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GEORGE WILLIAM HILL.

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With the death of Dr. George William Hill, on April 16, 1914, this country lost one of its most eminent men of science. For nearly forty years he was regarded by competent judges, both in Europe and in America, as one of the few great masters of Celestial Mechanics. Academies and scientific societies had honored him with medals and universities had granted him their degrees. Men of the greatest reputation, such as Poincaré and Darwin, had confessed their indebtedness to him.

Dr. Hill was a scientific man of the old school. He was retiring and modest to the verge of timidity. He was absorbed in his own work but never inflicted it on others. In fact, he would hardly discuss it when others desired him to do so. He never made himself prominent at meetings of scientific societies, and never appeared in the popular magazines. He seemed altogether indifferent to his reputation with all except a few high authorities, and he knew he did not need to give any thought regarding his reputation with them. As he never married, he had neither the satisfaction nor the distraction which a family brings. He was, in short, a man of exceptional abilities who found keen pleasure in devoting a long and serene life to the pursuit of science.

Dr. Hill, of English-Huguenot descent, was born in New York City on March 3, 1838. Consequently he was more than seventy-six years of age at the time of his death. He graduated from Rutgers College in 1859 at the age of twenty-one years. While Rutgers College is not one of the more prominent American institutions of learning, it then had on its faculty a scholarly man, Dr. Theodore Strong, who exercised an important influence on Hill's life.* In some brief autobiographical

* For these facts I am indebted to an excellent biographical sketch of Dr. Hill published in the *Astronomical Journal*, No. 668, by President Robert S. Woodward, who was long his acquaintance and friend.

notes Hill said: "Having shown some aptitude for mathematics it was decided to send me to college; and, in October, 1855, I took up residence at Rutgers College, New Brunswick, N. J. Here I found Dr. Theodore Strong, professor of mathematics, who was a friend of Dr. Nathaniel Bowditch, the translater of Laplace's *Mécanique Céleste.* I remember seeing in Dr. Strong's library the presentation copy of this work. Under his guidance, I read such books as LaCroix, *Traité du Calcul Différential et Intégral;* Poisson, *Traité de Mécanique;* Pontécoulant, *Théorie Analytique du Système du Monde;* Laplace, *Mécanique Céleste;* Lagrange, *Mécanique Analytique;* Legendre, *Fonctions Elliptiques.* My professor was an old fashioned man; he liked to go back to Leonard Euler for all his theorems; as he said, 'Euler is our great master'. He scarcely had a book in his library published later than 1840."

In this brief sketch of Hill's college work there is a wealth of material for reflection. In the first place, it shows what fine and unobtrusive scholars are sometimes found in relatively obscure places, and how great their influence may be on budding genius. Note how Dr. Strong guided Hill into reading the works of the masters. How many of us who have attended small colleges have had a course which could at all compare with that taken by Hill? Or, how many who now are taking work in our leading institutions are directed to so good sources of information and inspiration? It is not to be supposed that all students at Rutgers College in Hill's day read Poisson, Lagrange and the others; but this one example proves that Dr. Strong was a scholar and that he recognized unusual ability. His high estimate of Euler, who is universally regarded by competent judges as one of the greatest mathematicians who ever lived, proves that he had an instinct for what is really good. And this leads me to say that the best way to master a subject is to go to the highest sources, though no one would now limit his library to books published before 1840. In some way the real creators of a subject put into their work a certain life and inspiration that are never obtained by commentators.

In the second place, Hill's autobiographical notes prove that, even as a boy, he had exceptional mathematical ability. There are but few undergraduates in any institution who would voluntarily read such authors as Poisson, Lagrange, and Laplace. But to those who have the taste for such things the elegant writings of Lagrange are poetry and the dazzling mathematical operations of Laplace are a never ceasing source of wonder. There can be no doubt that Hill was greatly benefited by going so largely to original sources. He was naturally independent, and his contact with great independent minds cultivated this talent. Euler, Lagrange, and Laplace were not men who, travelling in beaten paths, simply went a little farther than their predecessors; they were men who viewed the distant goal from the heights and made their own roads to it. And Hill was a remarkable man in the same way. The strongest impression I got when I began to read his work was that here, too, was an absolutely independent spirit. He gave new points of view and new methods of attack to almost everything he touched.

