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ADDRESS DELIVERED BY [PROFESSOR CAYLEY AS] THE PRESIDENT [OF THE ROYAL ASTRONOMICAL SOCIETY] ON PRESENTING THE GOLD MEDAL OF THE SOCIETY TO PROFESSOR SIMON NEWCOMB.

[From the *Monthly Notices of the Royal Astronomical Society*, vol. XXXIV. (1873—1874), pp. 224—233.]

THE Council have awarded the medal to Professor Simon Newcomb for his Researches on the Orbits of *Neptune* and *Uranus*, and for his other contributions to mathematical astronomy. And upon me, as President, the duty has devolved of explaining to you the grounds of their decision.

I think it right to remark that it appears to me that, in the award of their highest honour, the Council of a Society are not bound to institute a comparison between heterogeneous branches of a science, or classes of research—to weigh, for instance, mathematical against observational astronomy or astronomical physics; or, in the several branches respectively, the happy idea which originates a theory against the patience and the skilled labour which develope and carry it out; and still less to decide between the merits of different workers in the science. It is enough that the different branches of a science coming before them in different years, the medal should in every case be bestowed as a recognition of high merit in some important branch of the science.

Before speaking of the Tables, I will notice some of Professor Newcomb's other works.

Memoir "On the secular Variations and mutual Relations of the Orbits of the Asteroids," *Mem. American Academy*, vol. V. (1860), pp. 124—152. The object is to examine those circumstances of the forms, positions, variations, and general relations of the asteroid orbits which may serve as a test, complete or imperfect, of any hypothesis respecting the cause from which they originated, or the reason why they are in a group by themselves. Every *a posteriori* test is founded on the supposition, that the hypothesis necessarily or probably implies that certain conditions must be satisfied by the asteroids or their orbits, viz. in the one case the conditions are those which follow necessarily and immediately from the hypothesis itself, in the other case those which are deducible from it by the principle of random distribution. The two principal hypotheses are that of Olbers, where the asteroids are supposed to be the fragments of a shattered single planet: and the hypothesis that they were formed by the breaking up of a ring of nebulous matter. On the first hypothesis the orbits of all the asteroids once intersected in a common point; the second affords no conclusion equally susceptible of an *a posteriori* test.

But for a rigorous or probable test of either hypothesis, what is needed is rigorous expressions in terms of the time for the eccentricity, inclination, and longitudes of perihelion and node of each of the asteroids considered, or, what is the same thing, the computation of the secular variations of the quantities h, l, p, q, which replace these elements. The investigation is applied to those asteroids the elements of which were determined with sufficient accuracy, and the eccentricities and inclinations of which were sufficiently small (limit taken is 11°). And the backbone of the memoir is the investigation of the h, l, p, q, for twenty-five asteroids included between the numbers (1) and (40). In this calculation, as was clearly necessary, the action of the asteroids on the larger planets and on each other was neglected; the expressions for the h, l, p, q, of the larger planets are regarded as given—they are, in fact, taken from Le Verrier (as calculated by him before the discovery of *Neptune*, but afterwards partially extended to that planet). The effect is that the differential coefficients $\frac{dh}{dt}$, &c. are given each of them as a sum of sines or cosines of arguments varying with the time; and thus, although the calculation is sufficiently laborious, the process is not one of the extreme labour and difficulty which it is in the case of the larger planets. The resulting table of the h, l, p, q, of the twenty-five asteroids has, of course, a value quite independent of the theoretical part of the memoir. Of this it is sufficient to say here that the conclusion is on the whole against Olbers's hypothesis. The subject is resumed, and more fully examined in a paper in the *Astronomische Nachrichten*, t. LVIII.

