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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.

refers explicitly in an article of 1873 concerning a long-term inequality of Saturn; and a publication of 1874 shows his intensive study of Hansen's *Auseinandersetzung einer zweckmässigen Methode zur Berechnung der absoluten Störungen der kleinen Planeten*.[23] When in the decade 1882–1892 he developed the theory of Jupiter and Saturn, he chose to apply the method of the *Auseinandersetzung*, with the modification of taking the mean anomaly as independent variable, whereas Hansen had chosen the eccentric anomaly for this role.

Hill in an article of 1883 takes issue with Hansen's assertion that the long-period inequalities of the Moon due to planetary action are difficult to compute, and proposes an elegant method deriving from Cauchy.[24] Hill's memorandum regarding new tables of the Moon, which we have reproduced in an Appendix, makes evident Hill's strongly negative assessment of the future of Hansen's lunar theory. He saw no way in which, by various adjustments, it could be brought up to the standards of exactness and clarity he regarded as obligatory for the celestial mechanics of his day. He envisaged a theory transparent in the sense that each derived effect was clearly traceable back to the assumptions and numerical constants on which it depended. Hansen's theory could not be so described. When E.W. Brown's *An Introductory Treatise on the Lunar Theory* (Cambridge University Press, 1896) appeared, Hill wrote Brown to compliment him on the book, but, as Brown reports it, with one criticism:

> He thinks it would have been better to leave out Hansen – because he says 'it will probably never be used again'! Otherwise he is complimentary – but I don't think he appreciates what a student beginning the subject wants.[25]

Delaunay's lunar theory initially aroused Hill's enthusiastic allegiance. It had been published in two huge volumes in 1860 and 1867, and Hill had begun studying it early in the 1870s. This study influenced his interests and thinking pervasively, as articles published in *The Analyst* in 1874 and 1875 testify.[26] Delaunay had not given a derivation of the Hamiltonian-style canonical equations on which he based his theory, referring instead to a memoir by Binet published in 1841.[27] (Binet was the first to develop canonical equations in which the variables are the elliptical elements

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[23] The reference is given in note 8.

[24] G.W. Hill, "On certain possible abbreviations in the computation of the long-period inequalities of the Moon's motion due to the direct action of the planets," *American Journal of Mathematics, 6* (1883), 115–130.

[25] E.W. Brown to G.H. Darwin, 21 March 1896, CUL. MS. DAR.251:479.

[26] "Remarks on the Stability of Planetary Systems," *The Analyst*, *I* (1874), 53–60; "The Differential Equations of Dynamics," ibid., 200–203; "On the Development of the Perturbative Function in Periodic Series," *The Analyst*, *II* (1875), 161–180.

[27] M.J. Binet, "Mémoire sur la variation des constants arbitraires dans les formulas générales de la dynamique," *Journal de l'École Polytechnique*, Vingt-Huitième Cahier, *T.XVII* (1841), 1–94. Binet's work derives, not from Hamilton or Jacobi, but from Poisson (personal communication from Michiyo Nakane; see M. Nakane and C.G. Fraser, "The Early History of Hamilton-Jacobi Dynamics 1834–1837," *Centaurus, 44* (2002), 161–227.)

of motion of a planet or satellite; Delaunay used them with one change, indicated below.) In an article published in 1876, Hill derived Delaunay's equations, relying not on Lagrange's and Poisson's brackets, which, as he acknowledged, permitted the equations to be established in a very elegant manner, but "on more direct and elementary considerations."[28] He evidently saw his role here as that of presenting to American mathematicians a sophisticated development with which they were presumably unfamiliar. His first sentence conveys what he saw in it:

> The method of treating the lunar theory adopted by Delaunay is so elegant that it cannot fail to become in the future the classic method of treating all the problems of celestial mechanics.

