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SOME MATHEMATICAL ASPECTS OF COSMOLOGY[1]

I. COSMOGONY

THE intimate relationship between mathematics and astronomy, even in ancient times, is well known; and since the time of Kepler, Galileo and Newton astronomy has been the ideal exact science. From a mathematical point of view, it must be admitted that we have been very fortunate in the fact that the earth is merely one of a family of relatively small planets, and that the earth possesses an extraordinary satellite, so extraordinary that an inhabitant of Mars would doubtless say that the earth and moon is a double planet rather than a planet and satellite. We are fortunate also in the fact that the earth is only 50 per cent cloud-covered instead of 100 per cent. as may be the case on the planet Venus. Finally, the regular succession of day and night, the waxing and waning of the moon and the annual circuit of the sun among the stars are uniformities in nature that can escape the attention of no one, save perhaps one who lives in a large modern city.

The somewhat less regular motions of the planets with respect to the background of stars excited attention among the ancients and stimulated the search for simple schemes to account for them. Even some slight irregularities in the motions of the sun and moon were discovered more than two thousand years ago, and a scheme of epicycles was invented to account for them, a compounding of uniform circular motions which is the geometrical equivalent of our modern Fourier Series. A dynamical explanation was not sought because it was the age of geometry, and dynamics had not yet been dreamed of. Even Kepler, who devoted a lifetime to the discovery of uniformities in the motion of the planet Mars, and who discovered the three laws of planetary motion which bear his name, did not seek a dynamical explanation of these uniformities. He was content to ascribe them to the intelligence of an angel who guided the planets in their courses. His was the age of spirits, and Kepler's interpretation of uniformities was animistic.

The foundations of dynamics came only with the genius of a Galileo who had little liking for the conceptions of animism. His induction that the natural state of a body was uniform motion in a straight

[1] A symposium lecture read before the American Mathematical Society at its meeting in Chicago, April 10, 1925.

line, and that a departure from that state was due only to force, was one of those great breaks with the past which occur at rare intervals in human history, and which have raised the race of men to its present intellectual level. Galileo initiated a new age, the one in which we ourselves live, the age of dynamics. He was followed quickly by an even greater genius, Newton, who not only completed the foundations of dynamics, but also developed the mathematical concepts which were necessary for progress in the realm of dynamics. Not only was a new mathematical science, coordinate with geometry, brought into being, but mathematics itself was given a forward impetus that seems to gather headway with the passing decades. Furthermore, the consequences are not restricted to the domain of pure intellect; the entire human family is living in a new era. It seems safe to say that the development of the science of dynamics has been the most fruitful and beneficent development in the experience of the race.

The application of the new science of dynamics to the problems of astronomy was immediate. Newton himself was able to show that Kepler's three laws of planetary motion led directly to a law of attraction of the planets towards the sun according to which the force varies inversely as the square of the distance of the planet from the sun; and it required but a slight generalization of this to attain the extraordinarily simple law of gravitation. Newton completely solved the problem of two bodies, and obtained a considerable measure of success in the problem of the motion of the moon. Eclipses and the sudden appearances of comets, that formerly were sources of terror and fright, now fell into the class of orderly and interesting phenomena. Eclipses could be foretold with accuracy, and Halley even dared to predict the return after seventy-five years of the comet which is known by his name.

The differential equations of motion of three or more bodies were first published by Clairaut, who also gave the ten classical integrals of the center of gravity, moments of momenta and energy. Notwithstanding its apparent simplicity from a physical point of view, the complete problem of three bodies has resisted heroic efforts of the most eminent mathematicians from Newton's time to the present day. Regrettable as this may be, it was found possible to develop a mathematical theory of the motions of the planets and satellites of our system, including their mutual actions and reactions, which, for most of the purposes of astronomy, is satisfactory; even the very difficult lunar theory is almost, though not quite, all that could be asked. The brilliant work of Newton, Clairaut, Euler, Lagrange, Laplace, Gauss, Jacobi, Poincaré and many others in the fields of celestial mechanics have erected a monument to the human intellect that can never be forgotten as long as the mathematical faculties of men are active. When we consider the extremely accurate determinations of position that can be made with the telescope, and the very close agreement of theory and observation, it is easy to understand how celestial mechanics has been, and will continue to be, a source of inspiration to workers in every other field of scientific endeavor. It has virtually attained the goal which all other sciences seek, namely, complete and accurate prediction. It should not be forgotten, however, that the reason for this success lies in the extreme simplicity which characterizes our planetary system, together with its almost complete isolation from outside influences.

Indeed this simplicity, together with certain uniformities in the motions of the planets and satellites, led Laplace to depart from the purely mathematical fields, and to speculate upon how all this thing came to be. Whatever be the fate of the ideas which he put forth upon this subject, I can not but feel that this speculation was one of the most valuable things which Laplace did. With certain limitations as to rigor, he had proved the stability of the planetary system, that is to say that the major axes, eccentricities and inclinations of the system fluctuate only within small limits, and in the long run remain unchanged. Mathematically, his conclusions cover the past as well as the future. The system always has been this way and always will be. And yet, forgetting all this, he wondered how the system came to be as it is now. In doing so he recognized the fact, which many experts with mathematical formulae are prone to forget, that not all nature is contained in the differential equations of motion and that the conclusions drawn from these equations are valid only for those periods of time within which nothing extraneous intervenes.