"Investigation of the Distance of the Sun and of the Elements which depend upon it, from the Observations of *Mars* made during the Opposition of 1862, and from other Sources," *Washington Observations for* 1865, Appendix II., pp. 1—29. The chief part of this valuable Memoir is occupied with a determination of the solar parallax by the discussion of the observations of *Mars* made in 1862 on the plan of Winnecke: three partial discussions had previously appeared, but these having been by comparisons of pairs of observations, one in each hemisphere, many observations in one hemisphere were lost by want of a corresponding observation in the other hemisphere; and out of a total of nearly 300 observations, only 125 were utilised. The idea is, the perturbations of the Earth and *Mars* being perfectly known for the period under consideration, every observation of the planet would lead rigorously to an equation of condition between its parallax, the six elements of its orbit, and the six elements of the Earth's orbit—thus 13 or more observations, when compared with any theory, should suffice to correct the errors of that theory. But the observations extending only over a short interval, say one month, the coefficients would be so minute as to give no trustworthy value of the corrections; the equations only suffice to determine a few functions of the elements which, being determined, the equations will be satisfied by widely differing values of the elements, if only these values are such as to give to the functions their right values. And by fixing *a priori* the entire number of functions in question, and using them in place of the elements of the Earth and *Mars*, the equations will be practically as rigorous as if all the 13 unknown quantities had been introduced. By such considerations as these, each observation is made to give a relation between only 3 unknown quantities, the correction of the Sun's parallax being one of them.

The principle appears to be one of extended application, in regard to the proper mode of dealing with the constantly recurring problem of the determination of a set of corrections from a large number of linear equations; and it is used by the author in regard to the equations which present themselves in his theories of *Neptune* and *Uranus*.

Returning to the *Mars* observations, these were made at six Northern and three Southern Observatories, the total number being 154 Northern, and 143 Southern, together 297 observations. There was the difficulty of reducing to a concordant system the observations at the different Observatories, since (the whole number of comparison stars not being observed on each night) the adopted mean position of each of them was not unimportant. But this being carefully discussed and allowed for, the observations, extending from August 21 to November 3, 1862, are divided into five groups, and from these is deduced a correction to the provisional value 8″·9 of the parallax. The author then reproduces or discusses other determinations, from micrometric observations of *Mars*, the parallactic inequality of the Moon, the lunar equation of the Earth, the transit of 1769, and Foucault's experiment on Light—the last result, as not a strictly astronomical one, and with no means of assigning its probable error, is left out of consideration—and the combination of the remaining ones gives the author's concluded value of the parallax; from which other astronomical constants are deduced.

"On the Right Ascensions of the Equatoreal Fundamental Stars and the Corrections necessary to reduce the Right Ascensions of different Catalogues to a mean homogeneous System," *Washington Observations for* 1870, Appendix III., pp. 1—73.

This important Memoir is referred to in the Council Report for 1873. The object is to do for the right ascensions of the equatoreal and zodiacal Stars what had been done by Auwers for the declinations, namely, to furnish the data necessary to reduce the principal original catalogues of stars to a homogeneous system by freeing them of their systematic differences. The results are contained in two tables of corrections (as depending on the R.A. and N.P.D. respectively) to the several catalogues; and in a table of concluded mean right ascensions for the beginning of each fifth Besselian year, 1750 to 1900, of 32 fundamental Stars, and of periodic terms in the right ascensions of *Sirius* and *Procyon*.

The evil of systematic differences between the observations of different Observatories of course presents itself in every case where such observations have to be combined: for instance, in the just-mentioned determination of the solar parallax by the observations of *Mars*; and in the making of a set of planetary tables: and all that tends to remove or diminish it is most important to the progress of Astronomy. I cannot help thinking that there should be some confederation of Observatories, or Central calculating Board, for publishing the lunar and planetary observations, &c., reduced to a concordant system. It seems hard upon the maker of a set of planetary tables that he should not at least have, ready to hand for comparison with his theory, a single and entire series of the observations of the planet.

