SIMON NEWCOMB, 1835—1909.

Simon Newcomb was born in Nova Scotia in 1835, at Wallace, a pretty village at the mouth of the river of that name. His father was a country school teacher, a nomadic profession in a thinly-populated district. He was the most rational and most dispassionate of men. Newcomb in his autobiography (which will be freely quoted in this notice) tells us that his father had learned from careful study "that the age at which a man should marry was twenty-five. A healthy and well-endowed offspring should be one of the main objects in view in entering the marriage state, and this required a mentally-gifted wife. She must be of different temperament from his own, and an economical housekeeper. So when he found the age of twenty-five approaching he began to look about. There was no one in Wallace who satisfied the requirements.

"He therefore set out afoot to discover his ideal. In those days and regions the professional tramp and mendicant were unknown, and every farmhouse dispensed its hospitality with an Arcadian simplicity little known in our times. Wherever he stopped overnight he made a critical investigation of the housekeeping, perhaps rising before the family for this purpose. He searched in vain until his road carried him out of the province. One young woman spoiled any possible chance she might have had by lack of economy in making the bread. She was asked what she did with an unnecessarily large remnant of dough which she left sticking to the sides of the pan. She replied that she fed it to the horses. Her case received no further consideration.

"The search had extended nearly a hundred miles when, early one evening, he reached what was then the small village of Moncton. He was attracted by the strains of music from a church, went into it, and found a religious meeting in progress. His eye was at once arrested by the face and head of a young woman playing on a melodeon, who was leading the singing. He sat in such a position that he could carefully scan her face and movements. As he continued this study, the conviction grew upon him that here was the object of his search. That such should have occurred before there was any opportunity to inspect the dough-pan may lead the reader to conclusions of his own. He inquired her name—Emily Prince. He cultivated her acquaintance, paid his addresses, and was accepted." "My mother was the most profoundly and sincerely religious woman with whom I was ever intimately acquainted, and my father always entertained and expressed the highest admiration for her mental gifts, to which he attributed whatever talents his children might have possessed. The unfitness of her environment to her constitution is the saddest memory of my childhood. More I do not trust myself to say to the public, nor will the reader expect more of me."

His early years were passed amid social conditions of the utmost simplicity.

"The women sheared the sheep and made the clothes, but any man who allowed wife or daughter to engage in heavy work outside the house would have lost caste."

As a child, Newcomb was precocious in arithmetic, doing extraordinary calculations for his years with the assistance of a napped counterpane.

He was never known to deviate from the truth in one single instance either in infancy or youth. This high praise comes from his father, who adds a little later: "You were uncommonly deficient in that sort of courage necessary to perform bodily labour. Until nine or ten years of age you made a most pitiful attempt at any sort of bodily or, rather, handy work."

He was an omnivorous reader, and a very careful one, never passing a word that he did not understand.

Among his neighbours he acquired a reputation for learning that he felt was not appreciated, while he was painfully conscious of his inability to drive oxen. And he says: "My boyhood was, on the whole, one of sadness." At the age of sixteen Newcomb almost decided upon the trade of a carpenter, but at the last moment he was apprenticed for five years to a certain Doctor Foshay, who turned out to be a quack. While he was with the doctor: "A book peddler going his rounds offered a collection of miscellaneous books at auction. I bought, among others, a Latin and a Greek grammar, and assiduously commenced their study. With the first I was as successful as could be expected under the circumstances, but failed with the Greek, owing to the unfamiliarity of the alphabet, which seemed to be an obstacle to memory of the words and forms."

At the end of two years he ran away and worked his passage on board ship to Salem.

The year 1854 was spent as teacher in a country school. In 1855 he got a better position of the same character at Sudlersville.

