NATIONAL ACADEMY OF SCIENCES
OF THE UNITED STATES OF AMERICA

BIOGRAPHICAL MEMOIRS
PART OF VOLUME VIII

BIOGRAPHICAL MEMOIR

OF

GEORGE WILLIAM HILL

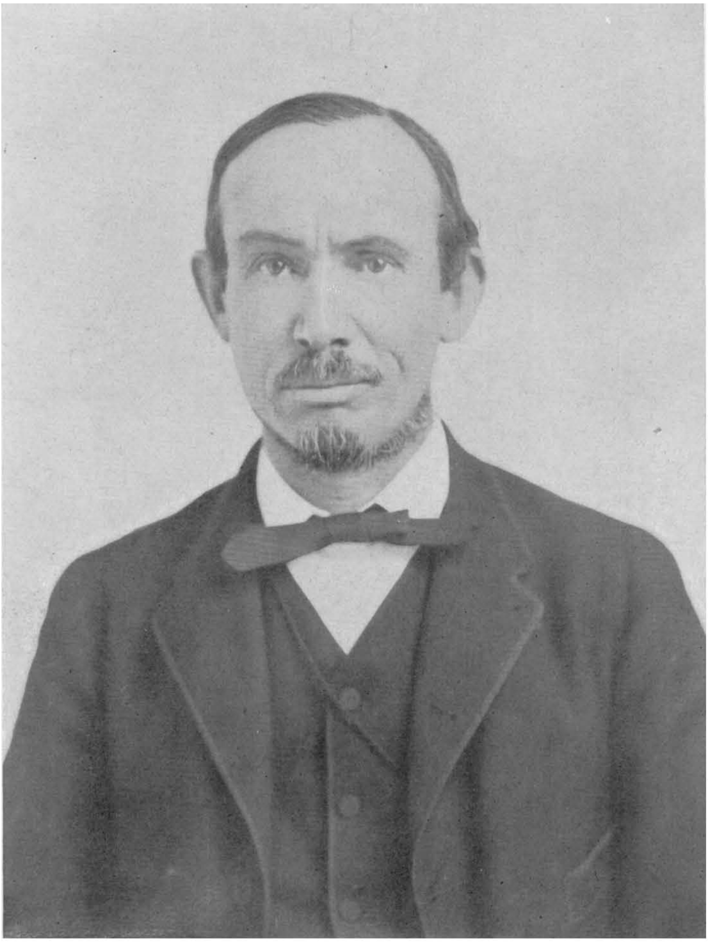
1838-1914

BY

ERNEST W. BROWN

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1915

CITY OF WASHINGTON
PUBLISHED BY THE NATIONAL ACADEMY OF SCIENCES
June, 1916



G. W. Hill

GEORGE WILLIAM HILL.*

BY ERNEST W. BROWN.

GEORGE WILLIAM HILL was the son of John William Hill and Catherine Smith, and was born in New York City on March 3, 1838. Both his father and grandfather were artists, and he himself was of English and Huguenot descent. His early education, like that of most of the men of his time in America, gave him few advantages. In 1846, when his father moved from New York to the farm at West Nyack, the country was too busy with material development to produce many teachers who could give any but the most elementary instruction, and the country school which he attended must have been inferior in this respect to those in the larger cities. Even at Rutgers College, in New Jersey, to which Hill was sent, owing to the exhibition of unusual capacity, and from which he took his degree in 1859, the course probably went but little beyond that now found in secondary schools. There, however, he came under the influence of a man whose ideas on education were unusual. Dr. Strong, according to Hill's evidence, believed only in the classic treatises; but little published after 1840 was admitted to his library. Under him, Hill read Lacroix's Differential and Integral Calculus, Poisson's Traité de Mécanique, de Pontécoulant's Système du Monde, Laplace's Mécanique Céleste, Lagrange's Mécanique Analytique, Legendre's Functions Elliptiques. Strong liked to go back for his theorems to Euler, whom he regarded as the greatest of analysts. Hill's sound knowledge of the fundamentals of his subject is doubtless due to this course of reading.

Hill's first paper, published in 1859, when he was but twenty-one years of age, and before he had taken his degree at college, is a half-page note on the curve of a drawbridge. Two years later he showed his capacity in the essay which gained

* The writer of this memoir desires to express his indebtedness to those who have assisted, either by their published accounts of Hill or by letters or in other ways. Due acknowledgment is made below in most cases.

a prize offered by Runkle's Mathematical Monthly for the best solution of a problem connected with the constitution of the earth. "This memoir," says Hill, in a footnote to the reprint of his collected works, "written at the end of 1859 and beginning of 1860, was designed to show how all the formulæ connected with the figure of the earth could be derived from Laplace's and Poisson's equations, combined with the hydrostatic equilibrium of the surface, without any appeal to the definite integrals belonging to the subject of attraction of spheroids. Some of the assumptions are quite unwarranted; nevertheless, I allow them to stand." President R. S. Woodward, who has himself worked much at this subject, says that the memoir is still worthy of careful reading.

In the year 1861 he joined the staff of the Nautical Almanac office, which then had its headquarters in Cambridge, Mass., and for a year or two he worked there, and thus had an opportunity for association with some of the ablest men of the time in astronomical science. But he soon obtained permission to do his calculations at the home in West Nyack, which he never seemed to leave willingly during the rest of his life. It was there that nearly all his best work was done. In fact, he was only away from it for one considerable period, and this is covered by his residence in Washington from about the year 1882 until 1892; even during that time the summers were generally spent in West Nyack.

In the first ten years after leaving college Hill only published eight papers, and none of them deal with celestial mechanics in the modern sense of the term. But from his output after that time it is evident that he had been reading and digesting the newer treatises and memoirs as they appeared. Delaunay's two magnificent volumes on the lunar theory were published in 1860 and 1869, respectively, and the methods of that investigator exercised a fascination over Hill for the rest of his life. He returned to them again and again, and several times not only expressed the opinion that they should be more widely used in planetary problems, but himself outlined ways in which this could be done. But his most important developments on the lines laid down by Delaunay all refer to the moon's motion. He gave the methods which have made them of practical advantage for the calculation of the effects produced by the planets and the figure of the earth on the moon; this was the step necessary for the completion of Delaunay's work, which had only taken account of the forces due to the earth and sun. The other great lunar theorist of the period, P. A. Hansen, had been explaining his methods for many years before this time, and Hill was probably one of the few men of his age who understood them thoroughly. He does not seem to have been particularly drawn to them, although they are used in his theories of Jupiter and Saturn with but little alteration. It is difficult to find many traces of other influences in his work. His most celebrated memoir, it is true, is based on one of Euler's numerous methods, as he himself tells, but after the start he proceeds entirely on lines of his own devising.

The publications which follow his first attempts during this early period exhibit knowledge of theoretical astronomy and the power to handle large masses of numbers rather than any unusual mathematical ability. In his discussion of the observations of the great comet of 1858, which was undertaken to obtain a satisfactory orbit (1867), he has to deal with 363 places gathered from many sources. As usual with Hill, he does not confine himself to the main point, but discusses systematic errors between the different observatories and those due to the size of telescope used. His final conclusion is that there is no evidence of any force other than gravitation influencing the motion of the comet.

It is probable that his work on this body was responsible for the next three papers—on the reduction of star places, the determination of the elements of a circular orbit, and the conversion of latitudes and longitudes into right ascensions and declinations—or, at any rate, that it drew his attention to these fundamental problems. But he was soon to lay them in the background for more original investigations in celestial mechanics proper. One can see in his published works his gradual approach to this subject. His tenth memoir is a correction to the elements of the orbit of Venus from observations extending over 33 years. It is followed by a derivation of the mass of Jupiter from the perturbations of certain asteroids and the calculation of an inequality of very long period in the motion of Saturn. Shortly before, however, he had been assisting in the campaign which had started years earlier to get the utmost out of the transits of Venus in 1874 and 1882. Part II of the Papers of the U. S. Commission relating to the transits is by his hand; it consists of charts and tables for facilitating predictions of the several phases at any place on the globe. In these days such work is largely a matter of routine for any efficient almanac office, but at the time of which we are writing much of it had not been systematized and the tables of the planet had not attained to the accuracy which they later received under the administration of Newcomb. And Hill's own work on fundamental astronomy undoubtedly contributed to some extent towards the systemization of the methods generally in use at the present time for the fundamental problems of the astronomy of position.