I need not relate that Laplace supposed that our solar system was once a great hot nebula, the boundaries of which extended beyond the orbit of the outermost planet; that by the radiation of its heat into space its size diminished, and therefore, owing to the conservation of angular momentum, it rotated more rapidly; that rings, formed around the equator, were abandoned by the contracting nebulous mass; and that the matter comprising these rings gradually gathered into the more compact and stable form of spheres, and that these spheres now constitute our planetary system. It was a beautiful idea, and it made a very strong appeal to the scientific imagination during the entire nineteenth century. It was simple; even a layman could understand it; it satisfied somewhat our natural curiosity to know the origin of things; and it was sponsored by a great mathematician. What more could be asked?

It must be admitted that ideas with respect to heat and energy were somewhat hazy in the days of Laplace; and it is indeed very fortunate for celestial mechanics that gravitation is independent of temperature. Shortly after Laplace published his hypothesis of the origin of our family of planets, it was proved by Count Rumford that heat is a mode of motion and therefore a form of energy, and by the middle of the century physicists had put forth the doctrine of the conservation of energy, recognizing clearly that energy exists in many forms which are convertible into one another. The doctrine of the conservation of energy is sometimes called the first law of thermodynamics. There is a second law of thermodynamics which states, in the words of Lord Kelvin, that the energy which is available for useful work always tends towards a minimum; or, in modern terms, that the entropy of an isolated system tends towards a maximum.

These new ideas with respect to energy enabled Helmholtz to make a great contribution to the nebular hypothesis of Laplace. Since gravitational potential energy is convertible into the energy of heat, it was possible to compute how much heat would be developed by a large gaseous mass in contracting from an infinite dispersion to the size of a given sphere as soon as the numerical relationship between ergs and calories was known. This fact was pointed out by Helmholtz in a popular lecture at Königsberg in 1854, a lecture which was translated and printed in the *Philosophical Magazine* of 1856, under the title "On the interaction of natural forces." Helmholtz computed that at the present rate of radiation the supply of the sun's heat thus generated would last for 20,000,000 years. He states that the duration of human history was only 6,000 years, but that the geologists estimated the entire period of organic evolution at from one to nine millions of years. Hence his theory was quite ample for all purposes.

About fifteen years later, Lane[2] and Ritter[3] proved that if the sun is a monatomic gas contracting under the action of gravitation and the radiation of heat, its temperature varies inversely as its radius, and therefore its temperature is increasing. The contributions of Helmholtz and Lane to the nebular hypothesis were very important. They are entirely in harmony with the idea of Laplace; they simplify it by removing the necessity of an initial high temperature and by providing a supply of heat, that, in the middle of the nineteenth century, seemed to be entirely adequate.

The satellite systems of the planets are miniature solar systems. In the case of Saturn and Jupiter the resemblance is striking except for the retrograde motion of the recently discovered outermost satellites. Granted the correctness of the Laplacian hypothesis of the origin of the planets, nothing particularly new was required to explain the origin of the satellite systems. It was necessary only to repeat the process over again. The orbits of the satellites lie very nearly in the plane of the equators of their primaries; their orbits are very nearly circular, and their motion is forward in all the satellites which were known to Laplace. None of them, with one exception, has a mass greater than 1/4000 of its primary, and most of them are much smaller. Among the planets the largest one, Jupiter, has a mass equal to 1/1000 of the mass of the sun, the others being much less.

The exceptional satellite just mentioned is our own moon. Its mass is 1/81 of the mass of the earth, and the inclination of its orbit to the plane of the equator varies from 18.5° to 28.5°. It is much more closely related to the plane of the ecliptic, to which it is inclined only 5°. Its orbit is nearly circular and its motion is forward. From its exceptional character one is tempted to regard the earth-moon system as a double planet. These facts led Sir George Darwin in 1879 to believe that in its origin it had not followed the Laplacian scheme. The planets and other satellites had been formed from a thin shaving taken from the equator of their primary while still in a gaseous condition. Darwin thought that the moon had separated from the earth while in a liquid state, owing to an instability arising from a too high angular velocity, and that after separation from the earth the two bodies continued for a time to rotate as a rigid system about their common center of gravity.

In 1880 Darwin summarized his ideas as to the early history of the moon as follows:[4]

> We begin with a planet not much more than 8,000 miles in diameter, and probably, partly solid, partly fluid, and partly gaseous. This planet is rotating about an axis inclined about 11° or 12° to the normal to the ecliptic, with a period of from 2 to 4 hours, and revolving about the sun with a period not much shorter than our present year.
>
> The rapidity of the planet's rotation causes so great a compression of its figure that it can not continue to exist in an ellipsoidal form with stability; or else it is so nearly unstable that complete instability is induced by the solar tides.
>
> The planet then separates into two masses, the larger being the earth and the smaller the moon. I do not attempt to define the mode of separation, or to say whether the moon was more or less annular. At any rate it must be assumed that the smaller mass became more

[2] J. Homer Lane: "On the theoretical temperature of the sun," *Am. Jour. Sci.* (1870).

[3] Various memoirs in Wiedemann's Annalen (1878–1883).

