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THE PLANETESIMAL HYPOTHESIS[1]

ON the eighty-fifth anniversary of his birth,[1a] September 25, 1928, Professor Chamberlin's latest book appeared from the press. In this volume he presents an orderly account of the researches he has made during the last thirty years respecting the origin and development of the planets and the other attendants of the sun. Lest any one should suppose that this work represents the feeble and distorted echoes of more vigorous years, I wish to say at once that it would be difficult to find in astronomical literature its superior in exhaustiveness and coherence of reasoning, in precision of statement and in the exercise of constructive imagination. In his alertness for significant clues and interrelations among phenomena and in the relentlessness with which he pursues the trails on which he enters, he reminds one of Charles Darwin in "The Origin of Species."

On the same day that Professor Chamberlin's book was received, I found in the October *Harper's Magazine*, page 574, in an article by Professor Eddington, of Cambridge, England, the following sentences:

> By elimination of alternatives it appears that a configuration resembling the solar system would be formed only if at a certain stage of condensation an unusual accident had occurred. According to Jeans, the accident was the close approach of another star casually pursuing its way through space. This star must have passed within a distance not far outside the orbit of Neptune; it must not have passed too rapidly, but have slowly overtaken or been overtaken by the sun. By tidal distortion it raised big protuberances on the sun, and caused it to spurt out filaments of matter which have condensed to form planets. That was more than a thousand million years ago. The intruding star has since gone on its way and mingled with the others; its legacy of a system of planets remains, including a globe habitable by man.

These sentences are the most recent expression of wholly erroneous views respecting the authorship of the theory to which they refer. They are the culmina-

[1] *The Two Solar Families: The Sun's Children.* By Thomas Chrowder Chamberlin. Chicago, The University of Chicago Press, 1928. 8vo., pp. xxi + 311; 52 illustrations. $2.50.

[1a] The death of Professor Chamberlin occurred on November 15, after this review of his book had been finished.

tion of an increasing disregard for facts which are incontestably established. Irrespective of the question of property rights, the interests of science itself demand that silence no longer be maintained on this subject. Consequently, and particularly because Professor Chamberlin himself has not referred to it, I shall make, in later paragraphs, such a clear and explicit statement of the actual history of the initiation and of the development of the ideas respecting the origin of the planets from the dynamic effects of a passing star that there will not again be any excuse for such a statement as that quoted from Professor Eddington. But before this matter is taken up, an attempt will be made to indicate the general scope of Professor Chamberlin's book.

Professor Chamberlin divided his book into three major parts. The first part contains an account of his (and my) critical examination and abandonment of earlier theories of the origin of the planets, work which was essentially completed by the year 1900; the second consists of the development of the planetesimal hypothesis, including as an essential part the rôle played by a passing star at the birth of the planetary system, a work all of whose main outlines were laid down and published by 1906; and the third is devoted to more recent auxiliary developments in various directions, and in particular to his theories respecting the origin of meteors and comets.

At first one might question, as I did, the advisability of using eighty pages in proving earlier theories erroneous, for it is now almost twenty-nine years since he and I, in separate investigations,[2] examined the Laplacian theory and found it absolutely untenable. The tests to which it was submitted were so conclusive as to leave no room for its continued acceptance. But inherited ideas are tenaciously held and new ones are adopted very slowly. For example, in 1911, Professor Frost, writing in *Popular Astronomy* (9: 466–7), excellently expressed the views generally held by astronomers in the following words: "But no adequate substitute [for the Laplacian theory] has been proposed, and the increased study of the different phases of development, as inferred from stellar spectra, supports the Laplacian theory surprisingly." Even in 1922, Dr. George E. Hale, in his book, "The New Heavens" (page 35), said: "Laplace's hypothesis has been subjected in recent years to much criticism, and there is good reason to doubt whether his description of the mode of evolution of our solar system is correct in every particular." It is only within the last four or five years that astronomers have shown any general inclination to regard the ideas associated with the Laplacian theory as untenable. For these reasons, as well as for the fact that many books on astronomy and geology still fail to point out the grave defects of the Laplacian theory, Professor Chamberlin is probably quite right in completely clearing the ground of inherited ideas.

One reason that the fundamental ideas which are basic in the planetesimal hypothesis have not been readily grasped is that this hypothesis is not simply a variant of the nebular theory of Laplace. The Laplacian theory and the doctrines associated with it constitute one genus of scientific theories; the planetesimal and associated hypotheses constitute a wholly different genus. The gap between these different genera of intellectual constructions is as profound as that between different genera of living organisms, and as difficult to bridge.

In order to illustrate the revolution in fundamental points of view which the new hypotheses require, the principal theories of the Laplacian group will be briefly enumerated and contrasted with the planetesimal hypothesis. Laplace started with a heated gaseous mass rotating as a solid. With loss of heat by radiation, it contracted and rotated more rapidly. At various stages of the contraction, the centrifugal acceleration at the equator of the rotating mass equaled the gravitational acceleration toward its center. At these places the contracting mass left behind gaseous rings which were concentrated into planets by the mutual gravitation of their parts. In six cases, after the contracting rings had assumed approximately spherical forms they similarly contracted and left behind smaller rings, which became satellites. This theory is delightfully simple and can be stated in a few sentences. It makes few demands upon the imagination to conceive of its various steps, and it requires no sustained mental effort to organize them into a unified whole. It raises no unanswere questions and arouses no doubts. The account of the creation and the origin of the earth in Genesis is not simpler.

