THE FIELD OF COSMOGONY

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AS THE nineteenth century opened, it was currently assumed by the learned men of the white race that the heavens and the earth, together with all things animate and inanimate, were created substantially as they appear at the present time in the space of six days by a fiat of Divine Will, approximately six thousand years ago. This cosmogony, which had been current in Europe for many centuries and which had been derived from the leaders of thought of the ancient Hebrews, is but one of a variety of cosmogonies which rest upon the concept of a creation by a Divine Being that has existed through all eternity and that itself required no explanation. It is not necessary here to enter into the fanciful details of such cosmogonies, or to attempt interpretations and explanations; but, on the other hand, it is not possible to ignore the existence of a type of human thought that has dominated the entire past, and still dominates a vast majority of men who regard themselves, at least, as more or less learned. Explorers in the difficult fields of cosmogony from a scientific point of view are still hardy pioneers—as little known to their contemporaries in general as were Vasco da Gama or Jacques Marquette.

When the telescope had revealed the fact that the planets were brothers and sisters of the earth, some larger and some smaller, and the new science of dynamics had made intelligible their apparent motions upon the skies, it was no longer possible to regard the earth as the central body about which the whole universe turned, and it was more difficult to believe that man was the central figure of existence, and that all things had been created for his benefit and profit. Astronomy offered to philosophers a larger, clearer vision than had previously been possible. It presented a very grand, but simple, picture of the solar system—a very large, massive, hot central body, the sun, about which the relatively small planets revolved in ellipses. The motion in these ellipses is definite and regular, in perfect accord with the three laws of motion and the law of gravitation. The sun rotates about its axis, and the planets do likewise. Most of the planets are accompanied by one or more satellites, and the satellites move about their primaries in accordance with exactly the same laws as the primaries about the Sun.

In addition to those uniformities which demonstrate the dynamical laws, there are other uniformities in the solar system which are very striking and which can be regarded only as links that connect the present state of the system with the past. They are of such a fundamental nature that they seem to relate, not to the immediate past, but to the very origin of the system. There are eight planets which vary in distance from the sun, from three-eighths of an astronomical unit for Mercury, to 30 astronomical units for Neptune. A priori, their orbits might have had any degree of eccentricity from 0 to nearly 1, and their inclinations to the plane of the ecliptic might have been anything from 0 to 180°. Had the sun picked up these planets, so to speak, one at a time, a random distribution of the eccentricities and inclinations would have resulted. Actually, however, the eccentricities and inclinations cluster about the zero point; that is, the orbits are all nearly circular, they all lie very near the plane of the earth's orbit, and they all move in the same direction. Obviously this is not a random arrangement, but a highly systematic one. But, as if to dispel any doubts which might arise upon the matter from so small a number as eight, there are more than a thousand planetoids, small bodies whose diameters range from a few miles to a few hundred miles, that conform to the same system, although not quite so closely as do the planets.

In a general way the rotations of the sun and the planets also conform with the motions of planetary revolution, but there are notable departures. The equator of the sun is inclined 7° to the plane of the ecliptic; for Earth and Mars, 23°–24°; Jupiter, 3°; Saturn, 28°; Uranus, 98°; Neptune, 170°, assuming that the satellites of Uranus and Neptune lie in the equatorial planes and indicate the directions of rotation, which cannot be determined from observations on the planet itself.

The satellite systems of the planets repeat the uniformities in the motions of the planetary systems, although the outermost satellites of Jupiter and Saturn have a retrograde motion.

In his *Système du Monde* of 1796, Laplace suggested that these manifest uniformities could be accounted for by supposing that initially the sun was a vastly distended, very hot, gaseous nebula in slow rotation. As the heat was radiated away, the nebula contracted, owing to the mutual gravitation of its parts; and, since the moment of momentum was constant, the angular rotation was increased. In the course of time the size of the nebula was notably diminished, and its rate of spin was notably increased. Eventually the nebula assumed a lens shape, rotating like a solid body. When the centrifugal force at the edge of the lens became as strong as the gravitational attraction of the entire mass, a ring all around the outer border failed to participate further in the process of contraction. It was therefore abandoned by the contracting mass, and the ring began a separate existence, just as the rings of Saturn have an individual existence.