The rudiments of Delaunay's method may be described as follows.[29] Let R be the disturbing function, and let the elements selected as canonical be: ℓ, the mean anomaly; g, the angle between the node on a fixed plane and the perigee; h, the angle between the node and a fixed line in the fixed plane; $L = \sqrt{(a\mu)}$, where a is the semi-major axis and μ is the sum of the masses of the Earth and the Moon; $G = L\sqrt{(1 - e^2)}$, where e is the eccentricity; and $H = G\cos i$, where i is the orbital inclination. The mean anomaly $\ell = nt + \varepsilon$ is an unexpected choice for an element since it is not a constant in the unperturbed elliptical orbit; Delaunay introduced it to replace one of Binet's elements (*viz.*, a factor entering into n), to avoid the emergence of terms proportional to t in the partial derivatives of R. Of Delaunay's set of elements, Hill remarks that "it does not appear that a better can be selected." For the disturbed ellipse Delaunay then obtained the canonical equations

$$\frac{dL}{dt} = \frac{\partial R}{\partial \ell}, \quad \frac{dG}{dt} = \frac{\partial R}{\partial g}, \quad \frac{dH}{dt} = \frac{\partial R}{\partial h},$$
$$\frac{d\ell}{dt} = -\frac{\partial R}{\partial L}, \quad \frac{dg}{dt} = -\frac{\partial R}{\partial G}, \quad \frac{dh}{dt} = -\frac{\partial R}{\partial H}. \tag{D.1}$$

Delaunay developed R as a series of cosines of multiples of the angles ℓ, g, h, and ℓ', where ℓ' is the mean anomaly of the Sun. If the unperturbed values of these variables are identified by the subscript "0," the resulting series may be written

$$R = F + \sum A \cos[i_1(nt + \ell_0) + i_2 g_0 + i_3 h_0 + i_4(n't + \ell')],$$

where i_1, i_2, i_3, i_4 are integers, and the summation extends to all sets of integers leading to detectable inequalities.

Delaunay's strategy in solving the equations was to separate R into two parts, R_1 and $R - R_1$, where R_1 is a single term in R, and solve the equations with R_1

[28] G.W. Hill, "Demonstration of the Differential Equations Employed by Delaunay in the Lunar Theory," *The Analyst*, *III* (1876), 655–670.

[29] Our account is based on Delaunay's earliest description: "Mémoire sur une nouvelle méthode pour la determination du movement de la lune," *Comptes rendus hebdomadaires des séances de l'Académie des Sciences, 22* (1846), 32–37.

substituted for R. The solution gave him new values of L, ℓ, G, g, H, h, which he substituted into $R - R_1$. Then he repeated the process, separating $R - R_1$ into two parts, R_2 and $R - R_1 - R_2$, and solving the equations with R_2 substituted for R. The process was to be repeated over and over, at each stage removing the largest remaining term in R, until all significant terms were removed. At each stage the literal expressions of L, ℓ, G, g, H, h approached more nearly to their final form.

According to Hill, Delaunay's procedure was based on the most advanced and elegant formulation of dynamics available, and it provided complete transparency in the relations between causes and derived effects. A distinct advantage was its enabling the calculator to focus on one term of the disturbing function at a time. Later, Hill would come to regard the large number of tedious transformations entailed by the method as a serious drawback.

At some point in the mid-1870s, Hill became aware of a more serious difficulty in the application of Delaunay's method to the Moon – a difficulty which, from the human standpoint, looked fatal. The series determining the coefficients of some perturbation terms converged so slowly that to obtain a result of the desired precision required a quite unreasonable expenditure of time and effort. It was the same difficulty that Plana and Pontécoulant had encountered, and that had led Hansen to choose a numerical form for his theory. Delaunay carried the development of his series to the eighth and sometimes to the ninth order of small quantities, still without attaining a final result of sufficient precision to match the precision of contemporary observations. Seeing the daunting number of further terms that would have to be calculated if he were to proceed to the next higher order, he introduced "probable complements", based on the rate of decrease of the last two or three terms calculated. Newcomb later found these complements, though in some cases roughly correct, quite illusory in others. Delaunay's method, after seeming to promise exact science, was here showing itself irremediably inexact.

For Hill, the recognition of this difficulty was a trumpet call. A new beginning was necessary. Hill opted for a radical departure from the tradition of past lunar theory.