The active period of Hill's work in celestial mechanics began in 1872. Between that year and 1877, when his two chief memoirs appeared, he published eleven papers on various phases of the subject, besides seven others in pure and applied mathematics and the long transit of Venus calculations already mentioned. Most of them are quite brief and call for no special mention. At this time he seems to have thought that the masses of some of the larger planets could be accurately found by the long period inequalities which they produce in the motions of certain of the asteroids, and he gives some preliminary work in the determination of the masses of Jupiter and the Earth. But there are several objections to this idea. The mathematical difficulties in the computation of the theoretical coefficients, to the degree of accuracy needed, are very great, while the observations themselves must be extended over very long periods. And, further, the later work of Newcomb and Hill himself on the great planets and of others on the satellites have given us far better values for the masses than we could hope to obtain from the asteroids.

A five-page note on the calculus of finite differences is fundamental in the problem of mechanical integration, and it was long afterward used by G. H. Darwin in his work on Periodic Orbits. But it is difficult to follow because of some confusion in the notation. Several other papers are new demonstrations of well-known formulæ. One memoir, interesting from a historical point of view, consists of some "remarks on the stability of planetary systems." Hill begins by pointing out that the ordinary definition of stability as given in the text-books is quite insufficient for application to planetary systems, and he then proceeds to discuss what the conditions should be. In editing this paper for insertion in his collected works he realized, as we see from the footnotes, that parts of it were rendered obsolete or shown to be erroneous by the researches of Poincaré. Nevertheless, it is a distinct advance on the ideas of the period; errors in accepted views are pointed out and conditions are laid down.

While these memoirs are good and exhibit both knowledge and insight, they could hardly have attracted any special attention outside the small circle of mathematicians and astronomers in America. It was not until he was 39 years old that the first of the memoirs which have given him a permanent place in the history of celestial mechanics appeared. Nearly all his best work was done in the decade from 1875 to 1885, when he was between 35 and 45 years of age. This was probably the natural development for one of Hill's powers, whose early education had been so much inferior in many respects to that which his peers in Europe had received. Even now the opportunities for a man of exceptional powers to obtain full development early are far fewer than those on the other side of the Atlantic, owing to the school and college systems in vogue in this country; in Hill's early years the difference, mainly from lack of well equipped teachers, was far greater. The circumstances under which he lived seem to have contributed to a natural development of his genius, but little affected by external circumstances. From the time he left Cambridge to work at home, probably about 1863, he could have had but little intellectual companionship. And it was twenty years before he went to Washington to live in order to undertake the theories of Jupiter and Saturn. Ten years later he returned to the farm, and lived there until his death. It is worth noting that the time of Hill's most original productivity was during the greater part of the decade preceding his residence in Washington. Apparently the change affected him very little. He resided in the same boarding-house during the whole time he was there, seeing but few people, and returning to his farm or traveling during the summers.

This sketch of Hill's mental development up to the point where his chief papers were produced is perhaps all too short, but the materials at the disposal of the biographer are scanty: almost nothing is available for the purpose except the few dates and places recording his movements and the internal evidence of his published work. One can only gather that even in his earlier years he lived and worked much alone, gaining his knowledge and receiving stimulus mainly from books. A wonderful calm pervades everything that he wrote. Even after the execution of some analytic *tour de force* like the solution and development of the infinite determinant and the successful reduction of the complicated analytic expression to numbers, there is not the slightest trace of exultation visible. With one case of this the writer happens to be familiar. In the development of the infinite determinant the most troublesome part is the summation of a triply infinite series into an expression involving cotangents and powers of π and consisting of a dozen terms. Two or three times, long after the publication of the result, Hill had been requested to give the method by which he reached it, for the only statement in his paper with reference to the actual work is "The single triple summation may be treated in an analogous manner" [to the double summations]. In a letter dated November 8, 1911, he says: "I spent six weeks of very hard and distracting labor to get the result through the process of summations. I must have done it about six times, scarcely no two being alike in the final result. These results were compared with the check founded on a procedure independent of summation, and [I] found one result which was in agreement with the check. This I had no doubt was right and that the other results were in error, because I had slipped up in the analysis of the triple summation. Had I printed in full the steps of my process it would have added about 12 pages to my paper and the printing would have cost me $50 additional; hence I limited myself to saying that I had arrived at the result by a method similar to that employed for the simpler summations. The method used for the check was laborious, but quite plain sailing." The writer succeeded in verifying Hill's printed result for the triple summation, but he was quite unable to do it in a manner which could be called "analogous" to the double summations; other devices had to be employed, and later, in looking over some notes of J. C. Adams, who had also done the summation, it was seen that the latter had been obliged to use similar devices. It would be interesting to know why Hill published this paper at his own expense. Surely the ordinary facilities of the astronomical journals in 1877 were quite equal to printing such memoirs, and Hill was even then not unknown as an able mathematical astronomer. In fact, the paper was reprinted nine years later in the Acta Mathematica.

In order that the value of Hill's contributions to celestial mechanics, and more particularly to the lunar theory, may be made clear, it is necessary to say a few words as to the condition of the subject at the time they were published. For two hundred years mathematical astronomers, many of them of the first rank, had been devoting their energies to furnishing a complete demonstration of the power of the law of gravitation to account for the motions of all the bodies in the solar system within the degree of accuracy of the observations. In the third quarter of the nineteenth century it was evident that this demonstration would soon be made. Leverrier was publishing his tables for the positions of the great planets, while Hansen and Delaunay had completed their work on the moon. For the purposes of navigation, all needed accuracy had been obtained, and from the scientific side there seemed to be but few matters which needed explanation: the final polish which a few industrious workers might give was the last step. There was thus danger that the subject of celestial mechanics might encounter a blank prospect. The number of investigators began to dwindle. At the same time, pure mathematics and physics were showing vast territories to be explored, while the discovery of spectrum analysis and the use of the photographic plate attracted many astronomers who earlier would have devoted themselves to the mathematical side of the subject. From the old point of view this attitude on the part of astronomers was justifiable. But Hill saw that there were problems other than the mere verification of the law of gravitation by comparisons of theory and observation of the chief bodies in the solar system which would demand solution. He also saw, partly from the industrious work of Newcomb on old and modern observations of the moon, that even the enormous labors of Hansen and Delaunay on the theory of its motion would demand extension and verification if a test of the Newtonian law to the degree of accuracy of the observations were required. For the former object, a new set of problems must be formulated and a start made towards their solution; for the latter, a new method of procedure was practically necessary, for it was almost certain that no one would repeat calculations which appeared to have been pushed as far as was humanly possible with the adopted methods. These two sides of Hill's work are quite distinct, even though they both start from the same memoir.

His own point of view is well expressed in the opening paragraph of his chief memoir, "Researches in the Lunar Theory" (1877): "When we consider how we may best contribute to the advancement of this much-treated subject, we cannot fail to notice that the great majority of writers on it have had before them, as their ultimate aim, the construction of tables; that is, they viewed the problem from the standpoint of practical astronomy rather than of mathematics. It is on this account that we find such a restricted choice of variables to express the position of the moon, and of parameters, in terms of which to express the coefficients of the periodic terms. Again, their object compelling them to go over the whole field, they have neglected to notice many minor points of great interest to the mathematician, simply because the knowledge of them was unnecessary for the formation of tables. But the developments having now been carried extremely far, without completely satisfying all desires, one is led to ask whether such modifications cannot be made in the processes of integration, and such coördinates and parameters adopted, that a much nearer approach may be had to the law of the series, and, at the same time, their convergence augmented."

In the following description of this memoir I have made but little attempt to keep these two sides separate, and, indeed, no such separation is indicated by Hill. He lays, in fact, more stress on the practical side of obtaining better expressions for the moon's motion than on the theoretical questions which he raises. Moreover, rather than the adoption of the modern point of view initiated mainly by Poincaré, it is more correct for historical purposes to approach its main ideas as far as possible with Hill's point of view.

The older lunar theorists had taken the ellipse as a first approximation—that is, at the start the action of the sun was neglected. Hill proposed a first approximation in which a portion of the sun's action shall be taken into account. If an examination of Delaunay's final expressions for the longitude, latitude, and parallax be made, it is seen that the infinite series proceed along powers of five parameters, and that the rate of convergence along powers of one of these, the ratio of the mean motions of the sun and moon, is far more slow than along powers of the others, owing to the presence of large numerical factors. Hill conceived the idea of neglecting all these other parameters, and then finding the series in powers of this ratio alone with all needed accuracy. He set up the equations of motion, solved them, and gave formulas of recurrence which enabled him to avoid the slow approximation methods which generally advanced the degree of accuracy by only one power of the ratio at each step; in his method it advances by four powers of the ratio. The expressions are worked out both literally and numerically, the latter being taken to fifteen significant figures, a number not very much in excess of what is actually required.