Gradually the material in the ring gathered together in the more stable form of a sphere—and a planet was born. Eight separate times this process was repeated, and from these rings came the eight planets. In one case the ring failed to concentrate into a single large planet but formed a large number of smaller bodies, the asteroids or planetoids. The larger planets in their process of contraction passed through the same experience as did the sun, thus giving rise to the satellites. Comets and meteors do not exhibit the same regularities of motion as do the planets and satellites. They exhibit, rather, the random distribution of motion that is to be expected in objects which have been picked up one at a time.

Such was the famous "nebular hypothesis" of Laplace. The great fame of its author as a mathematician and astronomer, together with its own inherent simplicity and attractiveness, made its appeal universal during the entire nineteenth century. Professor Chamberlin characterized this hypothesis as "perhaps the most beautiful and fascinating ever offered to the scientific public." It played a rôle the importance of which can scarcely be overestimated in the young sciences which were coming into existence or growing in strength during that period. When the physicists, for example, had clarified their ideas with respect to the properties of energy during the fifth decade of the century, they opened the way for Helmholtz, in 1854, to show that vast quantities of heat would be generated by a contracting sun, and that, in contracting to its present size, our own sun would have generated sufficient heat to last, at the present rate of radiation, for approximately twenty million years. In 1870 Lane showed, in addition, that as long as the sun was in the state of a monatomic gas, its temperature was inversely proportional to its radius, so that it was no longer necessary to assume, as Laplace had done, that the original nebula was very hot. Even though it had been intensely cold, it would have generated and retained the heat necessary to bring it to its present state in the process of contraction. It is scarcely necessary to remark that the contributions of Helmholtz and Lane were of the utmost importance to the nebular hypothesis. They showed that it was in accord with modern ideas in physics and, furthermore, that it gave a quantitative basis for the time element, a basis which is always desirable and eagerly sought for every hypothesis.

Such, in a brief way, is the contribution of the mathematicians to the hypotheses of cosmogony, a subject that was old and hoary before the science of mechanics had been dreamed of. Their contributions began and ended during the nineteenth century. Whatever one may think of their particular group of hypotheses, one thing at least has been made clear to anyone who would enter this field: No hypothesis of cosmogony, from whatever source it may come, will be deemed worthy of attention unless it satisfies the somewhat exacting demands of celestial mechanics. The choice of hypotheses is thereby strictly limited; but mechanics alone can never quite close the doors to new possibilities, even though it does seem to have the power of excluding one that has already been admitted.

Modern geology began with Hutton, Lamarck, and Cuvier. According to Hutton, it was not the business of a geologist to speculate upon the origin of the earth, or upon its end; his attention should be confined strictly to the geological evidence, in which he maintained there can be found "no traces of a beginning, no prospect of an end." But this advice, wholesome as it is, is too restrictive of the human spirit, for it would deny the possibility, at least to a geologist, of a wider point of view than that given by geology, a point of view in which evidences of astronomy, geology, physics, chemistry and biology unite, under the guidance of an exacting mechanics, in telling a larger, perhaps much more thrilling, story in which the entire earth plays but a minor part. At any rate, the earth is but a minute part of the solar system, and the solar system is but a single unit in the vast galaxy of stars. It is as impossible, therefore, for the geologist to ignore the general setting in which his science finds itself as for the biologist to ignore the environment of the living organism. Interpretation of the geological evidence necessarily rests upon the point of view which is entertained with respect to this wider relationship.

It is certain, also, that the geologists did fall under the magic spell of the nebular hypothesis of Laplace. This famous hypothesis not only made a strong appeal to the aesthetic sense on account of its simplicity, but, at least during the first half of the nineteenth century, it seemed to meet all of the requirements of mechanics, astronomy, and geology. When, about the middle of the century, the physicists, under the leadership of Carnot, Joule, and Lord Kelvin, developed new ideas with respect to energy, it was found by Helmholtz and Lane that these ideas fitted the nebular hypothesis perfectly, and merely added new beauties to those which had already been recognized. It is small wonder, therefore, that to the scientists of the first half of the century it was a noble hypothesis and that to the scientists of the second half it rose to the grandeur of a tradition and a dogma.

