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BY

FRANK SCHLESINGER and DIRK BROUWER

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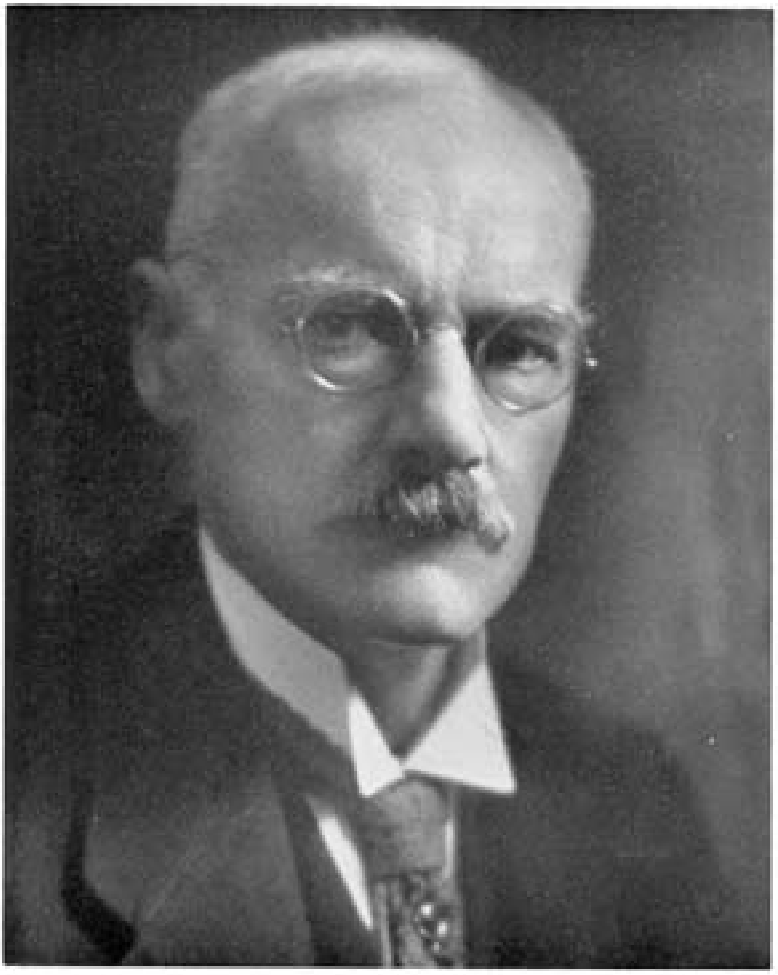

Ernest W. Brown

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Ernest William Brown's forbears on both sides lived at Hull, England, or in its immediate neighborhood. His father's father, William Brown (born 1806) was in early life a sailor, later a ship owner and a ship broker. His father (1837-1893), also named William, was for part of his life a farmer and later a lumber merchant. In 1863 he married Emma Martin (1839-1870), by whom he had four children, two boys and two girls. Of these Ernest (1866 November 29 to 1938 July 22) was the second oldest. In 1870 a scarlet fever epidemic carried off his mother and his younger brother. Ernest was not quite four years old at this time and he and his two sisters were looked after by a maiden aunt for about five years, when his father married again.

When Ernest was six years old he began to attend a day school in Hull. The master was at once impressed by his talent for music and urged his father to let the boy prepare for a musical career. This plan seems to have been given serious consideration; and later Ernest, and his elder sister Ella, studied the piano under the guidance of their step-mother. But later his tastes turned toward mathematics in which he greatly excelled both at the day school and at the Hull and East Riding College. Upon graduation from the latter institution he won a scholarship at Christ's College, Cambridge, tenable for three years.

This was in 1884. Then, as now and in all the intervening years, Cambridge was a great center for mathematics and the mathematical sciences; and then, as now, there were gathered together a company of mathematicians who have left a deep impression upon the science of the day. Of this company John Couch Adams (1819-1892) was the one whose work bore closest relationship to Brown's future research. One would have thought that it was he to whom Brown would be most deeply indebted for inspiration and guidance. But as a matter of fact they saw very little of each other, and whatever benefit Brown may have derived from Adams was chiefly through a perusal of his published work. It was George Howard Darwin (1845-1912), Plumian Professor of Astronomy and Experimental Philosophy, who was to prove his mentor. Darwin early recognized Brown's ability and admitted him into an intimate friendship that was to last till his death in 1912.

Brown was graduated B.A. in 1887 as sixth wrangler. This ranking was a surprise and a disappointment to his friends at Cambridge who were confidently expecting him to gain the senior wranglership. But Brown had no excuses to make and looking back, after the lapse of years, modestly said that his was a case of late development. He remained at Cambridge for three more years as a Fellow of Christ's College, receiving his master's degree in 1891. Then as now Cambridge excelled in mathematical training without paying too much attention to what application the student proposed to make of his mathematics; and it is interesting to know that Brown very narrowly missed going into meteorology, and, on another occasion, into practical astronomy. His definite turn to mathematical astronomy as a career came in 1891 when he was offered a place as instructor in mathematics at Haverford College near Philadelphia, a post that carried with it the directorship of the observatory. He was soon (1893) promoted to a full professorship. Here he remained until 1907 when he was called to Yale University.

