayer (1723–1762)), published initially in 1753. They were later refined and submitted to the British Admiralty. In 1760 James Bradley, the Astronomer Royal, compared them with 1100 observations made at Greenwich, and found $1'.25$ as the upper bound of the errors. The Admiralty Board at length adopted Mayer's tables as the basis for the lunar ephemerides in the *Nautical Almanac*, which appeared annually beginning in 1767. Whence the superior accuracy of Mayer's tables?

We are unable at the present time to answer this question definitively, but it appears that empirical comparisons had much to do with the accuracy achieved.

[18] A somewhat fuller account is given in "Newton on the Moon's Variation and Apsidal Motion," as cited in the preceding note, 173ff.

Mayer began with a Newtonian-style theory.[19] At some date he carried out an analytical development of the lunar theory, following, with some variations, the pattern laid out in Euler's theory of Jupiter and Saturn of 1749; he carried the analysis so far as to exhaust, as he said, "nearly all my patience." Many of the inequalities, he found, could not be deduced theoretically with the desired accuracy unless the calculation were carried still farther. From Euler's prize essay on Saturn's inequalities he had learned how the constants of a theory could be differentially corrected by comparison with large numbers of equations of condition based on observations; and he had applied such a process in determining the Moon's librations (slight variations in the face that the Moon presents to an Earth-bound observer, due primarily to variations in the Moon's orbital speed combined with the Moon's almost exactly uniform axial rotation). But of the processes he used in determining the Moon's motions in longitude, he gives us no description. We know that he assembled a large store of lunar observations, many of them his own, including extremely accurate ones based on the Moon's occultations of stars. Presumably he once more constructed Eulerian-style equations of condition, solved them approximately, and thus refined the coefficients of his theoretically derived terms to achieve a superior predictive accuracy.

Mayer's tables, being semi-empirical, did not answer the theoretical question as to whether the Newtonian law could account for all lunar inequalities. But they met the navigator's practical need, supplying a method for determining longitude at sea – at first the only method generally available. In later years, as marine chronometers became more affordable and reliable, the chronometric method was understandably preferred. The chronometer gave the time at Greenwich, and this, subtracted from local time as determined from the Sun, gave the difference in longitude from Greenwich. The method of lunar distances, by contrast, required a much more extended calculation. The latter method was long retained, however, as supplying both an economical substitute for the chronometrical method and an important check on it.

In 1778 Charles Mason revised Mayer's tables, relying on 1137 observations due to Bradley, and using, we assume, a similar deployment of equations of condition. It was in the same way, apparently, that Tobias Bürg revised Mason's tables early in the 1800s; he used 3000 of the Greenwich lunar observations made by Maskelyne between 1760 and 1793. From Mayer's theoretical derivation (published by the Admiralty in 1767), Mason deduced eight new terms, and Bürg added six more, to be included in the tables. But the accuracy of the tables depended crucially on the empirical refining of constants.

When Laplace undertook to deduce the lunar motions from the gravitational law, he saw these semi-empirical tables as setting a standard of accuracy difficult to surpass (*Mécanique Céleste*, Book VII, Introduction). Laplace's theory was considerably more accurate than the earlier analytical theories of Clairaut, Euler, and d'Alembert. This was principally because of Laplace's discovery of new inequalities by deduction from the gravitational law. Among these new inequalities were

[19] Private communication from Steven Wepster of the Mathematics Department, University of Utrecht.

two arising from the Earth's oblateness (the decreasing curvature of its surface from equator to poles). Moreover, Laplace for the first time supplied a gravitational explanation for the Moon's secular acceleration, as arising indirectly from the secular diminution of the eccentricity of the Earth's orbit; his deduced value for it was in good agreement with observations. (In the 1850s it would be found to be theoretically in error, so that a drastic reinterpretation was required – a topic that we shall return to in Part III.) The greatest difference between the predictions of Laplace's theory and Bürg's tables was 8.3 arc-seconds; thus the theoretical deduction fell little short of the accuracy attainable by comparisons with observations. The day was coming, Laplace confidently predicted, when lunar tables could be based on universal gravitation alone, borrowing from observation solely the data required to determine the arbitrary constants of integration.