Associated with the Laplacian theory is the theory that the earth is fluid except for a relatively thin crust. The phrase "the crust of the earth" occurs in geology about as frequently as the words "air" or "water." Dynamic geology has had this concept as its foundation.

Helmholtz's contraction theory of the sun's heat is another member of the same group of hypotheses. It relieves the Laplacian theory of the necessity for an excessively hot original nebula, and it measures the life of the earth and the other planets. Consequently it assigns superior limits to the past duration of the geological processes and the evolutionary stages of

2 *Journal of Geology*, 8 (1900): 58–73; *Astrophysical Journal*, 11 (1900): 103–130.

living organisms, and it foretells the time when they will have come to an end. The time-intervals are measured in tens of millions of years at the most.

Still another theory built upon the same ideas and harmonious with them, though insisting upon a variant in the case of the moon, is Sir George Darwin's splendidly elaborated tidal evolution. In thoroughness of examination and in clarity and fairness of exposition, Darwin's work is a model. But the body tides of the earth are now known not to be a dominant factor in the evolution of the earth-moon system, because the inherited ideas that the earth has a fluid or viscous interior have been found to be erroneous.

In the celestial spaces astronomers have held unquestioningly the same order of ideas. According to views current almost up to the present time, each star is going through a somewhat similar evolution. In some cases, the original mass undergoes fission into two comparable masses, a binary star; in others, presumably rings are left behind and planets are formed; in still others having very slow initial rotations, the nebulae concentrate into single stars unattended by planets. The evolution of each star, with very rare exceptions, is wholly independent of every other star. Observations are held to support the theory. Some stars have simple spectra—they are the stars recently concentrated from nebulae. Astronomical literature is filled with references to "young stars" and "early stars," and this simple theory of stellar evolution has dominated astronomical thought. When Professor Campbell finds that stars of "early" spectral types move much more slowly than the others, he questions whether the explanation of the fact may not be that they had not been long enough in the star state for gravitation to produce large relative velocities ("Stellar Motions" [1913], p. 216). In interpreting the giant and the dwarf stars in connection with their spectral types, Professor Russell follows out the contraction theory of stellar heat more consistently. He has the stars contracting from nebulae, and in the process running up in temperature to a maximum, depending upon their masses, and then declining to cold and dark bodies, the whole series of changes requiring (at least until almost the present time) only a few tens of millions of years. Hale whole-heartedly adopts the same ideas. He says in 1922 (*op. cit.*, pp. 53–54):

> Stars in an early stage of their life history may be regarded as diffuse gaseous masses, enormously larger than our sun and at a much lower temperature. . . . Their density must be very low, and their state that of a perfect gas. . . . In the slow process of time they contract through constant loss of heat by radiation. . . . Finally comes extinction of light, as the star approaches its ultimate state of a cold and solid globe.

Creation at one end and stagnation and death at the other, unless a new creation rejuvenates the system! What simplicity!

In striking contrast with the foregoing, consider the planetesimal hypothesis. The fundamental point of view adopted in it is that the stars of our galaxy constitute a group of mutually related objects, the evolution of each depending in part upon its relationships to the others. They mix and mingle with one another, in the course of time, somewhat like molecules in a gas. At the time of the dynamic adventure of a suitable near approach of one star to another, planets are born from the parent suns. These planets grow up around nuclei by the accretion of countless little planets (planetesimals) born at the same time. Not only in the broad sweep of events leading to the birth of the planets as independent objects does this theory differ completely from the Laplacian, but also all the dynamical considerations involved in the growth and evolution of the planets are wholly different. More than one commentator on the planetesimal hypothesis has regarded with favor the origin of the planets by dynamic approach as being likely, and has then utterly failed to realize that the growth and evolution of the planets could not have been along the lines that are consonant with the Laplacian theory. The new hypothesis gives an entirely new earth and lays down a new basis for the development of dynamic geology.

A moment's consideration shows that the intervals of time required for the events contemplated by the planetesimal hypothesis are of an entirely different order from those that have been current in connection with the Laplacian theory. Instead of tens of millions of years, thousands of millions of years are necessary. Astronomers naturally could not feel at home in the order of ideas that underlie the planetesimal hypothesis until they grew out of the inherited conceptions of the restrictions of time imposed by the contraction theory of stellar energies. Some detailed quotations bearing upon this point will be given, both because of its fundamental relationship to the problem under consideration, and also because of some astonishing recent claims respecting priority in an order of ideas that is rapidly coming to be looked upon with favor by astronomers.

In 1899 Lord Kelvin gave an address, "On the Age of the Earth as an Abode Fitted for Life" (SCIENCE, May 12, 1899, pp. 665–674, and May 19, pp. 704–711). His basis for conclusions was the simple Helmholtzian contraction theory of the sun's heat, which provides a lifetime for the earth of the order of 25,000,000 years. With a dogmatism that the entire history of philosophy and of science and a realization of our own ignorance should warn us against, he marked out definite time limits within which he asserted all terrestrial phenomena must be included. In SCIENCE for June 30, 1899, pp. 889–90, and July 7, pp. 11–18, Professor Chamberlin challenged both Kelvin's premises and his conclusions, and the events have shown that the challenge was fully justified. No one now would take seriously what Kelvin called "sure assumptions," "certain truth," and "no other possible alternative." After putting the date of the surface cooling of the earth between twenty and forty millions of years in the past, Kelvin speaks of "one year after freezing," "half an hour after solidification," and a "crust of primeval granite" having a depth of "several centimeters." I do not wish to emphasize so much the fact that Professor Chamberlin challenged the ideas on which the now passing generation of astronomers was reared, as to insist that he anticipated by nearly thirty years the basic ideas concerning the time-scale of the cosmic processes that are now rapidly winning favor. In 1899 (*op. cit.*, p. 889), he gave expression to the following startlingly prophetic words:

> Here [in the work of Kelvin] is an unqualified assumption of the completeness of the Helmholtzian theory of the sun's heat and of the correctness of deductions drawn from it in relation to the past life of the sun. There is the further assumption, by implication, that no other essential factors entered into the problem. Are these assumptions beyond legitimate question? In the first place, without questioning its *correctness*, is it safe to assume that the Helmholtzian hypothesis of the heat of the sun is a *complete* theory? Is present knowledge relative to the behavior of matter under such extraordinary conditions as obtain in the interior of the sun sufficiently exhaustive to warrant the assertion that no unrecognized sources of heat reside there? What the internal constitution of the atoms may be is yet an open question. It is not improbable that they are complex organizations and the seats of enormous energies. Certainly, no careful chemist would affirm either that the atoms are really elementary or that there may not be locked up in them energies of the first order of magnitude. No cautious chemist would probably venture to assert that the component atomecules, to use a convenient phrase, may not have energies of rotation, revolution, position, and be otherwise comparable in kind and proportion to those of a planetary system. Nor would he probably feel prepared to affirm or deny that the extraordinary conditions which reside in the center of the sun may not set free a portion of this energy. . . .

That Professor Chamberlin should have so exactly anticipated the discoveries of the last twenty-five years in subatomic structures and energies is extraordinary and to some extent accidental. But that he should have clearly perceived that there are vast sources of energy not theretofore taken into consideration was not accidental. It was an inescapable conclusion from several classes of facts that were then well established. The great age of the earth, as proved by geological evidence, made drafts on the bank of time far beyond the capacity of the contraction theory to meet; and the known scale of the stellar system made it as absurd to assume that stars are born and evolve and die in a few million years as it would be to assume that mountains are raised up to the clouds and are washed away in a summer's day.

For more than twenty years following 1899 nearly the whole scientific world continued serene in its inherited ideas respecting sources of energy and the time-scale of geologic and cosmic processes. Now, since 1920, the tide has set in toward the adoption of a time-scale measured by thousands—or even hundreds of thousands—of millions of years, and strangely enough the change is taking place without the discovery of any essentially new facts or the development of any new formulae bearing on the subject. The significance of the pertinent facts and of the formulae long available is just now being apprehended, and the conclusions from them are only now becoming mental property.

The main point that it is desired to make here, however, is that these ideas respecting energy and time, which are essential to the planetesimal hypothesis, have been held consistently by Professor Chamberlin since about 1900, or for more than twenty years before scientific men generally made them a part of their scientific thought.

Volume II of Chamberlin and Salisbury's "Geology" was published in 1905. Ideas respecting the duration of the sun that are essentially the same as those which astronomers are now adopting were set forth on pp. 51–2 in the following words:

> That self-compression is a potent source of heat is not questioned, but the Helmholtzian theory takes no account of sub-molecular and sub-atomic sources of energy. The transcendent potency of these sources of energy has been for some time suspected, and is now being revealed by refined physical research. The extraordinary energies displayed by radio-active substances are doubtless but an initial demonstration of immeasurable energies resident in other forms of matter and in the constitution of the sidereal system, and competent for its maintenance for unassignable periods. It does not appear, therefore, in the light of recent revelations in physics, or recent discoveries in the constitution of the stars and the stellar system, that there is any sufficient reason for setting narrow limits to the life of the sun. It seems more in accord with recent advances in knowledge to place the compressional theory of the sun's heat in the category of the earlier chemical and meteoritic theories, as true and contributory, but as only partial and inadequate. . . .

In the first edition (1906) of my "Introduction to Astronomy" (pp. 395–7 and 485–6), the same order of ideas was expressed in clearest terms. In the 1916 edition, pp. 360–4, 443–4, 495–8, they were repeated and amplified. Besides this, it was emphasized on pages 500–504 that the approximately steady state reached in the enormous globular clusters implies a dynamic evolution of these groups, and an existence of the stars of which they are composed, extending over thousands of millions of years. This argument is, in fact, the most conclusive we have.

The hypothesis that our planetary system may have had its origin in the dynamic effects of a passing star first began to be favorably considered by astronomers in about 1919. In that year Dr. J. H. Jeans published his "Problems of Cosmogony and Stellar Dynamics." His introductory chapter contains an excellent brief summary of theories of cosmogony, and on p. 16 he writes: "The most complete form of tidal-action theory is found in the 'Planetesimal Theory' of Chamberlin and Moulton." He had not, however, at that time changed his old ideas respecting the time-scale of cosmical processes. On p. 17 he states:

> Perhaps the most obvious criticism that can be brought against this and all other tidal theories is that they require the close approach of large astronomical bodies, and that such close approaches are very rare events. Calculations which will be given later seem to show that this consideration must lead to the abandonment of all tidal theories, including the planetesimal, as explanations of *normal* cosmogonic processes.

In order to avoid this difficulty respecting the time required and other embarrassments of the same sort, he made the astounding conjecture that our galaxy of stars has only recently expanded to its present dimensions—an assumption which Professor MacMillan showed in his review of Dr. Jeans's book[3] violates the dynamical assumptions upon which Dr. Jeans's reasoning was based. In discussing the time-scale, Dr. Jeans says (in 1919) on p. 286: "It hardly appears probable that the sun can have other sources of energy comparable with its gravitational energy." And on p. 289:

> We conjecture that something like 300 million years ago our sun experienced an encounter of this kind. . . . At this epoch the sun is supposed to have been dark and cold, its density being so low that its radius was perhaps comparable with the present radius of Neptune's orbit.

