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THE DERIVATION OF ORBITS, THEORY AND PRACTISE[1]

LESS than twenty-five years ago it was commonly accepted among astronomers and mathematicians alike that the orbit problem had been solved both in theory and in practise. Without detailing the well-known history of the development of orbit methods before that time it is sufficient to remind you that although Newton, after successfully integrating the differential equations in the problem of two bodies and verifying Kepler's laws, proposed a geometrical method which was successfully applied by Halley particularly in determining the orbit of the well-known comet which bears his name, the integrals derived by Newton were not translated into a thoroughly practical method for determining the constants or elements from the initial conditions furnished by observation until 1797 when Olbers published his famous method of determining parabolic orbits for comets from three observed positions. This special method was followed at the dawn of the last century by the general method of Gauss which permits of the determination of the elements from three observations without previous hypothesis regarding the eccentricity, a method applicable equally to comets and to planets. It is to be noted that both Olbers's and Gauss's methods rest on the previous analytical solution by Newton of the equations of motion in the two-

[1] Address of the vice-president and chairman of Section A, American Association for the Advancement of Science, read at a joint meeting of Section A, the American Mathematical Society and the American Astronomical Society, on Thursday, December 28, 1917, at New York.

body problem. In fact these orbit methods may be characterized as an evaluation of the numerical values of the constants or elements from given positions on the basis of the integrals found by Newton. It might be supposed that the mere evaluation of the numerical values of the constants of integration in a given case when the form of the integrals is known ought not to involve any considerable difficulties. But the solution of the unknown elements from the given equations of condition leads to very complicated expressions which can be solved only by successive approximations. This unfavorable condition arises from the occurrence of series in which the coefficients depend upon the unknown elements. Until the early nineties of the last century the chief aim of astronomers and mathematicians had been to modify the methods of Olbers and Gauss by transformations which would increase the degree of accuracy of the first and the convergence of later approximations. The most successful orbit methods would then be those which would yield the elements with the greatest degree of accuracy and with the minimum of numerical work.

The observations in general furnish three directions of three heliocentric positions of the body, each seen from one of three different positions of the observer. The problem of the older methods is to pass a plane through the center of the sun which cuts the three directions in such a manner that the body moves in accordance with the law of areas in the conic, which is defined by the three intersections of the plane with the directions, and by the center of the sun. It is evident at once that if the three directions are taken at short intervals they must be given with the utmost precision so that the parameters of the conic may be determined with any degree of accuracy.

In general a very large number of planes satisfying the required conditions may be drawn within the unavoidable errors of observation, so that every preliminary orbit is more or less indeterminate. Thus while a perfect theory might be available for the evaluation of the elements, in practise the numerical accuracy of the orbit will be limited. This limitation of accuracy in general increases with the ratio of the errors of observation to observed motion. In addition, even with perfect observations distributed over a sufficiently long heliocentric arc, cases occur in which the mathematical expressions for the solution of the elements lead to indeterminate forms. In some cases these indeterminate forms are inherent in the physical conditions of the problem. In other cases they may be avoided by proper mathematical devices or by a different mathematical treatment of the problem. One of the best known cases of indeterminateness arising from physical conditions is that in which the orbit plane coincides with the ecliptic. In this case the position of the orbit plane, usually defined by two elements, is given at once, but since each of the three observed directions furnishes but one independent condition, namely the longitude, while four elements remain to be found, the problem becomes indeterminate and requires a fourth observation for its solution. One of the best known cases of the other type of indeterminateness arising from the mathematical formulation of the orbit method is the so-called Ausnahmefall (exceptional case) of Oppolzer in Olbers's parabolic method. When the orbit is supposed to be parabolic only five elements need to be determined from the six conditions furnished by observation. The observed direction is usually given in right ascension and declination and may be considered as the intersection of two planes which may be introduced as given conditions. Since the choice of these planes is arbitrary, as long as their intersection coincides with the line of sight Olbers reduces the number of available conditions by rejecting one of the arbitrary planes for the middle place or second observation and adopts for the other arbitrary plane that which corresponds to a great circle drawn through the observed place of the body and through the sun.