Shortly after graduation from college Hill was given a position in the Nautical Almanac office, then at Cambridge, Mass., but removed to Washington in 1867. He remained at this work until he reached the retiring age. He faithfully performed the routine duties incident to his position, but for more than thirty years he spent his leisure hours on the more difficult parts of Celestial Mechanics. Hill and Newcomb placed American mathematical astronomy on the same high plane that observational astronomy has always occupied in this country. In 1905-1907 Hill's Collected Mathematical Works were published by the Carnegie Institution of Washington in four quarto volumes containing together 1735 pages, and one volume of the researches which he has made since the earlier ones were printed remains to be issued. Hill is another of the splendid examples Astronomy has so many times offered of men whose intellectual activity has continued almost without decline to the end of their lives.

Hill's work included nearly every branch of Celestial Mechanics, and obviously it is impossible to give any adequate idea of all of it. Consequently I shall limit myself largely to his rescarches on the lunar theory, which contain the most splendid examples of his originality and rare powers of invention. Moreover, this is the work which gave him his greatest reputation and which he himself considered as his most important contribution to science.

Every one knows that one of the most famous problems in mathematical astronomy is the Problem of Three Bodies. The motion of the moon around the earth as disturbed by the sun is a special but very important case of this problem. Its discussion started with Newton, in 1686, and now after a lapse of over two hundred years and the work of a great number of mathematicians it can not be regarded as completely solved, both from the practical and the logical point of view. In the discussion of this problem there are four principal epochs, the fourth of which is due to the work of Hill.

One of the applications of the law of gravitation, made by Newton, was to the explanation of the motion of the moon. It had long been known that there is a regression of the nodes of the moon's orbit, an advance of the perigee, a periodic perturbation known as "the variation", another still larger one called "the evection", and numerous smaller irregularities. If the law of gravitation is true it will not only explain the conic section motion of the planets, but also the deviations from the elliptic motion of the moon due to the perturbing action of the sun.

Newton recognized the fact that the motion of the moon gave a critical test of the law of gravitation. He also recognized its great difficulties. But this did not deter him from attempting to solve the problem of determining the effects of the attraction of the sun. In Section XI, Book I, of the *Principia* he gave a masterly analysis of the problem in what Sir George Airy said was "the most valuable chapter that was ever written on physical science."

The method of Newton was to regard the moon's orbit as an ellipse, but one which is continually changing. It is easy to see that, since if at any time the sun's attraction were henceforth to cease to act the moon would from that instant move in a fixed ellipse about the earth, the actual motion may be regarded as taking place in an ellipse whose elements vary constantly. Newton by a very skilful use of geometrical considerations and numerical approximations obtained a good first approximation to the way in which the elements vary. It should be noted, however, that his results, as published in the *Principia*, were much in error for the motion of the perigee; but in unpublished papers first brought to light in 1872 this discussion was supplemented by another which gave nearly the true value.

The second period in the lunar theory is that following Newton when continental mathematicians, and particularly the French, took up the task of applying the more powerful methods of analysis instead of the special geometrical ones which the English still used. It reaches from the work of Clairaut, d'Alembert, and Euler, beginning about 1745, down to that of Delaunay about one hundred years later. To this period belongs the work of Laplace, Poisson, Plana, Lubbock, de Pontécoulant, and others. It is not to be inferred that there were not differences in the treatments of these various writers. To all external appearances the various theories were quite distinct. But the fundamental point of view of all was the same, the differences consisting largely in the choice of variables and parameters and the modes of procedure in obtaining the coefficients.

The third period in the history of the lunar theory is that due to the work of Delaunay which was published in 1860. He carried to its logical end the process of considering the elements of the moon's orbit as varying. Suppose the undisturbed elliptic orbit of the moon is caused to vary by the attraction of the sun. The solution will depend upon certain constants, equal in number to the elements of the orbit, which specify how the elements are varying, But if the moon departs from its elliptic orbit by the variations just considered the disturbing force of the sun is changed, and it is necessary to cause to vary the constants which were introduced at the first stage of the solution.

These deviations are specified by a new set of constants which give rise to a new set of disturbing forces, and so on in an unending series. Delaunay developed a systematic process for varying first the elements, and then the equivalent constants at successive steps, and carried out the details to an extent never before approached. The development of the final literal series for the three coördinates of the moon took twenty years, and several hundred quarto pages were required to print them. The death of Delaunay terminated his work before he succeeded in reducing his series to numbers. The method he used is one of great elegance which Hill especially has shown is conveniently applicable to many other problems.