As obtained, the coördinates are referred to axes which move with the mean velocity of the sun round the earth, and in this form the expressions involve the time through its presence in multiples of a single angle. In the transformations which are necessary to convert rectangular coördinates to polars, Hill makes full use of the method of "special values," or, as it is now called, of harmonic analysis and synthesis. He was always very fond of this kind of transformation, using it much in later years and even attempting to systematize its use when many hundreds of terms were present.

It would be unjust in this connection not to mention the indebtedness of Hill to Leonard Euler, probably the greatest of lunar theorists since Newton. Euler, as Hill remarks, had had the idea of starting the theory in the same way with moving rectangular axes and with the same first approximation, and had carried it out to a considerable extent in his theory published in 1772. The debt would have been greater had Hill been aware of another paper of Euler's, published in 1768 in the memoirs of the Berlin Academy and entitled "Sur la variation de la lune." In this publication Euler obtained the first two or three terms of the same series which Hill treated so amply, remarking that he believed that a full investigation from this point of view would leave almost no difficulties to be solved. With the observational accuracy of his time, this was true; for, although he did not treat the motions of the perigee and node, approximation methods could have been applied which would have satisfied the needs of the astronomers of his day.

The further steps outlined by Euler and quoted by Hill consist of the determination, step by step, of the terms arranged in powers of the parameters which had been neglected. Each step is to consist of the complete calculation with all needed accuracy of the function of the time and the ratio of the mean motions which multiplies each combination of powers of the remaining parameters. There are several difficulties in following this process. The chief one, which Hill solved in the memoir on the Perigee of the Moon, is the determination of the first new angle, containing the time, which arises. In later approximations this angle also involves all the parameters, and other methods are needed to find the new portions depending on them. Euler possibly foresaw this; Hill certainly did, but he never carried his work to the degree of approximation which would need them. The method has been used by the writer for the construction of a complete theory of the moon's motion.

The expressions for the coördinates referred to the moving rectangular axes have another property; they form Fourier series, and are therefore periodic. The resulting orbit in this moving plane is consequently closed. Recognizing this fact, Hill draws the curve. But he saw that the orbit was of interest apart from its application to the lunar problem, for he immediately proceeds to trace, with some care, orbits for values of the ratio of the mean motions other than that which holds for the actual moon and sun. He thus obtains a family of such orbits. It is Hill's idea of the periodic orbit which, developed chiefly by Poincaré and G. H. Darwin, has given new life to the whole subject of celestial mechanics and has induced many mathematicians to investigate on these lines. The treatise of the former, "Les méthodes nouvelles de la mécanique céleste," is based mainly on the idea. Darwin actually traced many such orbits under varying conditions. During the last twenty years numerous mathematicians and astronomers have treated different phases of the subject which, according to Poincaré, is the only means of approach to regions of research previously considered impenetrable.

There is still another portion of this memoir which has been largely used as a foundation for investigations into the stability of celestial systems. If the eccentricity of the earth's orbit round the sun be neglected, it is possible to write the relative energy equation in a finite form. Referred to the same moving axes, the square of the velocity can, in fact, be expressed as a finite algebraic function of the coördinates. Since the square of the velocity can never be negative, this function equated to zero gives the equation to a surface which the moon cannot cross. As the surface consists of various ovals and folds, we can obtain certain limitations on the path of the moon, and therefore carry forward the question of the stability of its motion one important step. Hill draws the surfaces for a limited case. Darwin made extensive use of a similar diagram for a more extended case, and many others have followed on the same lines.

Thus this memoir of but fifty quarto pages has become fundamental for the development of celestial mechanics in three different directions. It would be difficult to say as much for any other publication of its length in the whole range of modern mathematics, pure or applied. Poincaré's remark that in it we may perceive the germ of all the progress which has been made in celestial mechanics since its publication is doubtless fully justified. It has sometimes been said that Hill did not appreciate at the time the importance of his work. Hill was far too modest about his own achievements to lay any such stress on his productions as has the scientific world. But it does not require an extended study of his memoirs to see that his vision often went beyond the particular matter in hand. A deviation into some problem arising from work on which he was engaged, or a suggestion for future investigation at the end of a paper, is almost characteristic, and the value of these suggestions, shown later, proves that he either saw further or had developed the idea more fully than his remarks would indicate.

The second memoir of 1877, "On the part of the motion of the Lunar Perigee, which is a function of the mean motions of the Sun and Moon," has already been referred to. It is essentially a continuation of that part of the researches which deals directly with the lunar problem, although published earlier. While not so far-reaching from the point of view of future developments, it is even more remarkable as an exhibition of Hill's powers of analysis. In it the determinant, with an infinite number of elements, is raised from a nebulous possibility to an instrument of computation. Hill's periodic orbit contained only two of the four arbitrary constants which the complete solution of his differential equations requires. He therefore proceeds to find an orbit—no longer periodic—differing slightly from the periodic orbit, but still satisfying the differential equations to the first power of the small variation. The equations obtained are two of the second order, and linear with respect to the two unknown dependent variables. An able analysis, with the use of known integrals, enables him to reduce the solution to that of one of the second order in the normal form:

$$\frac{d^2p}{dt^2} + Vp = o$$

where V is a known Fourier series depending on the time. Knowing the form of the solution:

$$p = \Sigma\, a_i \cos \left\{ c\,(t - t_1) + 2\,i\,(n - n')\,(t - t_0) \right\},$$

from previous work in the lunar theory which he justifies by general considerations, Hill substitutes and obtains an infinite series of linear equations for the determination of the unknowns a_i. But c is also unknown and it does not enter in a linear form. The a_i are eliminated by means of a determinant with an infinite number of rows and columns equated to zero; this is, therefore, a determinantal equation to find c, the main object of the investigation.

Then follows a remarkable series of operations. The determinant is reduced to a convergent form (though it was left to Poincaré to furnish the proof of convergence) by dividing each row by a suitable factor which reduced every element of the principal diagonal to unity. Next, the unknown c must be isolated; Hill achieves this by recognizing that if c be a root, so must $c + 2i$ be also a root, and that therefore all the roots can be expressed by a cosine function. On the assumption that there are no other roots, he equates the determinant to the cosine function, obtaining the constant by comparing the highest (infinite!) power of c on each side of the equation. A particular value of c (not a root) can also be inserted in the identity thus obtained. In this way Hill reduces the work to the computation of an infinite determinant every element of which is known. He gives a general method for its expansion which enables him to tell at once the order of the terms neglected by cutting off the series at any place. Each term of this series, however, consists of singly, doubly, . . . infinite series which must be summed. The labor at this stage was very great, and it caused a corresponding liability to error. Hill carried it through with complete success in its general form, afterwards substituting numbers and determining c to sixteen significant figures. The principal part of the motion of the moon's perigee is immediately deducible from c. President Woodward relates that the determinant was solved during one of two trips which Hill made to the northwest region of Canada; I imagine, however, that this statement refers to the method adopted rather than to the actual computation.

The story of these two memoirs is incomplete without a notice of the work of J. C. Adams on somewhat similar lines. Almost immediately after their publication a brief paper by him appeared in the Monthly Notices of the Royal Astronomical Society. After acknowledging the importance of Hill's work, he proceeds to give some account of his own researches. He had also seen the value of Euler's method, and had obtained Hill's periodic orbit to a high degree of accuracy. But he had used polar coördinates, and consequently the rapidly converging series which Hill had deduced with such elegance for rectangular coördinates referred to moving axes were not available. Also, instead of continuing the natural development to obtain the motion of the perigee, he had turned aside to find the principal part of the motion of the node. In so doing, the equation for p (see above) comes naturally into the normal form of the second order. At this point his method of investigation is almost parallel with Hill's. He sets up the infinite linear system, introduces the infinite determinant, expands it, and makes the numerical application to the nodal motion. His results only are given, but further information as to the extent of his actual work can be obtained from the extracts out of his diaries and unpublished manuscripts now printed in the second volume of his collected works. As Dr. Gaisher remarks in his memoir of Adams, the coincidence of the methods of Hill and Adams is very striking. It was the second important occasion that Adams had been forestalled, but here his inveterate habit of postponing publication in order to put a final polish on his work must be blamed. Indeed, much of this particular work on the lunar theory was done by Adams before Hill had left college.

It is convenient at this stage to take up Hill's work rather by subject than in chronological order. The periodic orbit used with such excellent effect in the lunar theory is tried later (1887) on the motion of the satellite Hyperion as disturbed by Titan, and the results applied in a following paper to obtain the mass of the latter. These were written before the publication of Poincaré's researches. Only on one occasion did he make it the subject of a theoretical research, and it was then probably stimulated by reading Poincaré's Mécanique Céleste. As the title, "Illustrations of Periodic Solutions in the Problem of Three Bodies," indicates, it consists of applications to certain bodies in the solar system.