The next year he taught in the family of a planter named Bryan, some fifteen or twenty miles from Washington. His first visit to the capital had been in 1854, but now they became frequent. In 1856, in the Smithsonian Library he first saw Laplace's "Mécanique Céleste." "About December, 1856, I received a note from [Mr. J. E. Hilgard, assistant in charge of the Coast Survey Office], stating that he had been talking about me to Prof. Winlock, Superintendent of the 'Nautical Almanac,' and that I might possibly get employment on that work. When I saw him again I told him that I had not yet acquired such a knowledge of physical astronomy as would be necessary for the calculations in question; but he assured me that this was no drawback, as formulæ for all the computations would be supplied me. I was far from satisfied at the prospect of doing nothing more than making routine calculations with formulæ prepared by others; indeed, it was almost a disappointment to find that I was considered qualified for such a place. I could only console myself by the reflection that the ease of the work would not hinder me from working my way up." The result was that one frosty morning in January, 1857, he took his seat in the office of the "Nautical

Almanac," at Cambridge, Mass. He was then in his twenty-second year. From this time onwards his career was one of unchequered brilliancy. In 1860, he went on an eclipse expedition up the Saskatchewan River. The weather was cloudy and nothing was seen of the eclipse.

In 1861, he was appointed Professor of Mathematics in the United States Navy, and as such he commenced transit instrument work at Washington Observatory on October 7.

Although he had been on an eclipse expedition in the previous year, he had never been inside an observatory, except on two or three occasions at Cambridge as a visitor. In September, 1863, he took charge of the mural circle. At this time it was usual at Washington for each observer to reduce his own observations. Newcomb contrived to introduce a uniform system of reduction in imitation of the system already introduced by Airy at Greenwich.

In October, 1865, the new transit circle arrived from Berlin. In the following years Newcomb succeeded in eradicating a vicious practice that obtained not only at his own observatory, but all over the world. He pointed out that clock stars ought only to be kept for place when at least a twelve-hour group has been obtained. For if an error depending on the sine or cosine of the right ascension exists in the clock star list, and observations only extend over six hours, the same error will be reproduced with only an infinitesimal degree of damping; whereas with twelve-hour groups the error is quickly damped out. In 1869, he observed an eclipse in Iowa, and in that year he began to turn his earnest attention to the problems presented by the moon's motion.

In 1870 he visited Europe for the first time, partly for an eclipse at Gibraltar that was obscured by cloud, and partly to search through the records of various observatories for seventeenth century observations of the moon.

In 1875 Newcomb was offered and declined the directorship of Harvard Observatory. On September 15, 1877, he took charge of the Nautical Almanac Office, a post which he held until his retirement in 1897.

The beginning of Newcomb's astronomical career coincided with the publication of Hansen's tables of the moon. These tables were an enormous advance on those previously in existence. Hitherto errors in computed coefficients had in many instances exceeded two or even three seconds of arc. Hansen's tables contain two or three errors in computed coefficients exceeding half a second of arc, but as a rule he attains a far greater accuracy.

The chief defect of the tables lies in the determination of the arbitrary constants, and in the omission of a whole group of planetary terms, the existence of which was not then suspected. A passage in Newcomb's autobiography throws much light on the cause of the former defect.

Hansen worked with the assistance of one computer only; this was no hardship while he was engaged on his theory. It would, in fact, require some management to assign work to a much larger staff simultaneously.

But when he came to compare his theory with observation and to determine his arbitrary constants, he was exceedingly short-handed. Instead, therefore, of making a detailed comparison with all existing observations, he based his comparison on a few years only. The result was utterly unworthy of his great theory. His parallactic coefficient is two seconds in error, his principal elliptic term half a second in error, and so on. He also postulated a mechanical ellipticity in the moon's figure at least four times too large. With all these defects his tables mark an enormous advance, and his contemporaries believed that "our troublesome satellite has been at length reduced to order."

At the end of Newcomb's career the theory of E. W. Brown, which is to replace Hansen's, is complete, and tables based upon it are in the course of preparation. The advance upon Hansen will be greater than Hansen's advance upon his predecessors, and yet no one believes that the problem of the moon is solved.

Newcomb was in touch with all the work done in this period of fifty years, and great portions of this work were done by himself. His first investigation connected with the moon was a redetermination of the elliptic elements of the moon's orbit.