"Théorie des Perturbations de la Lune, qui sont dues à l'action des Planètes," *Liouville*, t. XVI. (1871), pp. 1—45. This is a very important theoretical Memoir on the disturbed motion of three bodies: a problem which, so far as I am aware, has not hitherto been considered at all. I have elsewhere remarked that the so-called "Problem of Three Bodies," as usually treated is not really this problem at all, but a different and more simple one—that of disturbed elliptic motion. Thus, in the planetary theory, each planet is considered as moving in an ellipse, and as disturbed by the action of forces represented by means of a disturbing function peculiar to the planet in question. An approach is made to the problem of three bodies when, as in memoirs by Hamilton and Jacobi, the (say) two planets are replaced by two fictitious bodies, and instead of a disturbing function peculiar to each planet, the motion of the system is made to depend on a single disturbing function. And there are memoirs by Jacobi, Bertrand, and Bour, which do relate to the proper problem of three bodies, viz. to their undisturbed motion. But in the present Memoir, Professor Newcomb starts from this problem as if it were actually solved, viz. he takes the coordinates of the three bodies (Sun, Earth, and Moon) as given in terms of the time and of 18 constants of integration *. And then considering the system as acted upon by the attraction of a planet, represented by means of a disturbing function, he applies to the system of the three bodies the method of the variation of the elements. The six elements which determine the motion of the centre of gravity of the system are left out of consideration; there remain to be considered 12 elements only; six of these are ϵ_0, π_0, θ_0, ϵ_0', π_0', θ_0' (initial mean longitudes and longitudes of pericentre and node): but the other six k_ϵ, k_π, &c., are functions the invention of which is a leading step in the theory, and it is in fact by means of them that the investigation is brought to a successful conclusion: the expressions of the last-mentioned six functions can, it is stated, be formed with facility by means of the developments (obtainable from the lunar theory) of the rectangular

* Of course the expressions actually used must be approximations: the centre of gravity of the Earth and Moon is regarded as moving round the Sun in an ellipse affected by a secular motion of perihelion (ultimately neglected); and the coordinates of the Moon in regard to the Earth are considered to be given by Delaunay's Lunar Theory. The centre of gravity of the whole system (in the undisturbed motion) moves uniformly in a right line, viz. the coordinates are $a+a't$, $b+b't$, $c+c't$; and we have thus the whole number $6+6+6$, $=18$, of arbitrary constants.

coordinates x, y, z, as periodic functions of the time. With these twelve elements, the expressions for the variations assume the canonical form

$$\frac{dk_\epsilon}{dt}=\frac{dR}{d\epsilon_0},\quad \frac{d\epsilon_0}{dt}=-\frac{dR}{dk_\epsilon},\ \&c.$$

The concluding part of the Memoir contains approximate calculations which seems to show that the whole process is a very practicable one: but the author remarks that it is only doing justice to Delaunay to say that, starting from his (Delaunay's) final differential equations, and regarding the planet as adding new terms to the disturbing function, there would be obtained equations of the same degree of rigour as those of his own Memoir.

Everything in the Lunar Theory is laborious, and it is impossible to form an opinion as to the comparative facility of methods; but irrespectively of the possible applications of the method, the Memoir is, from the boldness of the conception and beauty of the results, a very remarkable one, and constitutes an important addition to Theoretical Dynamics *.

I come now to the planets *Neptune* and *Uranus*: it is well-known how, historically, the two are connected. The increasing and systematic inaccuracies of Bouvard's Tables of *Uranus* were found to be such as could be accounted for by the existence of an exterior disturbing planet; and it was thus that the planet *Neptune* was discovered by Adams and Le Verrier before it was seen in the telescope, in September 1846. It was afterwards ascertained that the planet had been seen twice by Lalande, in May 1795. The theory of *Neptune* was investigated by Peirce and Walker: viz. Walker, by means of the observations of 1795, and those of 1846—47, and using Peirce's formulæ for the perturbations produced by *Jupiter*, *Saturn*, and *Uranus*, determined successfully two sets of elliptic elements of the planet. The values first obtained showed that it was necessary to revise the perturbation-theory, which Peirce accordingly did, and with the new perturbations and revised normal places, the second set of elements (*Walker's Elliptic Elements II.*) was computed. With these elements and perturbations there was obtained for the planet from the time of its discovery a continuous ephemeris, published in the *Smithsonian Contributions*, *Gould's Astronomical Journal*, and since 1852 in the *American Ephemeris* and the *Nautical Almanac*. The theory was next considered by Kowalski in a work published at Kasan in the year 1855. The long period inequalities are dealt with by him in a manner different from that adopted by Peirce, so that the two theories are not directly comparable, but Professor Newcomb, by a comparison of the ephemerides with observation, arrives at the conclusion that the theory of Kowalski (although derived from observations up to 1853, when the planet had moved through an arc of 16°) was on the whole no nearer the truth than that of Walker;