Dr. Jeans appears now (1928) completely to have reversed his ideas respecting the time-scale. In the *Annual Report* of the Smithsonian Institution for 1926, in an article entitled "The New Outlook on Cosmogony," he writes:

> The ages we must now attribute to our sun and the other stars are many hundreds of times longer than was, until quite recently, thought probable or even possible [by Jeans and his followers]. This extension of the time-scale will call for a rearrangement of ideas in many departments of cosmogony and astronomy.

There is another reason why the planetesimal hypothesis has not rapidly won favor. Although to discuss it is to digress somewhat, the point to which I refer is so important in the present connection and to the progress of science in general that I shall venture to make a few comments upon it.

It seems to be an almost universal human characteristic to demand a formula to explain things. In most matters the formula is a combination of words constituting some so-called principle or law; in physical science, it is generally a mathematical expression. The simpler the formula is, the more highly it is likely to be regarded. Presently it is not alone an epitome of past experience and of our knowledge; it becomes, as it were, something inspired, something that gives us complete truth and leaves us comfortably free from uncertainties. Our scientific literature is filled with statements that this law or that law "governs" a certain class of phenomena. Now, what is a law of nature? It is not something ultimate and of divine origin. On the contrary, it is only our formulation of the way we have perceived certain phenomena; it is subject to all the imperfections of our knowledge and to all our inherited and acquired prejudices. It seems clear from the nature of the case that no law of nature or formula that we shall ever construct will be universally applicable. Yet we all strive for the formula as though it were more than it is, and we distrust anything for which a formula has not been made. There are many instances in the work of Dr. Jeans where he has arrived at a formula, derived at great labor on the basis of a whole series of uncertain assumptions, where general commonsense reasoning would have been safer.

The foregoing general statements are excellently illustrated by theories of the source of the energy radiated by the sun. The formula of Helmholtz, which is simplicity itself, speedily found wide acceptance. It held such sway over the minds of scientists that it seemed completely to paralyze progress for twenty years after it should have been regarded as inadequate. Only when the Einstein formula for the relation between mass and energy gradually became known and could be made to take its place was the Helmholtzian formula abandoned. This appears to be the explanation of the complete change of position

[3] *Astrophysical Journal,* 51 (1920): 309–333.

of Dr. Jeans between 1919 and 1926. Certainly he considers no evidence in the latter year that was not fully available and that he did not discuss in the former.

We may now inquire on what authority we should accept unquestioningly Einstein's formula and apply it, with no misgivings, to the most far-reaching speculations. It was not given us by the Almighty on tablets of stone; it is not even an absolutely necessary consequence of Einstein's general theory. And Einstein's theory itself has been verified in only a few classes of phenomena, in all of which large percentages of uncertainty remain. The tests that have been made of the theory certainly are not of a nature to justify us in concluding that now, contrary to all past experience, we have arrived at ultimate and absolute truth, expressible in a few mathematical symbols. Even if the general theory of relativity is substantially correct, there are reasons to doubt the general validity of the formula expressing the relation between mass and energy. The energy it provides is substantially equal[4] to the electrostatic potential energies of the electrons of which atoms are composed, and, therefore, if it is correct there can be no important sources of energy still unknown. Suppose the electrons are composite and contain internal energies of an order as much higher than those now known as the energies of the electrons are higher than those of the atoms formerly known, and that they may be transformed into electronic energies as electronic energies are in some cases transformed into molecular energies. The theory does not provide for these possibilities, and even, I think, probabilities. The statement that positive and negative electrons combine and disappear accompanied by the appearance of something entirely different, called radiant energy, is merely a formula in words that cloaks our ignorance and that is likely to stifle our curiosity.

If the planetesimal theory had been expressed by a formula, it undoubtedly would have been accepted more readily by many minds, even though the formula added nothing to its content or probability. But the planetesimal hypothesis can not be expressed by a simple formula, such as that in Genesis or that which describes the Laplacian theory. It involves many complexities and a wide range of auxiliary theories, and instead of closing a chapter of ideas it opens up volumes of new ones. In spite of this, Dr. Jeans and Dr. Jeffreys have attempted to put the planetesimal hypothesis into a formula under the name of the "tidal theory"; but, as I shall later point out, their formula does not rest upon a substantial foundation.

In the second part of his book, Professor Chamberlin takes up the development of the planetesimal hypothesis, and an outline of the history of its development is important for later parts of this review. The first published approach to its underlying ideas was Professor Chamberlin's "The Possible Function of Disruptive Approach."[5] This paper was referred to with some degree of approval by Alfred Russell Wallace in his "Man's Place in the Universe" (1903; p. 185). In the Fairchild revision edition of LeConte's "Elements of Geology" (1903; pp. 293–4), it was also considered; and still again, in Miss Clerke's "Problems in Astrophysics" (1903; p. 445). In 1905 the second volume of Chamberlin and Salisbury's "Geology" (pp. 38–81), contained a full discussion of the planetesimal hypothesis. In the same year I described it in the *Astrophysical Journal* (22: 165–181). It was also treated in detail in the first edition of my "Introduction to Astronomy" (1906; pp. 463–487). From this time on, references to the planetesimal hypothesis appeared frequently in astronomical and geological literature. For example, Sir George Darwin, in his third (1911) edition of "The Tides" (pp. 412–426), described the planetesimal theory with keen penetration respecting its bearing upon scientific thought and with gratifying cordiality. In an earlier part of the final chapter, he went far toward abandoning the Laplacian theory, which he had always theretofore accepted, and he closed his book with the following paragraph:

> The authors [Chamberlin and Moulton] frankly admit that their hypothesis [the planetesimal] may need revision in many respects, and this is no doubt inevitable in so ambitious an attempt. Whatever be its fate they are to be congratulated on having advanced views of extraordinary interest; and whether the theory be sound or not in all its parts they have made a contribution to cosmogony of great importance.