In this paper he brought to light an empirical term that is now known as the Jupiter evection term. It manifested itself as a fluctuation in the moon's eccentricity and perigee with a period of seventeen years. Some years earlier Airy had analysed eighty years of observations, and had been almost within touch of this term, but had wrongly identified the period as that of the moon's node, nineteen years.

Newcomb at this time did the bulk of his own computing, and to this fact his superior success is plainly due.

The explanation of the term was quickly assigned by Nevill to the action of Jupiter. At a time when most astronomers hardly realised that Hansen's tables needed correction, Nevill was being the pioneer in a new branch of the lunar theory.

The question of planetary inequalities was subsequently taken up by G. W. Hill, by Radau, by Newcomb himself and by E. W. Brown. It may now be considered as worked out. At this point we may notice one other contribution of Newcomb's to the gravitational theory of the moon, viz.: a beautiful theorem for obtaining the secular accelerations resulting from the secular diminution of the eccentricity of the earth's orbit round the sun.

Hansen, like Laplace, had assigned 12 seconds as the secular acceleration of the moon's mean motion; Adams had shown that this quantity was twice too large; but Adams' accuracy was not immediately admitted.

Some years later Newcomb produced his theorem, and again quite recently E. W. Brown, with the help of Newcomb's theorem, has practically reproduced Adams' value, which by that time was generally accepted, in a paper so short and simple that one wonders how there could ever have been any controversy on the subject.

We turn now to Newcomb's work of comparison of observations with theory.

His great work entitled "Researches on the Motion of the Moon" secured for its author the Copley Medal of the Royal Society.

In the first section he considers the ancient and mediæval eclipses. He rejects the solar eclipses, he ignores the magnitudes of lunar eclipses; and he shows the times of the lunar eclipses to be fairly consistent among themselves and with a secular acceleration slightly greater than the theoretical.

He assigns the excess to tidal friction. This no doubt is a *vera causa*, but there is at present no independent measure of its magnitude. In the concluding part of the "Researches" he gives the results of occultations observed in the century preceding 1750. These occultations were not only worked up by Newcomb, but actually extracted by him from the archives of European observatories. He has since extended his series of occultations down to 1898 and the results were published early this year. The older occultations required immense diligence. The observers' hieroglyphics had to be collected in many cases, in order to decipher their meaning. Clock errors had to be obtained in any way that was possible. Finally taking all the other quantities as known, a somewhat rough determination of the moon's mean error of longitude is obtained. From that it appears that in the moon's observed motion there exists a term unknown to theory with a period of about three hundred years.

To Newcomb, and to him alone, we owe such knowledge as we have of the moon's motion in the century preceding 1750. In his autobiography, Newcomb says: "One curious result of this work is that the longitude of the moon may now be said to be known with greater accuracy through the last quarter of the seventeenth century than during the ninety years from 1750 to 1840." The reductions for the latter period leave very much to be desired, but Newcomb's remark is too drastic. For instance, Newcomb has with his occulations traced an empirical term of sixty years' period back to 1820; before that date it is lost in the accidental error of his material. That term can be traced in the meridian observations back to 1750.

Newcomb's 'Researches' also contain the first recognition of the error of Hansen's mean motion of the moon's node. He deduces his correction by a comparison of an eclipse of 1715 with transit observations in or about 1868. Although the time interval is large, the position of the node on the first occasion is subject to much uncertainty.

The exact measurement of this motion is of great interest in view of the discrepancy from theory exhibited by the perihelion of Mercury.

Turning now to planetary theory, Newcomb's first paper was an investigation on the orbits of minor planets, with the object of ascertaining whether an explosion of a single planet could be assigned as their origin.

If such an explosion really took place, and if all secular changes affecting asteroids were already recognised, it would be possible to assign the place and time of the catastrophe; and the date, if obtained at all, would be obtained with an exactness unparalleled in other speculations as to the past history of the universe. Unfortunately, Newcomb's conclusions were negative.

At this time Leverrier was still working out his theory of the larger planets, going outwards from the sun. He had not yet reached Uranus and Neptune, so Newcomb took up the orbits of these two planets, and also of their satellites, in order to determine their masses.