Bürg's tables were published by the French Bureau des Longitudes in 1806. In 1811 J.K. Burckhardt presented new lunar tables to the Bureau; they were based on 4000 observations as well as on the terms newly discovered by Laplace. A commission compared Bürg's and Burckhardt's tables with observations of the Moon's longitudes and latitudes from around the orbit, using the method of least squares to assess the goodness of fit (this appears to have been the first published use of MLS). In 167 observations of the Moon's longitude, the root mean square error of Bürg's tables was $6''.5$, compared with $5''.2$ for Burckhardt's tables; in 137 observations of the Moon's latitudes, the corresponding numbers were $6''.0$ and $5''.5$. Consequently Burckhardt's tables were adopted as the basis of the lunar ephemerides in the French *Connaissance des Temps* and in the British *Nautical Almanac*. They would continue in that role, with some later corrections, through 1861.

For its prize contest of 1820, the Paris Academy of Sciences, at Laplace's urging, proposed the problem of forming tables of the Moon's motion as accurate as the best current tables [i.e., Burckhardt's] on the basis of universal gravitation alone. Two memoirs were submitted, one by the Baron de Damoiseau (1768–1846), director of the observatory of the École Militaire in Paris, the other by Giovanni Plana (1781–1864) and Francesco Carlini (1783–1862), directors, respectively, of the observatories in Turin and Milan. Both memoirs were Laplacian in method. Damoiseau proceeded more systematically than had Laplace. From the start he put the reciprocal radius vector (u) equal to $u_0 + \delta u$, and the tangent of the latitude (s) equal to $s_0 + \delta s$, where u_0 and s_0 are the elliptic values of u and s, and δu and δs are the modifications produced by perturbation. He developed the expressions for u and s to the sixth order inclusive in the lunar and solar eccentricities and inclination of the lunar orbit, whereas Laplace had stopped at the fourth order. He put δu, and also δs, equal to a set of sinusoidal terms, with the coefficient of each such term containing an undetermined factor; there were 85 such factors in the expression for δu and 37 in the expression for δs. Substituting the expressions for u and s into the differential equations, replacing the arbitrary constants by their empirical values, and setting the coefficient of each sine and cosine term equal to zero, Damoiseau obtained 207 equations of condition, which he solved by successive approximations for the undetermined factors. Because he substituted numerical values of the arbitrary constants from the start, his theory is called a *numerical* theory; it is to be contrasted with a *literal* theory in which the coefficients are expressed as algebraic functions of the arbitrary constants. Comparing Damoiseau's tables with 120 observations, and finding them to be of the same order of accuracy as Burckhardt's tables, the prize commission deemed them worthy of the prize.

Plana and Carlini in their memoir undertook to achieve a strictly literal solution of the differential equations. The coefficients of the sinusoidal terms of the theory are functions of certain constants of the theory – the orbital eccentricities of the Moon and the Sun, the tangent of the Moon's orbital inclination to the ecliptic, the ratio of the Sun's and Moon's mean motions, the ratio of the mean Moon-Earth and Sun-Earth distances. But these functions are far too complicated to be represented analytically, except in the form of infinite series in the powers and products of the constants involved. Our authors accordingly introduced such series into the representation of the theory – an important innovation, revealing the causal provenance of each term, and permitting the effect of any revision of a constant to be immediately calculated. The numerical factor that multiplies any term in such a series can be determined not merely approximately but exactly, as a numerical fraction, and the approximate character of the coefficient is due only to the series having to be broken off after a finite number of terms rather than being summed as a whole.[20] Unfortunately, for some of the series the rate of convergence was excruciatingly slow. Where denominators were produced by the integrations, Plana and Carlini developed their reciprocals as series and multiplied them into the numerators, often with a decrease in rate of convergence. At the time of the contest deadline they had not yet constructed tables, but they showed that their coefficients for the inequalities in longitude were in close agreement with Burckhardt's. In view of the immense labor that their memoir embodied, and the value of the resulting analytic expressions, the Academy decreed that they, like Damoiseau, should receive the full value of the prize as originally announced.

Plana went on to achieve a more complete development of the Plana-Carlini theory in three large volumes published in 1832. Here the dependent variables u and s emerge in successive approximations. Volume II gives the results accurate to the fifth order of small quantities, while Volume III gives the developments required to proceed to still higher orders.