To the geologists this hypothesis indicated that, in its earliest planetary stage, the earth was a liquid globe of molten material surrounded by a dense atmosphere which contained not only all of our present atmosphere but also all of the waters of the earth and the carbon and carbon dioxide which was later locked up in the coal deposits and in the carbonate rocks. In the course of time the liquid globe cooled, and a solid crust, thought to be at present some 30 or 40 miles thick, was formed upon the liquid surface, just as ice is formed upon the surface of water. Because of the loss of internal heat, the globe contracted. In adjusting itself to the shrinking interior, the crust was broken and crumpled. Throughout the geological ages this process has continued and is still in action. To this agency was attributed the mountain- and continent-building process. Certain portions of the crust are heavier than other portions, and the heavier portions were thought to have sunk deeper into the liquid below, forming the depressions which hold the ocean waters, the lighter ones rising to form the continents.

Other changes were going on in the atmosphere. When the temperature was sufficiently reduced rains began to fall, and the long process of erosion began. The original plutonic surface was worn away and the detritus, deposited in lakes and oceans, formed the stratified rocks. The early climates were warm and moist, and there was much evaporation and precipitation. Life eventually appeared upon the hitherto barren and desolate surface. Microscopic at first, the living forms slowly increased in size and complexity, and differentiated into the vegetable and animal types. The warm, moist climate of the Carboniferous period, with its atmosphere laden with carbon dioxide, was extremely favorable to the vegetable life, and the forests and swamps were luxuriant. Buried beneath deposits of muds and sediments, the swamp growths were converted into coal. The carbon which they contain was taken from the atmosphere, so that the climates of succeeding geological periods were colder and drier; and if this depletion of carbon dioxide finally resulted in the great ice sheets of the Pleistocene, it was thought not surprising. The earth was believed to be destined to a cold and frigid end. As Professor Chamberlin himself expressed it, the Pleistocene "was but an October frost; December was yet to come."

It was in the field of glaciation that Chamberlin won his first reputation as a geologist. This particular direction of his activities was guided largely by the accidental fact that Wisconsin, where he had been raised and educated and in which he had spent nearly his entire life, had been heavily covered at several different times by thick fields of ice, and the evidences of glaciation were abundant nearly everywhere. It was not sufficient to him to work out and describe the comings and goings of these strange ice invasions. There was the ever-insistent question of why these invasions had occurred, and the puzzle was tremendously increased when it was recognized that similar, even greater, periods of glaciation had occurred far back in Permian times and geographically nearer to the earth's equator, and that there was good evidence of glaciation even at the base of the Cambrian.

The theory that the great ice fields of the Pleistocene had resulted from a gradual depletion of the carbon dioxide of the atmosphere in the formation of the coal and limestone rocks, accorded in a general way with the hypothesis of Laplace. But why had the process of refrigeration been reversed, as evidenced by the disappearance of the ice sheets? What has caused the return, again and again, of more genial climates? Why were not the successive returns of the ice invasions characterized by a more and more intense refrigeration rather than by a diminishing intensity? The general character of the successive ice invasions is not in harmony with the theory, and Chamberlin's faith in the idea that the earth was rather rapidly approaching a frigid end was weakened; and when the evidence for even more remarkable episodes of glaciation in the ancient geological periods was beyond doubt, faith in the Laplacian hypothesis as a basis for a theory of climates was difficult indeed. In 1897 he said: "The nebular hypothesis correlates a wonderful array of remarkable facts and has gained a profound hold upon the convictions of the scientific world, yet some of its great pillars of support have recently weakened or have fallen away entirely." Not only had glaciation occurred far back in geologic time, contrary to the theory that the early climates were warm, but evidence had also been accumulated of aridity at certain times in the early geologic record. Not only is the climate not extraordinarily dry at the present time, but the driest epoch apparently was as far back as the Permian. The theory of climates growing out of the Laplacian hypothesis is clearly out of harmony with the general outline of the facts. The coldest, driest period occurred far back in geologic time.

Professor Chamberlin was dealing with these matters in a series of papers in the *Journal of Geology* as the nineteenth century was drawing to a close. He proposed a modification of the carbon-dioxide theory in which the proportion of carbon dioxide in the atmosphere varied from time to time, and the climate varied with it; but the Laplacian concept had no place in this theory. Indeed, his faith in the entire Laplacian structure had been so weakened that he began to search for a new hypothesis of the origin of the earth. Criticism alone is not sufficient, but

to admit a competitive hypothesis to the working list is a concrete form of embodying a doubt respecting existing hypotheses, and serves better than any abstract skepticism to keep alive the sources of doubt. I assume that the system of multiple working hypotheses is accepted as furnishing the most wholesome conditions for research, and that any additional hypothesis not in itself incredible will be welcomed.