All lunar theorists from Kepler to Delaunay, Euler alone excepted, had taken a solution of the *two-body* problem in Newtonian theory – a circular or elliptical orbit of the Moon about the Earth – as their starting-point, then superimposed on it periodic variations as required by theory or observation. John Couch Adams, in opening his lectures on lunar theory in the 1860s, called this procedure "the method of the Lunar Theory":

> The Earth and Moon describe orbits round the Sun which are approximately ellipses, and the Moon might be regarded as one of the planets; but this point of view would not be a simple one; the disturbing action of the Earth would be too great, though it is never so great as the direct attraction of the Sun, that is to say, never great enough to make the Moon's path convex to the Sun. The more convenient method is to refer the motion of the Moon to the Earth, and counting only the difference of the attractions of the Sun

upon the Earth and upon the Moon, to find how this distorts the otherwise elliptical relative orbit. This is the method of the Lunar Theory.[30]

In contrast, Hill will take as starting-point an oval orbit of the Moon about the Earth – a circle flattened toward the Sun by the difference between the Sun-induced, Sun-ward accelerations of the Moon and of the Earth; it is a periodic solution of a simplified version of the *three-body* problem. It was in fact the same curve that Newton had found as an effect of the Sun's action on the Moon. As previously noted, and unbeknownst to Hill, Newton in the 1680s had computed an ellipse which approximated this "Variation curve" with considerable accuracy. And J.A. Euler, son of Leonhard Euler, had calculated in 1766 the first two terms giving the Moon's motion on the Variation curve (Euler's coefficient for the second term is mistaken, owing to a simple numerical error).[31] Young Euler's article contains the statement, "I dare assert that if anyone succeeded in finding a perfect solution [to the problem of the Variation], he would scarcely find any further difficulty in determining the true motion of the real Moon." Leonhard Euler, the father, was likely the source of this claim, but its decisive substantiation would have to await the elaboration of the Hill–Brown lunar theory. We shall find Hill proceeding just as if he had read and accepted Euler's pronouncement (we have no evidence that he in fact had seen it).

In his final lunar theory, published in 1772, Leonhard Euler chose rotating rectangular coordinates, the x and y coordinates rotating in the plane of the ecliptic about the z axis with the mean angular speed of the Moon. His objective was to obtain series that converged rapidly. He separated the periodic developments of the lunar coordinates into classes according to the parameters on which they depended: K, the eccentricity of the lunar orbit; i, the inclination of the lunar orbit to the ecliptic; κ, the eccentricity of the solar orbit; a, the ratio of the Sun's parallax to the Moon's parallax; p, the difference between the mean motion of the Moon and the mean motion of the Sun, which Euler calls the "mean elongation;" q, the mean anomaly of the Moon; r, the mean argument of latitude; and t, the mean anomaly of the Sun. The stress on inequalities dependent on p does not appear to be present; p is simply one of eight parameters on which the Moon's motion depends. Euler was thus proposing to develop his mathematical theory systematically in terms of the successive powers and products – of one, two, three, and higher dimensions – of these small parameters. It was a new way of proceeding, which could guarantee the correctness of the theory to any pre-chosen level of precision.

Why did Euler (father and/or son) in the paper of 1766 claim that, given a perfect solution of the problem of the Variation, the further development of the lunar theory would be without difficulty? Euler does not say, but the following considerations were probably part of his thinking.

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[30] J.C. Adams, "Lectures on the Lunar Theory," in *The Scientific Papers of John Couch Adams*, II (Cambridge: Cambridge University Press, 1900), 6. The lectures were given with successive refinements from 1860 to 1889.

[31] J.A. Euler, "Réflexions sur la variation de la lune," *Histoire de l'Académie Royale des Sciences et Belles-Lettres*, Berlin, 1766, 334–353.

For earlier investigators, the Variation was a single term, a sinusoidal term discovered empirically by Tycho, with argument equal to twice the difference between the mean longitudes of the Moon and the Sun, i.e., $2(n - n')$. Euler's paper of 1766 derives not only this term but a second term, with double the argument of the first term, i.e., $4(n - n')$. Euler knew his solution to be an approximation at best; with more investment of labor, further terms could be derived. This discovery can have been a stepping-stone to Euler's project in the theory of 1772: to develop the entire lunar theory in terms of the powers and products of small parameters, by successive approximations.

