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A GEOLOGIST AMONG ASTRONOMERS: THE RISE AND FALL OF THE CHAMBERLIN–MOULTON COSMOGONY, PART 1

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1. INTRODUCTION

At the end of December 1905 Edwin B. Frost, recently appointed Director of the Yerkes Observatory, received the following announcement from the chairman of the Geology Department of the University of Chicago:

> I believe it is the practice of celestial astronomers (which should certainly be followed by telluric astronomers) to notify their colleagues of important events impending in any part of the universe. This is therefore to inform you that on and after January 1st proximo, the solar system will be run on the new hypothesis. It is not expected that the transition will be attended by any jar or other perceptible perturbation or that the change from a gaseous to a planetesimal feed will occasion any nausea. Everything is expected to work smoothly. There will be somewhat more play of the parts than heretofore and it will not be necessary to put the screws on quite so much to keep the machine running according to the rules. The planets will be allowed to rotate as fast or as slow as they please without regard to the speed of their satellites, and these will be permitted to go round forwards or backwards as they see fit without incurring the suspicion of being illegitimate members of the family. The inclination of the Sun's axis will not be regarded as a moral obliquity but merely as a frank confession that once on a time he flirted with a passing star and got twisted a bit, as was natural. The solar family will not appear as prim and precise as heretofore, and at first the neighbors may cast some reflections on it, but it is thought that in time they will come to rather like the new regime and will take kindly to the notion of new families springing up here and there in a legitimate way without sending the whole celestial concern to smash.
>
> Please post notice according to the rules of the game.
>
> Yours truly,
> (Signed) T. C. Chamberlin,
> Late of Yerkes Observatory
> (N.B., Telluric Annex.)[1]

The occasion for Chamberlin's letter was the forthcoming publication of vol. ii of his textbook on geology, containing a comprehensive exposition of his 'planetesimal hypothesis' for the origin of the solar system and its application to the formation of the Earth.[2] In this article I trace the early development and subsequent fate of Chamberlin's hypothesis as an episode in the history of astronomy (its geological aspects will be discussed elsewhere). As the above quotation suggests, the story involves not only the interaction of theory and observation but also the personal characteristics of an unusual scientist—a man who, in his late fifties, jumped into a new field and had a major impact on it.

In the first two decades of the twentieth century, the nebular hypothesis for the origin of the solar system was replaced by the hypothesis that the planets were formed as a result of a close encounter of the Sun with another star. In modern terms, a 'monistic' cosmogony was replaced by a 'dualistic' one (though the replacement was incomplete since the new hypothesis unlike the old one did not attempt to explain the origin of the Sun itself). Several competing theories based on the dualistic hypothesis were proposed during this period. The most comprehensive, and at first the most successful, was that of Chamberlin and his junior colleague at Chicago, the astronomer F. R. Moulton. Chamberlin and Moulton's effective critique of the nebular hypothesis was also in large part responsible for allowing alternative theories to receive a fair hearing, including the 'tidal' theory of James Jeans and Harold Jeffreys that subsequently was adopted by astronomers as the successor to the Chamberlin–Moulton theory. But the unique and in some ways most valuable aspect of the Chamberlin–Moulton theory—that the planets and satellites were formed by accretion of small solid particles ('planetesimals') rather than by condensation of gaseous or liquid material—was forgotten or rejected by most other cosmogonists.[3]

To Chamberlin the most objectionable feature of the nebular hypothesis was the inference that the Earth had begun in a hot molten state and gradually cooled down while solidifying. Lord Kelvin had estimated the time required for this cooling process and found it to be a few tens of millions of years, a result that contradicted geological evidence indicating much longer periods.[4] Moreover, Chamberlin found that the assumption of a dense primeval atmosphere rich in carbon dioxide, whose variation was frequently invoked to explain major climatic changes and glaciation, was untenable in the light of deductions from the kinetic theory of gases about the escape of atmospheric gases at high temperatures.

In addition to its geological implications the nebular hypothesis had an indirect influence on nineteenth-century biological theories, and its overthrow by Chamberlin and Moulton was associated with a major change in the scientific worldview. Laplace's cosmogony could be considered a paradigm of evolutionary naturalism, a plausible prehistory for theories of organic development. In England, early evolutionists like Robert Chambers and Herbert Spencer constructed schemes beginning with the formation of a nebula in space and running through the evolution of living forms including man.[5] In America, according to one historian, the debate about astronomical evolution helped prepare intellectuals to accept Darwin's theory of natural selection.[6]

Since the formation of a star involved the formation of planets, the nebular hypothesis suggested a plurality of inhabited worlds throughout the universe. Every planet should pass through a series of similar stages as it cooled down, and every civilization or race should pass through the same stages of cultural and scientific attainment as it developed, just as nineteenth-century anthropologists described all terrestrial societies in terms of their degree of advancement along a one-dimensional path, the highest point being that reached by white European males.[7]

As historians of the biological and social sciences have shown, evolutionary naturalism suffered a sharp setback in the early twentieth century at the same time mechanistic determinism was being discredited in the physical sciences.[8]

As in cosmogony, continuous predictable evolution in one direction was replaced by a discontinuous random process going in many directions.[9] The Chamberlin–Moulton theory is not, however, typical of twentieth-century theories which emphasize mathematical structure at the expense of intuitive comprehensibility; the Jeans cosmogony is more 'modern' in this respect. Chamberlin's personal achievement was against the trend of the times; he demonstrated the power of what he called a 'naturalistic' method of science, and showed that the qualitative reasoning of the geologist could contribute something in the domain of an exact quantitative science such as astronomy. Nevertheless the demise of this theory resulted from its failure to satisfy quantitative criteria.

2. COSMOGONY AT THE END OF THE NINETEENTH CENTURY

As late as 1906 a popular British writer on astronomy proclaimed that "the nebular theory, modified by subsequent research, seems destined to hold its own against all comers".[10] But the American astronomer Simon Newcomb warned that "cautious and conservative minds will want some further proof of the theory before they regard it as absolutely established" even though many facts support it.[11] Most scientists at the end of the nineteenth century favoured the nebular hypothesis but with reservations—they believed that the solar system had probably been formed by a process of separation and condensation of parts of a whirling cloud of hot gas, but they did not think that Laplace's original theory gave a completely satisfactory explanation of how this process worked.[12]

The following were considered to be the most significant facts and arguments:

(i) *Rotation of planets: orbital motions of satellites.* Laplace had constructed his hypothesis on the basis of the supposed fact that all planetary rotations and satellite orbits are *direct*, *i.e.* in the same direction as the orbital motions

TABLE 1. Knowledge of directions of planetary rotation and satellite motion. (D = direct, R = retrograde)

	Rotation			Satellite motion		
	1900	1920	1977	1900	1920	1977
Mercury	D	D	D			
Venus	D	D	R			
Earth	D	D	D	1 D	1 D	1 D
Mars	D	D	D	2 D	2 D	2 D
Jupiter	D	D	D	5 D	7 D, 2 R	9 D, 4 R, 1 ?†
Saturn	D	D	D	9 D	9 D*, 1 R	9 D*, 1 R
Uranus	R	R	R	4 R	4 R	5 R
Neptune	R	R	D	1 R	1 R	1 D, 1 R
Pluto			D			

* Themis, a tenth satellite of Saturn, was reported by W. H. Pickering but not confirmed by other observers; it is usually omitted in textbooks. See A. F. O'D. Alexander, *The planet Saturn* (London, 1962), 268. Another tenth satellite, Janus, was reported by Dollfus in 1966, but its reality "is questioned by some". William K. Hartmann, *Moons and planets* (Belmont, Calif., 1972, 1973), 20.

† C. Kowal reported a "Probable New Satellite of Jupiter" in *IAU circular* No. 2845 (3 October 1975); this would be the fourteenth. It was not yet established at the end of 1976 whether its orbit is direct or retrograde. See "All the Outer Satellites of Jupiter", *Sky and telescope*, li (1976), 242–3.

of the planets. No one doubted in 1900 that the rotations of the first six planets are all direct (see Table 1), though it was realized that reliable determinations of the periods of Mercury and Venus were still lacking because of technical difficulties. By the middle of the nineteenth century it had been established that the Uranus system is a clearcut exception to the rule; the rotation of the planet and the motions of all known satellites are around an axis nearly perpendicular to that of the orbital motion (obliquity 98°), so these motions are retrograde.[13] However, the fact that the rotation of the primary agrees with the motion of the satellites could still be regarded as a confirmation of the Laplacian viewpoint,[14] and the tilt of the entire system could be attributed to an accidental external cause. Hence when the orbital motion of Neptune's satellite Triton was found to be retrograde, it was generally assumed that the rotation of Neptune itself is also retrograde. If this assumption were granted, one could say that the rotations and satellite motions for the first six planets are all direct, those of the Neptune system are retrograde, and those of the Uranus system are about halfway between direct and retrograde.

To account for these facts—which in our superior wisdom we may consider somewhat theory-laden—it was generally assumed that there were two causes in operation during the formation of the system: one tending to produce direct rotation, the other retrograde. The first cause would have dominated inside the main part of the nebula, while the second dominated in its outer regions. In fact it was quite easy to think of a cause producing retrograde motion. If each ring thrown off by the whirling nebula consisted of fragments of material whose orbital motion was governed by Kepler's laws, those in the outer part of the ring would be moving more slowly than those in the inner part. If fragments from two adjacent parts of the ring collide and stick together, the excess speed of the fragment on the inner side will impart a retrograde rotation to the resulting compound. In order to avoid this result, Laplace had assumed that each part of the ring of material left behind by the contracting nebula would have the *same angular velocity* (as if it rotated like a rigid solid), so that fragments from the outer part of the ring would have a *greater linear velocity* than those from the inner part. Thus when the ring condensed to a planet the rotation would be direct.

In 1884 the French astronomer Hervé Faye proposed a plausible combination of these two causes. If planets form by aggregation of particles inside a nebula of uniform density, the particles would at first be subject to a central force proportional to the distance, and thus would move at constant angular velocity. Such planets would form with direct rotation. If there is a subsequent condensation of the nebula towards the centre, the force acting on the particles outside the condensation will change to an inverse square law, and their orbits will become Keplerian. Thus planets formed at the edge of the nebula will have retrograde rotation, in agreement with the "observed" facts.[15]

Faye's theory abandoned the Laplacian conception of fluid rings spun off by the nebula, held together and rotating at a common angular velocity. Instead, as G. H. Darwin pointed out, he assumed in effect that the planets were formed by aggregation of solid particles inside the nebula; moreover in his theory the inner planets would have been formed before the outer ones, reversing Laplace's sequence.[16]

A different mechanism for producing direct rotation had been proposed somewhat earlier by the American astronomer Daniel Kirkwood.[17] He assumed that the planets would first be formed with retrograde motion by aggregation of particles in Kepler orbits. Then tidal forces due to the Sun's gravity will damp out the rotation and produce a situation in which the planet always keeps the same face to the Sun. This amounts to a slow *direct* rotation. The planet will continue to contract and solidify; the rate of rotation will increase, tidal forces now being less effective. The tidal mechanism will not be able to eliminate the retrograde rotation if the planet is very far away from the Sun (Uranus and Neptune). This explanation appeared to be confirmed by Schiaparelli's conclusion (which was accepted until a few years ago) that Mercury always keeps the same face to the Sun.

One other observation in this category was that Phobos, the inner satellite of Mars discovered by Asaph Hall in 1877, moves faster in its orbit than the primary rotates. This behaviour is contrary to what one would expect from the Laplacian mechanism for satellite formation. A similar discrepancy was noted for the inner part of Saturn's rings.