The lunar theories of Clairaut, d'Alembert, Laplace, Damoiseau, and Plana all took as independent variable the true anomaly v, expressing the true longitude of the Moon from the lunar apse. Hence the variables u and s were obtained as functions of v, and so also was the mean anomaly ($[nt + \varepsilon]$ in Laplace's notation, where n is the mean rate of motion, t is the time, and ε the mean longitude at epoch). The resulting series, Laplace stated, converged more rapidly than the series obtained when the independent variable was the mean anomaly. The choice of v as independent variable meant that, to obtain u, s, and v as functions of t, it was necessary to obtain v as a function of the mean anomaly by reversion of the series for $nt + \varepsilon$ in terms

[20] A number of the points made here are due to J.C. Adams, "Address on presenting the Gold Medal of the Royal Astronomical Society to M. Charles Delaunay," *The Scientific Papers of John Couch Adams*, *I*, 328–340.

of ν. This operation becomes increasingly laborious as higher-order approximations are undertaken, and in 1833 Siméon-Denis Poisson (1781–1840) proposed that it be avoided by taking t as independent variable from the start. His former student Count Philippe G.D. de Pontécoulant was the first to carry through a complete development of the lunar theory on this plan. It was published in 1846 as Volume IV of Pontécoulant's *Théorie du système du monde*.

After completing the analytic development, Pontécoulant substituted empirical values for the constants in his formulas, and compared the resulting coefficients of terms in the longitude with those given by Damoiseau, Plana, and Burckhardt. His and Plana's coefficients agreed closely, despite the difference in their methods. Of Pontécoulant's 95 longitudinal terms, Plana gave 92. In eleven cases of discrepancy Pontécoulant traced the difference to errors in Plana's derivations – errors later verified and acknowledged by Plana. The differences between Pontécoulant's and Burckhardt's coefficients were generally small; in two cases they exceeded $2''$, and in 16 they exceeded $1''$. Pontécoulant believed the fault lay with the observations on which Burckhardt's tables were based.

In 1848 G.B. Airy published a reduction of the Greenwich lunar observations for the period 1750–1830. To compare the sequence of resulting positions of the Moon with theory, he turned to Damoiseau's tables of 1824, but with the coefficients modified to agree with Plana's theory, including all corrections so far found necessary. From Plana's theory and the observations, Airy then obtained corrected orbital elements for the Moon. Airy's lunar elements were the basis on which Benjamin Peirce of Harvard founded his *Tables of the Moon* (1853, 1865), from which were derived the lunar ephemerides published in the *American Ephemeris and Nautical Almanac* from its inception in 1855 through 1882.

For accuracy, however, lunar theories and tables from Damoiseau's to Pontécoulant's were outdistanced by the *Tables de la lune* of Peter Andreas Hansen (1795–1874), published in 1857. Deriving perturbations from gravitation alone, Hansen achieved an accuracy superior to Burckhardt's. His tables were adopted for the British and French national ephemerides beginning with the year 1862, and for the American *Nautical Almanac* beginning with the year 1883; they would remain in that role till 1922.

Hansen's method differed from that of any earlier theory. He had devised his way of computing perturbations in the course of preparing a memoir for submission in the Berlin Academy's contest of 1830. The problem posed by the Academy concerned Laplace's and Plana's conflicting results for second-order perturbations of Saturn due to Jupiter. Contestants were asked to clarify the issues involved.

The difficulty in deriving analytically the motion of the Moon's apse in the 1740s had led to the recognition that perturbations must necessarily be computed by successive approximations. Often the first approximation would prove sufficiently precise, but if greater precision were needed, the approximations could be arranged in a series with respect to powers of the perturbing force. For instance, to compute Saturn's perturbations of the first order with respect to Jupiter's perturbing force, you started from assumed approximate motions for the two planets (motions, say, following Kepler's "laws"), and on this basis calculated the attractions whereby Jupiter perturbs Saturn. To obtain the second-order perturbations of Saturn, the first-order perturbations of Jupiter due to Saturn, as well as the first-order perturbations of Saturn due to Jupiter, had to be taken into account. Thus the approximations initially assumed were to be progressively refined. When the corrections became smaller than the currently attainable observational precision, the result could be accepted as sufficiently precise.