Since the three distances of the body are not furnished by observation they enter the problem as additional unknowns. Usually the distances are derived first, whereupon the solution of the elements becomes comparatively simple. In Olbers's method one of the fundamental relations for the determination of the distances at the first and third dates has the form $\rho_{\mathrm{III}} = M\rho_{\mathrm{I}}$, where M is equal to the product of the ratio of two triangular areas into the ratio of the trigonometrical sines of the perpendicular arcs drawn from the first and third observed places, respectively, to the great circle through the sun and the second observed place. The ratios of the triangles referred to form a very important consideration in many orbit methods. The triangles are contained between successive radii vectores from the sun to the body. For short arcs or intervals these triangles differ but little from the corresponding sectors bounded by the conic, and since according to the law of areas the sectors are proportional to the intervals, the triangles are very nearly proportional to the intervals. The ratios of the triangles may then be developed in series of which the first term is identical with the ratio of the intervals and of which the later terms contain the powers and products of the intervals, the inverse powers of the heliocentric distances r and their derivatives. They may be made to depend on the second heliocentric distance r and its derivatives. Since r and its derivatives depend on the elements in the orbit their values in general can not be known until the first approximation has been accomplished by placing the ratios of the triangles equal to the ratio of the intervals. The series representing the ratios of the triangles have been the subject of intensive study in connection with the orbit methods resting on the integrals of Newton. The most exhaustive study of the true radii of convergence of series of this type is due to Moulton. He demonstrates analytically the empirical conclusions of astronomers that the series may lose their applicability for comets observed near perihelion at a moderate distance from the sun, while for minor planets in general they give universal satisfaction.

In referring to the indeterminateness in Olbers's method I am not at this moment concerned with any inaccuracies that may arise from his using in the first approximation the ratios of the intervals for the ratios of triangles. The indeterminateness I am referring to arises from the fact that when the first and third observed positions lie on the auxiliary great circle through the second place and the sun, referred to above as being introduced by Olbers, then both the perpendiculars from the first and third places on this great circle become zero and M becomes indeterminate. It becomes nearly indeterminate when the three observations lie approximately in the great circle through the sun, and the degree of indeterminateness in such cases depends upon the magnitude of the errors of observation as compared with the magnitude of the perpendiculars. It is evident that perpendiculars of but a few seconds accurately derived from precise observations would still yield a working first approximation, while larger perpendiculars comparable to the errors of observation would lead to fallacies or yield nothing. Here we have a significant contrast of the interpretations with reference to accuracy obtainable from theory and practise. Theoretically small perpendiculars in Olbers's method would lead to indeterminateness, but in practise it is not the absolute magnitude of the small perpendiculars that counts so much as their uncertainty due to errors of observation. To return once more to Olbers's method, when his mathematical formulation leads to a practical indeterminateness the difficulty may be removed at once by substituting for Olbers's great circle through the second place and the sun a great circle perpendicular to it. This choice of great circle evidently produces a maximum value of the perpendiculars drawn to it from the first and third places, so that the effect of the errors of observation is minimized.

It must not be supposed that the conditions of indeterminateness just referred to were not known to theoretical astronomers until recent times. In his classic "Lehrbuch zur Bahnbestimmung," the second edition of which was published in 1882, Oppolzer sets forth clearly and concisely the significance of errors of observation with reference to small quantities which are theoretically of a high order of smallness, when the intervals or motion are considered quantities of the first order. My own aim and that of those associated with me at the University of California has been to treat each case on its own merits from the numerical point of view and to ascertain at the outset the uncertainty which must necessarily remain in the result. As this uncertainty corresponds to a region of possible numerical results clustering around the physical solution or in case of multiple mathematical solutions around these, all of which correspond to orbits that will satisfy the observations within their errors, I introduced the term *range of practical solutions* in a paper read at the International Congress of Arts and Sciences at St. Louis in September, 1904, and have at the same time and again later set forth the numerical conditions producing a range of practical solutions. In the modifications of the formulæ, for computing orbits so as to secure the greatest accuracy with the least expenditure of numerical work this principle has been constantly borne in mind. I emphasize this point because this distinction between practise and theory has not been well understood. Moulton, in a very exhaustive memoir on the "Theory of Determining Orbits," published in the *Astronomical Journal* in 1914, to which further reference will be made later, also seems to have failed to recognize the significance of our work in this regard, although it was set forth in detail in another form in Buchholz's "Klinkerfues Theoretische Astronomie," third edition, 1912, which Moulton has reviewed. In the first example published in this work I was careful to place a dot over the last digit of every fundamental quantity that could be relied upon.

To facilitate our further discussion it may be well to trace in outline the fundamental principles of the methods of Olbers and Gauss as set forth by Oppolzer in a masterly manner. Olbers's and Oppolzer's parabolic methods yield a solution of the first and third geocentric distances from the equation $\rho_{\mathrm{III}} = M\rho_{\mathrm{I}} + m$ and the well-known Euler's equation expressing the intervals between the first and third dates in terms of the sums of the radii vectores drawn from the sun to the first and third places and the chord joining these two places. With Olbers's choice of the great circle through the middle place m becomes zero. In both methods the ratios of the triangles are replaced in the first approximation by the ratios of the intervals. Even then the solution is accomplished only by successive approximations or trials in the course of which, however, higher terms of the series in the ratios of the triangles may be taken into account. It is customary to assume as a first approximation that the sum of the first and third radii vectores is equal to 2 astronomical units. Convergence of the approximations has been facilitated by Oppolzer by differential relations which give the correction to be applied to the initial value of $r_{\mathrm{I}} + r_{\mathrm{III}}$ so that it may agree with the value derived at the end of the trial. In the course of ordinary trials without the use of differential relations the final values of the distances of one trial form the initial values in the next trial. In the method of differential corrections such corrections to the initial value of one trial are derived differentially from the differences between the initial and final values in the same trial as will produce an agreement of the initial and final values in the next trial. The number of approximations required by the ordinary trials is in general far in excess of that required by the method of differential correction.