(ii) *Angular momentum distribution.* The Sun rotates much too slowly to be the end result of a process that formed planets moving at their present speeds. This difficulty was evident to Descartes as long ago as 1644; he assumed that the rate indicated by sunspot motion applied only to the Sun's outer atmosphere and that the inner part of the Sun really moves much faster.[18] In the nebular hypothesis the problem is especially severe if one postulates that the nebula was originally of uniform density and rotated with the same angular velocity throughout; this was pointed out by J. Babinet who calculated that the present rotation period of the Sun (25 days) corresponds to the angular momentum of a nebula occupying the volume spanned by Neptune's orbit and rotating with a period of about 27,000 centuries.[19] That would of course be too slow to produce enough centrifugal force to spin off Laplacian rings, to say nothing of a ring moving with the present speed of Neptune. Babinet did not advance this as an objection to Laplace's theory but rather to the idea that the nebula was originally of uniform density; he argued that one should return to the Laplacian assumption that the planets were formed from the tenuous atmosphere of a highly condensed Sun. Fouché brought up this point again, noting that not only must one assume that the Sun started out highly condensed, but also that it later became *less* condensed; this is exactly the opposite of the basic assumption of Faye's theory.[20]

(iii) *Age and heat of the Sun.* One reason for assuming that the nebula originally had uniform density was that this provided a plausible theory for the origin of the Sun itself as well as the planets. Hermann von Helmholtz had argued in 1854 that the gravitational contraction of an enormous diffuse mass of gas into a sphere the size of the Sun would raise it to a high temperature (by conversion of mechanical energy into heat energy).[21] Lord Kelvin accepted this idea and estimated that the contraction would provide enough energy to keep the Sun shining at its present rate for somewhere between 20 and 100 million years; he later favoured the lower limit.[22] This calculation seemed to conflict with geological evidence indicating a much greater age of the Earth.[23] James Croll, a Scottish geologist, suggested that the available time could be prolonged by 50 million years by postulating that the nebula had been formed by direct collision of two bodies, each having half the Sun's mass, with speeds of 476 miles per second.[24] But Kelvin's decreasing estimates of the age of the Earth itself, from heat-conduction theory, maintained an even more direct conflict with the geological time scale.[25] These estimates were based on the assumption that the Earth started as a sphere at a uniform high temperature and were themselves regarded as consequences of the nebular hypothesis. The discrepancy would not be considered a serious objection to the hypothesis except by those who were convinced of the validity of the geological estimates.

(iv) *Formation of rings and planets*. Laplace had asserted without proof that the contracting nebula would shed a discrete ring when the centrifugal force balanced the gravitational force at the outer edge, and that the ring would subsequently collapse into a single planet. Kirkwood, earlier one of the strongest supporters of the nebular hypothesis, argued in 1869 that one would expect continuous ejection of material from the nebula rather than discrete rings. Moreover, he thought it would take hundreds of millions of years for the material in Neptune's ring to collect into a single planet. This would exceed the estimated age of the Sun; and by the time the planets formed they would have cooled off so much that they would be solid and thus unable to form satellites by the spin-off process.[26] Kirkwood's objections were frequently cited; Roche's theory, which claimed to account for the formation of discrete rings by the sudden falling of material from the shoulders of the nebula, was not widely known.[27]

(v) *Observations of nebulae*. The plausibility of the nebular hypothesis had originally been supported by the apparent existence of clouds of gaseous matter—"shining fluid" as William Herschel expressed it—from which solar systems might be formed. When Lord Rosse succeeded in resolving some of these nebulae into distinct stars in 1845, the death of the nebular hypothesis was widely proclaimed.[28] Rosse's discovery was not, however, regarded as a true refutation of the nebular hypothesis by those who were firmly convinced of its validity on other grounds, but only as a removal of one of the arguments in its favour.[29] On the other hand it was generally regarded as a dramatic confirmation of the theory when William Huggins in 1864 found that the spectrum of a nebula resembled that of a gas (not a star).[30] In the 1880s the techniques of time-exposure photography were used to produce striking pictures of spiral nebulae; several people regarded these as visual demonstrations of the Laplacian process of planetary formation.[31] But further research led to the conclusion that most nebulae do not have the annular structure one would expect from Laplace's theory; the spiral form is most common.[32] Thus cosmogonists in 1900 were faced with the question: Can a planetary system evolve from a spiral nebula? And could the theory of such an evolution legitimately be called a "nebular hypothesis"?[33]

(vi) *Religious belief in creation*. The religious objection to a naturalistic account of the origin of the world had been voiced with some intensity in the middle of the century, especially when it appeared that the nebular hypothesis would be made the basis of a general theory of the evolution of living organisms. The criticism came from scientists as well as clergymen.[34] Defenders replied that God was still needed to create the original matter of the nebula, and that the attempt to explain its development by natural law was no more atheistic than the application of laws in other areas of science; the existence of a law necessarily implies the existence of a law-giver.[35] By the end of the century religious opposition had largely subsided, although as late as 1904 two amateur astronomers stated at a meeting of the British Astronomical Association that they could not accept the nebular hypothesis because it conflicted with the Biblical account of creation.[36] The related question of a plurality of inhabited worlds was still open to discussion; Alfred Russel Wallace revived the controversy in 1903 when he argued that the Sun is at the centre of the universe, ours is the only planet with the right combination of conditions needed for the development of life, and all this cannot be attributed to chance but implies a definite purpose.[37]

The nebular hypothesis had few serious competitors in the nineteenth century. One was the 'meteoritic hypothesis', the assumption that the planets formed by accretion of small solid particles rather than as hot gaseous or liquid balls.[38] It was not clear that any of the major difficulties of the nebular hypothesis could be solved in this way; the meteoritic hypothesis only had the advantage of relying on phenomena now observed to be in operation, and thus conformed to the uniformitarian principle of geology. G. H. Darwin, in a frequently-cited paper of 1888, argued that a swarm of meteors would behave like a fluid, hence one might construct a Laplacian nebula out of them.[39] But he did not show how planets formed from such a nebula could have direct rotation, or how an Earth built by accretion of meteoric particles would provide a superior basis for geology. In his widely cited book on *The meteoritic hypothesis* (1890), J. N. Lockyer did not even attempt to explain the formation of planets.

Aside from Croll's postulate of stellar impact (which he did not regard as a replacement of the nebular hypothesis but an attempt to explain the origin of the nebula), there was only one definite anticipation of the encounter hypothesis before 1900. Alexander William Bickerton (1842–1929), a British engineer who became Professor of Physics and Chemistry at Canterbury College in Christchurch, New Zealand, published a long series of papers on his theory of 'partial impact' of stars beginning in 1878.[40] His work was not generally known to European and American astronomers until 1910, by which time his interests had shifted from planetary cosmogony to the theory of novae.[41]

3. THE ASSAULT ON THE NEBULAR HYPOTHESIS

The overthrow of the nebular hypothesis was primarily due to the efforts of two Americans, the geologist Thomas Chrowder Chamberlin (1843–1928) and the astronomer Forest Ray Moulton (1872–1952). Chamberlin was clearly the dominant partner who generated most of the ideas, but Moulton's contribution was essential in making the ideas consistent with celestial mechanics and thus acceptable to astronomers.

Chamberlin began working on his planetesimal hypothesis in 1892, which was the year he came to the University of Chicago to head its geology department. By then he had already established a considerable reputation as a geologist and educator. He had been in charge of part of the Wisconsin Geological Survey in the 1870s, and was head of the glacial division of the United States Geological

Survey from 1881 to 1904. He served as President of the University of Wisconsin from 1887 to 1892. As founder-editor of the *Journal of geology* (1893–) he continued to exert considerable influence on American geology through the 1920s. He was also well known for his 'method of multiple working hypotheses'—his 1897 article on that subject was so often cited that it was reprinted in *Science* in 1965.[42]

Moulton, who received his B.A. degree in 1894 from Albion College (Albion, Michigan), went to the University of Chicago as a graduate student, and was appointed assistant in astronomy in 1896. He was awarded the Ph.D. in astronomy and mathematics in 1899, and later became professor of astronomy at Chicago. He published several articles and books on topics in astronomy, in addition to his work with Chamberlin on the origin of the solar system.[43]

Chamberlin's initial reason for doubting the validity of the nebular hypothesis had nothing to do with any of the well-known astronomical problems discussed in the previous section. His studies in glacial geology led him to investigate theories of the ice age, in particular the suggestion that it was caused by a decrease in the amount of carbon dioxide in the atmosphere.[44] He then undertook a more general study of the early atmosphere, starting from the usual assumption that the Earth was formerly molten, with a dense atmosphere rich in carbon dioxide.

At this point a paper by the Irish scientist G. Johnstone Stoney (1826–1911) exerted a crucial influence on Chamberlin's thinking. Stoney had been one of the first to take up the kinetic theory of gases and used it to estimate the size of an atom; he is now known mainly for having invented the word 'electron'.[45] In 1892 Stoney wrote a paper "On the Cause of the Absence of Hydrogen from the Earth's Atmosphere and of Air and Water from the Moon" in which he pointed out that gaseous molecules whose velocities exceed the 'escape velocity' of a planet or satellite would be lost from its atmosphere.[46] According to the kinetic theory of gases the average molecular speed varies as the square root of the absolute temperature divided by the molecular weight [$mv^2 \propto kT$] so the lighter gases such as hydrogen are most likely to escape.

If the atmosphere were in thermal equilibrium at all heights, the number of molecules having velocities high enough to escape could be calculated from Maxwell's velocity distribution law. Stoney did not believe this law would apply to planetary atmospheres. Instead he argued in a more detailed paper in 1897 (of which Chamberlin obtained an advance copy through G. E. Hale) that the Earth's atmosphere has retained water vapour but not helium, hence there will be a significant loss of a gas when the escape velocity is less than about 10 times its average molecular speed, but not when it is 20 times or more.[47]

Chamberlin used Stoney's arguments in a paper on climatic change, presented at a meeting of the British Association for the Advancement of Science in Toronto in August 1897. He began by challenging the "time-honored conception of an exceedingly extensive, dense, warm and moist atmosphere" of the primeval Earth, and the assumption that this atmosphere contained the huge amount of carbon dioxide that is now locked up in mineral deposits on the Earth's surface.[48] He noted that the idea of a "vast original atmosphere" is based primarily on the assumption of the Earth's "supposed original molten state" which in turn is based on the nebular hypothesis.[49] But, in his first published remark on cosmogony, Chamberlin stated:

> There is still some ground to doubt the nebular hypothesis and to entertain some of the various phases of the meteoroidal hypotheses. 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. Of the 5000 known nebulae to which we naturally look for analogy very few, if indeed any, strictly interpreted, exemplify in a clear and decisive manner the systematic annular evolution postulated by Laplace. The photographs of the nebula of Andromeda, that were hailed with such delight on their first appearance as exemplifying the Laplacean hypothesis, appear upon more critical study to support it only in vague and general terms, if indeed they lend it support at all. The Saturnian rings, the trite source of illustration and analogy, prove under the test of the spectroscope to be formed of discrete solid particles, and not of gas, and the investigations of Roche have put a new phase on their theory. While in their form they tally with the annular hypothesis they do not support its gaseous phase, if indeed they lend it any support at all.[50]

Turning to the supposed molten state of the primeval Earth, Chamberlin raised the question of whether such an Earth could have retained gases such as oxygen and carbon dioxide long enough for them to form an atmosphere. To answer this question he presented tables of the escape velocity (called 'parabolic velocity' here) at various heights above the Earth's surface, computed for him by F. R. Moulton. The escape velocity varies from 11181 m/sec (= 6·9 miles/sec) at the Earth's surface ($r = 6{\cdot}38 \times 10^6$ m from the centre) to 565 m/sec at a distance 25×10^8 m from the Earth's centre. But when the centrifugal force due to the Earth's rotation is taken into account, the escape velocity decreases rapidly above that height and becomes zero at 304×10^8 m from the Earth's centre.[51] He then gave a table of average molecular velocities of helium, water vapour, carbon dioxide, oxygen, and nitrogen, computed by A. W. Whitney.[52] Another table shows the proportion of molecules having speeds various multiples of the average speed, for temperatures up to 4000°C; this table is based on the Maxwell velocity distribution law with no mention of Stoney's reservations about the applicability of that law to atmospheres.[53] Using Maxwell's estimates of the frequency of molecular collisions, Chamberlin then computed the times required for all the molecules to acquire the escape velocity.

Chamberlin recognized the difficulty of drawing definite conclusions in a situation where there are so many unknown or conjectural factors, but the main line of his argument was fairly convincing. If the Earth were molten, its temperature must have been at least 4000°C during its condensation, and this high temperature would have been shared by the gases in the upper atmosphere where the escape velocity is relatively small. Moreover, if one assumes that the Moon was created from the primeval Earth, either by condensation from a secondary Laplacian ring or by fission as in G. H. Darwin's hypothesis,[54] then the rotational speed of the Earth at that time must have been much greater than now; the period of rotation may have been as short as 1h 24m, corresponding to equality of centrifugal force and gravitational force at the equatorial surface. Under such conditions the escape velocity would be even further reduced; "indeed it is difficult to see how the Moon could have separated from the Earth without carrying away the atmosphere".[55] In any case, the numerical estimates from kinetic theory suggested that if the temperature were higher than 3000°C, there was no hope of retaining much more than a small residue of the atmosphere, and the only question was whether the gases would disappear in a few seconds or a few hundred years. The same arguments would apply with even greater force to the earlier stage of the nebular hypothesis, the supposed gaseous rings from which the Earth condensed; only by assuming that such a ring rapidly cooled and crystallized into small solid particles could one avoid the conclusion that its material would have been dissipated into space.[56] The only way out of the difficulty, if one still accepted the nebular hypothesis, would be to assume that the original Earth–Moon ring was not gaseous but composed of solid particles.