[4] "Scientific Papers of Sir George H. Darwin," Vol. II, 367.

or less conglomerated, and finally fused into a spheroid—perhaps in consequence of impacts between its constituent meteorites, which were once part of the primeval planet. Up to this point the history is largely speculative, for though we know the limit of stability of a homogeneous mass of rotating liquid, yet it surpasses the power of mathematical analysis to follow the manner of rupture when the limiting velocity of rotation is surpassed.

We now have the earth and moon nearly in contact with one another and rotating nearly as though they were parts of one rigid body. . . .

As the two masses are not rigid, the attraction of each distorts the other; and if they do not move rigorously with the same periodic time, each raises a tide in the other. Also the sun raises tides in both.

In consequence of the frictional resistance to these tidal motions, such a system is dynamically unstable. If the moon had moved orbitally a little faster than the earth rotates she must have fallen back into the earth; thus the existence of the moon compels us to believe that the equilibrium broke down by the moon revolving orbitally a little slower than the earth rotates. Perhaps the actual rupture into two masses was the cause of this slower motion; for if the detached mass retained the same amount of momentum that it had initially, when it formed part of the primeval planet, this would, I think, necessarily be the case.

In consequence of the tidal friction the periodic time of the moon (or the month) increases in length, and that of the earth's rotation (or the day) also increases; but the month increases in length at a very much greater rate than the day.

At some early stage in the history of the system, the moon has conglomerated into a spheroidal form, and has acquired a rotation about an axis nearly parallel with that of the earth.

He continues with an explanation of the effects of the tidal interaction upon the periods of rotation and revolution, upon the eccentricities of the orbit and obliquities of their axes; and states that it has required not less than 54,000,000 years to bring the system to its present state.

It will be noticed that Darwin was somewhat hazy at this time about the mode of separation, but was rather inclined to the idea of a ring which was gathered together to form a spheroidal mass which was just out of contact with the earth, the whole rotating as a rigid system.

Poincaré's attention was attracted towards the problem of the figures of equilibrium of rotating *incompressible* fluid masses and their stabilities by certain theorems which were stated, without proof, in Thomson and Tait's "Natural Philosophy." Poincaré's interest led him to publish a long and extremely interesting paper on the subject in *Acta Mathematica* for 1885.[5] MacLaurin had found in the eighteenth century that a series of oblate spheroids satisfied the conditions of equilibrium, and in 1834 Jacobi had found a series of ellipsoids with three unequal axes which also satisfied the requirements. Poincaré proved that for small angular velocities the MacLaurin spheroids were stable up to the point where they crossed the Jacobi series of ellipsoids. At the point of crossing the stability passes from the MacLaurin series to the Jacobi series. For higher rates of rotation the ellipsoids are stable up to a certain rate, at which the ellipsoids, too, become unstable.

For still higher rates of rotation Poincaré found, by the use of the ellipsoidal harmonics of Lamé, a series of figures which are unsymmetrical, and which were called pear-shaped by Darwin. Poincaré thought he had proved that this new series started out by being stable,[6] but Schwarzschild pointed out in 1896 that Poincaré had made an error in his criterion of stability,[7] which was admitted by Poincaré in 1901,[8] and the question of the stability of those figures was left open.

In the conclusion of his 1885 paper Poincaré said:

Let us consider a homogeneous, rotating fluid mass, and imagine that this mass contracts, with slow cooling, but in such a way as to remain always homogeneous. Let us suppose that the cooling is sufficiently slow and the internal friction is sufficiently great that the angular rotation remains the same throughout the fluid. Under these conditions the fluid will take always a figure of equilibrium which is stable, and the moment of momentum will be constant.

At the beginning the density will be very small, and the figure of the mass will be an ellipsoid of revolution differing little from a sphere. The cooling will at first increase the flattening of the ellipsoid, which, however, will remain an ellipsoid of revolution. When the flattening has become very nearly equal to 2/5, the ellipsoid will cease to be one of revolution and will become a Jacobi ellipsoid. The cooling continuing, the mass ceases to be ellipsoidal; it becomes unsymmetrical with respect to the yz-plane, and takes the form given in the figure on page 347 (the pear-shaped figure). As we have already observed with respect to this figure, the ellipsoid seems to be compressed slightly around the middle but nearer one end of the major axis than the other; the larger part of the mass tends to approach a spherical form while the smaller part moves towards one end of the major axis as if it sought to detach itself from the larger mass.

It is difficult to state with certainty what will happen if the cooling continues, but it is permissible to suppose that the mass will become creased deeper and deeper, until it is cut through and finally separates into two distinct masses.

[5] "Sur l'Équilibre d'une masse fluide animée d'un mouvement de rotation," Acta Mathematica, VII (1885).

[6] *Loc. cit.*, 378.

[7] K. Schwarzschild, Münchener Inaug. Dissert. (1896).

[8] "Sur la stabilité de l'Equilibre des Figures Pyriformes affectées par une Masse Fluide en rotation," Phil. Trans. 196 (1901), 333.

One might try to find in these ideas a confirmation or a refutation of the hypothesis of Laplace, but it must not be forgotten that the conditions are very different, since our mass is homogeneous while the nebula of Laplace was strongly condensed at its center.