In his paper entitled "A Group of Hypotheses Bearing on Climatic Changes," he wrote:

> But if we question current conceptions we should present alternatives which account for the atmosphere. Let us therefore hastily follow the hypothetical growth of a planet built up by the slow aggregation of small bodies which join it at small velocities and develop a minimum heat. Let the case be purposely made rather extreme to develop sharply the difficulties springing from it. Let the infalling particles be small and their rate such as not to generate a high surface temperature. The growth of such a body up to the size of the moon may be taken as an hypothesis of lunar history, and the phenomena of the moon may serve as a check upon it. The moon may, however, have originated by fission even though the earth were built up by accretions. In the early stages of growth the gravity being low the aggregation may be supposed to have remained uncondensed. Volcanic aggregations of bombs, cinders and ashes are perhaps the nearest terrestrial analogues. The ingathering particles obviously carried with them so much of the atmospheric material as was entrapped or occluded within them in their solidification, or was absorbed into their pores or adhered to their surfaces. Judging from meteorites the amount of this might have been large. Gaseous molecules moving as independent bodies may have joined the aggregation and become absorbed in its porous body, but they would not have been collected into an appreciable atmospheric envelope until the body passed the size of the moon if the molecular considerations urged earlier in this paper hold good, though an atmospheric envelope would not have been entirely absent. As the mass grew the central pressure increased and condensation produced heat at the center in proportion to the work done, I find the explanation of internal heat chiefly in this self-condensation, it being essentially the application of the Helmholtz solar theory to a solid body. Tidal kneading and chemical action doubtless added their contributions. . . . .

This passage contains the germ of the idea which, in its mature development, Professor Chamberlin called the "planetesimal hypothesis": the idea that the earth has grown to what it now is by a process of gradual accretion of meteoritic or planetesimal material from a much smaller mass, or nucleus, and that in this process the earth has remained solid throughout, and its surface relatively cool.

The development of the new hypothesis required a re-examination of the entire field from the standpoint of both astronomy and geology. On the astronomical side it was necessary to examine critically the dynamical foundations and consequences of the hypothesis of Laplace and to find out whether the weaknesses which had developed in its geological aspects extended also to its astronomical aspects. Then, too, if the earth was built up relatively slowly, by a somewhat steady ingathering of planetesimal material, it was necessary to find somewhere in the astronomical field an adequate supply of such material.

Professor Chamberlin had not been trained in the rigorous methods of celestial mechanics, his training having been that of the naturalist. It happened, however, that in the department of astronomy of the University of Chicago there was a young instructor, Dr. F. R. Moulton, who was already interested in the astronomical implications of the doctrine of Laplace and who had begun to question its validity on purely astronomical grounds. The ideas of each were made known to the other by students who were attending courses in both astronomy and geology. Conferences were arranged, and there began not only a collaboration that was extremely fruitful but an intimate friendship that was terminated only by death.

The first fruits of this co-operation were two papers in the year 1900: "An Attempt To Test the Nebular Hypothesis by the Relations of Masses and Momenta," by T. C. Chamberlin, in the *Journal of Geology;* and "An Attempt To Test the Nebular Hypothesis by an Appeal to the Laws of Dynamics," by F. R. Moulton, in the *Astrophysical Journal.* Each of the two authors found that the Laplacian hypothesis was not able to bear even the astronomical evidence that was put upon it. The two papers were not independent, however. In his paper, Professor Chamberlin said:

> These results are the outcome of a joint inquiry by Dr. F. R. Moulton and myself. They are a part of the results of a more or less continuous study on related themes lying on the border-land of geology and astronomy, running through the past three years. Our relations have been so intimate and our exchanges of ideas so free and so frequent that it is impossible to apportion the responsibility for the various methods adopted and the modes of carrying them out. The higher mathematical work is, however, to be credited to Dr. Moulton. It has perhaps been my function in the main to formulate problems and suggest general modes of attack, and Dr. Moulton's to devise methods of analysis and bring to bear the mathematical principles of dynamics, but this has not been uniformly so. Quite often we have proceeded by successive alternate steps in which each was the parent of its successor.