Gauss's method as formulated by Oppolzer may be started from the equation $Ax + By + Cz = 0$, which expresses that the body moves in a plane through the sun, x, y, z being the heliocentric rectangular coordinates referred to the sun. When this equation is written out for each of the three places and when the eliminant of the three equations is written down in the form of a determinant this determinant may be developed either in terms of co-factors of the x, or the y, or the z. For instance, in co-factors of x we have

$$x_{\mathrm{I}}(y_{\mathrm{II}}z_{\mathrm{III}} - y_{\mathrm{III}}z_{\mathrm{II}}) - x_{\mathrm{II}}(y_{\mathrm{I}}z_{\mathrm{III}} - y_{\mathrm{III}}z_{\mathrm{I}}) + x_{\mathrm{III}}(y_{\mathrm{I}}z_{\mathrm{II}} - y_{\mathrm{II}}z_{\mathrm{I}}) = 0.$$

The coefficients of x here represent the projections of double the triangular areas $[r_i\ r_j]$ upon the yz plane. By dividing out by one of these areas the two resulting coefficients represent the ratios of the projected triangles and since the triangles are projected on the same plane these ratios are the same as the ratios of the triangles themselves. As stated before, instead of developing the determinant by co-factors of x it may also be developed by co-factors of y and z. We thereby obtain the same equation written in two additional forms. Every equation is identically equal to zero, if the terms are multiplied out. But if we can assume the numerical values of the ratios of the triangles to be known from other sources and if we express in each of the three equations the heliocentric rectangular coordinates in terms of the geocentric polar coordinates and of the solar coordinates so that, for instance,

$$\frac{[r_{\mathrm{II}}r_{\mathrm{III}}]}{[r_{\mathrm{I}}r_{\mathrm{III}}]}(\rho_{\mathrm{I}} \cos \alpha_{\mathrm{I}} \cos \delta_{\mathrm{I}} - X_{\mathrm{I}}) - (\rho_{\mathrm{II}} \cos \alpha_{\mathrm{II}} \cos \delta_{\mathrm{II}} - X_{\mathrm{II}}) + \frac{[r_{\mathrm{I}}r_{\mathrm{II}}]}{[r_{\mathrm{I}}r_{\mathrm{III}}]}(\rho_{\mathrm{III}} \cos \alpha_{\mathrm{III}} \cos \delta_{\mathrm{III}} - X_{\mathrm{III}}),$$

then we arrive at three equations with the geocentric distances as unknown quantities. Now if the ratios of the triangles could be known at the outset, it is evident that the geocentric distances can be obtained by the solution of these three equations. The first approximation, depending upon the degree of accuracy with which the ratios of the triangles are introduced, is generally referred to as the first hypothesis and the accuracy of the geocentric distances and therefore of the whole solution which depends upon them is referred to as being of the zero, first, or higher order with reference to the intervals or motions. The choice of equal intervals always increases the accuracy by one order. Simple as this process seems in theory, it becomes very complicated in practise, because in general a first approximation can not be obtained by merely using the ratios of the intervals as numerical expressions for the ratios of the triangles. It is necessary to introduce at the outset one or more of the terms involving the inverse powers of the heliocentric distance r and its derivatives, and these can not be known until the geocentric distances have been obtained, r being derived from the triangle which has at its vertices the observer, the sun, and the body. The angle at the observer is known by observation, the distance of the sun from the earth is known, and ρ being assumed, r may be found. But since neither r nor ρ is known at the outset, the solution must be accomplished by trial and error. Here, as before, the method of differentially correcting the first approximation on the basis of the difference between initial and final values in a trial is very effective. Thus the first hypothesis involves a series of trials for the solution of r and ρ, and it is accurate to zero, first, or second order, and so forth, according to the number of terms in the ratios of the triangles introduced in the first set of trials for the distances, which trials become, of course, the more complicated, the more terms are introduced. A practical limit is thus set at once. The second hypothesis depends upon the computation of the remaining terms of the series in the ratios of the triangles on the basis of values derived from the first hypothesis. While these values may be sufficient for the higher terms the lower terms taken into account in the first hypothesis still remain inaccurate since they do not contain the final numerical values of the unknowns. This is important because it involves successive resubstitution of the improved values in all terms of the series. We shall see later that these complicated manipulations have recently been removed by Charlier by completing a purely analytical solution proposed by Lagrange. In passing from one hypothesis to the next it is necessary, as previously stated, to calculate the remaining terms of the series representing the ratios of triangles. Oppolzer ingeniously computes the whole remainder in a closed form, but in determining the numerical value of the closed remainder must perform successive approximations, as I say, merely to get the remainder. These approximations involve the complicated expression of the ratio of a sector of a conic to the corresponding triangle. Thus we see that in Oppolzer's formulation, which is the most accurate yet proposed, it is necessary first: to undertake several hypotheses; secondly: within each hypothesis to perform a number of trials for the distances; and, thirdly: in passing from one hypothesis to the next to perform approximations involving the ratio of sector to triangle. The application of the method of differential correction, so successfully applied in the trials for the distances, in place of these several cycles will take up in one operation all of these cycles of approximation as will be referred to later.