From time to time a paper was published extending the applications to the lunar theory. In one, the periodic orbit is made to include the terms which depend on the ratio of the parallaxes of the sun and moon as well as on the ratio of the mean motions. Another contains the terms dependent on the latter ratio and on the first power of the solar eccentricity. In still another paper he calculates the expression for the principal part of the motion of the moon's perigee as far as m^{11} literally, in order to settle the correctness of Delaunay's value, which had been questioned as to certain of the earlier powers of m by Andoyer. Beyond these he seems to have made no effort to continue the work in this direction. Possibly this was due to the heavy labor on the theories of Jupiter and Saturn which engaged him at least until 1892. In fact, as early as 1888 he stated, in a letter to Sir George Darwin, that he scarcely expected to proceed with the subject.

Prof. F. R. Moulton, who has himself done much to develop the ideas of Hill and Poincaré, chiefly in the domain of periodic orbits, has given in Popular Astronomy an account of Hill and his work. In referring to the lunar theory, he writes:

"The question naturally arises why Hill did not complete his lunar theory, according to his announced intention and plans as set forth in his first paper on the subject? The question had often arisen in my mind. After one of the meetings of the National Academy in Washington, a few years ago, Hill asked me to take a walk with him. It was one of the splendid afternoons that are often found in Washington in late April. The bright green vegetation and newly opened flowers were particularly beautiful to us, because they were still held back by the cold winds which at that time of the year were sweeping across the Northern States. Hill laid our course down one of the beautiful avenues for which Washington is noted and out toward the country. Soon the invigorating air and the glow from the exercise put us in the best of spirits. I then asked him to give me some account of his work on the lunar theory. With the utmost modesty, yet with considerable freedom, he told me about it.

"At the time Hill was publishing his papers on the lunar theory Simon Newcomb was in charge of the American Ephemeris and Nautical Almanac office in Washington, and he had the worthy ambition to get out new tables of the motions of the planets which would surpass all those in existence. The most troublesome problem in this program was the determination of the mutual perturbations of Jupiter and Saturn. Newcomb recognized Hill's ability, and assigned him the task of developing the theory of the motion of these planets. The result of this assignment is comprised in the 577 pages of volume III of Hill's Collected Works, and required seven and one-half years of steady computation for its completion. Hill told me that he thought the greatest piece of astronomical calculation ever carried out by one man was Delaunay's lunar theory, and that his work on Jupiter and Saturn came second. Now the greater part of this work was straight computation by methods which were largely due to Hansen, and which could have been carried out under Hill's direction by men who did not have his great ability for original work. It seems probable that science lost much because Newcomb caused Hill to spend about eight years of the prime of life on this work. At any rate, this was the direct cause of his laying aside, as he thought for a time only, his researches on the lunar theory."

His fondness for Delaunay's methods has already been mentioned. One of his most valuable memoirs is an application of them to the calculation of the smaller perturbations of the moon's motion which arise from the action of the planets and the figure of the earth. It had long been accepted that the most efficient way of treating these was through the method of the variation of arbitrary constants. But most of Hill's predecessors had found their action independently of the solar perturbations of the moon—that is, the orbit was considered to be elliptic before the application of these small forces; if the action of the sun were taken into account at all, it was through a troublesome series of approximations. Hill, using Delaunay's methods and results, showed, in a short paper on the Jovian evection, that the whole action of the earth and sun on the moon could be treated as known from the start, and that, therefore, only one approximation was needed in order to get the effect of any disturbance whose square could be neglected. All later investigators have used this method. The formulæ of Delaunay are literal, while Hill's final equations for the calculation of the effect of any small disturbing force have the great advantage of well-determined numerical coefficients to be multiplied by the constants which depend solely on the nature of the given force.

In an earlier paper he had also shown how the disturbing function for direct planetary action can be expressed as a series of products, one factor in each product containing the coordinates of the earth and moon only, while the other contained those of the earth and planet only. The former could therefore be computed once for all; it was the latter which required separate computations for each planet. This paper has also formed the basis for all the complete calculations of the planetary disturbing forces which have been made since its publication in 1883.

But Hill's most extensive application of Delaunay's theory is made in its original form to the calculation of the inequalities produced by the figure of the earth. While he carried these to the degree of accuracy needed for observation, the method appears to be somewhat long and complicated. It has to be applied in a literal form, and this requires expansions which converge very slowly. As a matter of fact, a few days' work with his methods adopted for the planetary terms will furnish the inequalities with all needed accuracy. In the first part of this paper Hill, not content with the values for the flattening of the earth which were then in use, deduced one directly from a large number of pendulum observations all over the earth. The result, 1/288, is considerably larger than most of the other determinations, and notably so than that of Helmert, 1/298, deduced from the same class of observations. The memoir occupies over a hundred and forty pages, and must have demanded an enormous amount of careful and accurate algebraic computation. To complete the account of his work on the lunar theory, mention must be made of his calculation, by de Pontécoulant's method, of the principal inequalities produced by the motion of the ecliptic. Hansen was the only writer who had found the term in longitude as well as that in latitude, and nearly all his calculations of the small perturbations are doubtful. Hill, of course, obtained correct results as far as he went in the matter.

Newcomb had taken charge of the American Ephemeris in 1877. This gave him the opportunity he had longed for: to construct new theories and tables of the moon and planets, with constants which should be determined by a full use of the great mass of observations which had at that time accumulated. Even Newcomb, with his great energy and power for hard work, saw that this was a task beyond the capacity of one man, and for the most difficult part, the theories of Jupiter and

Saturn, there was but one man on whom he could rely. It needed great ability and knowledge of celestial mechanics, as well as accuracy and care, in working out the numerical details. He induced Hill to undertake the task, and, in order that he might make full use of the computing staff, to settle in Washington. Hill yielded, and for ten years did most of his work there. But he always found it difficult to make use of ordinary computers; he lacked the organizing power which Newcomb possessed in so high a degree and which enabled the latter to turn out such vast quantities of results. Hill did most of the original computations himself and only used computers to a small extent in the verification of his results.

The method adopted is that of Hansen, with only a slight modification, which consisted in expressing the computations directly in terms of the time instead of using two auxiliary angles. That he used an old method in preference to devising a new one is perhaps unfortunate, even though the result leaves little to be desired. Had he taken more time over the preliminary stages we should probably have had something new and original, for Hill was then at the height of his powers as a mathematician. But he was under some pressure from Newcomb, who wished to complete his great plan during his tenancy of the office of director, and Hill himself may have desired to finish the calculations as soon as possible in order that he might return to West Nyack. However this may be, he completed the task successfully, as may be judged from the small residuals which he obtains after a comparison with observations extending over 150 years. The tables which he formed from the theories of the two planets are now used in most of the national ephemerides.

In 1882 Hill published a memoir of some length on Gauss's method for computing the secular perturbations of the planets. Gauss had outlined only the general idea, which amounts to obtaining the mean values of certain functions at about a dozen points of the elliptic orbit of the disturbed planet, equally distributed with respect to the angle which is adopted. Much work has, however, to be done before this can be shaped for actual use. Hill takes it up, and develops in detail the formulæ to be used. In the course of the analysis he finds that a considerable portion of the calculation depends on three elliptic integrals which may be needed for values of the argument up to 50°. Consequently a large part of the paper consists of the tabulation of these to eight places of decimals at intervals of a tenth of a degree; the first and second differences are also printed, so that the tables are in form ready for interpolation. As an example, he computed the secular perturbations of Mercury by Venus with great accuracy. These long computations are an evidence of Hill's industry and full control of his methods; he had tried them out in detail, and could therefore give them in the most effective form. At the end of the paper is an appendix in which he gives a further improvement on the method as a result of his experience with it. The most complete application of Hill's devices is the computation of the secular perturbations of the four inner planets by Prof. Eric Doolittle. Hill returned to the subject in two memoirs published during the year 1901. He was obviously stimulated by reading the memoirs of Halphen and Bour on the subject, and seeks to extend the work of Halphen as he had previously done that of Gauss.

In these years Hill published a number of short papers in the Analyst, a journal no longer in existence. Sometimes they are merely solutions of well-known problems; at other times simplifications of proofs of theorems which had evidently presented difficulties to him, and which he felt needed elucidation or elaboration—two favorite words with him. But Hill was not a great expositor; even for those familiar with the subject his work is often difficult, and sometimes obscure. Newcomb used to say that if Hill had only had the faculty of explaining his own ideas, he (Newcomb) might have avoided many an error and saved much time. Hill's ability to assimilate and extend the work of others, at any rate in his earlier days, doubtless prevented him from appreciating the difficulties of others. When the reader is used to Hill's style of composition and his general plans in writing out what he had to say, his arguments are much more easily grasped, but he is rarely anything else but concise.