But that assumption, known in the nineteenth century as the meteoritic hypothesis, ran into other difficulties, as Chamberlin soon discovered when he began to read the literature on cosmogony. According to a recent English book, citing Faye's authority, planets formed by aggregation of solid particles would be expected to have retrograde rotation.[57]

Chamberlin detected a fallacy in the argument: it was based on the assumption that the particles move in circular orbits so that in an encounter of two particles the outer one will be moving more slowly. But in reality collisions occur only if and when the orbits of two particles intersect, and this implies that at least one of them moves in an ellipse. The geologist, applying Kepler's second law, noticed a point that seemed to have escaped the attention of the astronomers who had previously written on this subject: the orbits are likely to intersect at a place where the aphelion of the inner orbit corresponds to the perihelion of the outer orbit; *at this place*, the outer body will be moving faster than the inner one.

Puzzled that the eminent Faye and other cosmogonists could have overlooked such a simple argument, Chamberlin wrote to his former colleague George C. Comstock, Director of the Washburn Observatory at the University of Wisconsin, asking him to verify the reasoning.[58] Comstock replied that while Chamberlin's conclusion about speeds of particles in elliptical orbits was correct, retrograde rotation had not been a strong argument against the meteoritic theory since one could account for the change to direct rotation by invoking various other causes such as tidal friction.[59]

In spite of Comstock's warning that it was not decisive, Chamberlin incorporated the argument about collisions producing direct rotation in his early publications on cosmogony, beginning with a footnote in his 1897 paper.[60] The argument has not received much attention from astronomers, and it is not clear that it is valid in all cases.[61] However, calculations by R. T. Giuli in 1968 and A. W. Harris in 1977 indicated that it is probably correct.[62]

By the end of 1895 Chamberlin had begun to study other astronomical phenomena involving rings that might be comparable to those postulated in the nebular hypothesis. In the letter to Comstock mentioned above he inquired about the nature of Saturn's rings. He was aware that the rings were generally believed to be composed of separate particles (as proposed on theoretical grounds by James Clerk Maxwell in 1857,[63] supported by James Keeler's spectroscopic observations a few months earlier in 1895[64]) but was not sure if this meant that the particles each moved in separate orbits around the planets. If so, and if they were subsequently to condense into a satellite, would that satellite have a backward or forward rotation? Comstock replied that he thought the perturbations between particles did not cause "any radical deviation from approximately circular motion". But there was no chance of testing the theory of the direction of rotation of aggregated bodies in this case, since Edouard Roche had proved that the rings could never condense into a satellite; they are inside the 'Roche limit' (as it is now called) so tidal forces would prevent any large body from being in hydrostatic equilibrium.[65]

In a lecture on 27 January 1896, Chamberlin explored the geological implications of the nebular hypothesis and analyzed some of the astronomical evidence often given for it. His contact with Washington geologists, resulting from association with the U.S. Geological Survey, had led him to consider the hypothesis that the Earth's crust is mobile. That hypothesis, based earlier on the assumption that there is a fluid layer below the crust, had been attacked by physicists like Lord Kelvin who argued that the entire Earth must now be solid, since otherwise one would expect tidal effects different from those observed.[66] While many geologists had bowed to Kelvin's authority, Chamberlin reported that John Wesley Powell and Grove Karl Gilbert still believe in liquidity, while Clarence Dutton insisted on 'mobility' without using terms such as 'fluidity' or 'plasticity' to account for it.[67] Chamberlin was willing to consider these ideas, but differed from Dutton in one respect: he thought there was evidence for differential motion of parts of the crust (for example, the raising of mountain chains), while Dutton denied differential motion. If the Earth had been formed according to the nebular hypothesis, it would have had a liquid layer under the crust and differential motion would have been possible. But he felt uneasy about drawing such an important geological conclusion without first examining with a critical eye the fundamental hypothesis on which it seemed to depend. At first sight, the rings of Saturn seemed to provide striking support for the nebular hypothesis. So did the photographs of nebulae showing a ring-like structure. But then he realized that only a few of the thousands of known nebulae really had anything like a ring structure, and on closer examination those looked more like spirals than rings.

About halfway through this lecture, according to the notes that have been preserved, Chamberlin confessed his attraction to the meteoric cosmogony, though not yet to the exclusion of the nebular hypothesis. If a nebulous mass, extending far out into space, throws off a ring, this ring would be very attenuated and would rapidly lose heat; probably it would solidify into separate particles, such as those in Saturn's ring, before condensing to a planet. If the Earth were formed by aggregation of solid particles, heat would be generated during its formation, and the age of the Earth might be considerably greater than that estimated by assuming it had simply cooled down from a molten or gaseous state.[68] Thus the geologists might be able to escape the restrictive time limits imposed by physicists.[69]

These arguments were elaborated in Chamberlin's 1897 paper at Toronto. If the Earth was formed by the *slow* accretion of solid particles it may have remained solid and cold throughout its history, or until it grew to such a size that gravitational contraction generated internal heat as in the Helmholtz solar theory.[70] The gases trapped inside would then be expelled to the surface, which would still be cold enough to hold most of them in an atmosphere. Conditions favourable to life could thus be present early enough "to satisfy the most strenuous demands of theoretical biology. Most of the restrictive arguments of Lord Kelvin and others lose their application under this hypothesis."[71]

Chamberlin did not want to reject the nebular hypothesis without giving it a fair chance to overcome its difficulties. In November 1897, Moulton presented him with calculations indicating discrepancies between the observed rate of rotation of the Sun and that expected from Laplace's theory. Chamberlin suggested that "at the time of formation of Neptune, the material of the Sun was so concentrated toward the center that the faster moving outer portion was just able to accelerate the central or axial slow moving portion, so as to give the present Sun's energy of rotation. Then suppose that previous to the separation of each subsequent planet the central concentration had relatively increased and the density of the peripheral portion had relatively diminished, so that in each instance the same energy of rotation was present (neglecting the discharged portions) and yet the ratio between the slow moving central portion and the fast moving outer portion was such as to give in each case the present solar rotation . . .".[72] Moulton replied that while this might be the only possible explanation of the slow rotation of the Sun on the assumption of contraction from a large homogeneous mass, he preferred to consider the question more carefully before reaching a definite conclusion.[73]

But Chamberlin had moved too far from the nebular hypothesis to invest any further effort in patching it up; he was already beginning to realize the enormous changes in geological theory that would result from adopting the meteoric theory instead. In January 1898 he warned his Wisconsin colleague Charles Van Hise, who was about to publish a major paper on crustal shortening in Chamberlin's *Journal of geology*, that quantitative estimates of forces determining the structure of the Earth's crust might well be affected by the choice of a theory of the Earth's origin.[74] A few days later he wrote to the publisher Henry Holt, to whom he had promised a comprehensive new textbook on geology, asking for an extension of time in order to work out the consequences of his new interpretation of the history of the Earth.[75] The extension was granted; it was not until 1904 that the first volume of *Geology* appeared, co-authored by Rollin D. Salisbury, head of the Geography Department at Chicago.[76]

Astronomy might also be affected by taking seriously the hypothesis that a nebula is composed of cold solid particles. Previously it had been claimed that the light emitted by nebulae is conclusive evidence that they are composed of hot gases, and this result was also considered favourable to the nebular hypothesis.[77] But, Chamberlin asked E. E. Barnard at the Yerkes Observatory, might it not be shown spectroscopically that the luminosity of nebulae is actually due to "an electrical or phosphorescent condition" rather than to high temperature?[78]

In May 1899, *Science* reprinted Lord Kelvin's address on the age of the Earth, delivered in London two years earlier.[79] Chamberlin quickly prepared a sharp critique, which appeared in the issue of 30 June.[80] This was quite a memorable event for his biographers: finally a geologist dared to attack the time limits imposed by calculations of heat conduction in the Earth, striking at the foundations of Kelvin's argument rather than haggling about the numerical details. According to Carroll and Mildred Fenton, Chamberlin "tore the restrictions to shreds while the more conservative Britishers gasped at New World temerity".[81] His son Rollin Chamberlin recalled in 1934: "Not a few British scientists expressed surprise that anybody should have the temerity even to reply to Lord Kelvin, let alone to attempt to unhorse him in his own field."[82]

Chamberlin attacked Kelvin's "very sure assumption that the material of our present solid Earth all-round its surface was at one time a white-hot liquid". Though widely accepted, this assumption does not rest on "any conclusive geological evidence". He noted that Kelvin himself had at times favoured the idea that the Earth was built up by the infall of meteorites; the main difference between them seems to be whether the process was so rapid that the material vaporized by heat of impact, or whether the heat was generated more slowly.[83]

In addition to his argument that a hot fluid Earth could not have retained water and atmospheric gases, Chamberlin also seems to have had some more philosophical reasons for his belief in a slow process of accretion. As Winnik has noted,[84] Chamberlin thought of natural processes as resulting from purposeful design, allowing progressive opportunities for man to participate in shaping his future. With this attitude he could not accept the pessimistic cosmology leading to the 'heat death', which Kelvin and others had inferred from the nebular hypothesis and the second law of thermodynamics.[85] In his 1899 critique of Kelvin, Chamberlin gave a remarkable comparison of the two hypotheses for the Earth's history, his own and the accepted one of a cooling contracting ball:

> The slow-built meteoric Earth delayed the exercise of thermal agencies until the life era and gradually brought them into play when they were serviceable in the prolongation of the life history, whereas the liquid Earth exhausted these possibilities at a time of excessive conversion of energy into heat and thus squandered its energies when they were not only of no service to the life history of the Earth, but delayed its inauguration until their excesses were spent. . . .[86]

Implicit in Chamberlin's theory was a radically new philosophy of natural science. The nineteenth-century worldview was evolutionary in a very limited sense, especially when applied to the history of the Earth: a simple cooling, solidification and contraction, giving little scope for fluctuations in temperature (as needed to account for glacial epochs) or for the hundreds of millions of years of temperate conditions postulated by uniformitarian geologists and evolutionary biologists.[87] Now at last with Chamberlin's theory, a more complex type of evolutionary development of the Earth was being introduced, compatible with modern theories of the development of living organisms on the Earth's surface. The evolutionary process could yield complexity and novelty rather than degeneration and death as nineteenth-century scientists had supposed. With Chamberlin's view of geological processes involving rearrangement of the solid materials of the Earth's interior, lubricated by temporary melting and driven by the heat of gravitational contraction, one might have expected a more favourable climate for hypotheses such as continental drift. But in fact Chamberlin did not establish a new world-view, and did not support Wegener's continental drift hypothesis in spite of his remarks in the 1896 lecture (above) about differential motion of the crust. What he did was important enough without having to be exaggerated: he liberated astronomers and geologists from the restrictions of the nebular hypothesis and the assumption of a uniformly cooling, solidifying Earth.

Early in 1900 the Scottish geologist James Geikie reviewed the geological objections to Kelvin's theory of the cooling of the Earth from a molten state.[88] He quoted at length from Chamberlin's "very interesting essay" on Kelvin's theory, concluding that Chamberlin's concept of a solid Earth built by slow accretion of meteors, with heat generated over a long period of time by consolidation and rearrangement of materials, is just as plausible as Kelvin's on astronomical grounds and much more satisfactory from a geological viewpoint.