While he was still at Cambridge, Darwin had called his attention to the great memoir on the lunar theory by George William Hill (1838-1914), published in 1877, with the advice that the memoir was well worthy of careful study. By the time that Brown came to America he had made up his mind to undertake the formidable task of computing the moon's orbit on this theory. In an address in 1914 he said: "My own theory, which was completed a few years ago, is rather a fulfillment to the utmost of the ideas of others than a new mode of finding the moon's motion. Its object was severely practical—to find in the most accurate way and by the shortest path the complete effect of the law of gravitation applied to the moon. It is a development of Hill's classic memoir of 1877." It does not give the emphasis that it should to his own resourcefulness in finding the most accurate solution by the shortest path. Hill's theory left plenty of room for the exercise of such resourcefulness before it could be numerically applied. In a series of publications beginning in 1891 Brown presented theoretical developments and their numerical application to the coefficients of certain classes of inequalities in the motion of the moon.

Brown's work on the theory of the moon's motion may be divided into two separate chapters: (1) the solution of the main problem, that is the motion of the moon under the attraction by the earth and the sun only, all three bodies being treated as spherical and the center of mass of the earth-moon system being supposed to move in an elliptic orbit round the sun. (2) The evaluation of the effects upon the moon's motion due to gravitational causes that were ignored in the solution of the main problem. These are mainly the direct and indirect attractions of the planets, and the deviations from mechanical sphericity of the earth and the moon.

There is nothing haphazard about his execution of this task. Each new phase of the work was preceded by careful preparatory studies. Once these had led him to adopt a definite plan, he would begin the systematic work, and let nothing interfere with its progress. This careful planning explains the absence of serious delays by unforeseen difficulties. It also accounts for the long period that preceded the systematic development of the theory which did not begin until 1894, when he completed the plan for the solution of the main problem (8).* At that time he had become so familiar with the entire subject that writing his "Introductory Treatise on the Lunar Theory" (11), still the standard text-book on this subject, hardly retarded the progress of his own theory.

The "Theory of the Motion of the Moon" was published in five parts in the Memoirs of the Royal Astronomical Society, 1897-1908, (19). The fourth part, 1905, concluded with a summary of the solution of the "main problem." Both as to completeness and accuracy this solution surpassed the work of Brown's predecessors to a remarkable degree. Few terms having coefficients in longitude and latitude exceeding ″.001

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* These numbers refer to the bibliography that follows.

were not included, and in the great majority of terms the uncertainty did not exceed ″.001. In Hansen's theory some coefficients were in error by some tenths of a second of arc; Delaunay's theory, on account of the slow convergence peculiar to his development, contained a few terms that were in error by as much as a whole second of arc.

The accuracy was confirmed by the preliminary results of the numerical verification of this part of the lunar theory that was carried out by his former pupil, Dr. W. J. Eckert, during the last few years of Brown's life.

The remaining parts of the lunar theory, and, more especially, the planetary perturbations in the moon's motion are among the most difficult subjects in celestial mechanics. Brown's most original work was in this field. One phase of this subject is the theory of the secular accelerations in the moon's motion produced by the secular diminution of the eccentricity of the earth's orbit. For over a century it had been among the most laborious and controversial parts of the lunar theory. Newcomb, in 1895, derived a remarkable theorem that rendered the derivation a relatively simple matter. But Newcomb used Delaunay's developments exclusively; the slow convergence produced an uncertainty that amounted to about five percent of the secular acceleration in the moon's mean longitude, exclusive of uncertainties present due to imperfect knowledge of the planetary masses. Brown succeeded in deriving a new theorem, related to Newcomb's, that enabled him to abbreviate the necessary computations, and to reduce the uncertainty of the result in the case of the mean longitude to only one-third of one percent. In order to accomplish this, judicious use was made of both Delaunay's expressions and the results obtained in his own theory. This derivation (15, 16) was one of Brown's favorite contributions to celestial mechanics.

When Brown began his work on the lunar theory it was known by Newcomb's researches that large unexplained differences existed between Hansen's theory and the moon's observed motion. The question whether these differences could be ascribed to imperfections of the gravitational theory thus became one of the most urgent problems in gravitational astronomy. Its solution required a reliable determination of the planetary perturbations in the moon's motion. This work was done independently by Radau (1835-1911), Newcomb (1835-1909), and Brown. Of these determinations Brown's was the most complete; moreover, his comparison (46) of the three results left very few discrepancies unexplained.

The direct planetary perturbations were published separately as an essay which obtained the Adams Prize in the University of Cambridge for the year 1907 (44). The fifth and last part of the lunar theory appeared a year later (19).