It is not necessary to go into the various and numerous devices which have been proposed during the past century to facilitate the various cycles of approximation referred to. Be it sufficient to say that the highest degree of accuracy has been obtained in this country by Gibbs in his vector method, which in the first hypothesis takes account of terms of the fourth order in the ratios of the triangles. But although this method is the most accurate of all the so-called methods in the first hypothesis it unfortunately requires too large an amount of numerical work in the first approximation and does not readily lend itself for application to a second hypothesis. It has, therefore, failed to come into universal use.

I have already referred to the methods hitherto described as "the older methods." They have been set down in various excellent formulations, particularly by Klinkerfues, by Watson in this country, by Oppolzer, by Buchholz, by Tisserand, and others, and are in use to the present day in accordance with the various formulations, but unfortunately without being duly appreciated in all cases by computers with reference to their numerical significance, that is, with reference to the validity of the results which they produce as conditioned by partial indeterminateness or range of practical solutions. Furthermore, to cite from my paper on "Preliminary Statistics on the Eccentricities of Comet Orbits":

Ever since the first computation of a comet orbit was made, it has been customary to derive a parabola as a first approximation, and to attempt a more general solution only if the deviations of the observed positions from the most probable parabola were in excess of the probable errors of observation. This custom has become so thoroughly fixed in astronomy that even now it would be considered absolutely unwarranted to suspect a comet of moving in an ellipse if by a little stretching of the probable limits of observational error a parabola could be found to represent the observed positions. A prejudice has always existed and exists now in favor of the parabola for comets. This prejudice is largely due to the many published parabolic comet orbits. A further reason lies in the fact that the first geometrical and analytical methods for solving a comet orbit were parabolic. The solution of an elliptic orbit was originally possible only in cases like Halley's comet, in which more than one appearance had been observed so that one of the unknowns, the period, became known.

The procedures in the older methods for the derivation of a parabolic and the derivation of a general solution are so different that when it is recognized that a parabolic orbit or conditioned solution is not possible, a fact which does not reveal itself until after many fruitless attempts at a parabolic solution have been made, it is necessary to discard most of the previous numerical work and to start anew with the formulæ for a general solution. An illustration of the labor involved in this antiquated process is furnished by the published work of one of the leading European astronomers on the preliminary orbit of comet 1892 II. (Holmes). Three observations at equal intervals of four days were available in this case. The computer attempted a parabola and, finding that he could do nothing with the ratios of the triangles in improving his orbit, finally resorted to an arbitrary variation of M referred to in Olbers's method as the ratio of the third to the first geocentric distance, thus producing four different parabolic orbits with ephemerides from which to choose, none of which admittedly represented the given observations. Only later and still greater discrepancies between observation and the predicted path lead the computer to resume the computation without hypothesis regarding the eccentricity. In due course of time this general solution yielded a short period ellipse. Here is a bit of practise still in use among many computers which, if applied in the business world, would involve an enormous cost of operation. Mr. Shane, one of my students, applied my formulation of the Laplacean method to this case and obtained the true ellipse in the first approximation or by a direct solution without difficulty in a few hours.

Nor is it always safe to assume the nature of the object and the character of its orbit from its appearance. Thus some comets are of a star-like appearance when discovered and can not be distinguished from asteroids, and to prejudice the character of the orbit at the outset may lead to unnecessary complications. It is not improbable that some of the short periodic orbits published for supposed minor planets which have become lost are really very eccentric orbits of comets which would account for their failing to be reobserved in their predicted places. Just how many of the published orbits of the hundreds of planets and comets are entirely reliable is difficult to say until a thorough examination shall have been made of the range of practical solution for each case. When a planet or comet has been observed for a considerable length of time, or on several returns, the elements require no further examination if they have been properly corrected on the basis of the observational material. But the comet and planet lists are full of orbits based upon comparatively short arcs and the lists contain little indication of the degree of accuracy in each case.