Chamberlin wrote immediately to Geikie, thanking him for his support, and telling him that in a forthcoming paper on the nebular hypothesis "I have been rash enough to try to carry the war into Africa, if I may speak so slightingly of our mathematical and physical friends. I may come home on a stretcher, but at any rate it seems worth while to direct our friends' attention to the p's and q's of their own domain. In any case the war is likely to be quite as wholesome for being less purely on the defensive on our part."[89]

In order to "carry the war into Africa" and defeat the nebular hypothesis on the astronomical battlefield, Moulton's efforts were essential. But in the summer of 1899 Moulton had no assurance that he would be able to remain at Chicago to collaborate with Chamberlin after receiving his Ph.D. He expressed his financial anxieties in letters to George Ellery Hale at Yerkes Observatory. (Hale was also on Moulton's examining committee at Chicago.) Moulton complained that President (William Rainey) Harper of Chicago would not pay him enough to live on; he was now in debt, and intended to leave Chicago as soon as he could find a suitable position elsewhere.[90] Hale replied that Harper had told him he was pleased with Moulton's work, though he could not yet promise a promotion.[91] Shortly after that the situation seemed to improve.[92]

Moulton completed his Ph.D. requirements at the end of 1899; on 3 January 1900 he wrote to Hale outlining his results on the nebular hypothesis, with a view toward possible publication in Hale's *Astrophysical journal*.[93] One result had already been announced at the first meeting of the Astronomical and Astrophysical Society, held at Yerkes Observatory the previous September: "if we supposed that the condensation towards a planet had gone on to a considerable extent the chances are against its completing itself."[94]

Hale encouraged Moulton to send his critique of the nebular hypothesis for publication in the *Astrophysical journal*. In another letter Moulton stated that "I have been associated with Prof. Chamberlin in attacking the problem, and I have obtained many valuable ideas from him, but of course all the mathematical features are mine".[95] The paper was accepted for publication and appeared as the first article in the issue of March 1900.[96]

Most of Moulton's arguments against the nebular hypothesis had already been made by other writers, but his presentation was exceptionally forceful and comprehensive; this paper is probably his most important contribution to cosmogony. He gave some rather convincing new arguments, on the basis of the Lagrangian solution of the 3-body problem and Roche's theory of stability, that a Laplacian ring could not condense into a single planet. He claimed that the angular momentum objection is so simple and certain that "it seems to render the nebular hypothesis in the simple form in which it has usually been accepted absolutely untenable unless some fundamental postulates now generally accepted are radically erroneous. It seems a necessary inference from the results of the discussion that the solar nebula was heterogeneous to a degree not heretofore considered as being probable, and that it may have been in a state more like that exhibited in the remarkable photographs of spiral nebulae recently made by Professor Keeler."[97]

In an account published 35 years later, Moulton seemed to have forgotten these first tentative steps toward the new theory; he wrote that in 1900 Chamberlin and Moulton "occupied the almost unique position in science of abandoning a theory because of its own weaknesses rather than because of the superior merits of a rival hypothesis". Implying a complete absence of any preconceptions about an alternative theory at that time, Moulton said they felt like Adam and Eve on leaving the Garden of Eden—"The world was all before them".[98]

The spiral nebulae were clearly regarded as the key to an alternative theory. On 30 January 1900, Chamberlin wrote to James Keeler that he and Moulton were considering alternative theories of the origin of the Earth, including "an origin from a spiral nebula". For this purpose he asked Keeler to send him copies of the photographs of spiral nebulae currently being made at the Lick Observatory.[99] The request was granted; a month later Chamberlin wrote to express his thanks and to promise Keeler a copy of his forthcoming paper. "I sincerely hope," Chamberlin told Keeler, "you may be able to come to the relief of geologists in the dilemma in which we seem to be finding ourselves as the result of the apparent breaking down of the accepted form of the nebular hypothesis."[100]

The paper just mentioned was "An Attempt to Test the Nebular Hypothesis by the Relations of Masses and Momenta" which Chamberlin published in the January–February (1900) issue of his own *Journal of geology*. The opening paragraph pointed out the link between geology and astronomy: recent work in glacial geology "had made it imperative to seriously consider inherited views relative to the nature of the Earth's early atmospheres, and this in turn forced an inquiry into the current postulate of a primitive, vast, gaseous envelope exceptionally rich in carbon dioxide; for the special heat-absorbing qualities of this constituent render it doubtful whether its presence in large amount is compatible with glaciation". But that postulate now seemed inconsistent with the Laplacian hypothesis that the Earth had been formed from a hot gaseous ring, in the light of calculations of molecular velocities based on the kinetic theory of gases.[101] As he had suggested earlier, carbon dioxide and other atmospheric gases including water vapour would largely have escaped from the primeval Earth, if it was so hot that its mineral components were in a liquid state.

Possibly having in mind current disputes about the applicability of the kinetic theory of gases to atmospheres,[102] Chamberlin pointed out that his conclusions did not depend on the precise validity of his computations of molecular velocities. The qualitative results of kinetic theory were in any case completely in agreement with the known absence of atmospheres from all satellites and asteroids, the thin atmosphere of Mars, the absence of the lightest gases (hydrogen and helium) from the Earth's atmosphere, and the "vast and deep atmospheres" of Jupiter and Saturn—"in short, there is a general correspondence between the mass of the atmosphere and the gravitative competency of the body".[103]

The major astronomical test of the nebular hypothesis was a comparison of calculated and observed distributions of angular momentum among the planets and the Sun. The ancestral nebula was assumed to be a sphere just extending to the orbit of Neptune, rotating with the present angular velocity of that planet. This assumption would lead to a *lower limit* for the angular momentum, since the equatorial bulge of a rotating mass would increase its angular momentum for a given speed of rotation, and since it was likely that the nebula actually extended some distance beyond the orbit of Neptune; hence, as will be seen, it was *favourable* to the hypothesis being tested. The distribution of density found by G. H. Darwin[104] was used; the details of the calculation are attributed to Moulton. The result was that *the ancestral nebula must have had an angular momentum at least 213 times that of the present solar system*, according to the nebular hypothesis.[105]

The discrepancies between hypothesis and astronomical observation became even more striking if the outer planets were omitted and the hypothesis was applied only to the formation of the inner planets and the Sun. For example, if the Sun, Mercury, Venus and Earth were formed from a nebula rotating with the present angular velocity of the Earth, that nebula must have had angular momentum 28 times as great as the Sun for those bodies, even making the most favourable assumption that the Sun is homogeneous in density rather than condensed toward the centre.

In the next section Chamberlin pointed out that the ratios of masses to momenta of bodies in the solar system are highly irregular; for example, Jupiter, having less than one-tenth of one per cent of the total mass of the nebula, carried away 95 per cent of its angular momentum. While this circumstance does not seem to be forbidden by the nebular hypothesis, Chamberlin implied that it was a fact that must weigh against the regular development usually associated with that hypothesis.

Chamberlin concluded by stating that the relationships of mass and momentum "are seemingly altogether incompatible with an evolution of the solar system from a gaseous spheroid controlled by the laws of hydrodynamic equilibrium and developing by secular cooling. The argument is equally cogent against an evolution from a meteoroidal spheroid controlled by the laws of convective equilibrium" (as in G. H. Darwin's theory).[106]

He then gave a hint of how he might develop his own theory. It was necessary to explain how "the peripheral portion of the system acquired all but a trivial part of the moment of momentum, while it possesses but a trivial part of the mass. The first suggestion of these conclusions was the possible formation of the system by the collision of a small nebula upon the outer portion of a large one. . . . Following a purely naturalistic and inductive method, it would seem that the spiral nebulae, whose abundance is attested by the recent notable success of Professor Keeler in photographing numerous small ones, offer the greatest inherent presumption of being the ancestral form".[107]

Shortly after the publication of Chamberlin's and Moulton's separate papers in 1900, James Keeler gave an extended discussion of his photographs of spiral nebulae. He concluded:

> The idea at once suggests itself that the solar system has been evolved from a spiral nebula, while the photographs show that the spiral nebula is not, as a rule, characterized by the simplicity attributed to the contracting mass in the nebular hypothesis. This is a question which has already been taken up by Professor Chamberlin and Mr. Moulton.[108]

A few months later Chamberlin and Moulton, in a joint article in *Science*, agreed with Keeler that the consideration of spiral nebulae should have "precedence in attempts to find analogies for the origin of our system . . . in the absence of any knowledge of the origin of spiral nebulae, it is possible to conjecture that they arose from peripheral collisions of antecedent nebulae".[109] This hypothesis was explored but eventually dropped; according to a memorandum written in 1906, published by Chamberlin and Moulton in 1909, the investigation of collisions between "nebulous bodies" as a mode of origin of spiral nebulae was "futile . . . no escape was found from the high probability, amounting almost to certainty, that the resulting orbits would be too eccentric to fit the case of the solar system in any instance that was likely to occur".[110]

In the meantime Chamberlin had become interested in solar eruptions, possibly as a result of studying photographs of the Sun taken during the eclipse of 28 May 1900.[111] He began to study the problem of "disruptive approach", in particular "the projective effect developed in a body of enormous elasticity already under high pressure and affected by violent local explosions which were subject to intensification by the changes of gravity brought to bear on them by a passing body . . . the explosive projections from suns under the influence of the passing body gave a reason for the two-armed feature of most spiral nebulae".[112] Just as he was completing an article on this subject, the idea of a catastrophic origin of spiral nebulae received unexpected support from an astronomical event.

4. COMPETING THEORIES 1901–1912

Just as the discovery of Ceres on 1 January 1801 ushered in a century in which knowledge of the smaller members of the solar system was greatly extended, the observation of Nova Persei on 22 February 1901 heralded a period of discovery for stellar astronomy. The *Astronomisches Jahresbericht* recorded 228 publications dealing with Nova Persei or novae in general in 1901, 97 in 1902, 27 in 1903, and 13 in 1904. (Nova Geminorum stimulated another 40 items in 1903.)

Many astronomers attributed novae to the collision of a star with other bodies;[113] the new observations indicated that the nova might subsequently develop into a nebula. Since the solar system was still thought by almost everyone to have evolved from some kind of nebula, the conclusion drawn by several writers was that the solar system originated in a stellar collision or encounter of some kind.[114]

Chamberlin had apparently written the following sentence in his paper "On a Possible Function of Disruptive Approach" just before learning about Nova Persei:

> That disruptions or explosions of some kind actually take place in the heavens, and that not uncommonly, seems to be implied by the sudden appearance of new stars, often with great brilliancy, followed by rapid decline to obscurity or extinction.[115]

A footnote to this sentence adds:

> A fact which has become very familiar and impressive, since this was written, by the appearance of *Nova Persei.*

As Chamberlin pointed out, the probability that two stars actually collide would seem to be much too small to account for the observed frequency of novae; five had been reported in the decade before 1901. But, he argued, collisions are not necessary to produce such an explosion; if the distance of closest approach in an encounter of two bodies is less than the Roche limit, tidal forces will tear apart at least one of them.[116] The fragments will form comets, or in some cases a spiral nebula.

Chamberlin considered several possible types of disruptive encounter between astronomical objects (including both gaseous stars and solid bodies) without explicitly proposing any one of them as a theory of the origin of the solar system.[117] He presented a more definitive suggestion in a talk to the geology and geography section of the American Association for the Advancement of Science at its meeting in Denver during the last week of August 1901. The talk, "Report on some Studies Relative to Primal Questions in Geology", was not published, but a brief and rather confusing summary of it by E. O. Hovey appeared a month later in the *Scientific American supplement*. The main point seemed to be that if one believes the Earth was formed from meteorites, it is necessary to explain the fact that some of them contain hydrocarbons and therefore can never have been subjected to the extremely high temperatures one would expect if they originated in the Sun. The solution was to postulate that a small body (comet or planet) passed near a large gaseous or nebular body and was disrupted by the encounter without being subjected to great heat. But the nebula itself would also explode, and its fragments would form a spiral shape.[118]

Spiral nebulae now seemed more than ever to be the necessary foundation for the new geology Chamberlin hoped to develop. On 30 September 1901, he wrote to W. W. Campbell, Director of the Lick Observatory, asking to see Keeler's latest photographs of nebulae as soon as possible.[119] (Keeler had died 12 August 1900; his work with the Crossley reflector was being completed by C. D. Perrine, and was not published in full until 1908.) Campbell's response was prompt and apparently so generous that Chamberlin felt compelled to explain in some detail his need for the photographs. Now that the nebular hypothesis had collapsed as a basis for geological reasoning, it was imperative to develop a satisfactory alternative hypothesis, especially since the geological interpretations had been so greatly hampered in the past few decades by deductions based on the Laplacian theory. "I am not a little like the poor heathen Goths of old, who, driven by the Huns and Vandals behind, could not well avoid invading the precincts of the sacred city, however reprehensible it was in itself."[120]

The guardians of the sacred city of astronomy did not seem particularly offended by Chamberlin's incursion. One of the earliest responses came from George Ellery Hale, who wrote in *Scientific monthly* for February 1902 that the nebular hypothesis "has encountered fresh attacks on the part of Chamberlin and Moulton, and it now seems doubtful whether it will be possible to overcome their criticisms, which are based on dynamical considerations. It may prove to be sufficient, however, to forsake the lenticular mass of vapor predicted by Laplace in favor of the spiral form which Keeler has shown to characterize so many nebulae."[121]

But the same evidence that led some scientists to give favourable consideration to the Chamberlin–Moulton theory also inspired them to think of their own hypotheses. Since I now want to survey some of the alternative theories proposed in the early twentieth century before returning to the development of the Chamberlin–Moulton theory, it is appropriate to mention here another piece of evidence that emerged in 1904 to help discredit the nebular hypothesis by destroying the regularity of satellite motions mentioned in Section 2.