The successful completion of the theory was merely a milestone in Brown's work on the moon's motion. It was followed immediately by the construction of new lunar tables and a comparison of observations with the new theory. The latter had to be carried out simultaneously with the construction of the tables in order to secure the best possible constants for the final tabulations.

The construction of the tables presented numerous problems totally different from those present in the development of the mathematical theory. Brown's theory, because it was so much more complete than Hansen's, required the tabulation of over three times the number of terms included in Hansen's tables. In order not to triple the work of the ephemeris computer, improved methods of tabulation were required.

With the very efficient aid of Dr. H. B. Hedrick (1865-1936), who was employed as chief computer for nine years, Brown proved himself equal to this task. Hedrick had been connected with the Nautical Almanac Office in Washington for twenty-four years, and possessed exceptional qualities as a computer in the highest sense. In after years Brown grasped every opportunity to emphasize the importance of Hedrick's share in the construction of the lunar tables.

The most ingenious new feature of the tables is the arrangement of the single-entry tables. They occupy less space than in Hansen's case, and are more convenient to use. This is particularly due to their being completely re-entrant; that is, after the value of the argument of each table has been found for a certain instant, the tabular values for succeeding half-day intervals for a whole year can be found without recomputation of the argument or change of the interpolating factor.

The construction of the tables and their publication were financed by the Lunar Tables Fund provided by Yale University; it involved an expenditure of some thirty-four thousand dollars. The three volumes of the "Tables of the Motion of the Moon" (95) were published by the Yale University Press in 1919. They had been printed at the Cambridge University Press. The 660 pages of tables and explanations for their use set a standard of perfection for works of this nature that will not easily be surpassed.

The numerical values for the constants used in the tables had been obtained in a series of contributions (63, 67, 68, 69, 71, 74, 75) from a comparison with the Greenwich observations in the years 1750 to 1900. This mass of some 20,000 observations had been prepared and analyzed by Cowell, a continuation and extension of an undertaking started by Airy (1801-92). The accuracy of the constants is, therefore, primarily due to Cowell's admirable analysis. An interesting confirmation was obtained in 1932 by Spencer Jones who, in a revision of Newcomb's occultation work, made an independent determination of the constants from totally different material, and derived results that are in excellent agreement with Brown's values.

With a few specific exceptions the tables represent the moon's motion according to gravitational theory with the best available constants. These exceptions are: (1) The motions of the perigee and node as used in the tables are the observed values rather than the theoretical ones. The main disagreement was in the motion of the perigee. Shortly before his death Brown showed (190, 192) that this was caused by the omission of certain terms of high order in the theory. These additional terms were then found to bring the theoretical value close to the observed motion. Further clarification of this point, that has been the subject of so many discussions, is expected to be one of the most interesting results of Eckert's numerical verification now in progress at Columbia University. (2) An empirical term with coefficient 10″.71 and period 257 years was included in order to eliminate the major part of the fluctuations in the moon's mean longitude during the past three centuries. (3) The oblateness of the earth's figure as used in the tables is 1/294, the value adopted by geodesists being 1/297. (4) The quantities depending upon the moon's figure are tentatively assumed values that must eventually be improved after a more complete knowledge of the moon's physical librations has been gained. (5) The secular acceleration in the moon's mean longitude as used in the tables is the theoretical value. It is known that this amount should be increased on account of the tidal retardation of the earth's rate of rotation. When the tables were constructed this subject was in such a state of flux that Brown preferred to omit this addition. The theoretical evaluation of tidal friction in shallow seas by Taylor and Jeffreys and also the definitive evaluation by Fotheringham (1874-1936) of the secular accelerations of the moon and the sun from a discussion of ancient eclipses, occultations, and equinox observations all appeared shortly before or shortly after the publication of the lunar tables.

For ordinary purposes of computation of the moon's position these details are of little or no consequence. It is only in refined special investigations that they become important. As a whole, after a lapse of more than twenty years, we conclude that Brown used excellent judgment in deciding upon these matters.

The new tables have been used for the calculation of the moon's place in most national ephemerides since 1923. The great improvement over Hansen, especially in the short-period terms, is immediately apparent from the small range of the residuals from observations in any one year.

That the new theory did not account for the large fluctuations in the moon's mean longitude must have been a great disappointment to Brown, but this negative result stimulated him to further research. His first publication on the subject (52) was in 1910; he continued to deal with it in occasional publications (63, 66, 73, 96, 113) until at last, in 1926, he published his well-known paper "The Evidence for Changes in the Rate of Rotation of the Earth and their Geophysical Consequences . . ." (120). In this publication he accepted the explanation that the fluctuations in the moon's mean longitude including the great empirical term, were no real deviations from its gravitational path, but caused by irregular variations in the earth's rate of rotation.