Laplace gave no systematic procedure for perturbations beyond those of first-order. Second-order perturbations, he believed, would need to be calculated only in special cases – where, for instance, the first-order perturbations were large. He failed to recognize the need for a systematic way of obtaining higher-order perturbations. It would later become evident that he had omitted second-order perturbations as large as those he calculated. Nor did Plana, though questioning Laplace's second-order results, supply a systematic procedure.

A systematic and rigorous procedure for first- and higher-order perturbations, however, was already at hand. It utilized formulas in the second edition of Lagrange's *Mécanique analytique* (1814). These formulas expressed the time-rates of change of the orbital elements as functions of these same elements and of the partial derivatives of the disturbing function with respect to them. (The disturbing function, a Lagrangian innovation, is a potential function from which the force in any direction can be derived by partial differentiation.) These formulas were rigorous, and remarkable in their independence of the time. Lagrange was imagining the planet or satellite as moving at each instant in an ellipse characterized by its six orbital elements, with the elements changing from instant to instant due to perturbation. Second- and higher-order perturbations were derivable by applying the well known "Taylor's theorem".

This procedure, however, was time-consuming. The perturbations of all six orbital elements had to be computed, whereas it was only the perturbations of the coordinates, three in number, that were required practically. The perturbations of the elements were often larger than those of the coordinates, so that a smaller quantity would have to be determined from the difference of two larger ones, giving a result of uncertain precision. Hansen therefore set out to transform Lagrange's formulas, so as to obtain a more direct route from disturbing function to the perturbations of the coordinates.

Two simultaneous processes had to be taken into account: the continuous change in shape and orientation of the instantaneous elliptical orbit in which the perturbed body was conceived to be traveling, and the body's motion along this protean orbit. The first of these processes was expressible through the Lagrangian formulas giving the rates of change of the orbital elements. The second process was governed by well-known elliptical formulas: the true anomaly of the body (its longitude from perihelion) was given, through an auxiliary variable, in terms of the mean anomaly; and the radius vector was given in terms of the true anomaly.

The main focus of Hansen's method was on the perturbations affecting the orbital motion in the instantaneous plane (he treated the perturbations in the position of the instantaneous plane separately). Here two processes needed to be kept distinct: change in shape and size of the ellipse and motion of the body along it. For this purpose Hansen introduced two variables for the time: t for the time in which changes in orbital elements are registered, τ for the time in which the motion along the orbit occurs. Eventually the two times would be identified as one, the single time of the ongoing, twofold process.

To have a single variable that would incorporate both aspects of this double process, Hansen introduced ζ as a function of both t and τ. To define it quantitatively, he stipulated that the true anomaly λ should be a function of ζ, and through ζ of t and τ. Hence

$$\frac{\partial\lambda}{\partial t} = \frac{\partial\lambda}{\partial\zeta} \times \frac{\partial\zeta}{\partial t},$$
$$\frac{\partial\lambda}{\partial\tau} = \frac{\partial\lambda}{\partial\zeta} \times \frac{\partial\zeta}{\partial\tau}. \tag{Ha.1}$$

The quotient of the first of these equations by the second is

$$\frac{\partial\zeta/\partial t}{\partial\zeta/\partial\tau} = \frac{\partial\lambda/\partial t}{\partial\lambda/\partial\tau}. \tag{Ha.2}$$

Now $\partial\lambda/\partial t$ is given in terms of the Lagrangian formulas for rates of change of the orbital elements; and $\partial\lambda/\partial\tau$ in terms of known elliptical formulas. Hence the quotient on the right side of (Ha.2) is expressible in terms of explicitly defined quantities.

To obtain an expression for ζ, Hansen proceeded by successive approximations. In the first approximation, he set $\partial\zeta/\partial\tau$ equal to 1, so that $\zeta = \tau$. Equation (Ha.2) then simplifies to an expression for $\partial\zeta/\partial t$ which can be integrated with respect to t, yielding a first-order expression for ζ. Differentiating this expression with respect to τ, Hansen obtained an improved value of $\partial\zeta/\partial\tau$, which he substituted back into (Ha.2). The resulting expression when integrated with respect to t gave the second-order approximation to ζ. Higher-order approximations were obtained by repeating this process. At the end of each stage of approximation, Hansen replaced τ by t, and ζ by z. Thus in descriptions of Hansen's method the variable z is sometimes referred to as "the perturbed time", and nz as "the perturbed mean anomaly."