There is also a similar statement in the paper of Dr. Moulton's.

The next step was a study of possible sources of meteors, planetesimals, and dispersed matter in general. The results of this study were contained in a paper by Professor Chamberlin which was published in the *Astrophysical Journal* in 1901 under the title "On a Possible Function of Disruptive Approach in the Formation of Meteorites, Comets and Nebulae." Its central theme was the effects of the close approach of two stars without collision, and in it he showed how such an approach would result in the dispersion of a portion of the sun's own material, and a possible fragmentation of already existing solid bodies such as planets.

On the geophysical side, there were problems relating to the origin and distribution of the internal heat of the earth; the sources of the hydrosphere and atmosphere; atmospheric balance according to the kinetic theory of gases; changes in the rate of rotation of the earth; internal pressures, compressions, and molecular rearrangements; the elasticity and rigidity of the earth; effects of the solar and lunar tides; and so on. No one person could be competent to deal authoritatively with all of the many problems which were raised by the new point of view, nor would he have the time to deal with them even if he were competent. For this reason Chamberlin sought the advice and assistance of experts in many of the fields of physical science, such as Dr. Lunn, Dr. Stieglitz, Professor Hoskins, Professor Schlichter, and many others whose names are not recorded here. He was very eager for help and for suggestions, and very generous with acknowledgment of any assistance which he had received. A collection of papers relating to these problems by various authors was published in 1909 as *Publication No. 107 of the Carnegie Institution of Washington*, under the title "The Tidal and Other Problems."

Although the new hypothesis had been widely discussed with and by scientific men in this country from the year 1900 on, its first appearance in print with a full discussion was in the *Year Book No. 3 of the Carnegie Institution of Washington*, for the year 1904. Other accounts of the hypothesis appeared shortly afterward in Chamberlin and Salisbury's *Geology*, Volume II (1905), and in Moulton's *Introduction to Astronomy* (1906). In the introduction to the discussion in the *Year Book* of 1904, p. 210, Professor Chamberlin says:

As the basis for developing the typical form of the planetesimal hypothesis, I have assumed that the parent nebula had a planetesimal organization from the outset. The conception is a rather radical departure from the gaseous conception of the familiar Nebular Hypothesis, and from the meteoritic conception of Lockyer and Darwin, so far as fundamental dynamics, and mode of evolution are concerned. To develop the hypothesis as definitely and concretely as possible, I have further chosen a special case from among those that might possibly arise, viz., the case in which the nebula is supposed to have arisen from the dispersion of a Sun as a result of close approach to another large body. The case does not involve the origin of a star, nor even the primary origin of the solar system, but rather its rejuvenation and the origin of a new family of planets. The general planetesimal doctrine does not stand or fall with the merits or demerits of this special phase of it, but to be of much real service in stimulating and guiding investigation, a hypothesis must be carried out into working detail so that it may be tested by its concrete and specific application to the phenomena involved, and hence the reason for developing a specific sub-hypothesis. This particular sub-hypothesis was selected for first development (1) because it postulates as simple an event as it seems possible to assign as the source of so great results, (2) because the event seems very likely to have happened, (3) because the form of the nebula supposed to have arisen in this way is the most common form known, the spiral, and (4) because spectroscopic observations seem at present to support the constitution assigned this class of nebula, although it must be noted that spectroscopic observations have not reached such a stage of development as to demonstrate the motions of the nebular constituents.

It is evident from this passage that the fundamental hypothesis was the existence of a nebula in which the motions of the particles were dominantly revolutions about a central mass rather than dominantly of the gaseous type of motion. This was the hypothesis to which his geologic and climatic studies had led him, and to which he clung tenaciously. The organization of such a nebula by the disruptive action of a passing star was, at first, a subhypothesis—a hypothesis to be replaced by an alternative in case it failed. The distinction between the hypothesis and the sub-hypothesis is highly illuminating. It marked clearly the geological character of the path along which he had arrived at his goal. Had he been an astronomer, had his thoughts been riveted upon the stars, it is altogether probable that the rôle of hypothesis and sub-hypothesis would have been interchanged.