Before taking this stand he had examined and rejected the theoretical possibility of numerous other hypothetical explanations. Any possible cause, even if it did not appear to hold out much promise, was submitted to a searching test. Some of these hypotheses were his own, some were suggested by other astronomers. Most frequently mentioned were the following: perturbations produced by an assumed equatorial ellipticity of the sun; magnetic attraction between the earth and the moon, and between the sun and these two bodies; a resonance effect produced by the physical librations in the moon's rotation; deviations from the Newtonian law of gravitation; perturbations by intramercurial planets; the shading of gravitation by interposing matter; perturbations by a massive meteor swarm. For a long time he considered with some favor the hypothesis of magnetic attraction between the earth and the moon, and possibly the sun.

Looking backward it may seem surprising that he did not accept the variability of the earth's rotation earlier. Since the explanation requires that other bodies in the solar system exhibit fluctuations similar to those in the moon's mean longitude but with smaller amplitude, the residuals in the longitudes of these other bodies provide the essential test. Newcomb had found some similarity with residuals obtained from transits of Mercury, but not enough to be quite convinced of its reality. In 1914 Brown exhibited the similarity with the curves obtained from the residuals in the longitudes of the sun and of Mercury; Glauert added Venus, and Ross added Mars. Brown discussed the subject again in 1920 (96). While mentioning all this evidence in favor of the explanation, he rejected it because "causes requisite to produce the amount required seem to be absent." This statement throws light upon his method of attack. In dealing with this subject, as well as on many other occasions, he showed his strong preference for emphasizing the mechanical side of a problem, instead of arriving at its solution by a thorough discussion of observational data.

The mechanical explanation of changes in the rate of rotation presents serious difficulties. It requires oscillatory changes in the earth's moment of inertia about its axis of rotation. If the entire mass expands and contracts uniformly the maximum change in the radius required is five inches below or above its mean value; if it is a crustal phenomenon, say to a depth of

80 kilometers, the changes in radius required would be thirty times as great, or about twelve feet below or above its mean value. No physical or chemical causes for such changes are known.

In a final review of the subject (194) Brown stated: "My own idea is to imagine the existence of a layer of material not too far from the surface which is at or near a critical temperature, the latter being defined as one in which a small change of temperature produces a relatively large change of volume. . . ." He then suggested that the hypothesis be tested by a device which could measure very small changes in the opening of a fissure. If a number of them were placed in various parts of the earth, especially in those regions where mountain building is known to be going on, the information needed could be obtained when the next great change in the earth's rate of rotation occurs. The formulation of this *ad hoc* hypothesis is again an illustration of his desire to penetrate to the mechanical cause of the phenomenon.

The immediate reason why Brown took up the subject in 1926 was the appearance, a year earlier, of a paper by Innes (1861-1933) in which he concluded, mainly from a study of transits of Mercury: "When allowance is made for the variability of the rotation of the earth, the moon's motion will probably be found to be purely gravitational. The inclusion of empirical terms confuses." Brown's own presentation contained a very full discussion of the entire subject, but the observational evidence was limited to a qualitative demonstration of the general similarity between the curves obtained from the residuals in the sun's longitude and the total fluctuation in the moon's mean longitude. One of the important results of his publication was that it brought to an end the unfortunate distinction between major and minor fluctuations that had confused the issue for many years. The eagerness with which the astronomical world received his contribution was a striking demonstration of Brown's authority. The fact that he had accepted the variability of the earth's rotation immediately raised the subject from the level of doubtful conjecture to one of full scientific standing. Numerous further discussions by other astronomers appeared in rapid succession. Outstanding among these were the discussions by de Sitter (1872-1934) and by Spencer Jones whose thorough analyses of all relevant observational data essentially confirmed Brown's conclusion.

From 1926 on he gave much attention to comparisons of observations of the moon with the tables. This led to his occultation campaign, supported by many astronomers all over the world.

In the course of his long career he found several opportunities to apply his knowledge of the moon's motion to the study of related problems. After a discussion with Jackson, who had developed a plan to apply Delaunay's theory to the motion of the eighth satellite of Jupiter, Brown took up this problem (102). He overcame the extremely slow convergence by ingenious extrapolations, but was apparently not satisfied with the results. Soon afterwards he began an entirely numerical development of the theory by a totally different method (155, 187).

During the last three years of his life the main problem of the lunar theory again held his almost exclusive attention. In this period he treated the stellar case of the problem of three bodies (188, 189, 190, 191), and followed with the greatest interest Eckert's numerical verification of the solar perturbations in the moon's motion, undertaken at Brown's suggestion. The presentation of the particular form of the equations of motion used in this undertaking is contained in his last publication (195).

Next to the lunar theory he was attracted primarily to the problems of planetary motion. Until 1908, when he finished the lunar theory, his explorations in this field had always been closely connected with the study of the moon's motion. From then on they became independent. A theoretical study of the motion of bodies near the Lagrangian triangular points (54, 55) was later followed by a general theory applicable to all planets of the Trojan Group, and particularly to orbits with large amplitudes of libration (103, 104). Soon after his earlier work on this problem he began a more general study of resonance in planetary motion (58, 61, 105, 106). This problem, related to that of the gaps in the ring of asteroids, had been treated on numerous previous occasions by other mathematical astronomers. Brown's contributions had many original features;

he was probably the first to see clearly that "the calculus of probabilities is more likely to lead to further information than the logical processes of analysis" (137).