By this time the planetesimal hypothesis was generally recognized by geologists as being a hypothesis worthy of respect and consideration. For example, Pirsson and Schuchert expounded it on pages 530–7 of their "Geology" (1915).

Dr. Jeans appears to have referred to the planetesimal hypothesis first in 1919 in his "Problems of Cosmogony and Stellar Dynamics," fourteen years after it had appeared in leading scientific magazines and in Chamberlin and Salisbury's "Geology" and in my "Introduction to Astronomy." In an introductory chapter, under the sub-heading "The Tidal-Action Theory," he states: "The most complete form of tidal action theory is found in the 'Planetesimal

4 Wm. D. MacMillan, "Some Mathematical Aspects of Cosmology," SCIENCE, 62 (1925): 125.

5 The *Astrophysical Journal*, 14 (1901): 17–40, and the *Journal of Geology*, 9 (1901): 369–392.

Theory' of Chamberlin and Moulton." In mentioning it, however, he gives no reference to its early publication or dates, but contents himself with citing generally Chamberlin's "Origin of the Earth," which he points out was published in 1916. A reader unacquainted with the facts from independent sources would be likely to infer that the work of Professor Chamberlin did not antedate much, if any, that of Dr. Jeans. In this section he explains that the theory involves the close approach of one star to another with resulting tidal forces and the generation of a spiral formation. In his concluding (XII) chapter, entitled "The Origin and Evolution of the Solar System," the Laplacian theory is examined and disposed of as Professor Chamberlin and I disposed of it in 1900 and for substantially the same reasons. The "tidal theory" is developed at some length with no reference whatever to the planetesimal hypothesis. In three later publications Jeans has explained the so-called tidal theory, and in none of them has he made any reference, direct or indirect, to the planetesimal theory or to Chamberlin. One of these papers is published in the *Annual Report* of the Smithsonian Institution for 1924; another is in the same publication for 1926; and the third is in a collective work to which further reference will be made.

Dr. Harold Jeffreys entered the field of the planetesimal hypothesis in several papers which appeared in the *Monthly Notices of the Royal Astronomical Society* in 1916–1918. These discussions, revised somewhat as a consequence of the work of Dr. Jeans, are included in his book, "The Earth," which was published in 1924.

It follows from the foregoing historical sketch that it is an inexcusable violation of the facts to state or imply that Dr. Jeans was the author of the theory that the planets had their birth at the time some star passed near our sun. The theory had appeared in many publications and had been presented to thousands of students of geology and astronomy for more than ten years before Dr. Jeans published a word upon the subject. The position taken by Drs. Jeans and Jeffreys, which has spread to others, as is illustrated by the sentences quoted from Dr. Eddington at the beginning of this review, is an astounding phenomenon. The way in which the planetesimal hypothesis faded from view between the first and last chapters of Dr. Jeans's book has been noted. But in the book of Dr. Jeffreys, it is the tidal theory throughout the text, with the planetesimal hypothesis relegated to an appendix, except for a brief reference in Chapter II. In sharp contrast with his treatment of the planetesimal hypothesis, Dr. Jeffreys devotes the entire first chapter to the Laplacian theory, which he rejects on essentially the grounds developed by Chamberlin and me in 1900, but with no reference to our work. In the matter of credit for essential ideas, the book of Dr. Jeffreys appears to be unique. In scores of references to his own work and to that of Dr. Jeans and other British writers, he in all cases follows the usual custom of giving the name of the publication, the volume, the year of publication and the pages in the volume. Such information enables a reader to fix clearly in his mind the historical sequence of things to which references are made, and it enables him easily to examine any original sources he may wish to consult for details. But nowhere in the book of Dr. Jeffreys is there a single corresponding explicit reference to the work of Professor Chamberlin or to my own writings. In not a single instance is a specific date given and in only three instances are the correct titles of the publications given, and they are titles of text-books without the dates of publication. There is no reference whatever to original publications in scientific journals. To such an extent has the "made in England" been pushed that Dr. Jeffreys, like Dr. Jeans, ascribes Helmholtz's contraction theory of the sun's heat to Lord Kelvin, and he makes no reference whatever to Helmholtz in his book. He has several chapters on tidal and other deformations of the earth, but he remains completely silent on the incomparable earth-tide experiments of Professors Michelson and Gale.

Thirteen distinguished British scientists and philosophers wrote "Evolution in the Light of Modern Knowledge," which was published in 1925. Dr. Jeans is the author of Chapter I, "Cosmogony," and Dr. Jeffreys wrote Chapter II, "The Evolution of the Earth as a Planet." Since the book is a non-technical discussion of a broad field, suitable for the intelligent general reader, it can not justly be expected to contain exhaustive references to original sources. Yet there are references in the text, and in bibliographies at the ends of the first two chapters, to Lucretius, Descartes, Swedenborg, Kant, Thomas Wright (of Durham, England), Laplace, Babinet, Dr. Eddington, Darwin (Sir George), and especially to Drs. Jeans and Jeffreys; there is a two-page discussion of the theory of Kant, an equally long discussion of the Laplacian theory and a four-page discussion of the tidal theory; but the planetesimal hypothesis and Chamberlin and Michelson and Einstein do not exist.