The fourth period in the history of the lunar theory dates from Hill's publication, in 1877, of his memoir "On the Part of the Motion of the Lunar Perigee which is a Function of the Mean Motions of the Sun and Moon." This paper was republished in *Acta Mathematica*, Vol. VIII (1886). But it is logically preceded by the memoirs which were published in 1878 in Vol. I of the *American Journal of Mathematics.*

Let us first consider the memoirs published by Hill in the *American Journal of Mathematics.* In the introduction he explains how he is dividing up the problem, and at this time he intended to give it a complete treatment, for he said: "A general method will also be given by which these investigations may be extended so as to cover the whole ground of the lunar theory. My methods have the advantage, which is not possessed by Delaunay's that they adapt themselves equally to a special numerical computation of the coefficients, or to a general literal development. The application of both procedures will be given in each of the just mentioned five classes of inequalities, so that it will be possible to compare them." As a matter of fact, Hill treated only the first two of the five divisions of the subject.

In the papers published by Hill in the *American Journal of Mathematics* there is introduced for the first time a very radical and important idea. Up to this time the orbits of the moon and planets were considered as being ellipses which continually change. The problem was to find the changes in the ellipses, or the deviations from the initial ellipses. That is, the ellipse was taken as a first approximation to the orbit of the body under consideration. Hill proposed to take a certain simple type of periodic orbit as a first approximation. He proved the existence of the periodic orbits by numerically integrating the differential equations in numerous special cases by a process known as mechanical quadratures. These were the first periodic orbits of the problem of three bodies having a practical use, and the first ones known to exist beyond the simple ones which were discovered by Lagrange. It should be added that Hill omitted a small part of the disturbing action of the sun, viz., that which is said to depend upon the solar parallax; but his method would have applied without sensible modification to the rigorous problem. In fact, in all his researches on the problem of three bodies, Darwin used methods which differ from those of Hill only in the variables employed and in inconsequential details.

Having proved by computation the existence of periodic orbits, the problem was to devise mathematical expressions for representing them. This Hill did with extraordinary skill and elegance. As he stated, his methods were adapted either to a literal or to a numerical treatment. He carried them out in both ways. I once heard a prominent astronomer say what seems to be a common impression among astronomers, viz., that Hill's work was purely theoretical. Nothing could be farther from the truth. No earlier work had approached it in practical applicability, and no subsequent work has surpassed it. For example, he gave the coefficients of the periodic orbit which was to serve as a first approximation to the orbit of the moon to fifteen decimals, and worked out all terms which were sensible with this degree of accuracy.

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

This was a remarkable statement to have been written in this country in 1878. It shows a clear understanding of all that was known at the time of the subject of the convergence of solutions of differential equations in series. Hill had a point of view and familiarity with the literature altogether beyond any other astronomer in this country, and it is questionable if he was then surpassed by any mathematician. Since Hill's work there have been few attempts at proving the convergence of the series used in the lunar theory, and no real progress in the matter has been made. For the most part the question has been entirely ignored. Yet, as Hill indicated, the lack is a real defect, and mathematicians will not abandon the subject until it is fully remedied; for, if the series are not known to converge, what seems to be a solution of the problem may be an illusion. It should be remarked that the series appear to converge rapidly, they do converge (as I have proved) for a moon whose period is not too great, and there is nothing to make us suspect they do not converge for our satellite. The problem is one of great difficulty, and there is no hope whatever of solving it except by the use of such powerful methods as those Cauchy introduced in his theory of functions of a complex variable.

Hill wrote a number of later articles, published in the *Astronomical Journal*, on periodic orbits. They are all of a high order of merit, but in spirit and method do not surpass his original work, and they show no influence of the epoch-making researches of Poincaré. On the whole they are of a practical character, and one was for the purpose of developing a theory for the orbit of Saturn's satellite Hyperion.

Hill's memoir in *Acta Mathematica* is generally regarded as his most brilliant work. He so regarded it himself. The purpose of the paper was, supposing that at a given instant the coördinates and components of the moon's velocity in the plane of the ecliptic differ slightly from what they would be in the periodic orbit which he had found, to find the character of the deviations about the periodic orbit. It is not at once clear how this is attached to the lunar perigee and to the eccentricity of the moon's orbit. As a matter of fact, there is no eccentricity to the moon's orbit in the ordinary sense, and writers have differed as to what they have called its eccentricity. But there is a real physical idea involved, and in Hill's work it comes out in an altogether new way.