Before 1904 it was believed that all satellites of a planet must move in the same direction that the planet rotates. Phoebe, the ninth satellite of Saturn, was discovered by W. H. Pickering in 1899, but he was not able to analyze its orbit until five years later. He found that Phoebe moves in the retrograde direction, unlike the eight satellites discovered earlier.[122]

A. C. D. Crommelin, who came to the same conclusion on the basis of a smaller number of observations of Phoebe, stated that the retrograde motion

> undoubtedly is unfavourable to the nebular hypothesis, and I anticipate that upholders of that hypothesis will say that the satellite is not an original member of the Saturnian family, but a later capture, which is, of course, possible, though the chances of such a capture are slender indeed.[123]

Retrograde satellites of Jupiter were discovered a few years later. While it could be argued that these discoveries, added to all the other objections, were fatal to the nebular hypothesis,[124] they did not have as much impact as one might have expected. Pickering himself thought the apparent irregularities of satellite rotations could be explained by a 'tidal inversion' mechanism that would change most but not all spins from retrograde to direct as the primary contracted, in agreement with the nebular hypothesis.[125]

It seems odd that Chamberlin and Moulton are usually credited with the first twentieth-century encounter theory, since two eminent scientists had suggested such a theory, though rather vaguely, before the Chamberlin–Moulton publications of 1905. Arrhenius in 1901, and Lowell in 1903, proposed that two stellar bodies could collide, forming a nebula from which a planetary system could develop.

Svante Arrhenius (1859–1927) was a Swedish physical chemist; he received the Nobel Prize in 1903 for his theory of electrolytic dissociation in solutions. His ideas on cosmogony were developed in a short article in the *Archives Néerlandaises* (1901) and at greater length in his *Lehrbuch der Kosmischen Physik* in 1903. His approach was somewhat similar to that of Chamberlin; he was interested in the role of atmospheric carbon dioxide in terrestrial climates and glacial epochs,[126] and cited Stoney's kinetic-theory calculations to show that the Earth could not have retained atmospheric gases if it had been formed as a hot gaseous ball. He mentioned the possibility that two stars could collide to form a nebula, and discussed briefly the formation of planets at condensation points in a spiral nebula. He suggested that the planets will be heated by their contraction, then cool by radiation.[127]

Arrhenius developed his theory in more detail in *Världarnas Utveckling* (1906), soon translated into German and then into English as *Worlds in the making* (1908). He emphasized the cosmic role of radiation pressure, predicted by Maxwell's electromagnetic theory (1873) and recently discovered experimentally by P. N. Lebedev in Russia (1899) and confirmed by E. F. Nichols and G. F. Hull in the United States (1901). According to Arrhenius, radiation pressure pushes dust particles away from stars. The material collects in nebulae, where it is reorganized in an entropy-reducing process similar to that effected by Maxwell's Demon. In this way the universe can be rejuvenated in cycles rather than succumbing to the Heat Death. Stars, which gradually cool off, will form crusts that keep their energy bottled up inside for billions of years until released by collisions, thus producing a new spiral nebula and a new planetary system.[128]

Percival Lowell (1855–1916) was probably the best-known planetary astronomer in America at the turn of the century; his speculations about life on Mars caught the public fancy, and his initials were commemorated in the name and symbol of the new planet whose discovery he persistently sought. (Pluto was finally spotted by Clyde Tombaugh in 1930 at the observatory Lowell had founded.) In 1903, in a book on *The solar system*, he asserted that the nebular hypothesis is erroneous because the different parts of a gaseous nebula would not all revolve at the same angular velocity; hence Laplace's explanation for the common direction of revolution and rotation of the planets fails. Lowell suggested that the angular-momentum distribution might be explained by assuming that the mass of the nebula is concentrated at its centre—"This would be the case if the present system had been formed by the collision of two bodies." The collision would scatter meteorites through a large space, and from their distribution Lowell suggested one might explain the retrograde motion of the outer planets.[129]

Lowell's biographers have stated that he supported the Chamberlin–Moulton theory, apparently misled by the usual statement that this theory was published in 1901.[130] In fact Lowell attacked the Chamberlin–Moulton theory in 1909, while at the same time elaborating his own somewhat similar theory; he rejected the Chamberlin–Moulton explanation of the production of direct rotation by particle impacts, relying instead on tidal forces to reverse the initially retrograde rotations.[131] Reviewers of his book missed the distinction and thought he was simply presenting the Chamberlin–Moulton theory.[132] Apparently Lowell never advanced any claim for his own priority.

By 1910 several other cosmogonies involving collisions of stars and/or nebulae had been proposed. Only one of them received much notice, and that was primarily because of the persistence of its author in publicizing it. Thomas Jefferson Jackson See (1866–1962) was an American astronomer who started his career with a dissertation on the theory of binary stars at the University of Berlin. This work, based on an extension of G. H. Darwin's tidal theory of rotating liquid masses, established See's reputation; he eventually obtained an appointment as professor of mathematics in the U.S. Navy, assigned to the observatory at Mare Island, California. (He failed to attain the more prestigious posts to which he aspired, and later became known as a crackpot for his attacks on Einstein and for his own strange theories.)

In 1897 See pointed out that the nebular hypothesis should be revised in the light of Darwin and Poincaré's (and his own) results; nebulae do not shed rings, but split into globular masses.[133] See was responsible for popularizing the phrase 'Babinet's criterion' in connection with the angular-momentum objection to the nebular hypothesis. I suspect he first learned about Babinet's paper from Agnes Clerke's book on cosmogony, which he reviewed in 1906.[134]

Previously sceptical of theories of the solar system based on analogies with spiral nebulae,[135] See proposed in 1909 that "our system was formed by the unsymmetrical meeting of two streams of nebulosity or by the mere gravitational settling of a single nebula of curved and unsymmetrical figure, but without hydrostatic pressure as imagined by Laplace". As an example he pointed to a photograph of H. V. 2 Virginis, "a spiral nebula just beginning to coil up and form a system . . . as the mass whirls and condenses under resistance, it will necessarily retain and draw down most of the nebulosity into the plane of motion".[136] Having previously claimed to have proved that the Moon was captured by the Earth rather than being spun off by it (as G. H. Darwin and others believed), See now argued that the planets were all formed independently in the nebula, then were captured by the Sun into nearly circular orbits with the help of a resisting medium. The planets were gradually built up to their present size over a period of up to a billion years by accretion of dust and meteoric matter.[137]

One other theory must be noted because, though generally ignored in the decade after it was proposed, it was later recognized as a precursor of Alfvén's cosmogony. Kristian Birkeland (1867–1917), a Norwegian geophysicist, studied cathode ray discharges from magnetized globes in connection with the aurora and other solar-terrestrial electromagnetic phenomena. In 1912 he proposed that the Sun emits charged particles into space; some of them cluster into orbits determined by the solar magnetic field and eventually form planets.[138]

5. THE PLANETESIMAL HYPOTHESIS

Chamberlin first introduced the term 'planetesimal' in a paper presented to the Washington meeting of the Geological Society of America on 1 January 1903. In the published abstract he distinguished his theory from "the Laplacian and other gaseous hypotheses, and from the meteoroidal hypothesis as set forth by Lockyer and Darwin". In those theories, "the aggregation is the simple work of gravity" whereas "in the planetesimal hypothesis the aggregation is dependent on orbital conjunction" and is relatively slow, involving much lower temperatures. 'Planetesimals' were defined simply as "infinitesimal planetoids" from which the planets grew by aggretion.[139]

As might be expected the discussion of Chamberlin's theory on this occasion centred on its geological aspects.[140] Harry Fielding Reid (later known for his 'elastic-rebound' theory of earthquakes, and a severe critic of the Chamberlin–Moulton theory 21 years later) asked friendly questions about possible evidence from rocks and from the study of other planets. George Perkins Merrill, an expert on meteorites as well as geology, objected that acid rocks in the Earth's crust did not have the same character as meteorites, to which Chamberlin replied that "the hypothesis was not meteoroidal, but nebular. That he considered meteoroidal material a negligible quantity."[141] Grove Karl Gilbert, another prominent geologist, "cited the results of his study of the Moon as showing the effects of the impact of masses falling upon it, and supporting in this way the hypothesis".[142]

By this time Chamberlin realized that he needed more help in developing his theory than Moulton could give, and was beginning to call on other Chicago scientists for assistance. He wrote to Robert A. Millikan on 19 December 1902, asking about a problem involving the cooling of the Earth, and received a quick reply citing Lord Kelvin's solution of the problem in 1862.[143] But the people he really needed to work on his research programme were for the most part occupied with their own projects; presumably most graduate students in the Geology Department did not have the required expertise in mathematics, physics and astronomy to solve the special problems involved in working out the planetesimal hypothesis.

Fortunately the means for organizing an interdisciplinary research team were at hand. The Carnegie Institution of Washington had recently been established to finance scientific research, and one of its trustees was Chamberlin's friend Charles D. Walcott, Director of the U.S. Geological Survey. On 23 January 1902, Chamberlin wrote to Walcott, alluding to an earlier conversation—"Since you were kind enough at Rochester to mention what you had in mind for me in connection with the Carnegie Institution"—and requesting a grant that would allow him to reduce his commitment to teaching and administrative duties, as well as to hire research associates.[144] Enclosed with the letter was a detailed memorandum, outlining a number of related problems in geophysics and astronomy.[145]

Walcott's response was favourable, though the negotiations proceeded rather slowly (detailed procedures for handling grant applications had not yet been worked out) and it was not until 2 January 1903, that Chamberlin submitted a formal application to the Carnegie Institution for a grant of $6000.[146] Walcott immediately approved the application, and Chamberlin informed him about how the money was to be spent in a letter dated 10 January 1903.[147] Moulton was to work on the project half-time at the rate of $1000 per year; L. M. Hoskins of Stanford would "undertake certain computations relative to the competency of certain shells of the Earth to *accumulate stresses*";[148] C. S. Slichter of the University of Wisconsin would study the "changes of form of the Earth due to supposed changes in the rate of rotation and the geological consequences of this";[149] and A. C. Lunn of the Mathematics Department at Chicago planned to investigate the "development and distribution of the heat of the Earth's interior".[150] Hoskins, Slichter, Lunn and others recruited later were paid on a *per diem* basis. Chamberlin himself received \$3500, half of his annual salary, from the grant.[151]

The relationship between Chamberlin and the Carnegie Institution was mutually beneficial though not without some friction and misunderstanding. By 1910 Chamberlin had received a total of \$34,000, more than any other grantee; the next highest amount was \$33,000 to Simon Newcomb.[152] But Chamberlin frequently resented the need to make an annual plea for funds, feeling that once the importance of his work had been recognized he should be able to count on automatic renewal of his grant without more paperwork.[153] The Carnegie Institution gained the public credit for having supported one of the major American scientific achievements of the early twentieth century; the first detailed statement of the complete Chamberlin–Moulton theory was published in the Institution's *Yearbook* for 1904.[154] Chamberlin also served on the Geology Advisory Committee and gave the Institution the benefit of his long experience in conducting and administering scientific research; much of his advice is recorded in correspondence with Robert S. Woodward, President of the Carnegie Institution from 1904 to 1920.[155] However, Chamberlin was not able to prevent a major change in policy that eventually denied Carnegie funds to most academic scientists; Woodward apparently found that the aggravation of dealing with individual applicants for grants was too burdensome, and decided that the endowment should be used primarily to support a small number of major laboratories directly administered by or affiliated with the Institution.[156] The grants to Chamberlin and Moulton were continued after this policy change had been set, though on at least two occasions permission to hire relatives as assistants was denied,[157] and in another case the existence of a large unexpended balance from a previous grant was used to deny a new allotment, much to Chamberlin's dismay.[158]

At the same time that he was contracting for geophysical calculations to support his planetesimal theory, Chamberlin continued to pursue the astronomical side with Moulton. On 12 January 1903, he wrote:

> On mulling further over our problems, I am wondering whether the best step to take first may not be to prepare a preliminary discussion of *the possible methods of nebular aggregation and their limitations*. The purpose would be to clear up the ground and present some general fundamental statements worked out in mathematical form which would serve as an introduction and groundwork to the more special problems to be taken up afterwards. The proposition would include a more elaborate treatment of the limitations of gaseous and quasi-gaseous aggregation which appeared in your paper on the nebular hypothesis. . . . it would be an extension of celestial dynamics into a field so far, I think, much neglected.
>
> There would, I think, be two rather distinct classes of aggregations to be considered, first, those in which the constituents were moving in essentially parallel concentric orbits and were only influenced by the

differential attraction of the central body (and slightly of other bodies). Second, those in which there was kinetic action between the constituents within the aggregation....