Sometimes when physical and mathematical indeterminateness does not prevail the range of solution with precise observations is quite limited even for a very short arc, and the resulting orbit is fairly accurate. In other cases conditions may be such that even for a comparatively long arc the orbit is inadequate to secure rediscovery at a later return. Comet *e* 1913 Neujmin was of a star-like appearance and admitted of a high degree of accuracy in the observed positions. The first three observations at one-day intervals admitted of the determination of a periodic orbit agreeing closely with a more accurate orbit determined from a 38-day arc. On the other hand, orbits of planet (702) discovered in 1910 based on arcs of several months show a wide range of practical solution amounting to about four degrees in the eccentricity.

An accurate knowledge of the eccentricities of comet orbits is of importance in determining the origin of comets. On theoretical grounds it has long been recognized that parabolic orbits are practically impossible if comets came from without the solar system, formerly a favorite theory of astronomers. The majority of orbits should be elliptic if comets have their origin within the solar system. A rare parabola and some hyperbolas might, of course, be accounted for through the perturbations of the major planets on an original ellipse. But if comets came from without they should be predominatingly hyperbolic. Now the published comet lists show that about three fourths of all comet orbits have been found to be parabolic. A study of the published eccentricities of comet orbits on the basis of the accuracy with which, and the length of time during which they were observed has shown conclusively that all comet orbits are elliptic if observed with sufficient accuracy and for a sufficient length of time. This conclusion was received with doubt when I first announced it in 1907 on the basis of a study of the eccentricities of comet orbits. That three fourths of the comet orbits are parabolic is due to the fact that comet orbits in general have a high eccentricity and that the parabola lies within the range of possible solutions. Since the lower limit of the eccentricity has never been sought the orbits have simply been set down as parabolic and much confusion has been created with reference to the determination of the origin of comets. A few well-determined hyperbolas do exist, but Stroemgren has shown that these are accounted for by perturbations of the original ellipses on the part of the major planets. The high eccentricity found for comet orbits lies in the nature of things. Long-period comets can not come within the range of visibility from the earth unless their orbits are highly eccentric. The others must remain invisible. It is only within recent years that the old idea that comets are visitors from without the solar system has been abandoned. Reference to the existing confusion in regard to the origin of comets is made here only because it is clear that if in the past astronomers had worked by methods which readily enable the computer to ascertain the range of possible solutions, particularly the lower limit, three fourths of all comet orbits would not have been set down as being parabolic and much analytical work with reference to the origin of comets would have been avoided. Yet there is a practical advantage in adopting a preliminary parabolic solution for a comet when the range of solutions is very large and when this range includes the parabola. For since the majority of comet orbits have high eccentricities the adoption of a preliminary short-period orbit would later involve a more radical correction than the adoption of a preliminary parabola. Of course within the physical indeterminateness or the practical range of solutions any and all of the orbits satisfying the observations within their errors are equally justified, but even to this day it is a reflection on the astronomer if the period or eccentricity of his preliminary orbit must be increased to satisfy later observations, while it is quite the proper thing to publish a parabola and later to find the orbit short period even if such short period and eccentricity could have been derived at the outset. Thus in the case of comet Neujmin referred to above parabolic orbits were still insisted upon, while the comet closely followed our short-period orbit from a two-day arc. It has been my frequent experience that elliptic orbits with a fair degree of accuracy could be determined from the first three observations, while other computers continued to produce parabolas which could be shown not to lie within the range of solution and resulted from the use of approximate methods.

In anticipation of stating the many advantages introduced by the modernization of the method originally proposed by Laplace I have already dwelt on three important considerations, namely, first, on the complications involved in the successive hypotheses and approximations of the older methods; secondly, on the waste of time in applying different formulas for a conditioned and a general solution so that with the abandonment of a parabola it is necessary to make a new start; and thirdly, on the significance of the range of solution as derived from partial indeterminateness depending upon the shortness of the arc and upon the effect of error of observation on small significant coefficients. My own attention was directed to Laplace's method by a memoir of Harzer. Laplace's method appeared in 1780 in a memoir and later in his Mécanique Céleste. Prior to him, in 1771 Lambert had produced his famous theorem based on geometrical considerations. Later, in 1778 Lagrange showed that Lambert's equation leads to an equation of the seventh degree, the fundamental equation of the orbit problem which also occurs in the older methods and which Charlier proposes to designate as Lagrange's equation. Laplace and Lagrange mutually inspired each other to further important developments of the orbit problem. Laplace starts with the three differential equations of motion of the second order for the two-body problem under the Newtonian law of attraction as applied to the motion of a material point (the object) about the sun. He then expresses the heliocentric rectangular coordinates of the object in terms of its geocentric coordinates and the heliocentric coordinates of the earth. The rectangular coordinates are then replaced in terms of polar coordinates, and thereby three equations are derived which give the geocentric distance ρ, its velocity ρ', and its acceleration ρ'' at the epoch in terms of the observed coordinates (for which we may choose α and δ, their velocities (α' δ'), and their acceleration (α'', δ''), and also in terms of the unknown heliocentric distance r of the object and known quantities, depending upon the motion of the earth about the sun.