It is evident at once that Poincaré's ideas exactly filled the gap in Darwin's theory, which has become known as the "fission theory." That they appealed strongly to Darwin goes without saying, and this interest resulted in a number of papers by Darwin during the succeeding twenty years. His first effort, in 1887, was a study of the figures of equilibrium of two fluid masses revolving as a rigid system. It is evident, if the two masses are far apart and the rate of revolution therefore is slow, that the figures of the masses are prolate spheroids which differ but little from spheres, and that these forms are stable. There are other forms in which the masses are very elongated, but these forms are unstable. They correspond to the very much flattened unstable MacLaurin spheroids.

As the distance between the two masses diminishes, the rate of revolution increases, and the masses become somewhat oval in shape with the small ends of the ovals pointing towards each other. If the two masses are brought close enough together to be touching, Darwin showed that the figures are unstable. Eventually, in 1906,[9] Darwin proved that the stability ceases for two equal masses when the distance between their centers is equal to 2.638 r, r being the radius of the sphere of the combined mass. It is very interesting to compare this value with the limiting distance of stability, 2.45 r, which Roche [10] had found in 1850 for an infinitesimal fluid satellite revolving about a rigid spherical primary. It seems to make little difference whether the mass is nearly all in one of the bodies or whether it is equally divided between the two; the distance of limiting stability is approximately two and one half times the radius of the sphere of the combined mass, and this distance is known as Roche's limit.

In 1901, Darwin attempted to prove the stability of the pear-shaped figures by numerical processes, and his conclusion was that, at least at the beginning of the series, they are almost certainly stable.[11] In 1905, however, Liapounoff[12] announced in St. Petersburg that he had obtained a rigorous solution of the problem and that the pear-shaped figures are unstable. Evidently Darwin did not see Liapounoff's proof, which was published only in abstract, for he found difficulty in accepting his conclusion. The matter was therefore uncertain until Jeans took up the problem in 1915[13] and proved definitely that Liapounoff was correct; Darwin, too, had made a mistake in his criterion of stability. The conclusion therefore seems to be final that there does not exist a continuous series of stable figures of equilibrium which connects the critical Jacobian ellipsoid with the limiting configuration of equilibrium of double masses.

Notwithstanding their instability, Jeans thinks that this pear-shaped series "are of the utmost importance in directing the course of dynamical or cataclysmal motions such as occur when statical evolution is no longer possible."[14] This opinion is somewhat difficult to understand. If a man, walking along a ridge, falls off, his subsequent motions depend very little upon the particular manner in which the ridge continues. The fact that the pear-shaped figures are unstable seems to make them of as little interest as all the other unstable series in this problem.

Jeans has also supposed[15] that the cataclysm which occurs after passing the critical Jacobian ellipsoid may be represented by a jump from the ellipsoidal configuration to the double-mass configuration of the same angular momentum, although he points out that this can occur only if the ratio of the masses is less than one third, since for greater ratios stable double-mass configurations do not exist. Since the moment of momentum of the system remains constant, the amount of this jump, computed on the assumption that the two masses are constrained to be spheres, is as follows:

Ratio of the masses	.3	.2	.1	1/81
Distance between centers (jump)	4.r	7.25r	22r	1047r
Energy of the system	.5403$\frac{M^2}{r}$	.5178$\frac{M}{r}$	.4765$\frac{M^2}{r}$	.4264$\frac{M^2}{r}$

From these figures it is seen that the jump increases rapidly as the ratio of the masses decreases, and the energy of the system also decreases. For the earth-moon system, in which the ratio is 1/81 and r about 4,000 miles, the jump is about 4,000,000 miles, or 16 times the present distance of the moon, and the energy lost in the transformation would be sufficient to heat the entire system some three or four thousand degrees.

It will be observed that there is only one ratio of the masses, about .29, for which both the energy and the angular momentum of the double mass system

[9] Scientific Papers, III, 513.

[10] La Figure d'une Masse fluide soumise à l'attraction d'une Point eloigné, Acad. de Montpelier, I (1850).

[11] *Ibid.*, III, 316.

[12] Sur un Problème de Tchebychef—Memoirs of the Imperial Academy of St. Petersburg, XVII (1905). See, also, Darwin, III, 391.

[13] Phil. Trans. 217 A., also, "Problems of Cosmogony and Stellar Dynamics," 101.

[14] "Problems of Cosmogony," 102.

[15] "Problems of Cosmogony," 134.

agree with the corresponding values of the critical Jacobi ellipsoid, the energy of the Jacobi ellipsoid being .5377 M^2/r. If such a jump occurs it seems certain that the ratio of the masses must lie between 1/4 and 1/3, a small range being necessary to allow for possible thermal changes.

It has been commonly assumed that if a satellite were brought within Roche's limit, that is, two and one half times the radius of the sphere of combined mass, by any means whatever, the satellite would be broken up by tidal stresses and its remains scattered about the primary in the form of a ring. This seems entirely reasonable, and I know of no dissenting opinion. The rings of Saturn apparently furnish an example of the consequences of this process, as the radius of the outermost ring is 2.3 times the radius of the planet, the radius of the nearest satellite on the same scale being 3. In 1859 Clerk Maxwell proved, under certain reasonable assumptions, that such a ring would be stable if its density were small enough.