The proposed discussion might well perhaps include a statement of the planetoidal or planetesimal method of aggregation, with certain general mathematical determinations relative to its applicability and limitations. I do not think there has ever appeared in print a full and explicit statement of this doctrine. I have never seen any statement of it in print, except those which I have myself made, and the fact that Fay[e] in his discussion of the origin of the Earth entirely ignores it and drew his conclusions without regard to it, and the fact that these conclusions seem to have been generally accepted, or at least to have escaped criticism among astronomers, leads me to think that the doctrine is essentially new....[159]

By the end of 1903 Chamberlin had consolidated his ideas about the initial disruptive encounter of the Sun with another star, and the shapes of the orbits into which the fragments torn from the Sun would be thrown. Moulton was in poor health during this time, spent the winter of 1903–4 in New Mexico, and thus was not able to contribute as much as might be expected to developing this part of the theory; but he reviewed all of Chamberlin's ideas and drafts by correspondence and thus helped prevent serious technical errors.[160]

In February 1904 Chamberlin was invited to present an account of his theory to the Washington Academy of Sciences.[161] Chamberlin later considered this to have been the "original announcement of the planetesimal hypothesis" and recalled that it was understood that his theory would be discussed "by Simon Newcomb, who was then regarded as the foremost mathematical astronomer in the country, by Grove Carl Gilbert, one of the most trusted geologists in this country, and by George E. Becker, who stood for a man using mathematics to a considerable extent".[162]

The talk at the Washington Academy was given on 29 March 1904. Chamberlin was somewhat disappointed that the three experts did not completely understand what he had done, even though he had sent them an advance copy of the exposition of his theory in his forthcoming geology text.[163] He reported to Moulton:

... While the presentation appeared to be satisfactory and to awaken a good deal of interest, the discussion did not amount to a great deal.

Dr. Newcomb had informed me beforehand that he could not undertake to seriously discuss the subject because the pressure of his duties would not permit him to give the previous consideration which was required. He was however present, and was good enough to say that he thought our studies were an advance, and that perhaps our views were the best now available. He discussed some minor points of no special moment.[164]

Becker had written out his criticisms, pertaining mainly to the change in angular momentum of the system; he also stated that the Chamberlin–Moulton theory had been anticipated by Buffon in the eighteenth century. Chamberlin thought he could dispose of the first objection without difficulty, though the details have not been preserved; and he protested that his own theory was fundamentally different from Buffon's.[165] Becker was an exponent of Kant's ideas,[166] and in his address to the Congress of Arts and Science at St Louis on 21 September 1904, he asserted that "every attempt to devise an essentially different hypothesis [from that of Kant and Laplace] has failed". Chamberlin's assumption that the accretion of the Earth was slow enough to avoid fusion was mentioned briefly but quickly dismissed.[167]

As for the third expert at the Washington meeting,

> Mr. Gilbert's discussion lay chiefly on geological lines, the leading point being that, as he conceived it, the hypothesis of a molten Earth permitted considerable heterogeneity in the distribution of density, while he thought that the accretion hypothesis would give rise to uniformity. Precisely the opposite seems to me to be the case. . . .
>
> On the whole, therefore, the discussion did not contribute much that was helpful to me. Of course the qualified endorsement of Professor Newcomb was gratifying. Many pleasant things were said privately.[168]

Chamberlin's theory now contained two new features. First, the 'ancestral Sun' was assumed to have had a family of planets, swept away by the encounter; second, the other body was several times as massive as the Sun. The connection between these two features is not made clear in published expositions of the theory, but is suggested in Chamberlin's letter to Moulton of 2 March 1904:

> The very small ratio of the mass of the planets to the mass of the Sun seems to make it evident either that the Sun did not go anywhere near the Roche limit of the disturbing body, or else that the disturbing body was small. As the postulated ancestral Sun may have had a family of planets, and as there are now no signs of these in the Sun's sphere of influence, it seems best to suppose that the disturbing body was massive enough to have stripped these away, while at the same time it caused the eruption of enough nebulous matter to form a new family.
>
> The preferable case, therefore, as I see it, is one which supposes that the ancestral Sun passed some distance outside of the Roche limit of a rather large body, say some few times as massive as the Sun, and that protuberances to the amount of one, two or three percent were shot forth on the opposite sides of the Sun in the line of differential attraction, with velocities for the outer parts or ends of the arms sufficient to carry them beyond the orbit of Neptune.[169]

In the report in the Carnegie *Yearbook* for 1904, the two assumptions are explained separately: (1) "The inference that a spiral nebula is formed by a combined outward and rotatory movement implies a preexisting body that embraced the whole mass. In harmony with this, an ancestral solar system has been postulated." (2) The other body is assumed to be "several times the mass of the Sun, since it is regarded as a small star".[170]

These two features were in turn connected to a third assumption, explained in a letter to Moulton on 16 May 1904. Instead of assuming, as he had in his 1901 paper on disruptive approach, that the tidal force due to the other body was the *cause* of matter being ejected from the Sun, Chamberlin now emphasized that the ejection of matter was a natural consequence of the Sun's normal activity; the only function of the other body was to neutralize the restraining effects of the Sun's gravity at two particular locations on its surface (toward and away from the other body), thereby allowing intensified eruptions at those places:

> ... I am dealing with protuberances which are not primarily dependent upon the disturbance of another body, so far as we know, for example, the protuberances which are constantly taking place in the Sun. These are supposed to be true explosions in the Sun, dependent upon conditions not understood. These, in the case of close approach, are supposed to be greatly augmented, but still to remain essentially the same in nature. It is merely their increased magnitude and localization which is due to the change in pressure, not their fundamental production. I was led to this special view by considering the magnitude of the protusions that seem to be required in the case of the solar nebula. As the planets are now only 1/700 of the whole mass, and as I could not see good reasons for supposing that many times as much of the nebular matter went back to the Sun as went into the make-up of the planets, it seemed that the best working hypothesis was one in which only a very small percentage of the Sun was shot out into the nebulous form. Now this could hardly be the case with general tidal deformation such as I discussed in the *Astrophysical Journal*. That would be applicable to those nebulae in which a large part of the whole mass was dispersed, and a comparatively small portion remained in the central nucleus, as is conspicuously true in some cases, judging from the photographs.
>
> ... it is obvious that the solar case is one of *distant* approach. The Sun could not have gone anywhere near the Roche limit of the disturbing body; otherwise, a very much larger proportion of the Sun's mass would have been shot out into the nebular form. The solar case must have been one in which the Sun's normal protuberances were merely intensified and localized.[171]

A further consequence of the smallness of the disruption was that the matter ejected first—and presumably shot farthest from the Sun—would originate in the surface layers of the Sun, while the later ejecta would come from the lower portions. If one assumes that elements of higher specific gravity have sunk toward the centre of the Sun, this would offer a possible explanation for the fact that the outer planets have lower densities than the inner ones.[172]

But the real difficulty still remained: what would happen to the ejected matter after the perturbing body had left the scene? Would all or most of it simply fall back into the Sun, or would it go into orbits around the Sun? It seemed clear that the action of the perturbing body on the ejected matter would impart some kind of rotation, simply because the perturbing body would be moving past the Sun and thus its gravitational force at a later time would have a component transverse to the line of the original tidal action. Moulton explained how the orbit could be computed by stepwise approximation,[173] but admitted that in problems of this kind "the results are often the opposite of what one would naturally expect".[174]

At the end of September 1904 Chamberlin submitted to the Carnegie Institution of Washington his first comprehensive report on the complete Chamberlin–Moulton theory.[175] The outline of events envisaged in the theory was now as follows: (1) the ancestral Sun is approached by another star (Moulton later calculated that it needs to come only as close as the orbit of Jupiter); (2) the tidal force of the intruder neutralizes the gravitational force of the Sun at two opposite points on its surface, allowing internal expansive forces to push out material in two protuberances; (3) as the intruder star swings around the Sun and goes off into space it draws these protuberances out into two spiral arms; (4) the arms contain 'knots' due to irregular pulsations in the process of emission from the Sun; these form nuclei for condensation of material into planets and satellites; (5) rapid cooling of the ejected matter by radiation from its surface reduces its refractory components to the liquid or solid state (planetesimals); (6) knots and planetesimals move in Kepler orbits around the Sun; (7) planetesimals are gradually captured by the knots; (8) growth of Earth and other planets proceeds as described earlier, with slow generation of heat by gravitational contraction.

Robert S. Woodward, who was about to succeed D. C. Gilman as President of the Carnegie Institution of Washington, expressed the first reaction to the theory in a letter to Charles D. Walcott on 2 November 1904, after reading the proofs of Chamberlin's report for the Carnegie *Yearbook*:

> Naturally such work must be, in its inception, somewhat speculative, and hence lacking in the definiteness to be expected in a completed investigation. However, the work foreshadowed in the report is not only very interesting but also very suggestive and far-reaching in its scope. It must rank, I think, with the work of Laplace, Roche, Croll, Kelvin, and G. H. Darwin in the domain of cosmogony.[176]

Woodward also praised the work of Chamberlin's associates Moulton and Lunn: "They come nearest to being successors to our distinguished George W. Hill of any men in America at the present time."[177]

Chamberlin's report was published early in 1905 in the *Carnegie Institution yearbook for 1904*.[178] It was a handsome dividend for the foundation that was investing heavily in his research. This form of publication called attention to the Carnegie sponsorship; curiously (from the modern viewpoint) neither Chamberlin nor Moulton acknowledged the source of financial support for their work in publications elsewhere.

Continuing the policy of separate publication, Moulton sent a paper under his own name to the *Astrophysical journal* on 30 August 1905; it appeared in the October issue.[179] He presented a brief discussion of the origin of spiral nebulae in encounters of two stars, not mentioning Chamberlin's assumption that the other star has a mass several times that of the Sun (see above) but promising numerical treatment of various special cases in a future publication.[180] Scheiner's observation of dark lines in the spectrum of Andromeda[181] was cited in support of the assumed constitution of nebulae, but some uncertainty about the alleged relation between spiral nebulae and planetary systems had begun to creep in:

> Doubtless those spirals which have been photographed are immensely larger than the one from which our system may have developed, and as a rule have relatively less massive centers.[182]

Most of Moulton's paper dealt with the orbits and rotations of the planets

TABLE 2. Knowledge of masses and eccentricity of planets.

	Mass		Eccentricity	
	1900*	1977	1905†	1977
Mercury	0·065	0·055	0·2	0·206
Venus	0·788	0·815	0·006	0·007
Earth	1·000	1·000	0·017	0·017
Mars	0·102	0·108	0·093	0·093
Jupiter	301·1	317·9	0·048	0·048
Saturn	90·1	95·2	0·056	0·056
Uranus	14·3	14·6	0·046	0·047
Neptune	16·0	17·2	0·009	0·009
Pluto		0·1 ?		0·25

* Chamberlin, *Journal of geology*, viii (1900), 61.
† Chamberlin and Salisbury, *Geology*, ii (1907), 79.

formed from the ejected matter. He argued, on the basis of approximate calculations, that the absorption of small particles by planetary nuclei would in most cases decrease the eccentricities of their orbits. The larger planets would thus be expected to have more nearly circular orbits. The nebular hypothesis, according to Moulton, predicts that the planets nearest the Sun would have the most nearly circular orbits because "friction would have destroyed most irregularities".[183]

The eccentricity test proposed by Moulton does not seem to me to be very conclusive. He says that "The orbits of the terrestrial planets average more than twice as eccentric as those of the great planets".[184] Using the data in Table 2, I find the average to be 0·079 for the terrestrial planets, compared to 0·04 for the great planets. Whereas Moulton points to Mercury, the smallest planet, as having the greatest eccentricity, and points out that some of the asteroids have even more eccentric orbits than Mercury, he does not mention the anomalously small (on his theory) eccentricity of Venus, or the anomalously large eccentricity of Jupiter and Saturn. On the other hand, the predicted correlation between eccentricity and distance from the Sun which he attributed to the nebular hypothesis was not generally considered a consequence of that hypothesis by its supporters, though Moulton had suggested this in 1900.[185]

Moulton repeated Chamberlin's argument that inelastic collisions of particles moving in Kepler orbits would tend in most cases to give a forward rotation to the resulting planets; moreover, one would expect to find the larger planets rotating more rapidly because there have been more such collisions. He did not indicate whether this expectation was fulfilled, perhaps because of the uncertainty of existing data; modern data indicates that there is a rough correlation of this type, if one ignores the *direction* of rotation.[186]