In his last years Hill still continued to publish in spite of failing health. He covered a variety of topics, several of them quite away from the region of celestial mechanics. That these papers have not attracted any very great attention may possibly be because they are on lines in which his name is not well known; time alone can furnish a judgment of their value. One of the most extensive of them is a memoir on dynamic geodesy, the last in the fourth volume of his collected papers, and not previously published elsewhere. In the prefatory remarks to it he says: "The endeavor in this memoir is to elaborate a theory of gravity which dispenses with the idea that we must know the elevation of the place of observation before we can make comparisons." In it he makes a variety of suppositions concerning the differences from an ideal earth for which exact formulæ can be supplied. Some of these are worked out in considerable detail, with occasional reductions to numerical results.

If an attempt is made to regard Hill's work as a whole and to try to find out his point of view, one thing stands out clearly: a desire to obtain exact knowledge about natural phenomena, in however limited a field, which could be expressed in a numerical form. He never seemed to hesitate about making long calculations, and apparently had a positive liking for obtaining his results to many places of decimals. But, unlike the tendencies of those who engage much in computation, his mind did not seem to get cramped by figures. Not only could he see both trees and wood, to adopt a familiar simile, but could trace paths in the wood and keep his eyes open for those roads which led in directions other than that he was exploring. He had remarkable ability for algebraic manipulation, which reached its highest manifestation in the memoir on the perigee of the moon. The more modern sides of mathematics appealed to him but little; if a formula or a series could be reduced to numbers, such questions as convergence did not trouble him much—a point of view which has later been fully justified by Poincaré. He seemed to take but little color from the work of others. Even when, as in many cases, he starts with the results of some previous investigator, his writing shows but little influence of the source of his ideas; it is individual, and carries the reflection and methods of his own mind. Above all, however much in detail he works out his themes, his memoirs carry that indefinable quality of the master which tempts the reader to explore further.

In the introduction to the four volumes of Hill's collected works, published by the Carnegie Institution of Washington, M. Poincaré has given an appreciation of their value. I quote the first and last paragraphs:

"M. Hill est une des physionomies les plus originales du monde scientifique américain. Tout entier à ses travaux et à ses calculs, il reste étranger à la vie fiévreuse qui s'agite autour de lui, il recherche l'isolement, hier dans son bureau du Nautical Almanac aujourd'hui dans sa ferme tranquille de la vallée de l'Hudson. Cette réserve, j'allais dire cette sauvagerie, a été une circonstance heureuse pour la science, puis qu'elle lui a permis de mener jusqu'au bout ses ingenieuses et patientes recherches, sans en être distrait par les incessents accidents du monde extérieur. Mais elle a empêche que sa réputation se répandit rapidement au dehors; des années se sont écoulées avant qu'il eût, dans l'opinion du public savant, la place à laquelle il avait droit. Sa modestie ne s'en chagrinait pas trop et il ne demandait qu'une chose, le moyen de travailler en paix." . . .

"Ainsi aucune des parties de la Mécanique Céleste ne lui a été étrangère, mais son ouvre propre, celle qui fera son nom immortel, c'est sa theorie de Lune; c'est là qu'il a été non seulement un artiste habile, un chercheur curieux, mais un inventeur original et profond. Je ne veux pas dire que ces méthodes qu'il a créées ne sont applicables qu'à la Lune; je suis bien persuadé du contraite, je crois que ceux qui s'occupent des petites planètes seront étonnés des facilités qu'ils rencontreront le jour où en ayant pénétré l'esprit ils les appliqueront à ce nouvel objet. Mais jusqu'ici c'est pour la Lune qu'elles ont fait leurs preuves; quand elles s'étendront à un domaine plus vaste, on ne devra pas oublier que c'est à M. Hill que nous devons un instrument si précieux."

The impressions of those who knew Hill personally give an idea of the man which is an attractive complement to the account of his scientific life. In the course of a brief account of him in the Astronomical Journal, President R. S. Woodward writes:

"Unfortunately for most of his contemporaries, but perhaps fortunately for him, Hill was too little known, and hence too little appreciated, especially by his fellow countrymen. He was so absorbingly preoccupied with his researches that he had scant leisure for association even with his more intimate friends. He held himself mostly apart from the ordinary affairs and amenities of society, and consecrated his energies to the less obvious realm of celestial mechanics. But this was not because he lacked the social instincts common to mankind. To those who knew him well he presented a singularly attractive personality. Although his innate modesty obscured his superior talents in a large or in a mixed company, he was a delightful companion for a long walk and a very stimulating host to, or guest of, his chosen friends individually. His transparent sincerity, his steadfast but unostentatious devotion to elevated ideals, and the effective simplicity of his life will be long remembered as among the finer qualities of the man Hill by those who had the good fortune to come into closer association with him."

An interesting picture has been contributed by Mr. A. S. Flint, of the Washburn Observatory. In a letter to the writer he says:

"I first met Dr. Hill early in my period of residence at Washington, 1881-1889. He was a regular attendant at the meetings of the then sole scientific society in Washington, the Philosophical Society. Any stranger would have supposed that Hill was some very ordinary farmer that had strayed into the meeting. Professor Ferrel was a good deal of a companion piece to him, and I think the two usually sat together at those meetings. No history of scientific Washington would be complete without a portrait of Dr. Hill, always dressed the same and always proceeding in a walk of short, quick steps, with his umbrella, hanging from one hand, held out forward, except when it was raised against rain or hot sun.

"I remember calling on him once in his large room in a rather economical quarter near Judiciary Square. He was very kindly. I was then enjoying my first experience in riding a bicycle, and I remarked to Mr. Hill that he ought to have one for traversing Washington's 'magnificent distances.' He replied quickly, with a farmer's abrupt tone, 'It would be a great extravagance!' But he had understood me to mean he should have a riding horse, and apologized gently when he understood that I had said 'a bicycle.'"

I am much indebted to Dr. Henry B. Hedrick for the following account of his association with Hill, which he has written at my request:

"My first recollection of George William Hill dates back to the early eighties, when I remember seeing him start out from a house on P street, in Georgetown, D. C., opposite our school, on his Sunday walks, with two companions, William L. Shoemaker, a poet, and Eben J. Loomis, a colleague in the Nautical Almanac office.

"A few years later, about 1890, I became the fortunate one to accompany Hill. At that time I lived at Glencarlyn, Virginia.

"Mr. Hill would walk from his room near the City Hall, in Washington, D. C., to my house, a distance of about eight miles, and then together we would explore the woods and streams for miles beyond my place, taking a light lunch with us, and returning to Glencarlyn. After dinner he would never permit me to hitch up the horse, but insisted on walking back to Washington. One serious objection to driving back was that he would miss the pretty path along Lover's Run. In this way he would walk 25 or 30 miles a Sunday, with little apparent exertion.

"He was a most agreeable companion. He knew the infrequent haunts of the fragrant arbutus and the wooded hillsides completely covered with laurel blooms. The whole country for twenty miles round Washington was an open book to him. I could never take him anywhere in Virginia that he had not been often before, and he was fully as familiar with the country in Maryland north of Washington. He knew all the trees and flowers, and in most cases their common and botanical names, genera, and species. He was as well informed on the botany of these regions as those who made such their special study. In Ward's 'Flora of Washington and Vicinity' it was at first stated that no hemlocks grow near Washington. Mr. Hill called the author's attention to a grove of hemlocks, far from any road, at the mouth of Scott's Run, where it makes its final fall into the Potomac, about 10 miles above Washington.

"Mr. Hill made two trips into the wilds of Canada and several walking trips through the mountains of Virginia, West Virginia, and North Carolina. Everywhere he went he used his marvelous powers of observation, and could talk about the flora he found on these longer trips with the same full knowledge he displayed about those round Washington or round his own home in the valley of Hackensack.

"The birds we saw interested him as well as the flowers. On my visit to his home in the spring of 1908 I found him in front of his house watching some birds in a tree. He said then he was unable to take any more long walks, but that he computed a little each day.

"He took some walks in Germany also, but told me it was not interesting to walk where people spoke a different language.

"Among his books were a great many on travels, especially of drives and walks. I remember one that interested him particularly, and which he lent me, was about a driving trip through West Virginia.

"He did not, I believe, have any botanical collection, leaving these matters to others, as he did physical observations in astronomy. In fact, in these walks of his, which were such a characteristic part of his life and perhaps his main pleasure outside of his work, he displayed the same striking qualities of mind as were manifest in mathematical fields. The roads and paths made by others he used whenever they took him in the direction he wished to go, but at the earliest moment possible he left the beaten roads and struck out into the pathless wood, ever searching for and finding the hidden, the unusual, and the beautiful.