But secondly, Euler may have come to see the Variation as more intrinsic to the lunar problem than any of the other inequalities found in the Moon's motion – inequalities dependent on eccentricity, inclination, or parallax. Let us imagine the eccentricities K and κ, the inclination i, and the ratio a of solar parallax to lunar parallax diminishing so as to become negligible or zero; a "Variation" would still be present in the Moon's motion, provided only that the Moon's mean motion n and the Sun's mean motion n' differed. Deriving the resulting motion of the Moon would be solving an essentially three-body problem. To cope with it, the analyst would no doubt proceed by successive approximations. The parameter in terms of which to develop these approximations could be $m = n'/n$ or $\mathbf{m} = n'/(n - n')$. While Newton was able to show by qualitative geometrical arguments that the Variation curve is some kind of oval, flattened along the line of syzygies, it is important to note that, except for successive approximations in terms of m or $\mathbf{m}$, no other avenue to learning the precise nature of the Variation orbit and motion was – or yet today is – known.

It will be worth our while to review certain general qualitative features of the Variation; see the figure below. The Moon moves about the Earth **E** in an orbit **abcd**, while the Earth-Moon system moves about the Sun **S**; we have exaggerated both the size of the orbit **abcd** relative to the distance **SE**, and the flattening of this orbit. The period of the Earth about the Sun, reckoned with respect to the stars, is 365.256 days. The period of the Moon about the Earth, reckoned again with respect to the stars, is 27.321 days. These two numbers, with their ratio, have been – at least until the introduction of atomic clocks in 1955 – as accurately known as any constants in all of astronomy.

Two further numbers are needed to determine the ratio of the forces of the Sun and Earth on the Moon. These two numbers are the Earth-Sun distance and the Moon-Earth distance. The mean ratio of these distances was already known in the

1760s to be about 380 or 390 to 1. J.A. Euler in his paper of 1766 assumed a solar parallax of 9″; this with the known lunar parallax of close to 57′.0 implies a ratio of 380:1. The senior Euler in his lunar theory of 1772 used the value 390:1 for this ratio. The accepted value today is about 389:1. These data, along with Proposition 4 of Book I of Newton's *Principia*, yield a value for the ratio of the Earth's force on the Moon to the Sun's force on the Moon. With Newton's value for the solar parallax, 10.5 arcseconds (corresponding to an Earth-Sun distance of 19,644 Earth radii), the Sun's force on the Moon comes out to be 1.8 times the Earth's force on the Moon. With Euler's value of 1/390 for the ratio of parallaxes, the Sun's force on the Moon is found to be 2.18 times the Earth's force.

Since the two forces act constantly, the Moon's path must at each instant be curved concavely toward *both* the Sun and the Earth. To understand how this can be, consider the Moon moving from **a**, where it is a new Moon, to **b**, where it is at the first quarter. Its path **ab** is shown in the preceding figure as convex toward the Sun, but this is an illusion due to the diagram's failing to incorporate time and motion. The Moon requires 7.4 days to move from **a** to **b**, an arc which at the Sun subtends an angle of 8.8 arcminutes, or less than one-sixth of a degree. But in 7.4 days the whole Earth-Moon system moves through 7°.293 about the Sun. The relatively tiny motion that takes the Moon around the Earth is dwarfed with respect to the larger sweep that takes the Earth-Moon system about the Sun. This larger sweep moves the Moon in an arc always concave toward the Sun, while the Moon creeps round the arc **ab** which, reckoned in the moving space with Earth at its origin, is always concave toward the Earth. Since the curvatures are inversely as the radii, the Moon's orbit about the Earth has a curvature 389 times the curvature of the Moon's path about the Sun. The curvatures are directly as the accelerative forces, but inversely as the 3/2 powers of the linear velocities. Given that the accelerative force of the Sun on the Moon is 2.18 times the accelerative force of the Earth on the Moon, the much larger curvature of the Moon's path about the Earth compared to the curvature of its path about the Sun is due to the much smaller linear velocity of the Moon's motion about the Earth – only about 1/90th of its velocity about the Sun.