But the sub-hypothesis did not fail in its purpose of harmonizing those simple and conspicuous relations among the attendants of the sun that had engaged the attention of Kant and Laplace. The facts that the orbits of the planets are circular, that they lie almost in a single plane, and that the planets all move in the same direction about the sun, follow as corollaries from the hypothesis of Chamberlin as simply as they superficially appear to do from the hypothesis of Laplace. Indeed, a careful scrutiny of the hypothesis of Laplace fails entirely to reveal what would happen. It is very doubtful whether there would have been any planets at all. According to the hypothesis of Chamberlin, the common plane of the planetary motion is nearly identical with the plane of the passing star, and the direction of the planets' motion is the same as the direction of motion of that star relative to the sun.

According to the hypothesis of Laplace, the present period of rotation of the sun should be but a few hours, and its axis of rotation should be perpendicular to the common plane of planetary motion; and neither of these expectations is in accordance with the facts. The period of the sun's rotation is nearly 26 days, and its axis differs by 7° from its expected position. According to the hypothesis of Chamberlin, the present rotation of the sun is the combination of its unknown ancient rotation and an unknown rotation about an axis perpendicular to the plane of the passing star which was impressed upon it by the passing star. The fact that the sun's axis is within a few degrees of this perpendicular suggests a preponderance of rotation impressed by the passing star, even though its actual position can be explained otherwise. The sun's rotation, however, is not a critical point under this hypothesis.

There is a slight tendency under the assumed mode of origin for forward rotation of the planets, but the preponderance is very small. If the rotations of Earth, Mars, Jupiter, and Saturn are forward and relatively rapid; if the rotations of Venus and Mercury are forward but very slow; and if the rotations of Uranus and Neptune are backward, as they seem to be, about all that can be said is that forward rotation preponderates; but again, the point is not critical, for initial individual peculiarities of rotation might easily outweigh statistical expectations.

Professor Chamberlin did not regard the formation of the satellite systems as a repetition in miniature of the formation of the planetary system. The nuclei of the satellites were from the beginning, or at least from very near the beginning, companions to the nuclei which later developed into the planets. They shared, therefore, in those events which controlled the rotations of the planets. If our own moon and a few of the outermost satellites of Jupiter and Saturn are excepted, the relations between the revolutions of the satellite systems and the rotations of the primaries seem to be rather closer than would have been anticipated. The satellites mentioned, however, are markedly exceptional.

It is seen from the foregoing brief sketch of the bi-parental hypothesis of the origin of the planetary system, to use another of Professor Chamberlin's phrases, when the origin of the planetesimals is under consideration, that the hypothesis accounts perfectly for those features of the system which stand out boldly and attract the attention, just as in a broad way modern geological theories give a satisfying account of the existence of mountains and valleys. Just as accidental peculiarities have determined the positions and shapes of the individual mountains or valleys, so the accidental distributions of nuclei and planetesimals and their velocities have determined the individual peculiarities of the various members of the planetary system.

The Laplacian hypothesis was highly attractive to mathematicians because its simplicity attracted the efforts of mathematicians, and its difficulties stimulated their best efforts without ever wholly satisfying them. It became almost a dogma that cosmogony was a mathematical subject. Anything would succumb under a sufficiently acute mathematical attack. It is evident, however, to one who meditates much upon the violence which attends the birth of a planetary system according to the bi-parental hypothesis, that the contributions of the mathematician, while extremely valuable, must always be limited to broader features only. Just what will occur when two hot, intensely energetic, rotating stars pass each other at relatively small distances on hyperbolic orbits, is a very beautiful mathematical problem, but it is an extremely complicated one. If the eruptive features of real stars are included, it ceases to be a mathematical problem, for it is no longer definite. The problem is much simplified if the stars are assumed to be quiescent and not rotating. It is conceivable that such a problem might be solved if the distance of approach were not too small. While the solution would give much valuable information, it would still be only an approximation, and this approximation would doubtless be greatly modified when the elements of eruption and axial rotation are included.