The more abstract phases of celestial mechanics could hold his attention for limited times only. Gradually he became more particularly interested in the study of practical methods for planetary theories. In this connection he dealt extensively with the development of the disturbing function (132, 133, 145, 146, 147, 148, 153, 167). This led eventually to the construction with Brouwer of the "Tables for the Development of the Disturbing Function" (172).

He concentrated upon two different forms of planetary theories: the variation of arbitrary constants, and a method in which a modified true orbital longitude is used as independent variable (143). In the former he limited himself mainly to the indication of abbreviated methods and to the computation of the most important terms of higher order due to the presence of small divisors. His true-longitude method is a modification of Laplace's method. It is not an easy matter to judge its value. Brown used it himself in its original form in the theory of the Trojan Group because it offered some advantages in obtaining the intermediate orbit. Later he decided that for the Trojan Group the method of the variation of arbitrary constants is superior. He used it again in the theory of the eighth satellite of Jupiter. In the introduction to the second part of this theory he wrote: "The method has given rise to complications which make the developments of the terms of higher order difficult to follow, requiring great care if errors are to be avoided." A complete application to an ordinary planetary theory has not yet been made.

In the treatise "Planetary Theory" (178), written in collaboration with Dr. C. A. Shook, a coherent presentation is given of most of his contributions to celestial mechanics that are not related to the lunar theory. If one leaves aside the subject of resonance in planetary motion, which has more theoretical than practical aspects, the general impression of all these contributions is one of great unity of point of view. He aimed at the development and improvement of methods for the numerical calculation of general orbits of planets with an accuracy comparable with that of modern observations. He stressed that for any particular problem a method should be chosen on account of its efficiency, and that no standard method can be made to fit the needs of every special case. His adaptation and critical use of harmonic analysis to problems arising in this field goes well beyond what had been attempted by others before him. He succeeded in demonstrating that a judicious use of numerical methods permitted their successful application to notoriously difficult problems in planetary motion.

Through most of his life he was working on extensive programs that required years of continued effort, but he would occasionally devote himself to the solution of some isolated problems. The great majority of these are of a gravitational type. To this group belong his contributions to the subject of tides and harmonic analysis of tidal observations (23, 60, 77, 114). His most important application in this field was the analysis of records of Shortt Clocks (158, 159) obtained in the laboratory of A. L. Loomis. In this analysis the minute lunar tidal effect upon the pendulum, and consequently upon the rate, was shown to be present. Another striking gravitational study was his critical discussion of the discovery of unknown masses in the solar system from their gravitational effect on other bodies (154, 160), written after the discovery of Pluto in 1930.

He was less successful with studies suggested by astrophysical problems. He attempted to explain the spiral structure of extragalactic nebulae by a gravitational theory (112, 134), but did not succeed in producing more than an interesting analysis of an improbable model. His method of attack required the postulation of a definite hypothetical model. Given such a problem, reduced to a complicated set of equations, Brown could remove its unessential features and penetrate to the heart of the problem with rare ability and certainty. But not all problems presented by nature can be treated in this manner.

In this connection mention should be made of his attempt in 1900 to explain the sun-spot cycle by assuming it to be caused by a tidal disturbance produced primarily by Jupiter and Saturn (24). The treatment of this problem was somewhat arbitrary, and in later years he rather regretted having written the paper.

The work of any scientist can be judged more accurately if it is placed in relation to that of his contemporaries in the same field. In Brown's case we have to reach back to an older group in order to find men of comparable stature, namely, Newcomb (1835-1909), Hill (1838-1914), and Poincaré (1854-1912). These four men and their work show as striking differences as are likely to be found among the great men of any period. In the following lines some of these outstanding differences are indicated.

To Poincaré, the pure mathematician, it was incidental that celestial mechanics had an important physical application to the motions of the bodies in the solar system. He was attracted by the mathematical difficulties of the three-body problem, and, even if he dealt with a problem presented by a practical case in astronomy, he would proceed at once to free it from any special features, and reduce it to its purely mathematical nucleus. Numerical applications did not appeal to him, although he thoroughly appreciated such work by others.

To Brown celestial mechanics was applied mathematics. His principal aim was the quantitative solution of the equations presented by specific problems in the solar system. He concentrated upon finding practical methods that would be adequate for attaining the numerical standard set by the accuracy of the observations. As an applied mathematician he did not concern himself too much with more theoretical questions such as those of convergence, although he was well aware of the limitations of the developments that he used. The comparison of theory with observations was to him an evil that he avoided whenever possible. Somehow he was never impressed by the value of a thorough discussion of observational data. His lunar tables would certainly have suffered on this account if Cowell had not made his analysis of the observations and if he had not had Hedrick's assistance.