* Since the above was written, Professor Newcomb has communicated to me some very interesting details as to the extent to which he has carried his computations, and in particular he mentions that, considering the action of each planet from *Mercury* to *Saturn*, he has (in regard to the terms the coefficients of which might become large by integration) estimated the probable limiting value of more than fifty such terms of period from a few years to several thousands without finding any which could become sensible, except the term leading to Hansen's first inequality produced by *Venus*.

he observed, however, that this failure is accounted for by an accidental mistake in the computation of the perturbations of the radius vector by *Jupiter*.

Professor Newcomb's theory of *Neptune* is published in the *Smithsonian Contributions* under the title "An Investigation of the Orbit of *Neptune*, with General Tables of its Motion," (accepted for publication, May 1865). The errors of the published ephemerides were increasing rapidly; in 1863 Walker's was in error by 33″, and Kowalski's by 22″; both might be in error by 5′ before the end of the century. The time was come when (the planet having moved through nearly 40°) the orbit could be determined with some degree of accuracy. The general objects of the work are stated to be:

(1) To determine the elements of the orbit of *Neptune* with as much exactness as a series of observations extending through an arc of 40° would admit of.

(2) To inquire whether the mass of *Uranus* can be concluded from the motion of *Neptune*.

(3) To inquire whether these motions indicate the action of an extra-Neptunian planet, or throw any light on the question of the existence of such planet.

(4) To construct general tables and formulæ, by which the theoretical place of *Neptune* may be found at any time, and more particularly between the years 1600 and 2000.