On the other hand, the fission theory requires us to suppose that after the critical ellipsoid of Jacobi is passed, either the mass separates at once into two parts which jump immediately beyond Roche's limit, or else it separates into many parts which eventually are reunited by their mutual actions into two masses outside of Roche's limit. The jump hypothesis involves the difficulty of radial velocities which would bring the two masses together again in a second cataclysm possibly greater even than the first. If one were compelled to admit, by concrete examples found in nature, that a single mass which becomes unstable through excessive angular momentum does separate into two stable masses, then the second hypothesis would seem to be the more reasonable one, but we should have to admit that we do not see how the thing is done. One can admit that eventually a ring is formed which is composed of small discrete masses, with a total mass and angular momentum sufficiently great to permit the primary mass to become spheroidal, but the satellite state is, at present, beyond us. There is nothing in the astronomical situation, however, which compels us to make such an admission, although Russell thinks the multiple stars are such as would be expected on the hypothesis of fission.[16]

Moulton[17] has made a critical examination of these abstract ideas as applied to the solar system. The celestial bodies are not homogeneous, and it is generally assumed that the density decreases from the center towards the circumference. As MacLaurin pointed out, this fact makes the celestial bodies less oblate than a homogeneous body for a given rate of rotation. Furthermore, we have to deal with a single mass whose angular momentum is constant and whose density is increasing, rather than with a mass whose density is constant and whose angular momentum is increasing. This is important since, if ω is the rate of angular velocity. and ϱ is the density, the eccentricity of the figure of equilibrium depends not upon ω^2 alone but upon the ratio ω^2/ϱ. As the fluid mass radiates its heat and contracts both ω and ϱ increase, in such a way however that ω^2/ϱ slowly increases. Admitting that the process goes on indefinitely, every rotating fluid mass would eventually reach the point where the MacLaurin spheroid branches into the Jacobi ellipsoid. If the earth and moon originally were a single homogeneous mass with the angular momentum of the present system this branch point would not be reached until the density of the mass was 215 times the density of water and its radius something less than one third of the present radius of the earth.[18] As heterogeneity of density only makes the situation worse, the demonstration is complete that the moon did not separate from the earth through rotational instability while the mass was in a liquid state.

So small is the angular momentum of the sun that it will not reach the branching point until it has shrunk to a size in which its equatorial radius is only 11 miles and its period of rotation is 55.4 seconds. The most favorable case in the solar system is Saturn, whose present density is .6 of the density of water and equatorial radius about 37,500 miles. When it reaches the same branch point, its density will be 21 times the density of water and its equatorial radius 14,200 miles; but it will not reach the critical Jacobi ellipsoid until its density is about 95, its longest radius 13,400 miles, and its shortest 4,700 miles. It is evident from these figures, which are due to Moulton, that the fission theory finds no application in the solar system.

As one of the attractive features of the hypothesis of Laplace was that it not only could be appealed to in accounting for the planetary systems, but was also available for explaining the satellite systems, so also is it an attractive feature of the fission hypothesis, which was devised to account for the origin of the moon, that it is also available in accounting for the origin of the binary stars. Aitken states in his book on the binary stars[19] that at least one star in 18, down to the 9th magnitude, is a close double

[16] Russell, H. N., "Origin of binary stars," *Astrophysical Journal* (1910).

[17] See "The tidal and other problems," Publication 107 of the Carnegie Institution of Washington, 79 (1909).

[18] *Loc. cit.*, 151.

[19] "The Binary Stars" (1918), 255.

star visible with the 36-inch Lick telescope. When the spectroscopic binaries are taken into account, however, it is estimated that 40 per cent., or even more, of the stars are double.[20] Here, then, is a great field for the fission hypothesis.

Stars, however, are certainly not incompressible liquids. With respect to the state of their interiors we know nothing at all, but as a mathematical model undoubtedly a compressible gas is much more acceptable than an incompressible liquid; but even a quiescent gas theory may be only a rough first approximation, notwithstanding the theory be mathematically complete, since a star is the most energetic thing we know anything about. The mathematical theory of the gas model, as might be expected, is much more difficult than the theory of the incompressible model[21] and there is little that can be stated that does not rest largely upon conjecture. We are virtually thrown back upon the incompressible liquid for our intuitions. So far as we can depend upon this model, the observations of the binary stars are unfavorable to the fission theory. We have already seen that for this model the ratio of the masses can not exceed one third. But the observations of the binary stars do not harmonize with this ratio.

If the fission theory is to apply to any stars at all it must be to the spectroscopic binaries, since Moulton[22] has shown, and the same results were obtained later by Russell[23] and by Jeans,[24] that after fission has occurred the mutual tidal actions can never separate the two stars very far, so that if the visual binaries were ever formed by fission they were formed while the mass was still in the nebulous state; or perhaps better, the nebulous mass had two centers of condensation from the start, and the theory of fission is not applicable.

Aitken gives a list of 32 spectroscopic binary stars[25] for which the ratios of the masses were known in 1918. In this list there are but two stars for which the ratio is less than one third, and the average ratio for the entire 32 is .748. In the third list of spectroscopic binaries issued by the Lick Observatory[26] which is complete to July 1, 1924, the ratios of the masses are given for 71 stars. There are only four stars in this list for which the ratio of the masses is less than one third, and the average ratio for the 71 stars is .746, which is practically identical with the average of Aitken's list. For 24 of these stars the ratio lies between .9 and 1.0, and for 14 it lies between .8 and .9. It is evident that an approximate equality between the masses is the rule.