To Newcomb, the astronomer, theories of the motion of planets and planetary tables were but the means toward obtaining a secure basis for the analysis of observations. Great as Newcomb's accomplishments are in the construction of theories and tables for the four inner and two outer planets, his principal contribution was the discussion of thousands of observations.

Upon this he built a structure of fundamental astronomy that, after forty years, still stands intact.

No such definite preference for one phase is noticeable in Hill's work. This creative genius was decidedly interested in the purely mathematical questions of the three-body problem. Yet his researches were, as a rule, guided by the practical needs of the solution of specific problems. Hill preferred to test his methods by numerical applications. In these he showed a love for numerical accuracy and the use of many significant figures that reminds one of some of Gauss' publications. This same theorist undertook the arduous task of constructing the theories and tables for Jupiter and Saturn, the most difficult planets in the solar system. He did not hesitate to make the laborious comparisons with observations. Apparently this was to him a necessary part of the whole, which he performed conscientiously, but without showing any of Newcomb's flair in arriving at his results.

The circumstances under which these men worked were quite different, and have a good deal to do with the nature of their accomplishments. Newcomb, the executive, simplified and standardized his methods in order to be able to have the aid of a staff of computers for the execution of the bulk of the computations, too much for any man to undertake alone. Brown did have the help of computers, one or two at a time, but it was a burden to him to supervise their work or to adjust his plans according to the ability of his helpers. He was happiest when he could have the assistance of men to whom he could leave the greater part of the responsibility and, above all, the details. Hill desired to work alone. Even for the theories of Jupiter and Saturn he made all the computations single-handed, using a computer for checking his own work. Poincaré, of course, had no need for computing aid.

The following extracts are taken from a statement prepared by Brown in connection with the semi-centennial celebration in 1938 of the American Mathematical Society. They refer to his relations with Newcomb and Hill.

"My first acquaintance with American mathematics arose from G. W. Hill's classic paper 'Researches in the Lunar Theory.' Professor G. H. Darwin, who had been my chief adviser during my year of postgraduate work at Cambridge, had recommended a study of his work and particularly this paper, and he added, 'No one seems to know much about it.' It was therefore not unfitting when, in the summer of 1891, Isaac Sharpless, then President of Haverford College, offered me a position on his staff that I should accept.

"My earliest recollections of mathematicians outside our little Haverford and Bryn Mawr group are connected with two visits that first winter to Baltimore and Washington, a letter of introduction to Craig and Newcomb being given me by Morley at Washington. At Washington I found Simon Newcomb in his office in the Department of the Navy—he was then head of the Nautical Almanac Office and had got well started on his chief life-work—the Theories of the Major Planets. I knocked at the door, heard a gruff 'Come', went in and presented my letter. 'Sit down', said Newcomb and read it. 'What do you want?' was his first remark. At that time a shy youngster, just 25 years old and looking about eighteen, I was completely nonplussed as to what to say. I think I managed to stammer out that I chiefly wanted to make his acquaintance and then bethought me to ask about logarithm tables—the only way we had of performing calculation at that time. It occurred to me then that he was Editor-in-chief of the American Journal of Mathematics which was printing my second paper on the Lunar Theory and by recalling this to him I managed to get down to some real talk. He became quite cordial told me not to hurry off and when I did move, gave me a letter to G. W. Hill with instructions where to find him.

"Hill, quiet soul, was the opposite in manner to Newcomb. He was busy calculating when I found him—I think on the theories of Jupiter and Saturn, and he probably gave me advice on the Lunar Theory, though I have no recollection of anything he said. As a matter of fact I had then formed my own plan of procedure and had started work on it, so that advice, if it were given, was somewhat too late for me to use. Not long after, when giving a paper before the National Academy of Sciences, I met Hill again and experienced his great generosity in mentioning a slight error I had found in one of his papers, although it had nothing to do with the paper being read.

"Hill was difficult to get to know. His chief interest outside mathematical astronomy was botany; on that subject my knowledge was and is almost *nil.* So that we had little in common outside the Lunar Theory when we met, and he was doing scarcely anything on that subject then or in later years. We corresponded occasionally with great satisfaction to me, as he wrote fully on any question which was put up to him, apparently finding it easier to express his ideas on paper than by word of mouth.

"My contacts and correspondence with Newcomb were much more frequent. He really deserved to add the Lunar Theory to his other achievements, but never found time to undertake the chief part of it—the solar perturbations. In fact he had persuaded Hill to undertake the theories of Jupiter and Saturn because he saw that they could not otherwise be completed during his tenure of office, and, as a matter of fact, Hill spent ten years over them with little else to do during that time. But Newcomb's work on another part of the lunar theory was immensely valuable—the collection and discussion of two centuries of occultations bringing to light the fluctation which he suggested might be an error in the theory, or might be due to a variation in the rate of rotation of the earth; the latter is now fully substantiated by comparison of the theories and observations of the sun and planets. His attempt to compute the planetary inequalities in the motion of the moon was less successful, partly because of an error, but mainly because he tried to carry it out almost wholly through a method for which it is poorly adaptable."