In discussing the deviations from the periodic orbit Hill was led to a fourth order system of linear differential equations having periodic coefficients. The solutions of such equations had never been treated. By means of the energy integral he reduced the system of equations to a single equation of the second order. He then inferred the character of the solution by analogy with a corresponding differential equation having constant coefficients, and from previous work on the lunar theory. The solution involved an unknown exponent c, which was to be determined, and the coefficients of an infinite number of terms. When the series with undetermined coefficients were substituted in the differential equation the condition that it should be satisfied led to an infinite series of linear homogeneous equations, each involving an infinite number of the unknown coefficients. The coefficients of these equations involved the unknown exponent *c*. If the number of equations is finite and the number of unknowns which occur linearly and homogeneously is equal to the number of equations, the condition that there may be a solution which is not zero is that the determinant of the coefficients shall be zero. Is the same thing true for an infinite system? Hill assumed it is true and thus introduced the infinite determinant. Each of its lines involved *c*. Hence it might be expected that the equation would be satisfied by infinitely many values of *c*. Such is the case, but the various values are simply related to a certan principal value. Hill found these relations by a course of brilliant reasoning and showed how to get the principal value. In discussing this determinant he made his loftiest flight. Moreover, he carried out the whole work numerically (and later literally) and gave the rate of motion of the moon's perigee so far as it depends upon the first power of the eccentricity of the moon's orbit to fifteen decimals, an accuracy never before approached and fully sufficient for all purposes.

Hill's work on the motion of the lunar perigee has been of great importance. It introduced into mathematics infinite systems of linear equations, a subject which is just now being cultivated by some of the most eminent mathematicians in the world. It called attention to the infinite determinant which has been the subject of memoirs by von Koch and Poincaré. The methods of Hill were used by Darwin without essential modification in determining the character of the stability of the periodic orbits which he discovered. In insisting that Darwin used the methods of Hill both in discovering periodic orbits and in examining their stability, it is not my purpose to take from him any of the large amount of credit which is his due for the enormous amount of work that he did; but it is desired to point out that in all essential respects Hill was the pioneer.

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 for the motions of the planets which should 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 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. In the mean time E. W. Brown, who had graduated at Cambridge, England, and had attracted the very favorable attention of Darwin, became interested in the lunar theory and recognized the great superiority of Hill's methods. In his first paper, published in the *American Journal of Mathematics* in 1892, he treated the fifth of the divisions into which Hill had separated the problem; and the methods he employed were so exactly those of Hill that Hill could not have treated the question without duplicating the work. However, it is not to be understood that this was done without Hill's consent; it simply explains why Hill never wrote on this phase of the subject.

A little later Brown took up the problem of developing a lunar theory on the general basis that Hill had laid down. While Hill had conquered the most important difficulties, many still remained and the task was one requiring enormous labor. The problem has been entirely completed by Brown in the most satisfactory manner. While his work is above criticism we can not but regret that we have not also the treatment of the subject that Hill might have given. In this connection I may translate from Poincaré's introduction to the first volume of Hill's Collected Works a few sentences: "It is on these (Hill's) principles that the new theory of Brown has been founded. This theory is much more perfect than all others hitherto known, and there are grounds for the hope that it will permit us to push the approximation much farther than it has been carried by Hansen and Delaunay. It would be unjust not to recognize the personal part that Brown has taken in this great work, and the originality of the ideas that he himself has brought to it. But it would be more unjust still to forget that it is Hill who has laid down the principles; that he has vanquished the first difficulties and that these difficulties were the greatest ones." Poincaré also expressed the opinion that the methods of Hill were capable of quite other applications than to the theory of the moon. and he said, "When they are extended to a domain more vast, we must not forget that it is to Hill that we owe an instrument so precious."

SHOOTING STARS.

CHARLES NEVERS HOLMES.

As a rule, the appearance and disappearance of so-called "shooting stars"—the sudden flash and more or less rapid vanishing of such swiftly moving aerolites—are very uncertain. Indeed, there are few times in the year when one can be sure that a larger number of these "stars" will fall; and, as a rule, he will watch quite a while before a single spectral "star" comes and goes. There are, however, a few times when he may be fairly certain of a larger exhibition of "shooting stars". On some of the nights of April, August and November, there are, as a rule, more of such aerolites flashing across the darkened firmament, and, occasionally, once in every thirty-three years, for example, one may witness a remarkable display of "shooting stars."

As a rule, such "stars" seen throughout the night are few and scattered. Now and then, after perhaps a half-hour's watching, there will come a sudden flash—more or less bright—and it may be that another such flash will follow soon; but appearances of aerolites upon most clear nights are rather few and far between. But around April 19-22, August 9-11, November 12-14 and 27, there are very likely to be excellent exhibitions of "shooting stars." That is, on these dates, our little planet, revolving around the sun, enters or crosses certain regions more closely filled with aerolites. As a result, a larger number of these little particles are captured by the attraction of the earth and are destroyed by their friction with the terrestrial