In view of these astounding tactics, the very least that could be expected is that Drs. Jeans and Jeffreys should have developed a tidal theory that in no way had its origin in the planetesimal hypothesis and that in its essentials was entirely distinct from the planetesimal hypothesis, or that they should have established conclusions that were of the very first order of importance. Let us examine the facts, first as to whether the so-called tidal theory descended from the planetesimal hypothesis. Dr. Jeans says, in his "Theories of Cosmogony" (p. 17): "The most complete form of tidal-action theory is found in the 'Planetesimal Theory' of Chamberlin and Moulton." In Appendix A, page 251, Dr. Jeffreys says: "The Planetesimal Hypothesis was historically the parent of the Tidal Theory of the origin of the Solar System, elaborated in Chapter II." That is, originally both Dr. Jeans and Dr. Jeffreys seem to have acknowledged that the tidal theory in some real sense was a direct descendant of the planetesimal hypothesis.

Let us compare the theories themselves. The planetesimal hypothesis ascribes (1903 and later) the birth of the planets to the dynamic effects of a passing star; the tidal theory (1919 and later) does exactly the same thing. The planetesimal hypothesis ascribes the separation from the sun of the materials of which the planets are composed to the combined effects of the tides generated in the sun by the passing star and to eruptive activities such as now are exhibited by the sun; the tidal theory ascribes the separation to the effects of the tides alone, at a time when the sun was much larger than it is at present. (This point will be discussed further.) The planetesimal hypothesis ascribes to this origin of the planets the relatively small amount of momentum of the solar system and its significant distribution between the planets and the sun; the tidal theory draws exactly the same conclusion in exactly the same way. The planetesimal hypothesis explains the fact that the planetary orbits are nearly coplanar, and the fact that the planets all revolve about the sun in the same direction, to the cross-component attractions of the passing star in the plane of its orbit; the tidal theory draws exactly the same conclusions in exactly the same way. The planetesimal hypothesis assumes that the planets have grown up about nuclei in the matter that left the sun, the exact characteristics of the nuclei being at present undeterminable; the tidal theory assumes that the planets grew up around nuclei and maintains that it has been proved that the nuclei were liquid, not solid, almost immediately after they left the sun. The planetesimal hypothesis explains the small eccentricities of the orbits of the planets as the effects of their growth by collisions with planetesimals; the tidal theory does the same except that Dr. Jeffreys claims to have shown that if the planetesimals were not originally molecular they would be reduced to that state and would have the properties of a gaseous resisting medium. The planetesimal hypothesis ascribes the present rotation of the sun to its original rotation and to the effects of the planetesimals that fell back upon the parent body; the tidal theory of Dr. Jeffreys makes exactly the same explanation. The planetesimal hypothesis explains the satellites as bodies that have grown up about secondary nuclei accompanying the original planetary nuclei or later becoming entangled with them; the tidal theory ascribes the satellites to matter tidally removed from the planetary nuclei by Jupiter and by the sun. Professor Chamberlin concludes that the earth has grown almost entirely from solid bodies and that it has been solid through and through, as it is now, during nearly all its growth; Dr. Jeffreys believes the earth-nucleus was liquid almost from the time it left the sun and that it grew quickly to its full size in the liquid state. Thus, in every essential concept the two theories are identical; yet Dr. Jeffreys sets them forth, point by point, in his Chapter II (*op. cit.,* 1924), "The Tidal Theory of the Origin of the Solar System," with not a word to suggest that they all had been published in detail nearly twenty years earlier. On the other hand, he is meticulous in acknowledging credit for things of much less importance to his discussion.

Now let us return to the tidal theory in which Drs. Jeans and Jeffreys claim that the planetary materials were separated from our sun by the tidal effects alone of a passing star. Let it first be noted that both Dr. Jeans and Dr. Jeffreys ignore the fact that comets, having very small masses and large dimensions, have been observed to pass very close to the surface of the present condensed (according to them) sun without undergoing the tidal disruption that they imply they have proved would necessarily follow, even in the case of much less tenuous stars approaching less closely a comparable tide-raising body. Since the tide-raising forces vary inversely as the cube of the distance from the tide-raising body and directly as its mass and the radius of the disturbed body, it follows that Drs. Jeans and Jeffreys would have us believe that tidal forces ten thousand times less than those to which comets have been subject without serious damage have quickly torn great Jupiter and Saturn from the side of our sun.