If, therefore, for the present, we assume the coordinates α, δ, their velocities α', δ', and their accelerations α'', δ'' to be known, we have three fundamental equations for the solution of the four unknowns ρ, ρ', ρ'', and r. The fourth equation is derived from the triangle sun-earth-object, and involves ρ, r, and known quantities. By elimination the problem reduces to Lagrange's equation of the seventh degree with not more than two positive real roots, which may be interpolated to six decimals from a table which I have prepared for this purpose, so that the solution may be accomplished without the hitherto necessary laborious numerical approximations.

The direct solution which has just been outlined corresponds to the so-called first hypothesis of other methods. It is evident that the accuracy of Laplace's direct solution depends upon the accuracy of the fundamental observational data for which we have chosen α, δ, α', δ', α'', δ''. If the epoch is chosen to coincide with the date of one of the observations, then α, δ are fixed numbers, and the accuracy of the Laplacean solution depends upon the accuracy of the adopted values of their velocities and accelerations or, which is an equivalent statement, upon the accuracy of their first and second differential coefficients. In practically all other methods the accuracy of the solution depends upon the accuracy of the adopted values of the ratios of the triangles. Unfortunately the method of Laplace has been prejudiced by Lagrange until recent times through a letter addressed to Laplace, in which he says that while analytically Laplace's method constitutes the simplest solution of the problem, in practise it does not afford corresponding advantages because the differential coefficients could not be determined with the necessary accuracy. This far-reaching statement Lagrange intended as a mere opinion which he proposed to verify later by mathematical demonstration. It is a remarkable fact that Lagrange's opinion, although never verified by himself, has been the chief cause of retarding the further development of the Laplacean method until recent times.

Nevertheless, several attempts at giving it a practical formulation have been made during the last century, but with indifferent success. With reference to the disrepute which Laplace's method and all formulations based upon the same have been held until recent times and for a statement of its actual merits I may refer you to my address delivered before the International Congress of Mathematicians in August, 1912. Laplace's method leads to the usual equation of the seventh degree, which, as stated above, we shall refer to as Lagrange's equation. The roots of this equation have been frequently studied by Cauchy, Mrs. Young (Grace Chisholm), Oppolzer, and others. In recent times a classic study of the equation has been published by Charlier, who not only clarifies the existing conditions which will lead to a multiple solution, but exhibits these conditions geometrically by dividing space into four regions symmetrical with reference to the line joining the earth and sun as central axis, and showing that two solutions exist when the body is in two of these regions, and one solution when the body is in the other two. In certain cases it is not possible to distinguish the mathematical from the physical solutions, so that either a fourth observation must be employed in the original solution or the mathematical solution must be eliminated on the basis of difference between theory and later observation.

My own formulations of Laplace's method need be referred to but briefly. The results are chiefly that the whole process has been extremely simplified. A conditioned solution may be made on practically the same plan as a general solution. Criteria have been introduced to distinguish between the feasibility of solution with or without assumption regarding the eccentricity. Provision has been made for passing from one class of orbit to another in the course of the computation without repeating the solution. Numerical criteria have been set up to distinguish the physical from the mathematical solutions in the case of three roots which may occur in the parabolic method. A method has been provided for completely eliminating the paralax, as has been done in the case of Planet *MT* by Dr. E. S. Haynes, since in this case the possibility of a solution rested on such elimination. The various approximations for the solution of distances is avoided. In a general solution the distances are taken from a table. The accuracy attainable in each case can be ascertained in advance and the range of solution definitely determined. Series corresponding to the ratios of the triangles, which do not enter, however, in the original solution but only later after the distances have been determined, have been replaced by closed expressions which avoid slow convergence or divergence in case of comet orbits observed near perihelion and at a moderate distance from the sun. The whole cycle of hypotheses and approximations of the older methods and all initial inaccuracies are taken up by a method of differential correction. In the case of highly disturbed satellites, such as the ninth satellite of Jupiter, the orbit solution has been made possible by extending the formula so as to take account of the perturbations in the first approximation. Closed expressions in the differential correction of an orbit now make it possible to apply the method to any and all conditions, particularly to arcs of any length.