"When I entered the Almanac office, in November, 1886, I was given a desk in Mr. Hill's room. At first there were two other assistants in our room, but later Mr. Hill and I had a room to ourselves, and continued so until he left the office, in June, 1892.

"In those days an able assistant was assigned a piece of work and left to choose his own method of accomplishing the desired result. Thus Mr. Hill was assigned the task of preparing the theory and tables of the motion of Jupiter and Saturn—a difficult problem, in which, however, Mr. Hill was greatly interested. He never had, while I was in the office, any routine computing, nor routine duties incident to his position. He was at all times the master of his work. It might appear that the greater part of the work was straight computation, which could have been carried out under his direction by men who did not have his ability for original thought: that is one of the consequences of efficient planning—the work looks so simple when it is finished.

"The best method to pursue requires a great deal of thought and originality, and often this knowledge can be obtained best by actually doing the work. Mr. Hill always had all the assistance he thought he could use to advantage. He told me once that he did not like having an assistant even to duplicate his work, as it would be impossible for the two to keep together, and it would necessitate his carrying in mind the formulæ not only of the part on which he himself was engaged, but also of the part his assistant was doing. He considered that he conserved time and energy by completely finishing up one part of the calculation, even though he did a major part himself. Then he was free to turn his whole attention to the next part. He was an astonishingly rapid and accurate worker. Several times I noticed that his calculations in a single day would cover two large sheets of computing paper, 16" x 21".

"Mr. Hill was most punctual about arriving at the office, but often worked after office hours in order to reach a desirable stopping place. At that time it was customary, instead of singling out delinquent ones for reprimand, to issue a general order to all the assistants covering the point in mind. To stop tardiness on the part of a few, an order was issued calling attention to the fact that office hours were from 9 to 4. Mr. Hill initialed the order as required, and never thereafter remained after 4.

"He received very few visitors during office hours. Among these callers were Dr. Robert S. Woodward and Prof. Asaph Hall, Sr., two of Mr. Hill's most intimate friends. When Mr. Hill first came to Washington he stayed with Professor Hall several years—that is, until Mrs. Hall's long illness, in the fall of 1881. After that he took rooms on Indiana avenue near the City Hall, where he remained the balance of the time he was in Washington. It was Mrs. Hall who entered Mr. Hill's name as a candidate for Professor of Mathematics in the Navy, the corps to which Professor Hall belonged. At the time there were two vacancies, and Mr. Hill passed a most brilliant examination, away ahead of all competitors, but for political reasons did not receive the appointment. Mr. Hill told me years afterwards that it was a very hard three-day examination, and that he thought the ordeal injured his health.

"Soon after the completion of his theory of Jupiter and Saturn—that is, in June, 1892—he resigned from the Nautical Almanac Office, where he had spent over thirty years, as he could finish the preparation of the tables of Jupiter and Saturn better at his home. When he was a small boy his parents moved from New York City and settled in the small village of West Nyack, N. Y., then called Nyack Turnpike. Both his father and grandfather were artists, and they were attracted to this place by the picturesqueness of the Hackensack meadows. After the death of his father, in 1879, he obtained a house his father had bought. Here he had his library and his desk. Here he worked and slept, taking his meals with his sister, at the family homestead, a few rods away.

"The house contained five rooms on the first floor, with a small attic, and stood near the road, in an acre lot, nearly surrounded by other houses. Although he always went wherever his work called him, it turned out that this place was exceptionally suitable. He lived here while he gave his lectures on Celestial Mechanics at Columbia University and while he was president of the American Mathematical Society. It is but 24 miles from New York City, on the West Shore Railroad, and thus as convenient to that city as could be desired. The roads in the neighborhood are kept in excellent condition and the country is well and thickly settled. He had many neighbors, but his house was sufficiently distant from the others so that he could work at his desk quietly, as all should be who work mentally.

"On entering his home at West Nyack one could not help being impressed by his library. There were bookcases in all his rooms, and they were all full. There were literally books everywhere. He received gratuitously the publications of observatories and mathematical and astronomical societies, and the purchase of every book he wanted was one item of expense in which he never restricted himself. For many years he had an order in Europe for a book he wanted that was out of print. When he obtained it he had to pay a good round price, but that was all right, as he said he could not get it for less. No price was too great for a good book. Before he died he gave some of his books to Rutgers College, and the balance he left in his will to Columbia University.

"He never appeared to be melancholy or morose. To him the world was always bright. He could tell amusing stories of some hardships on his excursions, but not a word of complaint. He mingled with his fellow-men wherever he went, and was always glad to see his friends, both abroad and at his home. He freely helped all who asked his aid.

"In the spring of 1908, when he attended the National Academy meeting, he presented to me a copy of a paper which he had just read before the American Mathematical Society, entitled Subjective Geometry, remarking, as he presented it, that he expected it to make a stir, and that the non-euclidian geometers would answer it.

"I remember how his face would fairly beam on the days the National Academy was in session, as he started off to attend the meetings of the council. He was always present at the meetings of the Academy as long as he was able to attend. I could always look forward to seeing Hill in the spring, when the Academy came to Washington. At these meetings he was retiring and modest, but not timid, as some one has said. It always appeared to me as though it were the other members who were the timid ones. Few spoke to him, but those who did get up courage so to do found him most agreeable and not in the least hard to approach, and a good conversationalist on almost any subject.

"Still, however much he enjoyed these gatherings, he did not let them interfere with his main purpose of life. Like his walks, they may be called his recreation, using that term in its best sense. These were not the incentives for his achievements, and so his life and work continued, after he became eminent and distinguished, in the same way as before.

"In order to accomplish his life work he early trained himself to avoid luxuries. A glass of wine a year with his friends would do no harm, but no feasts, which so often follow in the wake of renown, should turn him from his path of service. He never talked on religion or temperance, but rather preached by his example. His life was an inspiration to all who knew him.

"He was not the head of the Nautical Almanac Office, but he may well be called its spirit. All parts of it—the stars, moon, and planets—show the effects of his mind. He gave new methods to workers in celestial mechanics and extended the boundaries of mathematics. He never received pay for any of his epoch-making ideas; never retired or ceased his labor; never sought nor received a pension. All was given gratuitously for the benefit of his fellow-men. He was the ideal altruistic man of science."

Referring to Hill's connection with Columbia University, Professor Jacoby writes in the Columbia University Quarterly:

"It was in 1898 that Rees obtained from the late Catherine Wolfe a sum sufficient to engage Hill for several years, and he was appointed Lecturer in Celestial Mechanics. That such lectures would attract few students, or none, Rees had predicted to the writer. Hill, who had a high sense of honor, soon offered his resignation on the ground that he had no audience and was not earning his compensation. But Rees was ready with a plausible refusal, and insisted that Hill would amply satisfy the duties of the post if he would but write his lectures so that they might be printed for the benefit of future astronomers. Hill did so; but after the lapse of a couple of years he insisted on returning to Columbia the entire sum that had been paid him, this time on the ground that he did not want the money, and that the care of it was an annoyance to him. Rees accepted it as a gift to the department of astronomy and it was used for purposes of research."

Hill received numerous marks of recognition in this country and abroad. He was foreign member, or associate, of the Royal Society of London, the Paris Academy, the Belgian Academy, and several societies which are less national in their scope. He was awarded the Gold Medal of the Royal Astronomical Society in 1887. Later he received the Schubert prize from the Petrograd Academy, the Damoiseau prize from the Paris Academy, and the scientific blue ribbon of the British Empire, namely, the Copley Medal, from the Royal Society in 1909. Columbia, Princeton, and Rutgers conferred on him their honorary LL. D., and Cambridge University, England, its honorary Sc. D. The list is perhaps not so imposing as that of many a man whose reputation will endure for less time than Hill's, but some of these foreign decorations are achieved by few scientific men. Hill himself was so little known personally, even in his own country, that all such awards were based on his published work. He rarely attended congresses or scientific meetings, and so did not get an opportunity to explain in language which the majority of scientific men could understand the ideas which lay at the base of his chief memoirs. Almost the only occasions when Hill emerged from his retreat at West Nyack were from 1894–1896, when he was president of the American Mathematical Society, then in its early years, and in 1898 to 1901, when he was lecturer at Columbia on celestial mechanics. While living in Washington, however, he would attend the meetings of the Academy of Sciences, and in later years, when his health permitted, he usually made an effort to be present at the annual spring meeting of the National Academy.