He also made a series of observations for this special purpose, and his work was rewarded with the Gold Medal of the Royal Astronomical Society.

Before the close of his life Newcomb had constructed tables for all the larger planets, and in addition for the minor planet Polyhymnia, in order to determine the mass of Jupiter. G. W. Hill relieved him of "about the most difficult [part] in the whole work—the theory of Jupiter and Saturn. Owing to the great mass of these 'giant planets,' the inequalities of their motion, especially in the case of Saturn, affected by the attraction of Jupiter, are greater than in the case of the other planets.

"Leverrier failed to attain the necessary exactness in his investigation of their motion. . . . [G. W. Hill] laboured almost incessantly for about ten years when he handed in his manuscript of what now forms Volume IV of the 'Astronomical Papers.'"

Newcomb followed Leverrier's methods in essentials. "Two systems of computing planetary perturbations had been used, one by Leverrier, while the other was invented by Hansen. The former method was, in principle, of great simplicity, while the latter seemed to be very complex and even clumsy. I naturally supposed that the man who computed the direction of the planet Neptune before its existence was known must be a master of the whole subject, and followed the lines he indicated.

"I gradually discovered the contrary, and introduced modified methods, but did not entirely break away from the old trammels.

"Hill had never been bound by them, and used Hansen's method from the beginning. Had he given me a few demonstrations of its advantages I should have been saved a great deal of time and labour."

Possibly in order that his own work, regarded as a verification of Leverrier's, might be quite independent, Newcomb introduced some changes into the calculations.

Leverrier, for instance, used the mean longitudes of the planets. Newcomb used the mean anomalies. Leverrier develops algebraically, according to the mean angles, and then reduces to arithmetical values. Newcomb develops algebraically in eccentric anomalies, reduces to number, and then transforms to mean anomalies arithmetically in each separate case. In a later volume he gives algebraic formulæ for the mean anomalies. He has enormously improved Leverrier's notation by introducing an operator that he terms "D."

When the final perturbations are compared, we are struck by how little Leverrier left for his successors. When allowance is made for the difference in the assumed masses of the planets, the difference in the perturbations calculated by Leverrier and Newcomb respectively are not such as could be detected by observation. Newcomb has, however, improved the arbitrary constants, he has used the same planetary masses throughout all the tables, and finally he has enormously reduced the labour required for an ephemeris by following the methods used by Hansen for the moon.

Like the lunar theory, the planetary theory is not yet perfect. The principal outstanding problems are:—

(i) A centennial motion of forty seconds in the perihelion of Mercury, and
(ii) the orbit of Mars.

Since Leverrier's time, the adopted solar parallax has increased by nearly three per cent. and consequently the mass of the earth by eight per cent.

Mars is the planet whose tabular orbit is most affected by an erroneous mass of the earth; but although Newcomb was able to avail himself of the more accurate value of the mass of the earth, his tables of Mars are far less satisfactory than any other of his tables, and the problem has not yet been solved.

The discordance from theory in the motion of the perihelion of Mercury had been discovered by Leverrier from a discussion of the transits of Mercury.

Newcomb went over the ground again, with a little added material, and asking an additional question, "Are the errors of Mercury so related to those of the moon as to suggest that the earth is not a perfect timekeeper?" he found that he was not able to assert that such a relation existed.

Newcomb's Fundamental Catalogue and his "Astronomical Constants" must be mentioned, as well as his determination of Precession. These are great works in themselves, but to Newcomb mainly incidents in the thorough discussion of the motion of the moon and of the planets.

It was Newcomb also who assigned the lengthened period of latitude-variation to want of rigidity in the earth.

Newcomb visited Europe for the last time in 1908. Soon after his return his friends heard that he was hopelessly ill. He still continued his interest in his work, and passed through the press his last paper on the Moon.

He died on July 16, 1909, at the age of seventy-four. Twenty-two years had he spent in darkness before he became an astronomer, and subsequently the congenial nature of his work made the world for him one of "sweetness and light."

P. H. C.