It is not possible to dwell further on these advantages, yet reference may be made to some important results which make unnecessary extensive investigations hitherto in use. In the case of comet 1910*a*, which was discovered near perihelion and at a moderate distance from the sun, a variety of preliminary orbits were derived by various computers. Through the work of Oppolzer, Charlier, and myself, it was already known that cases of three mathematical solutions might be possible. Tscherny classified all the different preliminary orbits that had been derived for this comet and showed that clearly they represented three groups, each group representing a range of solution clustering about a mathematical solution. Each computer had produced a perfectly legitimate orbit within the errors of observation, none recognizing that his orbit was one belonging to one of the three ranges, or that multiple solutions existed. In my short methods simple criteria are given for determining the existence of three mathematical solutions of the equation of the sixth degree for a parabola. As soon as observations became available the method was applied by Miss Levy and three distinct values of the geocentric distance at the middle date and the range of each were obtained. A simple consideration leads to the elimination of the two fictitious parabolic solutions, as there can be at most two general solutions, also either one or three parabolic solutions, and as only one parabolic solution can agree with a general solution. By this process the physical solution was at once determined. The two general solutions corresponding to the problem are readily taken from the table so that all five roots, two general and three parabolic, are available simultaneously and with little effort. Therefore, there really exists no reason why hereafter a computer should ever be misled to derive a parabola corresponding to a solution other than the physical solution. By the method of the greatest common divisor Picard has reduced the equations for general and parabolic orbits to a linear equation giving the only possible parabolic solution.

The other point on which I desire to dwell is that of the identification of newly discovered planets or comets with objects previously observed and for which orbits are available. More than once it has been found in the case of a newly discovered comet that the inclination, node, and perihelion distance of the parabolic solution resemble within the range of the solution the corresponding elements of some former parabolic comet. By introducing a period corresponding to one or more revolutions between the dates of the perihelia of the two comets the original solution may be turned into a conditioned solution based on a definite period. In no case where definite reasons for such procedure existed did this experiment fail of proving the identity of two comets. Thereby the two objects instead of being different comets with parabolic orbits were recognized to represent a single comet, moving in a definite ellipse. It is quite probable that a proper study of the existing comet lists may readily lead to many identifications. Many pretty results might be cited in connection with the various advantages to which I have referred above as obtainable from a proper formulation of Laplace's method. Undoubtedly there are cases where Gauss's and Olbers's methods would converge more rapidly than my own formulation of Laplace's method, but these are readily ascertained at the outset. Orbits have been computed at Berkeley for practically every comet since the methods have been perfected, and so far every case has readily yielded to a solution.

In recent times notable memoirs have been written on orbit theory by Harzer, Charlier, and Moulton. I have not as yet had an opportunity to study Harzer's new geometrical methods with respect to their practical value. With the claims made in Moulton's memoir on the "Theory of Determining Orbits," published in the *Astronomical Journal* in 1914, I can not, unfortunately, find myself entirely in accord. The object of the memoir is set forth to be, on the one hand, to clarify the problem mathematically, and, on the other, to define the extent of the indeterminateness. In spite of the noted mathematical skill of Moulton it appears that although his forms have the merit of symmetry, his treatment of the problem which involves determinants of the ninth order, though readily reduced, offers no advantages over the simplifications arising from earlier combinations of geometrical and dynamical relations. To his misconception of the practise of the computers at Berkeley, with reference to the interpretation of the accuracy of their results, I have already referred. These misconceptions apply also to the significance of a number of theoretical and practical points, particularly to his interpretation of the vanishing of the chief determinant. Quite contrary to his statement in his "Celestial Mechanics" that in general the expressions for ρ and ρ' become indeterminate when the determinant referred to is zero and that they are poorly determined when it is small, it may be shown that the orbit is in general well determined when the determinant is definitely zero or definitely small, and that the determinateness of the solution does not depend so much on the numerical value of the determinant, but upon the accuracy with which this numerical value can be found. Thus a large range of solution may result for a comparatively large value of the determinant, if that value has a large percentage error. These conditions have been partially set forth in Buchholz's "Klinkerfues Theoretische Astronomie," but reference has been made by Moulton only to the first and initial draft of the methods as published in 1902. It is, of course, not my intention at this time to undertake a detailed analysis of Moulton's memoir. This must be deferred to some more appropriate time and must be done in more explicit form. With reference to the formulation finally adopted by Moulton it may readily be shown that it reverts to Gauss's method.