The foregoing sketch omits crucial detail, such as the steps required to determine the arbitrary constants introduced by the integrations, the processes for determining the radius vector as a function of ζ, and the procedure for finding the instantaneous plane in which the instantaneous ellipse is located. Among features distinguishing Hansen's development of the theory were his use of harmonic analysis (or "special values"), as advocated by Gauss, in determining the disturbing function, and his application of Bessel functions in the expansions. Like Damoiseau before him, he insisted on a *numerical* rather than a *literal* form for his theory, and introduced approximate numerical values for the orbital elements at an early stage, so as to avoid the problems of slow convergence of series encountered by Plana, and to make sure that all terms greater than an agreed-upon minimum would be included.

After completing his memoir on the mutual perturbations of Jupiter and Saturn (*Untersuchung über die gegenseitigen Störungen des Jupiters und Saturns*, Berlin, 1831), Hansen set out to apply his new method to the lunar problem. He described this application in his *Fundamenta nova investigationis orbitae verae quam luna perlustrat* (Gotha, 1838). Is the method really suitable to the lunar problem? Brouwer and Clemence in their *Methods of Celestial Mechanics* suggest that it is not. They give high marks to Hansen's method in its application to planetary perturbations, but they describe his adaptation of it to the lunar problem as a *tour de force*.[21] The method as set forth in the *Fundamenta* presents new complications, not easily susceptible of schematic description. We mention here only certain major new features. A full account is given by Ernest W. Brown in his *Introductory Treatise on the Lunar Theory*, Chapter X.

Hansen's earlier treatment of the latitudes had lacked rigor, while the lunar latitudes require an especially careful development. In the *Fundamenta* Hansen succeeded in deriving them as accurately as could be wished, taking account of the motions of the ecliptic as well as those of the instantaneous plane of the lunar orbit with respect to a fixed plane. Comparing the different derivations of the perturbations in latitude put forward by the celestial mechanicians of his day, the mathematician Richard Cayley found Hansen's alone to be strictly rigorous.[22]

A special difficulty in the lunar theory comes from the relatively large motions of the Moon's perigee and node in each lunar month, much larger proportionately than the motions of the perihelion and node of any planet during its sidereal period. In his theory of Jupiter and Saturn, Hansen had permitted terms proportional to the time (t) and its square (t^2) to be present, but in the lunar case such terms would quickly become embarrassingly large. To avoid them Hansen introduced a factor y, such that the mean rate of the perigee's advance is ny, where n is the mean rate of advance in longitude, and y is constant so long as only the perturbations due to the Sun are considered. He likewise used y in defining the mean rate of recession of the lunar node.

Another new feature in the *Fundamenta* was the introduction of a function W which, integrated twice, gave the perturbations in the instantaneous plane of the orbit. Initial values for the mean anomaly and radius vector were taken from an auxiliary ellipse of fixed eccentricity and unvarying transverse axis, the mean motion on it having a fixed rate n_0, and the perigee progressing at the steady rate $n_0 y$. The perturbed mean anomaly, nz, was obtained by the integration of W, and then substituted into the standard elliptical formulas to yield the true anomaly. To find the perturbed radius vector r, Hansen stipulated that $r = r_0(1 + \nu)$, where r_0 is the radius vector in the auxiliary ellipse, and ν is a small fraction which represents the perturbations and is obtained from the integration of W.

Hansen's lunar theory, Brown tells us, was "much the most difficult to understand of any of those given up to the present time [1896]." Presumably Hill, at an early stage in his studies, became acquainted with it, but there are no references to it in his writings of the 1870s. To Hansen's work on Jupiter and Saturn, on the contrary, Hill

[21] D. Brouwer and G.M. Clemence, *Methods of Celestial Mechanics* (New York: Academic, 1961), 335, 416.

[22] See R. Cayley, "A Memoir on the Problem of Disturbed Elliptic Motion," *Memoirs of the Royal Astronomical Society, 27* (1859), 1.