The Variation, more than the other parametric dependencies of the Moon's motion considered by Euler, must have led him to ponder more deeply the dynamic complexities presented by our Moon's motion. The curve the Moon follows in space is fully determinate, yet its essence, its mathematical formula, its exact individuality, is unknown, except the parameters governing it be extracted by successive approximations, step by step. Newton approximated the Variation curve with an ellipse, but it is not an ellipse or any other curve with a finitely expressible formula. In this respect the Variation resembles the lunar theory as a whole; the exact character of the motion is hidden in the dynamics. These realizations must have led Euler to propose that, of all the problems in the Moon's motion, the problem of the Variation should be tackled first, and independently of the other lunar inequalities.

Hill's acquaintance with Euler's theory came about in his undergraduate study at Rutgers in 1855–1859 under Theodore Strong, professor of mathematics. Strong, Hill later recalled, was old-fashioned, and liked to go back to Euler for all his theorems, asserting that "Euler is our Great Master."[32] Hill, in the introduction to his paper of 1878, explicitly cites Euler's lunar theory of 1772 as providing the model for his own partition of the inequalities into classes. Hill's papers and Euler's lunar theory also agreed in using rotating rectangular coordinates, but for Hill the coordinates rotated with the mean speed of the Sun, not the Moon.

The Eulerian roots of Hill's new theory are important. Without Hill's having previously become acquainted with Euler's theory of 1772, he might never have thought of developing the lunar theory along Eulerian lines.

Also important, however, were the respects in which Hill went beyond Euler. First, he had studied the methods of Hansen and Delaunay. Hansen's *Untersuchung* showed how all terms with coefficients greater than a pre-specified lower bound could be obtained – a kind of result that no earlier mathematical astronomer had achieved. Delaunay's completely literal lunar theory permitted each perturbational term to be traced back to the assumptions on which it was based. Hill undoubtedly saw the exactitude and transparency thus illustrated as standards that a new theory ought to meet.

Crucial to Hill's new solution of the lunar problem was the Jacobian integral, an integral of the equations of motion for a restricted form of the three-body problem. Nothing similar was available to Euler, who had long struggled to integrate the equations of the general three-body problem, and had at last given up the attempt. In the lunar case he made no use of general integrals, such as those for *vis viva* and angular momentum. Knowing in advance that the Moon's position depended on certain parameters, he formed differential equations each of which contained trigonometric terms deriving from just one of these parameters or the product of two or more, and solved the equations one after another by the method of undetermined coefficients. He did not attempt to calculate the motions of the apsidal and nodal lines, but used the values for these constants that Mayer had derived from observation. Other constants besides those introduced by integration, he suggested, might have to be evaluated observationally. His primary aim was to achieve a precision of one minute of arc, matching the precision of the available observations.

In contrast, Hill's solution will be controlled by the *vis viva* integral due to C.G.J. Jacobi and first published in the *Comptes rendus* of the Paris Academy in 1836.[33] According to Jacobi in his *Vorlesungen über Dynamik*, Euler had regarded the *vis viva* integral as valid only about a *fixed* center of attraction, whereas the Jacobian integral was here applied to a *moving* center; Jacobi credits Lagrange with the extension to moving centers.[34] For Hill, the Jacobian integral did yeoman service in determining the properties of the motion. It enabled him, for instance, to obtain

[32] See E. Hogan, "Theodore Strong and Ante-bellum American Mathematics," *Historia Mathematica*, 8 (1981), 435–455.

[33] C.G.J. Jacobi, *Comptes rendus de l'Académie des Sciences de Paris*, III, 5961; reprinted in C.G.J. Jacobi's *Gesammelte Werke*, IV (ed. K. Weierstrasse: Berlin: Reimer 1886), 35–38.

[34] C.G.J. Jacobi, Vorlesungen über Dynamik, in *Gesammelte Werke, Supplementband* (ed. A. Clebsch Berlin: Reimer, 1884), 10. For a detailed account of Jacobi's likely path in deriving his integral, see pp. 195–201 of M. Nakane and C.G. Fraser, "The Early History of Hamilton-Jacobi Dynamics 1834–1837," *Centaurus, 44* (2002), 161–227. The

the constants of the Variation orbit in *literal* form, as series in the constant **m**. By its means he obtained the terms of the Variation in longitude and radius vector with a precision far greater than ever before achieved.