Among the unpublished manuscripts left behind by Hill, and now deposited in the library of Columbia University, are two of some interest. The earlier is a carefully written diary, illustrated with photographs, containing an account of the second expedition which he made to the northwest of Canada. The route followed took him, with a companion and guides, by rivers and lakes from Lake Superior to Hudsons Bay and back. It must have been sufficiently laborious, with the numerous portages of the canoes and supplies, but Hill apparently found enough spare energy to record everything of sufficient interest to appeal to him.

The other manuscript must have cost him great labor. It consists of the lectures which he delivered as lecturer on celestial mechanics in Columbia University from 1898 to 1901. He evidently intended them to be a more or less complete account of the methods by which the motions of the moon and planets are computed. There are long algebraic developments which achieve the end he had in view, but the absence of explanations, except in a very concise form, would make them difficult for one not familiar with the subject.

Hill never married. In his last years he suffered from poor health, and rarely left his home; so that his death, on April 16, 1914, from heart failure, scarcely came as a surprise to those who knew him.

A great scientific figure has passed away of a type which is increasingly rare in these times of high organization in every line of educational and scientific activity. With calls on all sides for advice, assistance, lectures, committees, instruction, what chance is there to hope that we may produce scholars most of whose thoughts and time may be devoted to the increase of knowledge concerning the inner mysteries of science? Is it not to be feared that by our very efficiency in coördinating all means to this end we shall extinguish the spark which appears in but a few individuals of any one generation? Hill yielded but once to such a call, and although the result was an edifice to which any one might look back with pride, the world will be poorer because of its construction, if it prevented great discoveries in unknown regions. Whether this be so, we may not know; but we can at least hope that in the one great nation which is and may be free, if it will, from the great overturn of the ideals of our modern civilization, there may still arise a few who, possessing great powers, will also show the single-hearted devotion to science which is so amply exemplified in the life of George William Hill.

BIBLIOGRAPHY OF GEORGE WILLIAM HILL

On the curve of a drawbridge. Runkle's Math. Mon., vol. 1, 1859, pp. 174-175. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 1, p. 1.

Discussion of the equations which determine the position of a comet or other planetary body from three observations. Runkle's Math. Mon., vol. 3, 1860, pp. 26-29. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 2, pp. 2-4.

On the conformation of the earth. Runkle's Math. Mon., vol. 3, 1861, pp. 166-182. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 3, pp. 5-19.

Ephemeris of the great comet of 1858. Astron. Nachr., vol. 64, 1865, pp. 181-190. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 4, pp. 20-22.

On the reduction of the rectangular coördinates of the sun referred to the true equator and equinox of date to those referred to the mean equator and equinox of the beginning of the year. Astron. Nachr., vol. 67, 1866, pp. 141-142. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 5, pp. 23-24.

Discussion of the great comet of 1858, with the object of determining the most probable orbit. Mem. Amer. Acad. Arts and Sci., vol. 9, 1867, pp. 67-100. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 6, pp. 25-58.

On the derivation and reduction of places of the fixed stars. Star Tables of the American Ephemeris, 1869. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 7, pp. 59-76.

Determination of the elements of a circular orbit. Proc. Amer. Acad. Arts and Sci., vol. 8, 1870, pp. 201-209. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 8, pp. 77-84.

New method for facilitating the conversion of longitudes and latitudes of heavenly bodies, near the ecliptic, into right ascensions and declinations, and vice versa. Proc. Amer. Acad. Arts and Sci., vol. 8, 1870, pp. 210-213. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 9, pp. 85-88.

Correction of the elements of the orbit of Venus. Tables of Venus. Amer. Ephemeris and Nautical Almanac, Washington, 1872. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 10, pp. 89-104.

On the derivation of the mass of Jupiter from the motion of certain asteroids. Mem. Amer. Acad. Arts and Sci., vol. 9, n. s., 1873, pp. 417-490. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 11, pp. 105-108.

On the inequality of long period in the longitude of Saturn, whose argument is six times the mean anomaly of Saturn minus twice that of Jupiter minus three times that of Uranus. Astron. Nachr., vol. 82, 1873, pp. 83-88. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 12, pp. 109-112.

Charts and tables for facilitating predictions of the several phases of the transit of Venus in December, 1874. Papers relating to the Transit of Venus in 1874, part 2, 1872. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 13, pp. 113-150, 4 charts.

A method of computing absolute perturbations. Astron. Nachr., vol. 83, 1874, pp. 209-224. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 14, pp. 151-166.

On a long period inequality in the mean motion of Hestia arising from the action of the earth. Astron. Nachr., vol. 84, 1874, pp. 41-44. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 15, pp. 167-168.

Solution of a problem in the theory of numbers. Analyst, vol. 1, 1874, pp. 27-28. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 16, p. 169.

A second solution of the problem of No. 8. Analyst, vol. 1, 1874, pp. 43-46. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 17, pp. 170-172.

Remarks on the stability of planetary systems. Analyst, vol. 1, 1874, pp. 53-60. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 18, pp. 173-180.

Useful formulas in the calculus of finite differences. Analyst, vol. 1, 1874, pp. 141-145; vol. 2, 1875, pp. 8-9. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 19, pp. 181-185.

Elementary treatment of the problem of two bodies. Analyst, vol. 1, 1874, pp. 165-170. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 20, pp. 186-191.

The differential equations of dynamics. Analyst, vol. 1, 1874, pp. 200-203. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 21, pp. 192-194.

On the solution of cubic and biquadratic equations. Analyst, vol. 2, 1875, pp. 4-8. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 22, pp. 195-199.

On the equilibrium of a bar fixed at one end half way between two centres of force. Analyst, vol. 2, 1875, pp. 57-59. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 23, pp. 200-202.

The deflection produced in the direction of gravity at the foot of a conical mountain of homogeneous density. Analyst, vol. 2, 1875, pp. 119-120. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 24, pp. 203-205.

On the development of the perturbative function in periodic series. Analyst, vol. 2, 1875, pp. 161-180. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 25, pp. 206-226.

Demonstration of the differential equations employed by Delaunay in the lunar theory. Analyst, vol. 3, 1876, pp. 65-70. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 26, pp. 227-232.

Solution of a problem in the motion of rolling spheres. Analyst, vol. 3, 1876, pp. 92-93. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 27, pp. 233-235.

Reduction of the problem of three bodies. Analyst, vol. 3, 1876, pp. 179-185. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 28, pp. 236-242.

On the part of the motion of the lunar perigee which is a function of the mean motions of the sun and moon. Separately published, Cambridge, Mass., John Wilson & Son, 1877, pp. 28. Reprinted in Acta Mathematica, vol. 8, 1886, pp. 1-36. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 29, pp. 243-270.

Empirical formula for the volume of atmospheric air. Analyst, vol. 4, 1877, pp. 97-107. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 30, pp. 271-281.

On Dr. Weiler's secular acceleration of the moon's mean motion. Astron. Nachr., vol. 91, 1878, pp. 251-254. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 31, pp. 282-283.

Researches in the lunar theory. Amer. Journ. Math., vol. 1, 1878, pp. 5-26, 129-147, 245-260. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 32, pp. 284-335.

On the motion of the centre of gravity of the earth and moon. Analyst, vol. 5, 1878, pp. 33-38. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 33, pp. 336-341.

The secular acceleration of the moon. Analyst, vol. 5, 1878, pp. 105-110. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 34, pp. 342-347.

Note on Hansen's general formulæ for perturbations. Amer. Journ. Math., vol. 4, 1881, pp. 256-259. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 35, pp. 348-350.

Notes on the theories of Jupiter and Saturn. Analyst, vol. 8, 1881, pp. 33-40, 89-93. Carnegie Inst. Publ. No. 9, vol. 1, 1905, Mem. No. 36, pp. 351-363.

On Gauss's method of computing secular perturbations, with an application to the action of Venus on Mercury. Astron. Papers, Amer. Ephemeris, vol. 1, 1882, pp. 315-361. Carnegie Inst. Publ. No. 9, vol. 2, 1906, Mem. No. 37, pp. 1-46.

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. Amer. Journ. Math., vol. 6, 1883, pp. 115-130. Carnegie Inst. Publ. No. 9, vol. 2, 1906, Mem. No. 38, pp. 47-63.

On the lunar inequalities produced by the motion of the ecliptic. Annals Math., vol. 1, 1884, pp. 5-10, 25-31, 52-58. Carnegie Inst. Publ. No. 9, vol. 2, 1906, Mem. No. 39, pp. 64-79.

Elements and perturbations of Jupiter and Saturn. Astron. Nachr., vol. 113, 1886, pp. 273-302. Carnegie Inst. Publ. No. 9, vol. 2, 1906, Mem. No. 40, pp. 80-97.

A reply to Mr. Neison's strictures on Delaunay's method of determining the planetary perturbations of the moon. Mon. Notices, Roy. Astron. Soc., vol. 47, 1886, pp. 1-8. Carnegie Inst. Publ. No. 9, vol. 2, 1906, Mem. No. 41, pp. 98-105.