Although Brown was not very fond of teaching he fulfilled these duties conscientiously and with distinction. At Yale he was relieved from teaching as far as possible, his average being four hours a week. He did not take the trouble to prepare in detail what he was going to say, and as a result he occasionally found himself at a loss how to proceed from one equation to the next. But he always succeeded in promptly extricating himself from such a situation, and it was highly instructive to his pupils to see how accurately and unfailingly his mind worked under such circumstances.

Like some of his predecessors in Celestial Mechanics, Brown never married. His household was presided over for many years by his maiden sister Mildred, his junior by two years. For most of her adult life she made it her chief, almost her sole, concern to see to his comfort and shield him from cares and disturbances. She succeeded in utterly spoiling him. She died a few years before her brother. Thereafter his only close relatives were his widowed sister, Ella Yorke, and her children. They live in New Zealand and, of course, saw Ernest only on very rare occasions.

Brown knew how to play as well as to work. In his youth he was addicted to rowing and to mountain climbing. He kept up his piano playing and up to within a few years of his death he was an excellent performer, until in fact palsy made it increasingly difficult for him to strike the keys accurately. But he continued to take great pleasure in music in all its forms. He was for a time the head of the New Haven Oratorio Society. He was fond of chess and played a good game; of late years he gave up this amusement as being too severe a mental tax. He then took to cards, especially bridge, but he did not make a conspicuous success of this game. He was an authority on nonsense verse and could recite without a slip long extracts from Gilbert and Sullivan's operettas, from the Bab Ballads and from Lewis Carroll's verse. In his earlier years he read the English classics, but later he devoted his reading time to the detective story. He was an inveterate traveller and used to attend a great number of meetings, scientific and other. There is no doubt that his chief object in going to so many gatherings was to renew his many friendships among his colleagues.

His daily routine was unusual. He would retire rather early in the evening and as a consequence would awaken usually from three o'clock to five o'clock in the morning. Having fortified himself with a number of cigarettes and a cup of strong coffee from a thermos bottle, he would then set to work in earnest without leaving his bed. At nine o'clock he would get up and have his breakfast. Unless he had something especially exciting on hand, he would not return to mathematical work until the next morning, devoting the intervening time to correspondence, teaching and other similar duties. This program he carried out whenever possible, at home, at the houses of friends he was visiting, and even on board ship.

From his early manhood Brown was affected by bronchial troubles, probably as a result of his rowing activities. Just before his retirement in 1932 he suffered an attack of intestinal ulcers. He refused to take the usual treatment for this complaint, admonishing his physician not to try to prolong his life but simply to make him as comfortable as possible. Strange to say this illness cured itself, but left him in a much weakened condition, and the six years that were left to him were a constant struggle for health. But he went about his work undaunted and undismayed. He died at last of sheer exhaustion on July 22, 1938, in his seventy-second year.

CURRICULUM VITAE AND HONORS

B.A., Cambridge, 1887.
Fellow, Christ's College, 1889-95.
M.A., Cambridge, 1891.
Instructor in Mathematics, Haverford College, 1891-93.
Professor of Mathematics, Haverford College, 1893-1907.
Doctor of Science, Cambridge, 1897.
Fellow, Royal Society, London, 1898-1938.
Member, American Philosophical Society, 1898-1938.
Joint Editor, Transactions of American Mathematical Society, 1899-1907.
Vice-President, American Mathematical Society, 1905.
Professor of Mathematics, Yale University, 1907-21.
J. C. Adams Prize, Cambridge, 1907, for essay on the "Inequalities in the motion of the moon due to the direct action of the planets".
Gold Medal, Royal Astronomical Society of London, 1907, for researches in lunar theory.
Vice-President, American Association for the Advancement of Science, and Chairman Section A, 1910.
Joint Editor, Bulletin of American Mathematical Society, 1910-13.
de Pontecoulant Medal, French Academy of Sciences, 1910, for advancing knowledge of lunar motion.
Honorary Fellow, Christ's College, 1911-38.
Associate Editor, Astronomical Journal, 1912-38.
Fellow, American Academy of Arts and Sciences, 1912-38.
Doctor of Science, Adelaide University, 1914.
Royal Medal, Royal Society of London, 1914.
President, American Mathematical Society, 1915-16.
Vice-President, American Alpine Club, 1920-22.
Bruce Medal, Astronomical Society of the Pacific, 1920.
Sterling Professor of Mathematics, Yale University, 1921-31.
Correspondent, French Academy of Sciences, 1921-38.
Citizen of United States, January, 1922.
Vice-President, American Astronomical Society, 1923-25.
Member, National Academy of Sciences, 1923-38.
Corresponding Member, Belgian Academy of Sciences, 1926-38.
Josiah Willard Gibbs Lecturer, American Mathematical Society, 1927.
President, American Astronomical Society, 1928-31.
Josiah Willard Gibbs Professor of Mathematics, 1931-32 (first incumbent).
Professor of Mathematics, Emeritus, Yale University, 1932-38.
Doctor of Science, Yale University, 1933.
President, American Association of Variable Star Observers, 1934-36.
Doctor of Science, Columbia University, 1934.
LL.D., McGill University, 1936.
Watson Medal, National Academy of Sciences, 1937.