It is a fair guess, therefore, that the fission theory does not account for the spectroscopic binaries. It is not applicable to the visual binaries, and it does not fit anywhere within the solar system. I can not, therefore, but differ from Jeans when he states:[27] " . . . a double star must be supposed to be born as a result of cataclysmic motion," that is, by the process of fission; and agree with the opinion expressed by Moulton[28] when he states that his results "are so uniformly contradictory to its implications as to bring it into serious question, if not to compel us to cease to consider it, even as a possibility."

There is no doubt that from the mathematical point of view the theory of fission, as set forth by Poincaré and Darwin, is the most attractive portion of cosmogony. Like so much of his work, Poincaré's paper on the figures of equilibrium of rotating fluid masses is a masterpiece. Darwin's work is not characterized by mathematical brilliancy, but one can hardly read his memoirs on this subject without a feeling of the highest respect for his work. His patience and industry, his honesty and extreme modesty with respect to himself, his thoroughness in the examination of all details, command one's entire confidence, and make one feel that Darwin's attitude towards his problem is a model which should be emulated by all scientific workers who labor in regions in which definite conclusions can not be reached. One turns from this theory with a feeling of profound regret that the evidence seems to be fairly conclusive that, in the birth of the cosmic forms, nature has not followed this model.

During the entire nineteenth century work in cosmogony was entirely in the hands of the mathematicians. Contraction and rotational instability were the central features. During the last two years of the century T. C. Chamberlin[29] entered the field from the domain of geology. With him came a new set of ideas, and a somewhat new mode of treatment.

We do not generally regard geology as a mathematical science, but, notwithstanding this, we can not deny that a competent geologist has a right to cosmogonical opinions. Indeed, a geologist has a closer and more intimate experience with one of the cosmic bodies than either an astronomer or a mathematician, and if he ventures to formulate an opinion in the difficult field of cosmogony the abstract worker

20 *Loc. cit.*, 274.

21 See Jean's "Problems of Cosmogony," Chap. VII.

22 *Loc. cit.*, p. 107.

23 *Loc. cit.*, p. 191.

24 *Loc. cit.*, p. 260.

25 "The Binary Stars," 205.

26 Lick Observatory Bulletin No. 355.

27 "Problems of Cosmogony," 252.

28 *Loc. cit.*, 133. See also p. 160.

29 T. C. Chamberlin: "An attempt to test the nebular hypothesis by the relations of masses and momenta," *Jour. Geology* (1900).

must listen to his ideas with respect. A pure geologist, however, would be in danger of running amuck in the china closet of dynamics, just as did the philosopher, Kant; and in associating himself with F. R. Moulton,[30] Chamberlin formed a very happy combination of talents that gave promise of being fruitful. To-day no one would think of framing a hypothesis of the origin of the planets without considering very carefully its geological implications. The matter is no longer purely mathematical, nor purely astronomical. It is clearly a mathematical-astronomical-geological problem.

The nebular hypothesis of Laplace had ignored everything outside of our own nebula. It asserted that, once upon a time, the earth was an incandescent liquid mass slightly larger than at present, surrounded by an atmosphere which contained all the water which is at present in the ocean, and therefore 300 times as massive as it is at present. In addition to this it contained all the carbon dioxide which is at present locked up in coal and the sedimentary rocks. These conditions imply a climate, which, geologically speaking, became progressively cooler as the crust of the earth cooled, and the atmosphere was gradually relieved of its excess burden of water and carbon dioxide. Unfortunately, these relations can not be expressed in mathematical formulae; but they are very real for all that, and they must be checked up with the evidence of the rocks.

In the introduction to "The Origin of the Earth," Chamberlin writes:[31]

But this theory of a simple decline from a fiery origin to a frigid end, from a thick blanket of warm air to a thin sheet of cold nitrogen, consonant with the current cosmogony as it was, logical under the premises postulated, pessimistically attractive in its gruesome forecast, already in possession of the stage, with a good prospect of holding it—this theory of a stupendous descensus none the less encountered some ugly facts as inquiry went on. It seemed to accord well enough with an ice age *if* the ice age came *only* in the later stages of the earth's history, but it was ill suited to explain an ice age in the earlier geological eras. Unfortunately for it, there began to appear signs of ice ages far back in time, and, besides, some of these had their seats much nearer the equator, and, in other respects, were even stranger than the latest great glaciation. The evidence of these later and stranger glaciations was at first quite naturally received with incredulity, but the proof grew steadily stronger with every new test, and the range of evidence was found wider and clearer as exploration advanced. While all this should have weakened, and did weaken, the fundamental concept of great warmth and a rich atmosphere in the earlier ages, while it should have roused skepticism as to the verity of the cosmogony on which it was based, and perhaps did so, still the old thermal concept and the old cosmogony continued to hamper all attempts at a radical revision of glacial theories. . . .