Drs. Jeans and Jeffreys have put a considerable part of their discussion in mathematical terms and have wholly neglected the naturalistic method of approach. As a consequence, they have considered certain questions for which, on the basis of sufficient assumptions, formulae could be developed, and they have ignored the remainder. Among other things, they have given formulae and a diagram in explanation of the separation of the planetary masses from the sun under the influence of the tidal forces of a passing star. Dr. Jeans set up the equipotential surfaces for the force system of the sun and the visiting star substantially as I set up the same conditions in 1900[6] in examining the stability of the Laplacian ring. My conclusion was that when the surfaces were open the ring would not contract into a dense mass. Drs. Jeans and Jeffreys assert that when the equipotential surfaces were dumb-bell shaped, all the material of which the planets are now constituted "are shot away from the primary star" (Jeans) through the opening (the dumb-bell handle) from our sun toward the visiting star. They assign no reason for assuming that the motion would be from the sun toward the star rather than from the star toward the sun. They do not even attempt a determination of the rate that material would be "shot" in this way under any conditions, actual or hypothetical. They ignore the fact that the resultant gravitational accelerations are normal to the equipotential surfaces and not parallel to them, and that, consequently, the tendency to flow through the spout is not gravitational. They assume that matter, to escape from one star, must flow out from it through the spout, and then neglect to explain why it does not belong to the other star. They make no reference to the fact that as the visiting star recedes the dumb-bell breaks and leaves the "tidal filaments" interior to one of the ovals. They speak as though the cross-components of motion of the filaments were tidal effects, though in tides the wave-form and not the matter moves. Hence, even if they were wholly correct, it would be difficult to justify their title for the theory. They make no quantitative discussion of the short time the visiting star must have been near the sun. They speak of "slow encounters" and "transitory encounters," and Dr. Eddington in his recent paper (*loc. cit.*) speaks of the star having slowly overtaken or been overtaken by our sun; all in spite of the fact that the star came from stellar distances and has receded again to stellar distances, from which it follows that the relative velocity of the sun and the star must necessarily have at least equaled the parabolic limit. In short, they have not even remotely approached a mathematical demonstration of the validity of their assumption that tidal forces alone will account for the separation of the planetary material from an ancestral sun. And corresponding statements apply to their suggestions respecting the origin of the satellites and the early states of the planets.

I do not make the foregoing remarks in a harshly critical spirit, for the field of their discussions is beyond the range of sound mathematical treatment. The fact that their formulae and diagrams do not approach logical conclusiveness is of no consequence except in so far as the formidable appearance of these mathematical tools misleads the unsophisticated. Even if their conclusions were unimpeachable, I should not regard them as being of great significance, for they rest on assumptions that may not correspond with the facts. Let me make a much more general statement respecting theories in the domain of physical science. When a theory has been definitely formulated, it may be examined mathematically in order to determine, so far as may be possible, whether its various parts are consistent with one another and with other accepted facts and theories. If inconsistencies are brought to light, the theory must be modified or abandoned. If no inconsistencies are revealed, the theory may be somewhat more probable, but it is still uncertain. That is, mathematical processes may disprove a physical theory, but they can never completely establish one. For example, in 1900 Professor Chamberlin and I brought out fundamental inconsistencies in the Laplacian theory and we abandoned it. But we could not logically prove the correctness of the planetesimal hypothesis, nor have Drs. Jeans and Jeffreys succeeded in proving its correctness, or even approached such a proof.

As has been pointed out, a mathematical formula is not sacred and has no greater validity than the assumptions upon which it is based. Common sense supports this position and the whole history of science warns us against placing great faith in a formula, whether it be in words or in mathematical symbols. The Helmholtzian contraction theory of the heat of the sun excellently illustrates the point. The assumptions on which a formula is based are not only those that are explicitly expressed, but they are also those which are subconsciously held by its author and which make up his general point of view. It has already been remarked that Dr. Jeans in the *Annual Report* of the Smithsonian Institution for 1926 reversed his earlier views on the duration of the stars without using any new data. Dr. Eddington did the same thing in an article, "The Borderland of Astronomy and Geology," published in *Nature,* January 6, 1923, and reprinted in the *Annual Report* of the Smithsonian Institution for 1923. In the text, Dr. Eddington says: "There must have been a time when the sun's heat was from twenty to fifty times more intense than it is now." But in the Smithsonian reprint there is added the footnote: "New facts have emerged since this was written. I think we can now say fairly definitely that the sun's heat has not altered appreciably during the last ten thousand million years." One familiar with this field wonders what

[6] *Astrophysical Journal,* 11: 122–126.

"new facts" emerged in the interval of a few months to justify this astounding reversal of opinion, except that Dr. Eddington had changed his point of view.

The assumptions that underlie one's point of view are not easily recognized by the worker himself. If he overlooks their importance, the fact may be regarded with charity, for the fault is common. But it is not too much to expect that he will keep steadily in his own mind and place fairly before his readers the fact that his conclusions are as uncertain as his definitely expressed assumptions. He should not proceed as though the probabilities of uncertain hypotheses, sequentially introduced, compound by addition rather than by multiplication. The conclusions of Drs. Jeans and Jeffreys and Eddington are not usually characterized by conservative formulation. For example, in his "Problems of Cosmogony" (1919), under the subheading "The Time-Scale," Dr. Jeans writes on page 287:

> Taking the luminosity of the average star to be 1/10, we find that the contraction provides for radiation at this rate for 530 million years, a period which agrees well enough with our other estimates of the age of the universe.
>
> Thus as regards the universe as a whole, there is no difficult problem associated with the time-scale: the problem only arises in connection with special stars, and our sun happens to be one of these.

Would he now assert there is no difficulty in the time-scale he then held?

Then in 1925,[7] in discussing the same question and the same data, he says:

> The length of time to bring about an imperfect approximation such as is observed is found to be of the order of millions of millions of years. Although we can not say that any individual star has lived for this length of time, we can be fairly confident that the great majority of stars have done so.

Millions of millions of years! Fairly confident! One may inquire what in all the realm of science should be regarded as merely a hypothesis and held cautiously.