The formation of the tables of a planet may, I think, be considered as the culminating achievement of Astronomy: the need and possibility of the improvement and approximate perfection of the tables advance simultaneously with the progress of practical astronomy, and the accumulation of accurate observations; and the difficulty and labour increase with the degree of perfection aimed at. The leading steps of the process are in each case the same, and it is well-known what these are; but it will be convenient to speak of them in order, with reference to the present tables: they are *first* to decide on the form of the formulæ, whether the perturbations shall be applied to the elements or the coordinates—or partly to the elements and partly to the coordinates; and as to other collateral matters. These are questions to be decided in each case, in part by reference to the numerical values (in particular, the ratios and approach to commensurability of the mean motions), in part by the degree of accuracy aimed at, or which is attainable—the tables may be intended to hold good for a few centuries, or for a much longer period. The general theory as regards these several forms ought, I think, to be developed to such an extent, that it should be possible to select, according to the circumstances, between two or three ready-made theories; and that the substitution therein of the adopted numerical values should be a mere mechanical operation; but in the planetary theory in its present state, this is very far from being the case, and there is always a large amount of delicate theoretical investigation to be gone through in the selection of the form and development of the algebraical formulæ which serve as the basis of the tables. In Prof. Newcomb's theory the perturbations are applied to the elements; in particular, it was determined that the long inequality arising from the near approach of the mean motion of *Uranus* to twice that of *Neptune* (period about 4,300 years), should be developed as a perturbation, not of the coordinates, but of the elements. And it was best, (as for a theory designed to remain of the highest degree of exactness for only a few centuries) to take not the mean values of the elements, but their values at a particular epoch during the period for which the theory is intended to be used. The adopted provisional elements of *Neptune*, and the elements of the disturbing planets, are accordingly not mean values, but values affected by secular and long inequalities, representing the actual values at the present time. *Secondly*, the form being decided on and the formulæ obtained, the numerical values of the adopted provisional elements of the planet, and of the elements of the disturbing planets and their masses, have to be substituted, so as to obtain the actual formulæ serving for the calculation of a provisional ephemeris; and such ephemeris, first of heliocentric, and then of geocentric positions, has to be computed for the period over which the observations extend. *Thirdly*, the ephemeris, computed as above, has to be compared with the observed positions; viz. in the present case these are, Lalande's two observations of 1795, and the modern observations at the Observatories of Greenwich, Cambridge, Paris, Washington, Hamburg, and Albany, extending over different periods from 1846 to 1864: these are discussed in reference to their systematic differences, and they are then corrected accordingly, so as to reduce the several series of observations to a concordant system. In this way is formed a series of 71 observed longitudes and latitudes (1795, and 1846 to 1864); the comparison of these with the computed values shows the errors of the provisional ephemeris. *Fourthly*, the errors of the provisional elements have to be corrected by means of the last-mentioned series of errors: as regards the longitudes, the comparison gives a series of equations between $\delta\epsilon$, δn, δh, δk, and μ (correction to the assumed mass of *Uranus*). The discussion of the equations shows that no reliable value of μ can be obtained from them; it indeed appears that, if *Uranus* had been unknown, its existence could scarcely have been detected from all the observations hitherto made of *Neptune* (far less is there any indication to be as yet obtained as to the existence of a trans-Neptunian planet): hence, finally, μ is taken $=0$, and the equations used for the determination of the remaining corrections. As regards the latitudes, the comparison gives a series of equations serving for the determination of the values of δp and δq. And applying the corrections to the provisional elements, the author obtains his concluded elements; viz. as already mentioned, these are the values, as affected by the long inequality, belonging to the epoch 1850. *Fifthly*, the tables are computed from the concluded elements, and the perturbations of the provisional theory.

After the elements of *Neptune* were ascertained, the question of its action on *Uranus* was considered by Peirce in a paper in the *Proc. American Acad.*, vol. I. (1848), pp. 334—337. This contains the results of a complete computation of the general perturbations of *Uranus* by *Neptune* in longitude and radius vector, but without any details of the investigation, or statement of the methods employed: it is accompanied by a comparison of the calculated and observed longitudes of *Uranus* (with three different masses of *Neptune*) for years at intervals from 1690 to 1845, and for one of these masses the residuals are so small that it appears that, using these perturbations by *Neptune* and Le Verrier's perturbations by *Jupiter* and *Saturn*, there existed a theory of *Uranus* from which quite accurate tables might have been constructed. But this was never done. The ephemeris of *Uranus* in the *American Ephemeris* was intended to be founded on the theory, but the proper definitive elements do not seem to have been adopted: and in the *Nautical Almanac* for the years up to 1876, Bouvard's Tables of *Uranus* were still employed; for the year 1877 the ephemeris is derived from heliocentric places communicated by Prof. Newcomb.

An extended investigation of the subject was made by Safford, but only a brief general description of his results is published, *Monthly Notices, R.A.S.*, vol. XXII. (1862). The effect of *Neptune* was here computed by mechanical quadratures; and corrections were obtained for the mass of *Neptune* and elements of *Uranus*.