. . . In the course of this,[32] still further departures from the generalizations of the inherited view came to notice. Desiccation products were found to be scarcely less abundant and characteristic in the early strata than in the later, and no steady progress from humidity to aridity seemed to mark the progress of time; nor were there found any evidences of even an oscillatory progress from predominant humidity to predominant aridity. If the record favored any generalization it seemed to be that the severest and most prevalent period of aridity fell near the middle of the stratigraphic record.

The implications of the nebular hypothesis are out of harmony with the history of the earth as revealed by the geological record. Moulton found them to be out of harmony also with the present dynamic of the solar system. For example, the present angular momentum of the solar system is less than 1/200 part of the angular momentum which the system must have had when the ring of Neptune was formed, notwithstanding that the elementary principles of dynamics require that the angular momentum of the system shall be constant; the axis of rotation of the sun is 5° out of its proper position; when the ring of Jupiter was formed one tenth of one per cent. of the mass received 96 per cent. of the moment of momentum; some of the satellites of Jupiter have forward motion, some have backward motion; similarly, with respect to the satellites of Saturn; one of the satellites of Mars has a shorter period of revolution than Mars' period of rotation; similarly, the period of the inner ring of Saturn is shorter than Saturn's period of rotation; the high eccentricities and inclinations of the orbits of Mercury and the asteroids are unexpected. There are other objections, but these are enough. It is abundantly evident that the nebular hypothesis of Laplace does not tell the true story of how our planetary system was formed: both astronomy and geology cry out against it and demand that a new story of its birth shall be told. The concept of rotational instability has been tried out in its various aspects during an entire century, and it has been found wanting.

The planetesimal hypothesis[33] of Chamberlin and

[30] F. R. Moulton: "An attempt to test the nebular hypothesis by an appeal to the laws of dynamics," *Astrophysical Jour.* (1900).

[31] Chamberlin, T. C., "The Origin of the Earth," p. 4 (1916).

[32] Chamberlin, T. C., *Op. cit.*, p. 7.

[33] T. C. Chamberlin: "Fundamental Problems of Geology," Year Book No. 3 (1904) of the Carnegie Institution of Washington, p. 195–258, and subsequent issues.

F. R. Moulton: "Evolution of the solar system," *Astrophysical Journal* (1905).

Moulton appeals to another principle, namely, dynamic encounter of the sun with another star. In the zoological world the lowest types of animals multiply by simple division, much as Darwin and Poincaré supposed a rotating liquid mass to do. But in the higher types of life two parents are required in the process of generation; and this is remotely analogous to the generation of the sun's family of planets. It is a bi-parental process.[34] If I remember correctly, it was Lord Kelvin who likened the galaxy to a gas of which the molecules are stars. As in the kinetic theory of gases the collision of the molecules is a fundamental event, so in the dynamics of the galaxy the close approach of two stars is a fundamental event; the time scale in the two cases, of course, is very different. However improbable such an encounter may be for a given star and a given century, nevertheless in the course of time they are inevitable for all stars. If we take a sufficiently large survey of the galaxy we are compelled to face the question: What are the consequences of the close approach, but not collision, of two large, hot, highly active, gaseous masses which are moving on hyperbolic orbits with respect to their common center of gravity?

There are many variable factors in such a situation, and quite likely a great variety of consequences may follow. The problem can be narrowed down somewhat by assuming that the sun had such an encounter some ten or twenty billion years ago, that at the time of the encounter it was in substantially its present condition, and that the distance of closest approach was neither too great nor too small for our purpose, which is, of course, the generation of our planetary system. The first obvious effect is that we have a tidal problem on our hands. On second thought, this tidal problem is complicated with a rotation of the sun about an unknown axis, and at an unknown rate. By the time that we have become adjusted to this idea, it has occurred to us that this is not a quiescent sun, sleek and complacent, but one with a fiery disposition, subject to explosions and great gusts of uncontrollable passion.

I am sure that even such a redoubtable mathematician as Poincaré would have fled precipitately from such a problem, but Chamberlin fortunately is a geologist, accustomed to volcanoes and earthquakes; therefore he stood his ground and prepared to see what would happen. Quite naturally, what he tells us is not couched in mathematical terms, but with Moulton standing guard over the interests of mathematics and dynamics we may be sure that, judged from the point of view of present ideas, the picture presented is essentially correct and sound. If later research and study shows that this is not the correct story, the difficulties, I fancy, will not be perfectly obvious ones.

The planetesimal hypothesis tells us that great tides were raised upon the sun, so that the shape of the sun ceased to be spherical and became somewhat prolate, its longest axis pointing towards the visiting star, but deflected slightly by rotation. The violent ascending and descending convective currents, which are always a normal part of the sun's activities, and which are responsible for or, at least, accompany, the great sun spots and prominences which make the study of the sun so interesting, were greatly stimulated by these vast tides, and were particularly violent in the direction towards and away from the passing star. What are now merely prominences that shoot up a quarter or a half a million miles, only to fall back upon the sun, were then intermittent streams of matter that left the sun with somewhat higher velocities so that some of it doubtless escaped from the sun's control altogether, some of it quickly fell back upon the sun, and some, slightly more than one tenth of one per cent. of the sun's mass, was deflected from its radial motion by the attraction of the visiting star and given an angular momentum about the sun in the same direction as the motion of the visiting star, thereby reducing the eccentricity of the star's orbit.