In the Chamberlin–Moulton theory, satellites were formed by accretion of planetesimals by secondary nuclei that could have revolved in any way around the primary nucleus that formed the planet. However, the orbits of retrograde secondary nuclei would tend to become *more* eccentric and would thus tend to fall into the primary nucleus unless they moved at a considerable distance from it. Any surviving retrograde satellites would therefore be expected to be on the outside of the system, and to have highly eccentric orbits; this was indeed true of Phoebe (Saturn IX) and the recently-discovered seventh satellite of Jupiter. On the other hand, the behaviour of Phobos, which was an anomaly for the nebular hypothesis, presented no difficulty: "There is no reason why a satellite may not revolve more rapidly than its primary rotates, especially as its period continually diminishes as the mass of the planet increases."[187]

Moulton devoted the last part of his paper to an attack on W. H. Pickering's attempt to explain the retrograde rotation of Phoebe in a manner consistent with the nebular hypothesis.[188] But he misunderstood Pickering's theory, thinking it to depend on a tidal slowing and inversion mechanism like the one suggested originally by Kirkwood (see above), whereas what Pickering had in mind was a simple tipping over of the axis of rotation. Pickering's theory survived Moulton's attack, at least for a few years, and thus held out some hope of rescuing the nebular hypothesis.[189]

Moulton concluded his 1905 paper by suggesting some philosophical consequences of what he called the "spiral theory". Previously it was thought that cosmic processes tended to aggregate matter into larger bodies while dispersing energy; but we now recognize tendencies toward dispersion of matter. This suggests that the evolution of celestial bodies may have a cyclic character, though this would require the discovery of new types of energy sources, possibly involving atomic processes, to supplement the energy available to the stars from Helmholtzian contraction.[190]

(Part 2 of this article begins on p. 77 of our June issue.)

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29. According to a respected authority, Whewell in 1853 and Spencer in 1858 showed that "the conception of the nebulae as remote galaxies which Lord Rosse's resolution of many into stellar points had appeared to support" was untenable; nebulae and stars are found in the same regions, hence must be part of the same system (Agnes M. Clerke, *A popular history of astronomy during the nineteenth century* (4th ed., London, 1902), 422).
30. Huggins himself did not at first admit the relevance of his discovery to the evolution of the solar system because, he later confessed, he was "under the undue influence of theological opinions". See his "Celestial spectroscopy", presented to the British Association, 1891, reprinted in *Essays in astronomy by Ball, etc.* (New York, 1900), and "The New Astronomy: A Personal Retrospect", *Nineteenth century*, xli (1897), 907–29.
31. Isaac Roberts, "Photographs of the Nebula M31, h 44, and h 51 Andromeda, and M27 Vulpeculae", *Monthly notices of the Royal Astronomical Society*, xlix (1888), 65.
32. James E. Keeler, "The Crossley Reflector of the Lick Observatory", *Astrophysical journal*, xi (1900), 325–49. H. C. Wilson, commenting on the appearance of the Andromeda nebula, wrote that "A better confirmation of the Nebular Hypothesis of world formation can hardly be found" but he meant the formation of stars, not planets. See "The Great Nebula in Andromeda", *Popular astronomy*, vii (1899), 507–10.
33. Saubert, "Moultons neue Idee über Planetenentstehung", *Die Natur*, xlix (1900), 250; Adolf Hnatek, "Die Entstehung des Planetensystems", *Naturwissenschaftliche Wochenschrift*, xv (1900), 553–7; I. W. Heysinger, *Solar energy* (2nd ed., Philadelphia, 1901), 285–95; Friedrich Nölke, *Das Problem der Entwicklung unseres Planetensystems* (Berlin, 1908), 138ff.
34. David Brewster, *More worlds than one: The creed of the philosopher and the hope of the Christian* (London, New York, 1854), 45–46; *Memoirs of the life and discoveries of Sir Isaac Newton* (London, Edinburgh, 1855; New York, 1965), ii, 131. Louis Agassiz, introduction to Hugh Miller, *The foot-prints of the Creator* (Boston, 1851), p. xxv. Benjamin Peirce, "On the Constitution of Saturn's Ring", *American journal of science*, (2) xii (1851), 106–8 (see last paragraph).

35. Daniel Kirkwood, "On the Nebular Hypothesis", *American journal of science*, (2) xxx (1860), 161–81; William Whewell, *Astronomy and general physics considered with reference to natural theology*, Book II (London, 1833), ch. 7. One clergyman attacked the nebular hypothesis but also tried to show that even if it were accepted it would still illustrate the agency of divine power, through "mediate" rather than "immediate" creation. James Buchanan, *Modern atheism* (Boston, 1857), 56–60. See also Numbers, *op. cit.* (ref. 6).

36. Alexander Hutt and G. F. Chambers, discussion remarks, *Journal of the British Astronomical Association*, xiv (1904), 150–5. For a theological defence of the nebular hypothesis see George V. Leahy, *Astronomical essays* (Boston, 1910), 234, 253, 259.

37. Alfred Russel Wallace, "Man's Place in the Universe: As Indicated by the New Astronomy", *Fortnightly review*, (n.s.) lxxiii (1903), 395–411; *Man's place in the universe* (New York, 1903).

38. E. W. Brayley, "Physical Constitution and Functions of the Sun, and Sources of its Heat and Energies, as Recently Investigated", *Companion to the British almanac for 1866*, 75–113 (see p. 95); Richard A. Proctor, *Other worlds than ours* (London, 1870), 201–9; Joseph Prestwich, "On the Past and Future Work of Geology", *Nature*, xi (1875), 290–2 (report of Lockyer's theory). A. R. Wallace (*My life* (London, 1905), i, 427) mentioned an anonymous article, "The Birth of the Solar System. A New Theory", *Atlantic monthly*, xxiii (1869), 221–8.

39. G. H. Darwin, "On the Mechanical Condition of a Swarm of Meteorites, and on Theories of Cosmogony", *Proceedings of the Royal Society of London*, xlv (1888), 3–16; *Philosophical transactions*, clxxx, 1–69.

40. A. W. Bickerton, "Partial Impact (Paper No. 2): A Possible Explanation of the Origin of the Solar System, Comets and Other Phenomena of the Universe", *Transactions of the New Zealand Institute*, xi (1878), 125–32; "On the Genesis of Worlds and Systems", *ibid.*, xii (1880), 187–97; "On the Causes Tending to Alter the Eccentricity of Planetary Orbits", *ibid.*, xiii (1880), 149–54; "The Origin of the Solar System", *ibid.*, xiii (1880), 154–9; "Synoptic Statement of the Principles and Phenomena of Cosmic Impact: Prepared for the Criticism of Scientific Men and Societies", *ibid.*, xxvii (1895), 545–58; *The romance of the heavens* (London, 1901). Extracts from two of the 1880 papers are included in *A source book in geology*, ed. Kirtley F. Mather and Shirley L. Mason (New York, 1964), 574–7.

41. In 1910 Bickerton succeeded in raising funds for a trip to England, where he obtained a respectful hearing for his views. See: "A New Astronomy", *Popular astronomy*, xix (1911), 594; "Report of the Meeting of the Association, held on Wednesday, December 28, 1910, at Sion College, Victoria Embankment, E. C.", *Journal of the British Astronomical Association*, xxi (1911), 139–47; "Physics. Fresh Support for Professor Bickerton's Theory", *Knowledge*, n.s., viii (1911), 324; Sylvester N. E. O'Halloran, "The Second Law of Thermodynamics and Professor Bickerton's Theory", *Knowledge*, n.s., ix (1912), 361; J. McCarthy, "The New Astronomy", *Knowledge*, n.s., vii (1911), 426; Charles W. Raffety, "The Spectroscopic Aspect of the Impact Theory", *Knowledge*, n.s., ix (1912), 106; W. H. S. Monck, "The Collisions of Stars", *Observatory*, xxxiv (1911), 202–3.

H. N. Russell thought that some of Bickerton's ideas were sound but they suffered from overdiscussion; Bickerton tried to explain too much with his theory (Russell to L. Spitzer, Jr, 18 May 1939, copy in Box 35, Russell papers at Princeton).

42. For the bibliographic history of this article see *Science*, cxlvii (1965), 757. The most comprehensive source of information on Chamberlin is Susan F. Schultz, *Thomas C. Chamberlin, An intellectual biography of a geologist and educator* (Ph.D. Dissertation, University of Wisconsin–Madison, 1976). See also the article by his son, Rollin T. Chamberlin, "Thomas Chrowder Chamberlin", *Biographical memoirs of the National Academy of Science*, xv (1934), 307–407; H. C. Winnik, "Science and Morality in Thomas C. Chamberlin", *Journal of the history of ideas*, xxxi (1970), 441–56; Kirtley F. Mather, "Chamberlin, Thomas Chrowder", *Dictionary of scientific biography*, iii (1971), 189–91; articles by C. K. Leith, William C. Alden, R. A. F. Penrose, Jr, Charles Schuchert, William D. MacMillan, Bailey Willis, and F. R. Moulton, in *Journal of geology*, iv (May 1929); Bailey Willis, "Memorial of Thomas Chrowder Chamberlin (1843–1928)", *Bulletin of the Geological Society of America*, xl (1929), 23–44.

Chamberlin's papers, including draft manuscripts, letters to and from him, and autobiographical notes, are preserved and easily accessible at the University of Chicago (Department of Special Collections, Joseph Regenstein library). As stated above (ref. 1), this collection will be cited as C-UC. There are also some letters (primarily dealing

with administrative matters) at the University Archives of the University of Wisconsin, Madison; correspondence concerning research grants may be found at the Carnegie Institution of Washington (cited as CIW). Undoubtedly there are originals of letters from Chamberlin in the papers of many geologists and astronomers throughout the world; copies of most of these letters can be found in the Letterbooks in the Chamberlin collection at Chicago.

43. See anonymous article (signed "Fidus Achates") on "The Washington Moultons, Forest Ray, '94, and Harold Glenn, 1907", in the magazine of the Albion College Alumni Association, *Io triumphe*, (2) xii (March 1947), 19–22; Henry S. Tropp, "Moulton, Forest Ray", *Dictionary of scientific biography*, ix (New York, 1971), 552–3. Both the *Io triumphe* article and Tropp date Moulton's collaboration with Chamberlin from 1898, but the documents cited below indicate that it had begun at least as early as summer 1897. *Io triumphe* also confuses T. C. Chamberlin with his son Rollin.

One of Moulton's first publications advocated the presentation of the nebular hypothesis in elementary astronomy courses (cited in ref. 12).

44. T. C. Chamberlin, "Present Standing of the Several Hypotheses of the Cause of the Glacial Period", *Scientific American supplement*, xxxii (1891), 13075–6.

45. S. G. Brush, *The Kind of Motion We Call Heat* (Amsterdam, 1976), §5·4. J. G. O'Hara, "George Johnstone Stoney, F.R.S., and the Concept of the Electron", *Notes and records of the Royal Society*, xxix (1975), 265–76.

46. I am unable to locate a publication with this exact title; it is cited by Chamberlin in his 1897 paper (ref. 48, below), p. 658, and by Moulton in his 1900 paper (ref. 97, below), p. 111. Chamberlin's letter to G. J. Stoney, 6 September 1897, requesting a copy of this paper, indicates some bibliographic confusion (Letterbook IX, p. 428, C-UC).

47. G. J. Stoney, "Of Atmospheres upon Planets and Satellites", *Transactions of the Scientific Society of Dublin*, vi (1897), 305–28; *Astrophysical journal*, vii (1898), 25–55. Chamberlin wrote to Stoney on 10 December 1897, thanking him for a copy of the paper and stating that he had seen an advance copy sent by G. E. Hale (Letterbook IX, p. 624, C-UC). In a letter to C. R. Van Hise, 30 November 1897, he says he saw this paper "yesterday" (Letterbook IX, p. 592, C-UC).

48. T. C. Chamberlin, "A Group of Hypotheses Bearing on Climatic Change", *Journal of geology*, v (1897), 653–83, p. 656.

49. *Ibid.*, 656–7.

50. *Ibid.*, 657.

51. *Ibid.*, 660. This assumes a period of rotation of 23 hours 56·067 minutes. Another table gave the escape velocities for a rotation period of 1 hour 24 minutes (see text below).

52. *Ibid.*, 661. Letters from Chamberlin to Moulton, 10 September 1897, and to Whitney, 9 October 1897, indicate that there was some disagreement between Whitney and Moulton on the calculation of centrifugal force effects (Letterbook IX, pp. 434 and 476, C-UC).