The most notable and classic contributions to the orbit theory in recent years have been made by Charlier. In a number of memoirs he has set forth the fundamental principles of the problem and has thrown much light on the subject with reference to many details, but his most important contribution is the resumption of Lagrange's incomplete analytical solution, a pure analytical solution in series, which admits of determination of the higher terms by direct computation without involving successive approximations of any kind nor requiring an improvement of the lower terms. His theoretical developments hold out the highest promise of successfully conquering the problem in practise without the complications existing in the older methods. But at present serious practical difficulties still exist, chief among which is that the series involved become extremely complicated when a high degree of accuracy is required, and that the method is subject to several of the limitations of the older methods. If these complications and limitations can be removed the method will be the best in existence. One of the chief limitations affecting practise consists in the fact that it is a general method and that it therefore may lead to orbits within the range of solution which are not acceptable from experience. It has been applied to the computation of several planet and comet orbits by Charlier and his associates. In the case of Comet *e* 1906 the resulting orbit is an hyperbola with an eccentricity 1.46. This seems to represent a solution near the upper edge of the range. A parabolic solution has been produced from the observations without difficulty in the first approximation by my formulation of Laplace's method by Miss Levy. In the case of another comet the elements are slightly hyperbolic; in the case of planet (702) the orbit deduced by the Charlier-Legrange method from an arc of two months gives an angle of eccentricity differing by nearly four degrees from the corresponding angle deduced near the upper possible limit by Miss Levy. Under these circumstances it would be difficult to decide where to stop in the computation of the terms of his series in relation to the possible range of solution.

From the somewhat disconnected and incomplete observations which I have just made on the methods of determining orbits it is seen that the interest of investigators is directed along two distinct lines, purely mathematical and practical. A proper adjustment between the two is required by the demands of astronomical science. In this connection and in conclusion I may make reference to the possibility of determining the orbit of a highly disturbed satellite from a limited number of observations on the basis of Laplacean principles. It is not necessary to await the evaluation of all the 18 integrals of the problem of three bodies for the purpose of setting up a satisfactory orbit method. Laplace's method for the two-body case is not based on Newton's integrals, but by introducing numerical values for the geocentric velocities and accelerations in α and δ the differential equations are transformed into algebraic equations admitting of easy solution of r, ρ, its velocity and acceleration. This equation has been found to be of the seventh degree or of the eighth if the always existing root ρ equal zero be included. My equation for three bodies, that is, for the solution of orbits of disturbed bodies, is of the twenty-eighth degree and admits at most of three solutions, the examination of which makes it possible to decide whether a body is a satellite, planet, or comet, in cases where the physical appearance of the object does not settle this question in advance.

In spite of the extensive investigations that have been made on the orbit problem there is room for much improvement both in theory and in practise, improvement which can not fail to come through proper cooperation of astronomers and mathematicians.

A. O. Leuschner

University of California

SCIENTIFIC EVENTS

THIRD INTERSTATE CEREAL CONFERENCE

An executive committee, representing the U. S. Department of Agriculture and state experiment stations, has called a third interstate cereal conference to be held at Kansas City, Coates Hotel, June 12–14. This conference, which has the approval of the Secretary of Agriculture and the directors of the state stations, is for the purpose of discussing questions involved in and work essential to accomplish the enlargement of cereal production and the economic utilization of cereals during the existing war emergency. In addition to the representatives of the institutions mentioned, the flour mills, grain inspection departments, grain dealers and manufacturers of cereal foods and corn products of the grain states are invited to send delegates. Some of the subjects to be discussed are:

Agricultural War Measures in Kansas.
Waste in Cereal Production on the Farm.
The Importance of Good Seed.
The Proportion of Flour and By-products in Milling.
The Preparation of Land for Wheat.
The Use of Barley as a Food.
Corn as a War Crop.
Treatment of Seed Grain for the Prevention of Smuts.
Analysis of the World's Wheat Supply.
The Importance of Grain Sorghums.
Federal Standards for the Grading of Wheat.
Weed Seeds in Relation to Grain Grading.
The Importance of Testing Spring Wheat for Germination.
The Next Step in Improvement in Wheat Cropping.
The Work of Committee on Seed Stocks, U. S. Department of Agriculture.

The delegates to the conference are invited to Manhattan, Kansas, on June 15, to inspect the cereal field work of the Kansas Agricultural Experiment Station.

Charles E. Chambliss,
Secretary

U. S. Department of Agriculture,
Washington, D. C.

THE SOCIETY OF INDUSTRIAL ENGINEERS

The Society of Industrial Engineers, a national organization, the membership of which is to comprise men and women who are industrial engineers, professional technical engineers, accountants, managing executives of commercial and industrial activity, writers, educators and students, was planned in Chicago on May 26. The Society will be permanently organized in Washington, on June 15, on which date the directors have been called to meet.

Charles Buxton Going, for twenty years editor of the *Engineering Magazine*, New York, was chosen provisional President and *pro tem* chairman of the board of directors which was chosen at the session. This board, comprising 15 prominent men from various sections of the United States, the majority of whom have accepted, includes:

Charles Buxton Going, New York; C. E. Knoeppel, industrial engineer and organization counsel, New York; Frank B. Gilbreth, industrial engineer, Providence, R. I.; E. C. Shaw, vice-president The B. F. Goodrich Co., Akron, Ohio; Harrington Emerson, industrial engineer, New York; Charles Piez, president