After the star had passed on its way and the sun had returned to its lonely state, there existed a large amount of matter that had been torn from the sun moving about it in orbits that were in general highly eccentric, and in all of which the motion was forward. Much of this material consisted of free molecules, each of which moved in a Keplerian orbit until that orbit was changed by collision with other molecules; some of it was in large gaseous masses, which are called nuclei, whose gravitative power was sufficiently great to resist the gaseous tendency of expansion and dissipation, and thereby to preserve their identity; and some of it consisted of smaller gaseous masses which could not wholly resist expansion and dissipation, but large enough to delay the process until a certain amount of condensation from the gaseous state to a foggy or a liquid or a solid state had occurred. Thus, in a relatively short time there were large gaseous nuclei, small and smaller liquid masses, very small solid bodies, and free molecules, each pursuing its own path like a tiny planet about the sun—hence the term planetesimals.

Owing to the high eccentricities of the orbits of

Chamberlin and Salisbury: "Geology," Vol. II (1906).
F. R. Moulton: "Introduction to Astronomy" (1906); also (1916).

[34] Chamberlin, T. C., "Origin of the Earth," p. 102.

these planetesimals there was a vast amount of crossing of paths, jostling and collisions. The larger masses gradually absorbed the smaller ones, and, in accordance with well-known principles in celestial mechanics, their orbits became fat and round, and the inclinations of their orbits to the plane of the passing star tended towards zero. In this manner the planets with their nearly circular orbits came into being. Large inclinations and eccentricities are to be expected only in small bodies for which the integration process was small, and it is only in the small bodies that they occur. There are no difficulties with angular momentum, as in the nebular hypothesis; and one can not say that the sun's axis of rotation is 5° out of place. That the axis of rotation of Saturn is 27° out of the perpendicular to its orbit and for the earth and Mars it is 24° out is a blow to the nebular hypothesis, but it causes no disturbance here. The axis of Uranus may be 90° from the perpendicular, and Neptune may rotate backwards without any one being surprised. The fact that there are a thousand asteroids between the orbits of Mars and Jupiter merely tells us that there was no dominant nucleus in this region from the beginning; and the zodiacal light suggests that the process of aggregation is not yet fully completed.

That an eruption from the sun that produced the planetary nuclei also produced one or more smaller nuclei which were travelling at about the same speed seems not unlikely. If at the time of ejection such a system of nuclei were moving like a rigid system in rotation then, if they were not too far apart, they would continue to move as a dynamical unit and the smaller nuclei would move about the planetary nucleus as a system of satellites. Their direction of revolution would be the same as the direction of rotation of the planet and their orbits would lie in the plane of the planets' equator. This sub-hypothesis would account for the uniformity of motion of the larger satellites of the planets Jupiter, Saturn and Uranus. It would not, however, require that the plane of the planet's equator should coincide with the plane of the planet's orbit, nor that it should have any particular relation to the plane of its orbit. Observations on the planets themselves do not indicate that any relationship exists. Thus the inclination of Jupiter's equator is 3° and that of Saturn 27°, while the equators of the earth and Mars have sensibly the same inclination of 23½°. The inclinations of the planes of the orbits of the satellites of Uranus and Neptune are 98° and 145°, respectively; little is known about the planes of equators of the latter two planets.

There is also a possibility that a satellite was captured during the interval of time in which the process of aggregation of the planetesimal material was going on, and this may account for the fact that Jupiter and Saturn have satellites whose motion is retrograde and whose orbits have a high inclination to the plane of the planet's equator. The high inclination of the plane of the moon's orbit to the plane of the earth's equator suggests that the moon, too, is a captured satellite.

I can not, of course, enter into the wealth of details with which Chamberlin and Moulton support the argument for the planetesimal hypothesis. They will be found in Chamberlin's book "The Origin of the Earth," and a series of fifteen articles in the *Journal of Geology*, and in Moulton's "Introduction to Astronomy." To me the arguments are very persuasive, although they are, on the whole, qualitative and not quantitative. They appeal to one who loves nature rather than to one who loves merely mathematics. The planetesimal hypothesis is broad and elastic, capable of admitting much modification without losing its essential character. In this respect it contrasts sharply with the theory of a rotating fluid, incompressible mass, although it has yet to be proven that even this theory is precise after instability sets in.

Jeans has attempted to set up a mathematical model[35] for the planetesimal hypothesis by neglecting the sun's rotation and its violent internal activities, considering only the tidal actions of a quiescent gaseous mass moving in a hyperbolic orbit. But even this simplified problem is too difficult, and the orbital motion has to be eliminated. The results obtained for even this simplified model are valuable and interesting. The model is too inexact, however, to admit of any usable theorems, and in the present state of our mathematical development the naturalistic methods of Chamberlin, checked up mathematically in those places where the theorems of dynamics can be applied rigorously, give far the greater promise of progress. Certainty can not be reached by either method, for the naturalistic methods are not exact quantitatively, and mathematical models are not exact qualitatively. Our hope lies in a judicious combination of the two.

As the matter stands at present, the planetesimal hypothesis of the origin of the planetary system has a clear field, since no other adequate hypothesis is in sight.

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(To be continued)

[35] "Problems of Cosmogony and Stellar Dynamics." See, also, H. Jeffreys, "The Earth," Cambridge (1924).