Another important feature of Hill's treatment of the lunar problem was his use of the imaginary exponential as it relates to the cosine and sine:

$$e^{\pm(\sqrt{-1})\theta} = \cos\theta \pm \sqrt{-1}\sin\theta.$$

This relation had been used by d'Alembert in his lunar theory of 1754, but had not been employed by later celestial mechanicians until Cauchy started promoting it in the 1840s. The expression of cosines and sines of angles by the imaginary exponential is particularly useful when infinite series are to be multiplied. Hill's theory relied heavily on such multiplications. The expression of trigonometrical series by imaginary exponentials reduced the multiplications to a simple addition of exponents.

Hill's first use of this device was in his paper "On the Development of the Perturbative Function in Periodic Series," published in *The Analyst* in 1875.[35] This paper makes reference to a memoir of 1860 by Puiseux, also dealing with the development of the perturbing function.[36] Puiseux advocated use of the imaginary exponential with the mean anomaly or its multiples as argument:

> The consideration of this new variable allows us not only to assign the limits within which the coordinates remain convergent, but, as M. Cauchy has remarked, to calculate without difficulty the general terms of these developments. Moreover, the same method applied to the perturbing function furnishes the general term of this function developed according to the sines and cosines of multiple arcs of the mean anomalies of the two planets. The coefficients of the sine and cosine of a given argument are thus obtained directly in the form of series proceeding according to the integral powers of the two eccentricities, of the sine of the mutual half-inclination of the orbits, and of the ratio of the major axes – that is, under the most appropriate form for use in celestial mechanics.[37]

Puiseux is here following in the footsteps of A.-L. Cauchy, who in the Paris Academy *Comptes rendus* of the 1840s wrote frequently on ways to make rigorous and to streamline celestial mechanics. Puiseux refers in particular to Cauchy's report, in the *Comptes rendus* for 1845,[38] for a commission reviewing a memoir by Le Verrier on an inequality in the mean motion of the minor planet Pallas. The minor

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reconstructed derivation involves a time-dependent potential and thus a non-conservative dynamical system.

35 *The Analyst*, *II*, 161–180; *Collected Mathematical Works of G.W. Hill*, *I*, 206–226.

36 Puiseux, "Mémoire sur le développement en séries des coordonnées des planets et de la fonction perturbatrice," *Journal de mathématiques pures et appliqués*, Deuxième Série, *V* (1860), 65–102, 105–120.

37 Ibid., 65.

38 XX, 767–786.

planets so far discovered – there were just four of them – had all proved troublesome: orbital elements calculated from 1 year's observations disagreed with the next year's observations, and so it was unclear how to proceed in determining perturbations. Le Verrier had found that 7 times the mean motion of Pallas minus 18 times the mean motion of Jupiter was a very small angle (*viz.*, $27'11''$); an inequality with a period of 83 years would result, but being of the eleventh order in the eccentricities and inclinations, the question was whether it was in fact detectable. Only a detailed computation could decide the matter. The available methods for computing it stemmed essentially from Laplace, and were exceedingly laborious. Le Verrier carried out this computation, and found the maximum value of the inequality to be $14'55''$, and the phase difference from the mean anomaly, $-29°7'$. The commission desired to check Le Verrier's result without having to repeat his long calculation.

Cauchy had already shown how to do this: derive a *general* term of the perturbing function algebraically, then substitute into it the numbers appropriate to the inequality in question. No one earlier had carried out such a procedure. Applying it to Le Verrier's inequality, Cauchy first obtained a maximum of $15'6.6''$ and a phase difference of $-29°3'55''$, and then by a slightly different calculative route a maximum of $15'6.3''$ and a phase difference of $-29°3'25''$. The results agreed closely with each other and differed but slightly from Le Verrier's result; the difference, according to Cauchy, was of the order of the error arising from Le Verrier's use of 7-place logarithms.