KEY TO ABBREVIATIONS

Amer. Jour. Math.—American Journal of Mathematics.
Amer. Jour. Sci.—American Journal of Science.
Ann. Math.—Annals of Mathematics.
Astron. Jour.—Astronomical Journal.
Astron. Nach.—Astronomische Nachrichten.
Astrop. Jour.—Astrophysical Journal.
Biog. Mem. Nat. Acad. Sci.—Biographical Memoirs, National Academy of Sciences.
Bull. Amer. Math. Soc.—Bulletin, American Mathematical Society.
Bull. Nat. Res. Coun.—Bulletin, National Research Council.
Cambridge Rev.—Cambridge Review, Cambridge, England.
Haverford Coll. Bull.—Haverford College Bulletin.
Haverford Coll. Stud.—Haverford College Studies.
Jour. British Astron. Assn.—Journal, British Astronomical Association.
Jour. Franklin Inst.—Journal, Franklin Institute.
Jour. R. Astron. Soc. Canada—Journal, Royal Astronomical Society of Canada.
Mem. R. Astron. Soc.—Memoirs, Royal Astronomical Society.
Mo. Notices, R. Astron. Soc.—Monthly Notices, Royal Astronomical Society.
Observ.—Observatory.
Phys. Rev.—Physical Review.
Pop. Astron.—Popular Astronomy.
Pop. Sci. Mo.—Popular Science Monthly.
Proc. Amer. Acad. Arts & Sci.—Proceedings, American Academy of Arts and Sciences.
Proc. Amer. Assn. Adv. Sci.—Proceedings, American Association for the Advancement of Science.
Proc. Amer. Phil. Soc.—Proceedings, American Philosophical Society.
Proc. Cambridge Phil. Soc.—Proceedings, Cambridge Philosophical Society.
Proc. Fifth Int. Cong. Math.—Proceedings, Fifth International Congress of Mathematicians.
Proc. London Math. Soc.—Proceedings, London Mathematical Society.
Proc. Nat. Acad. Sci.—Proceedings, National Academy of Sciences.
Proc. R. Soc.—Proceedings, Royal Society of London.
Proc. Second Pan Amer. Sci. Cong.—Proceedings, Second Pan American Scientific Congress.
Pub. Amer. Astron. Soc.—Publications, American Astronomical Society.
Pub. Astron. Soc. Pac.—Publications, Astronomical Society of the Pacific.
Rev. Astron.—Revista Astronomica.
Science, n.s.—Science, new series.
Sci. Amer.—Scientific American.
Sci. Mo.—Scientific Monthly.

Smithsonian Rept.—Smithsonian Institution, Annual Report.
Trans. Amer. Inst. Elec. Eng.—Transactions, American Institution of Electrical Engineers.
Trans. Amer. Math. Soc.—Transactions, American Mathematical Society.
Trans. Astron. Observ. Yale Univ.—Transactions, Astronomical Observatory, Yale University.
Trans. Cambridge Phil. Soc.—Transactions, Cambridge Philosophical Society.

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1891

2. On the Determination of a Certain Class of Inequalities in the Moon's Motion. Mo. Notices, R. Astron. Soc., vol. 52, pp. 71-80.
3. On the Part of the Parallactic Class of Inequalities in the Moon's Motion, which is a Function of the Ratio of the Mean Motions of the Sun and Moon. Proc. Cambridge Phil. Soc., vol. 7, pp. 220, 221.

1892

4. Notes on the Lunar Theory. Mo. Notices, R. Astron. Soc., vol. 52, pp. 408, 409, 1892; vol. 54, p. 471, 1894; vol. 55, pp. 3-5, 1894.
5. On the Errors Produced in Numerical Calculations by the Use of Decimals. Haverford Coll. Stud., no. 11, pp. 50-55.
6. On the Part of the Parallactic Inequalities in the Moon's Motion which is a Function of the Mean Motions of the Sun and Moon. Amer. Jour. Math., vol. 14, pp. 141-160.

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1895

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1896

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15. On certain Properties of the Mean Motions and the Secular Accelerations of the principal Arguments used in the Lunar Theory. Proc. London Math. Soc., vol. 28, pp. 143-155.

1897

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1900

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before the Ithaca Colloquium of the American Mathematical Society, August 21-24, 1901.) Bull. Amer. Math. Soc., series 2, vol. 8, pp. 103-113.

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1905

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1911

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1915

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1916

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84. George William Hill (1838-1914). Proc. Amer. Acad. Arts & Sci., vol. 51, no. 14, pp. 890, 891.
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1917

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1918

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1919

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1920

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1923

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1924

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1925

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