It is evident, therefore, that with the planetesimal hypothesis, or better perhaps with the bi-parental hypothesis, cosmogony ceases to be a purely mathematical subject. The problem, complicated by many elements, requires the insight and the genius of a great naturalist, precisely such talents as Professor Chamberlin possessed; and when the lively imagination of genius is regulated and controlled by another who is highly gifted in the difficult fields of celestial mechanics, there is a concurrence of talents that is rare indeed. Such collaboration existed between Professor Chamberlin and Dr. F. R. Moulton through many years of intimate association. Indeed, in the end, Professor Chamberlin himself acquired an insight into and a mastery of the principles of mechanics that was highly remarkable when it is borne in mind that he was approximately at the age of sixty when these studies were commenced.

In the field of geology he was, of course, a master. That the planetesimal hypothesis met the demands of geology was to be expected, for it was the needs of geology that gave it birth. It is particularly evident that, on the basis of this hypothesis, glacial epochs are no more surprising in the Cambrian or in the Permian than in the Pleistocene. We may be in doubt as to why they have occurred at all, but at any rate the geological evidence does not tell us that there was glaciation at a time when the theory tells us that the climate was warm and moist. Again, the theory does not tell us that in the remote past the earth's rate of spin was much higher than it is at present, and therefore the day much shorter than now, when the geological and geographical evidence is all against it.

The geological and the biological evidence is all against the few million years that were allowed for the age of the earth, even by so eminent a physicist as Lord Kelvin, who regarded the age of the earth as rigorously limited by the theory of Helmholtz. To be sure, the origin of the sun's heat has nothing to do with the planetesimal hypothesis, further than the fact that Professor Chamberlin refused to acknowledge the validity of Helmholtz's theory when it ran counter to the geological evidence. He postulated a condition of the sun quite similar to the present one at the time when the planetary system was formed, perhaps billions of years ago. In doing so, he broke the fetters of astronomical and physical dogma just as truly as Galileo broke the fetters of theological dogma. He did not worry over the question of the origin of the sun's heat. It was evident to him on geological grounds that the sun had been in substantially its present condition throughout the entire history of the earth, perhaps billions of years. It was a problem for the physicist to discover the source, or the sources, of its energy. Nevertheless, thirty years ago, when Lord Kelvin asserted, in his Baltimore address, that gravitational potential was the only source of the stellar energies, Professor Chamberlin pointed out possibilities of subatomic energies which, he suggested, might be of a very high order of magnitude. The physicists at that time had not recognized these energies; but when the geological evidence seemed to demand them, Professor Chamberlin did not hesitate to suggest them and to deny the right of a physicist to assert, even implicitly, that only those things exist with which he, the physicist, is acquainted.

During the past thirty years, however, the physicists have succeeded in penetrating many secrets of the atoms and have given us the very wonderful electron theory of its structure. According to this theory, mass is a property of the electron and is due to the electric charge. The available electrostatic potential energies of its electrons are perhaps hundreds of thousands of times greater than the available gravitational potential energy of the sun, and dynamical astronomers no longer heed the time limitations which had been imposed by the theory of Helmholtz. Indeed, the dynamics of star clusters and galaxies are vastly more extravagant of time than is the science of geology.

It is greatly to the credit of Professor Chamberlin that in a broad way he anticipated these results. He stated that the close approach of our sun to another star was a likely event, because his mind had been freed from the fetters of the Helmholtz theory. To the astronomers who had been reared on the theory of Helmholtz, an event which occurs on an average only once in a million billion years seemed highly unlikely; and this divergence of view as to the time-scale was probably the greatest obstacle which the planetesimal hypothesis had to encounter. According to the nebular hypothesis of Laplace, all of the bodies in the solar system evolved from a common nebula which had once existed in a vastly diffused state. In a hazy way it was thought that all the other stars had done likewise, so that if there had not been a creation a few million years ago, at least all matter everywhere was in a state of diffused nebulosity, and it was only within the past few million years that galactic, stellar, and planetary organization has occurred, a simultaneous organization everywhere.

This was not the view of Professor Chamberlin. As he saw it, the galactic organization of stars was already very old when our present system of planets was formed, and the process of forming planetary systems has gone on with other stars in our own galaxy and in other galaxies through all past time and will continue throughout all time to come. He has led us up to the summit of a very high mountain, from which we have a clearer perspective of great and distant events, and a vision that will always bring joy to those who can live and breathe in the rare atmosphere of such lofty elevation.