Coplanar motion of two planets, one having a zero mass. Annals Math., vol. 3, 1887, pp. 65-73. Carnegie Inst. Publ. No. 9, vol. 2, 1906, Mem. No. 42, pp. 106-115.

On differential equations with periodic integrals. Annals Math., vol. 3, 1887, pp. 145-153. Carnegie Inst. Publ. No. 9, vol. 2, 1906, Mem. No. 43, pp. 116-124.

On the interior constitution of the earth as respects density. Annals Math., vol. 4, 1888, pp. 19-29. Carnegie Inst. Publ. No. 9, vol. 2, 1906, Mem. No. 44, pp. 125-134.

The motion of Hyperion and the mass of Titan. Astron. Journ., vol. 8, 1888, pp. 57-62. Carnegie Inst. Publ. No. 9, vol. 2, 1906, Mem. No. 45, pp. 135-143.

On Leverrier's determination of the second order terms in the secular motions of the eccentricities and perihelia of Jupiter and Saturn. Astron. Journ., vol. 9, 1889, pp. 89-91. Carnegie Inst. Publ. No. 9, vol. 2, 1906, Mem. No. 46, pp. 144-147.

The secular perturbations of two planets moving in the same plane, with application to Jupiter and Saturn. Annals Math., vol. 5, 1890, pp. 177-213. Carnegie Inst. Publ. No. 9, vol. 2, 1906, Mem. No. 47, pp. 148-177.

Determination of the inequalities of the moon's motion which are produced by the figure of the earth. A supplement to Delaunay's lunar theory. Astron. Papers, Amer. Ephemeris, vol. 3, 1884, pp. 201-344. Carnegie Inst. Publ. No. 9, vol. 2, 1906, Mem. No. 48, pp. 180-320.

On certain lunar inequalities due to the action of Jupiter and discovered by Mr. E. Neison. Astron. Papers, Amer. Ephemeris, vol. 3, 1885, pp. 373-393. Carnegie Inst. Publ. No. 9, vol. 2, 1906, Mem. No. 49, pp. 321-339.

A new theory of Jupiter and Saturn. Astron. Papers, Amer. Ephemeris, vol. 4, 1890. Carnegie Inst. Publ. No. 9, vol. 3, 1906, Mem. No. 50, pp. 1-577.

The secular variation of the motion of the moon's perigee. Astron. Journ., vol. 10, 1890, pp. 73-74. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 51, pp. 1-5.

Additional terms in the great inequalities of Jupiter and Saturn. Astron. Journ., vol. 11, 1891, pp. 49-51. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 52, pp. 6-10.

On the connection of precession and nutation with the figure of the earth. Astron. Journ., vol. 13, 1893, pp. 1-6. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 53, pp. 11-21.

On intermediate orbits. Annals Math., vol. 8, 1893, pp. 1-20. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 54, pp. 22-40.

Literal expression for the motion of the moon's perigee. Annals Math., vol. 9, 1894, pp. 31-41. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 55, pp. 41-50.

Discussion of the observations of Jupiter, with resulting values for the elements of the orbit and the mass of Saturn. Astron. Papers, Amer. Ephemeris, vol. 7, 1895, pp. 5-22. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 56, pp. 51-64.

Discussion of the observations of Saturn, with resulting values for the elements of the orbit and the masses of Jupiter and Uranus. Astron. Papers, Amer. Ephemeris, vol. 7, 1895, pp. 147-167. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 57, pp. 65-77.

The periodic solution as a first approximation in the lunar theory. Astron. Journ., vol. 15, 1895, pp. 137-143. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 58, pp. 78-93.

On the convergence of the series used in the subject of perturbations. Bull. Amer. Math. Soc., vol. 2, 1896, pp. 93-97. Carnegie Inst. Publ. No. 9, vol. 4, 1907, pp. 94-98.

Remarks on the progress of celestial mechanics since the middle of the century. Pres. address delivered before the American Mathematical Society, December 27, 1895. Bull. Amer. Math. Soc., ser. 2, vol. 2, 1896, pp. 125-136. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 60, pp. 99-110.

Jupiter-perturbations of Ceres, of the first order, and the derivation of the mean elements. Astron. Journ., vol. 16, 1896, pp. 57-62. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 61, pp. 111-122.

On the values of the eccentricities and longitudes of the perihelia of Jupiter and Saturn for distant epochs. Astron. Journ., vol. 17, 1897, pp. 81-87. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 62, pp. 123-135.

On intermediary orbits in the lunar theory. Astron. Journ., vol. 18, 1897, pp. 81-87. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 63, pp. 136-149.

Note on the mass of Mercury. Astron. Journ., vol. 19, 1898, pp. 157-158. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 64, pp. 150-152.

On the inequalities in the lunar theory strictly proportional to the solar eccentricity. Astron. Journ., vol. 20, 1899, pp. 115-124. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 65, pp. 153-168.

On the extension of Delaunay's method in the lunar theory to the general problem of planetary motion. Trans. Amer. Math. Soc., vol. 1, 1900, pp. 205-242. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 66, pp. 169-206.

Ptolemy's problem. Astron. Journ., vol. 21, 1900, pp. 33-35. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 67, pp. 207-211.

Normal positions of Ceres. Astron. Journ., vol. 21, 1900, pp. 51-54. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 68, pp. 212-218.

Secular perturbations of the planets. Amer. Journ. Math., vol. 23, 1901, pp. 317-336. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 69, pp. 219-235.

On the use of the sphero-conic in astronomy. Astron. Journ., vol. 22, 1901, pp. 53-56. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 70, pp. 236-243.

Illustrations of periodic solutions in the problem of three bodies. Astron. Journ., vol. 22, 1902, pp. 93-97, 117-121. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 71, pp. 244-261.

On the application of Delaunay transformations to the elaboration of the secular perturbations of the solar system. Astron. Journ., vol. 22, 1902, pp. 183-189. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 73, pp. 262-274.

Examples of periplegmatic orbits. Astron. Journ., vol. 24, 1904, pp. 9-14. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 74, pp. 275-286.

The theorems of Lagrange and Poisson on the invariability of the greater axes in an ordinary planetary system. Astron. Journ., vol. 24, 1904, pp. 27-29. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 75, pp. 287-292.

Comparison of the new tables of Jupiter and Saturn with the Greenwich observations of 1889-1900. Astron. Journ., vol. 24, 1904, pp. 60, 61. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 76, pp. 293-295.

Development of functions in power series from special values. Astron. Journ., vol. 24, 1904, pp. 123-128. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 77, pp. 296-307.

Deduction of the power series representing a function from special values of the latter. Amer. Journ. Math., vol. 27, 1905, pp. 203-216. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 78, pp. 308-319.

Integrals of planetary motion suitable for an indefinite length of time. Astron. Journ., vol. 25, 1905, pp. 1-12. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 79, pp. 320-344.

Application of the Delaunay transformation in the planetary theories. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 80, pp. 345-391.

Remarks supplementary to Memoir No. 79. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 81, pp. 392-397.

Development, in terms of the true anomaly, of odd negative powers of the distance between two planets moving in the same plane. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 82, pp. 398-407.

On the construction of maps. Carnegie Inst. Publ. No. 9, vol. 4, 1907, Mem. No. 83, pp. 408-418.

Dynamic geodesy. Carnegie Inst. Publ. No. 9 vol. 4, 1907, Mem. No. 84, pp. 419-452.

PAPERS BY GEORGE WILLIAM HILL, NOT INCLUDED IN HIS COLLECTED MATHEMATICAL WORKS, PUBLISHED BY THE CARNEGIE INSTITUTION OF WASHINGTON.

Tables of Jupiter. Astron. Papers, Amer. Ephemeris, vol. 7, pt. 1, 1895, pp. 1-144.

Tables of Saturn. Astron. Papers, Amer. Ephemeris, vol. 7, pt. 2, 1895, pp. 147-285.

Attraction of the homogeneous spherical segment. Amer. Journ. Math., vol. 29, 1907, pp. 345-362.

Subjective geometry. Bull. Amer. Math. Soc., ser. 2, vol. 14, 1908, pp. 305-313.

The Jovian evection in the lunar theory. Astron. Journ., vol. 25, pp. 193-196, 1908.

Motion of a system of material points under the action of gravitation. Astron. Journ., vol. 27, pp. 171-182, 1913.

The secular perturbations of the four outer planets. Astron. Journ., vol. 28, pp. 59-71, 1913.

Hypergeometric series and Walker's tables of the Leverrier coefficients. Astron. Journ., vol. 28, pp. 93-100, 1914.