For about fifteen years Drs. Jeans and Eddington have been making persistent, skilful and praiseworthy efforts to penetrate the difficult field of the internal constitution of the stars. Although it has been necessary for them to pile one assumption on another, at every stage of their work they have appeared to be confident of the substantial correctness of their results. Now, in *Monthly Notices of the Royal Astronomical Society,* for October, 1927, on pages 724–5, Dr. Jeans states that all their work had been based on an erroneous assumption respecting radiation pressure, and he concludes:

> This invalidates the whole of the discussions, and any apparent success they [the discussions] may have achieved must have been purely fortuitous. In liquid stars radiation pressures may in general be disregarded and the luminosity mass-temperature relation must be examined *de novo.*

So far as I know, for the time being the matter rests there.

The foregoing remarks have been made partly for the purpose of illustrating the danger of accepting results simply because they are clothed in more or less of mathematical garb, and partly for the purpose of throwing in sharp relief the naturalistic methods employed by Professor Chamberlin. The third major division of his book is devoted to the nature, the origin and the evolution of meteors, meteorites and comets. In a field beset with enormous difficulties, Professor Chamberlin exhibits skill of the highest order in seizing on significant facts and analogies; he pursues every hopeful clue; he brings into view and examines all promising hypotheses; he is bold in inventing new hypotheses, and he presents his conclusions as results to be tested by time and new discoveries. Even though one should disagree with some or many of the conclusions Professor Chamberlin regards as possible, yet one can not but learn much from his discussions and feel the stimulating effects of following a daring mind in its wide excursions in unfamiliar domains. However strange and strained some of his views may seem now, it may very well be that in thirty years he will be found to be now as much in advance of his times as it is now known that thirty years ago he was in advance of his times in respect to the heat of the sun. The differences in point of view are in this case no less radical and startling than they were in the former. What I wish to insist on is only that the fact that they depart from current ideas is not real evidence against their correctness, and the daring excursion will certainly benefit science. When considered broadly, Professor Chamberlin's contribution to the methods of thought in the field of cosmogony will be regarded as highly as the new ideas he has advanced.

In a time when many have talked of the creation and the final death of the physical universe and have made such concepts basic in their thought, he has entertained no such philosophic juvenilities, nor has he insisted on theories expressed in closed formulae, nor has he been depressed by the thought that much is unknown. It seems that many minds have a sort of horror of an unending past or future, or infinite

[7] "Evolution in the Light of Modern Knowledge," p. 19.

space, and that to avoid the terrors of open time they assume a creation, wholly unconscious of the profound and wide-reaching implications of the assumption.

For example, Dr. Jeans closes his paper in the October, 1927, number of the *Monthly Notices of the Royal Astronomical Society* with the astounding sentence: "As a corollary, it would be difficult to deny that all the matter of the universe may have been created at the same instant." To ascribe to such a conclusion the logical relationship of a "corollary," and especially to entertain it on any grounds whatever, means that in spite of the fact that Dr. Jeans appears to adopt the planetesimal hypothesis under the name of the tidal theory, and in spite of the fact that he has recently changed to the general order of ideas respecting the time-scale of the cosmic processes held by Professor Chamberlin since 1899, the fundamental philosophic point of view occupied by him and his followers is separated from that of Professor Chamberlin and his associates by an immeasurable gulf.

F. R. MOULTON

CHICAGO, ILLINOIS

OF HUMAN INTEREST

To
JOHN HENRY COMSTOCK
and to
ANNA BOTSFORD COMSTOCK
On this their Golden Wedding Day
Sunday, October seventh, 1928:
Be
Peace and joy and every happy memory
Though but a few of all the many who in these fifty years have felt the benediction of their home, we cannot let this anniversary day go by without our word of love and gratitude.

SUCH was the wording of a beautiful illuminated testimonial scroll signed by a group of immediate personal friends of the Comstocks and presented to them on Sunday morning, October 7.

On the preceding day the Ithaca *Journal-News* had given editorial expression to the esteem of the community, as follows:

> Cornell and Ithaca need no special occasion to remind them of Professor John Henry Comstock and Mrs. Comstock. The lives of both are too closely and too happily interwoven with that of the University and city communities to need such a reminder; but all will rejoice with them to-morrow on the occasion of their golden wedding anniversary, and many will take pleasure in recalling kindnesses received at the hands of these distinguished citizens.

Then after an enumeration of the books that they both have written and of the high honors that have come to both of them, the editorial continues:

> Their home has been a center of hospitality for a generation, and many boys and girls from the country have found comfort and inspiration in its atmosphere. No one will ever know how many careers have been formulated, how many lives have been given an upward bent through association with these two.

The editor further went on to express the

> Sense of gratitude felt in their home city for the half century in which they have lived together to the great enlightenment of their chosen scientific field, for the benefit of the community and for the enrichment of the lives of their many friends.

Similar tributes of esteem were editorially expressed in the *Cornell Daily Sun* and in the *Alumni News*. The latter said:

> They are a couple unique in university and scientific circles. Both are internationally known in their respective fields of science. With the exception of a few years devoted to study and to teaching in other institutions, they have been connected with the University since 1869.

Thus, during nearly the entire history of Cornell University they have lived and worked together. In the fullest sense of the word they have been colaborers. Their respective fields of labor, entomology and nature study, broadly overlap, and nothing in the province of the one has ever been too sacred for the use of the other, if needed. And back of the work and sustaining the work, there has always been a home of good cheer and genuine friendliness.

Professor and Mrs. Comstock, though stricken in health with the burden of the years, still live in their beautiful home among the trees on the brink of the gorge above the Primrose Waterfall. Here they are surrounded by personal treasures that have come to them from all over the earth. Here was the Mecca of many distinguished entomologists who came to the recent International Congress, bringing new tributes of respect. Here, also, their hospitable door is still open to the humblest student, and for him there is unending friendliness and encouragement within.