Since Hill gives us no specific references, we do not know which of Cauchy's writings he read. He was clearly aware of Cauchy's insistence on quantifying the error committed in breaking off an infinite series at any particular point. Hill in his paper of 1878 stated:

> I regret that, on account of the difficulty of the subject and the length of the investigation it seems to require, I have been obliged to pass over the important questions of the limits between which the series are convergent, and of the determination of superior limits to the errors committed in stopping short at definite points. There cannot be a reasonable doubt that, in all cases, where we are compelled to employ infinite series in the solution of a problem, analysis is capable of being perfected to the point of showing us within what limits our solution is legitimate, and also of giving us a limit which its error cannot surpass. When the coordinates are developed in ascending powers of the time, or in ascending powers of a parameter attached as a multiplier to the disturbing forces, certain investigations of Cauchy afford us the means of replying to these questions. But when, for powers of the time, are substituted circular functions of it, and the coefficients of these are expanded in powers and products of certain parameters produced from the combination of the masses with certain of the arbitrary constants introduced by integration, it does not appear that anything in the writings of Cauchy will help us to the conditions of convergence.[39]

[39] *The Collected Mathematical Works of George William Hill*, *I*, 287.

Thus Hill recognized the legitimacy of Cauchy's demand for tests of convergence, and though he was unable to give error-terms for the series he used, he demonstrated, as we shall see, that the apparent convergence of these series was exceedingly rapid. Both in his use of the imaginary exponential and in his concern with convergence, we must recognize the influence of Cauchy.

When Hill was first appointed to the Nautical Almanac Office in 1861, he spent a year or two in Cambridge, Massachusetts[40]; the office was located there from its inception in 1849 till 1866, primarily in order to benefit from the guidance of Benjamin Peirce, professor of mathematics at Harvard. Peirce had introduced Cauchy's work, including the *Cours d'analyse* of 1821, into the Harvard curriculum.[41] It can have been during Hill's time in Cambridge that he gained some acquaintance with Cauchy's writings. But he soon obtained permission to do his work at the family farm in West Nyack, and we do not know what works he had in his library there.[42]

In his paper of 1878, Hill solved his differential equations while leaving out of account the lunar orbit's eccentricity and its inclination to the ecliptic; he thus obtained a periodic orbit. His paper of 1877, proceeding from that same periodic orbit, introduced eccentricity into the problem, and set out to solve the differential equations that thus resulted. In this way he arrived at an infinite determinant, a kind of problem he was the first to confront. In the course of solving it he made crucial use of a summation which may be written as

$$\sum_{i=-\infty}^{+\infty} \frac{1}{\theta \pm i} = \pi \cot \pi\theta,$$

where θ is a constant. This formula, according to Hill, was "well known". It had first been derived by Euler, with the daring manipulative virtuosity for which he is famous, in a paper published in 1743[43]; and it is also given in Euler's *Introductio in analysin infinitorum*, I.[44] It can be derived more soberly in accordance with Cauchy's theory of residues, and is so derived in *Théorie des fonctions doublement périodiques* by Briot and Bouquet, published in 1859[45]; this book was a standard text for complex

40 R.C. Archibald, *A Semicentennial History of the American Mathematical Society, 1888–1938* (New York: American Mathematical Society, 1938), 117.

41 K.H. Parshall and D.E. Rowe, *The Emergence of the American Mathematical Research Community, 1876–1900: J.J. Sylvester, Felix Klein, and E.H. Moore* (Providence, RI American Mathematical Society, 1994), 18.

42 Hill bequeathed his library to Columbia University, according to his will, dated 15 April 1897, and published in the *Columbiana* at that time. But a list of the books thus donated to Columbia does not appear to have survived.

43 *Leonhardi Euleri Opera Omnia*, *I.17*, 15.

44 Ibid., I.8, 191. See also J.A. Euler, *Introduction to Analysis of the Infinite*, I (tr. John D. Blanton: New York, Springer-Verlag, 1988), 149.

45 See C. Briot and C. Bouquet, *Théorie des fonctions doublement périodiques* (Paris: Mallet-Bachelier, 1859), 126.

function theory in the late nineteenth century, and Hill may have met with his "well known" formula there.

In his founding of a new and more exact lunar theory, Hill was powerfully assisted by what he had learned from his study of Euler's writings during his college days and by his later solitary study of the mathematical literature of his own day. His construction of the new lunar theory was also solitary. Among mathematical astronomers in America, his preparation was altogether unique. Without that preparation, it is hard to see how his two seminal papers of 1877 and 1878 could have come to be.

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