Professor Newcomb's Tables of *Uranus* have only recently appeared. They are published in the *Smithsonian Contributions* under the title "An Investigation of the Orbit of *Uranus*, with General Tables of its Motion," (accepted for publication February, 1873), forming a volume of about 300 pages. The work was undertaken as far back as 1859, but the labour devoted to it at first amounted to little more than tentative efforts to obtain numerical data of sufficient accuracy to serve as a basis of the theory, and to decide on a satisfactory way of computing the general perturbations. First, the elements of *Neptune* had to be corrected, and this led to the foregoing investigation of that planet: it then appeared that the received elements of *Uranus* also differed too widely from the truth to serve as the basis of the work, and they were provisionally corrected by a series of heliocentric longitudes, derived from observations extending from 1781 to 1861. Finally, it was found that the adopted method of computing the perturbations, that of the "variation of the elements," was practically inapplicable to the computation of the more difficult terms, viz. those of the second order in regard to the disturbing force. While entertaining a high opinion of Hansen's method as at once general, practicable, and fully developed, the author conceived that it was on the whole preferable to express the perturbations directly in terms of the time, owing to the ease with which the results of different investigations could be compared, and corrections to the theory introduced; and under these circumstances he worked out the method described in the first chapter of his treatise, not closely examining how much it contained that was essentially new. With these improved elements and methods the work was recommenced in 1868; the investigation has occupied him during the subsequent five years: and, though aided by computers, every part of the work has been done under his immediate direction, and as nearly as possible in the same way as if he had done it himself: a result in some cases obtained only by an amount of labour approximating to that saved by the employment of the computer.

The leading steps of the investigation correspond to those for *Neptune*: there is, *first*, the theoretical investigation already referred to; *secondly*, the formation of the provisional theory with assumed elements; *thirdly*, the comparison with observation; and here the observations are the accidental ones previous to the discovery of *Uranus* as a planet by Herschel in 1781, and the subsequent systematic ones of twelve Observatories, extending over intervals during periods from 1781 to 1872; all which have to be freed from systematic differences, and reduced to a concordant system as before: the operation is facilitated by the existence, since 1830, of ephemerides computed from Bouvard's Tables serving as an intermediate term for the comparison of the observations with the provisional theory. *Fourthly*, the correction of the elements of the provisional theory, viz. the equations for the comparison of the longitudes give $\delta\epsilon$, δn, δh, δk, and a correction to the assumed mass of *Neptune*, which mass is thus brought out $=\frac{1}{19640}$. And the equations for the comparison of latitudes give δp, δq; there is thus obtained a corrected set of elements (Newcomb's Elements IV.), being for the year 1850, the elements as affected with the long inequality; these are the elements upon which the Tables are founded. But it is theoretically interesting to have the absolute mean values of the elements, and the author accordingly obtains these (his Elements V.) together with the corrections corresponding to a varied mass of *Neptune*, $\left(\text{that is, the terms in } \mu \text{ corresponding to a mass } \frac{1+\mu}{19700}\right)$; he remarks that, admitting the mass of *Neptune* to be uncertain by about one-fiftieth of its value, the mean longitude of the perihelion of *Uranus* is from this cause uncertain by more than two minutes, the mean longitude of the planet by nearly a minute, and the mean motion by nearly two seconds in a century. *Fifthly*, the formation of the tables, based on the Elements IV.; the tables calculated with these elements are intended to hold good for the period between the years 1000 and 2200; but by aid of the Elements V. they may be made applicable for a more extended period.

In what precedes I have endeavoured to give you an account of Professor Newcomb's writings: they exhibit all of them a combination, on the one hand, of mathematical skill and power, and on the other hand of good hard work—devoted to the furtherance of Astronomical Science. The Memoir on the Lunar Theory contains the successful development of a highly original idea, and cannot but be regarded as a great step in advance in the method of the variation of the elements and in theoretical dynamics generally; the two sets of planetary tables are works of immense labour, embodying results only attainable by the exercise of such labour under the guidance of profound mathematical skill—and which are needs in the present state of Astronomy. I trust that imperfectly as my task is accomplished, I shall have satisfied you that we have done well in the award of our medal.

The President then, delivering the medal to the Foreign Secretary, addressed him in the following terms:

Mr Huggins—I request that you will have the goodness to transmit to Professor Newcomb this medal, as an expression of the opinion of the Society of the excellence and importance of what he has accomplished; and to assure him at the same time of our best wishes for his health and happiness, and for the long and successful continuation of his career as a worker in our science.