53. Chamberlin's source of information about kinetic theory was a recent book by Allan Douglas Risteen, *Molecules and the molecular theory of matter* (Boston, 1895).

54. G. H. Darwin, "On the Precession of a Viscous Spheroid, and on the Remote History of the Earth", *Philosophical transactions*, clxx (1879), 447–538.

55. Chamberlin, "A Group of Hypotheses" (*op. cit.*, ref. 48), 666.

56. For further remarks about the bearing of the kinetic theory of gases on the origin of the Earth, see: Letter from T. C. Chamberlin to S. R. Cook, 14 January 1899, in Letterbook XI, p. 181, C-UC; T. C. Chamberlin, *The origin of the Earth* (Chicago, 1916), ch. 1.

57. T. C. Chamberlin to G. C. Comstock, 11 December 1895 (Letterbook VII, pp. 165–167, C-UC). The "recent English work" may be J. Ellard Gore, *The visible universe* (London, 1893), which includes an extensive discussion of Faye's work.

58. *Ibid.*

59. G. C. Comstock to T. C. Chamberlin, 13 December 1895 (Box I, folder 15, C-UC). For an account of Faye's theory and an explanation of how direct rotation might be produced he referred Chamberlin to C. A. Young, *A text-book of general astronomy for colleges and scientific schools* (Boston, 1895), 518–19.

60. Chamberlin, "A Group of Hypotheses . . . ", 668–9.

61. G. H. Darwin, "A Theory of the Evolution of the Solar System", *Internationale Wochenschrift*, (30) iii (24 July 1909), 921–32 (p. 930). This article was used as a chapter in the 3rd ed. of Darwin's book *The tides* (1911).

62. R. T. Guili, "On the Rotation of the Earth Produced by Gravitational Accretion of Particles", *Icarus*, viii (1968), 301–23. A. W. Harris, "An Analytic Theory of Planetary Rotation Rates", *Icarus*, xxxi (1977), 168–74. Dr Harris kindly sent me the following comments on the Chamberlin-Moulton theory (letter of 15 July 1977): "It seems to me that they correctly point out that the retrograde rotation paradox is resolved by the effect of eccentricity: the simple cases with both planet and planetesimals in circular orbits as shown in their Figures 27 and 28 do not apply because there is no means for planetesimals to reach the planet embryo from significantly far away from the planet orbit. In their solution, they propose eccentric orbits for the planet embryos, while retaining circular orbits for the planetesimals. More recent works (*e.g.* V. S. Safronov, *Evolution of the Protoplanetary Cloud*, trans. 1972) favor the reverse situation: the planetesimals travel in more eccentric orbits than the planets. However, exactly the same geometry applies, and their Figure 30 is an excellent illustration of the effect which I attempt to illustrate in my Figure 1 and develop into an analytical theory in my paper. My result is as they suggest, that 'the resulting rotation is likely to be relatively low, though the total force of impact be great'. In fact, I find that this mechanism produces rotation rates that are about an order of magnitude less than observed rotation rates, if one assumes eccentricities of planetesimals sufficient to deliver them to the planet orbits from distances midway in between. Satisfactory results can be obtained from this mechanism by assuming smaller (but non-zero) eccentricities, as was done by Giuli in his numerical theory."

See also: A. V. Artemev and V. V. Radzievsky, "The Origin of the Axial Rotation of the Planets", *Soviet astronomy AJ*, ix (1965), 96–99 (translated from *Astronomicheskii Zhurnal*, xlii (1965), 124–8); A. V. Artemev, "Planetary Rotation Induced by Elliptically Orbiting Particles", *Solar system research*, iii (1969), 15–21 (translated from *Astronomicheskii Vestnik*, iii (1969), 18–25).

63. Maxwell, "On the Stability of the Motion of Saturn's Rings", essay awarded the Adams Prize at Cambridge University, 1 June 1857; revised version published in 1858, reprinted in *The scientific papers of James Clerk Maxwell* (Cambridge, 1890), i, 288–376. See S. G. Brush, C. W. F. Everitt and E. W. Garber, *Maxwell on kinetic theory and statistical mechanics* (Boston, 1978), §1–10 and ch. x.

64. J. E. Keeler, "A Spectroscopic Proof of the Meteoric Constitution of Saturn's Rings", *Astrophysical journal*, i (1895), 416–27. This result was later confirmed by H. Deslandres, "Recherches Spectrales sur les Anneaux de Saturne", *Comptes rendus, Académie des Sciences, Paris*, cxx (1895), 1155–8.

65. E. Roche, "Mémoire sur la Figure d'une Masse Fluide, Soumise à l'Attraction d'un Point Éloigné", *Mémoires de la Section des Sciences, Académie des Sciences et Lettres de Montpellier*, i (1849), 243–62; i (1850), 333–48; ii (1851), 21–32; the application to Saturn's rings is on p. 258. On 6 March 1903, Chamberlin wrote to Comstock asking for the loan of Roche's work, which was apparently not available at Chicago (Letterbook XVI, p. 680; original at University of Wisconsin Archives, Astronomy Department General Correspondence series no. 7/4/2, box no. 7). See also Chamberlin's letter to the Librarian of Congress in 1903, Letterbook XVI, p. 681, C-UC.

66. William Thomson, "Review of Evidence Regarding the Physical Condition of the Earth", British Association for the Advancement of Science, Address to Section A; *Nature*, xiv (1876), 426–31, reprinted in *Popular lectures and addresses*, ii (London, 1894), 238–72. William Thomson and P. G. Tait, *Treatise on natural philosophy* (1912 edition, reprinted as *Principles of mechanics and dynamics* (New York, 1962)), i, part 2, 427–40.

67. T. C. Chamberlin, "Lecture, January 27, 1896" (typescript in Box 3, folder 17, C-UC).

68. *Ibid.*, 9–13.

69. See refs 4 and 25.

70. Chamberlin, "A Group of Hypotheses", 668–70. There is a letter to Comstock, dated 30 November 1897, thanking him for criticisms and suggestions on this point in the paper (University of Wisconsin Archives, Department of Astronomy, General Correspondence, Series No. 7/4/2, box no. 5). Earlier Chamberlin had asked Moulton to calculate the heat that would be generated in the Moon by its self-compression (letter dated 30 July 1897, in Letterbook IX, p. 380, C-UC). See also Chamberlin to Moulton, 24 November 1897 (Letterbook IX, p. 589, C-UC).

71. Chamberlin, "A Group of Hypotheses", 676.

72. Chamberlin to Moulton, 24 November 1897 (Letterbook IX, p. 587, C-UC).

73. Moulton to Chamberlin, 25 November 1897 (Box 5, folder "Correspondence with F. R. Moulton", C-UC).

74. Chamberlin to Van Hise, 22 January 1898 (Letterbook IX, p. 698, C-UC).

75. Chamberlin to Henry Holt, 27 January 1898 (Letterbook IX, p. 711, C-UC). See also Chamberlin to Holt, 13 August 1898 (Letterbook X, p. 470, C-UC).
76. *Geology*, i: *Geologic process and their results* (New York, 1904).
77. See ref. 30.
78. Chamberlin to Barnard, 1 April 1898 (Letterbook X, p. 172, C-UC).
79. *Science*, n.s., ix (1899), 704–11; see ref. 25.
80. T. C. Chamberlin, "On Lord Kelvin's Address on the Age of the Earth as an Abode Fitted for Life", *Science*, n.s., ix (1899), 889–901; x, 11–18. The manuscript was reviewed by Moulton before publication; see his letter to Chamberlin, 21 June 1899, in Box 5, C-UC.
81. Carroll and Mildred Fenton, *Giants of geology* (Garden City, N.Y., 1945), 312–13.
82. R. T. Chamberlin, *op. cit.* (ref. 42), 351.
83. T. C. Chamberlin, *op. cit.* (ref. 80), 892–3.
84. H. C. Winnik, *op. cit.* (ref. 42).
85. For references see Brush, *The temperature of history* (New York, 1977). Others at this time favoured a cyclic cosmogony in order to avoid the heat death, for example Svante Arrhenius.
86. T. C. Chamberlin, *op. cit.* (ref. 80), 897.
87. It is not really correct to say that nineteenth-century biology required a definite amount of time for evolution, in excess of that allowed by Kelvin. Kelvin gave this impression in his writings, and later commentators have adopted it uncritically; see e.g. J. N. Hattiangadi, "Alternatives and Incommensurables: The Case of Darwin and Kelvin", *Philosophy of science*, xxxviii (1971), 502–7. T. H. Huxley insisted that evolutionary biology only takes its time scale from geology and physics, and is not refuted by changes in that time scale; see his 1876 address, discussed by Brush, *The temperature of history*, 41.
88. J. Geikie, "A White-Hot Liquid Earth and Geological Time", *Scottish geographical magazine*, xvi (1900), 60–67. This issue (February 1900) was received by the U.S. Department of Agriculture Library on 23 February 1900.
89. Chamberlin to Geikie, 13 March 1900 (Letterbook XII, p. 235, C-UC).
90. Moulton to Hale, 12 June 1899 (Box 23 of letters to Hale, at Yerkes Observatory).
91. Hale to Moulton, 13 June 1899 (Letterbook 7, p. 153, at Yerkes Observatory).
92. "I am glad to hear that there is some prospect of your remaining at the University. If you decide to do so I shall be glad to recommend you for promotion next year" (Hale to Moulton, 22 June 1899 (Letterbook 7, p. 187, at Yerkes Observatory)).
93. Moulton to Hale, 3 January 1900 (Box 23 of letters to Hale, at Yerkes Observatory).
94. Only the title was published: "Laplace's ring nebular hypothesis", *Publications of the Astronomical and Astrophysical Society of America*, i (1910), 98.
95. Moulton to Hale, 15 January 1900 (Box 23 of letters to Hale, Yerkes Observatory).
96. "Your paper on the Nebular Hypothesis seemed to me to be all right and I therefore did not call. I hope you will not make any more changes in the galley proof than are really necessary as it is very expensive . . . " (Hale to Moulton, 20 February 1900 (Letterbook 8, p. 71, at Yerkes Observatory)).
97. F. R. Moulton, "An Attempt to Test the Nebular Hypothesis by an Appeal to the Laws of Dynamics", *Astrophysical journal*, xi (1900), 103–30 (quotation from p. 130). For Chamberlin's comments on the manuscript of this paper (chiefly pertaining to the discussion of G. H. Darwin's explanation of the motion of Mars and Phobos) see his letter to Moulton, 19 January 1900, in Letterbook XII, p. 161, C-UC.
98. R. R. Moulton, *Consider the heavens* (Garden City, N.Y., 1935), 143–4.
99. Chamberlin to Keeler, 30 January 1900 (copy from the tissue letter book, in Box I, folder 16, C-UC).
100. Chamberlin to Keeler, 28 February 1900 (copy from tissue letter book, in Box I, folder 16, C-UC).
101. T. C. Chamberlin, "An Attempt to Test the Nebular Hypothesis by the Relations of Masses and Momenta", *Journal of geology*, viii (1900), 58–73 (p. 58).
102. S. R. Cook, "On the Escape of Gases from the Planets according to the Kinetic Theory", *Proceedings of the American Association for the Advancement of Science*, 1899, 120–1; "On the Escape of Gases from Planetary Atmospheres according to the Kinetic Theory", *Astrophysical journal*, xi (1900), 36–43. G. J. Stoney, "Escape of Gases from Planetary Atmospheres", *Nature*, lxi (1900), 515. G. H. Bryan, "On the Permanence of

Certain Gases in the Atmospheres of Planets", *Report of the 69th Meeting of the British Association for the Advancement of Science*, 1899, 634–5; "The Kinetic Theory of Planetary Atmospheres", *Nature*, lxii (1900), 126.

103. Chamberlin, *op. cit.* (ref. 101), 59.

104. See ref. 39. Moulton (*op. cit.*, ref. 97) stated that he used Darwin's "Isothermal-Adiabatic" sphere given on p. 25. A year later Anne Sewall Young (niece of the astronomer Charles Augustus Young) attacked the problem from the other end, using the present distribution of angular momentum to find the law of density of the initial nebula and showing that this leads to impossible results. See her paper, "Density of the Solar Nebula", *Astrophysical journal*, xiii (1901), 338–43. Her work was apparently started under the direction of Moulton, who submitted it to Hale on 26 March 1901 with a letter saying that "In the light of these results many of Darwin's cease to have importance, and in my opinion, this might have been pointed out, but Miss Young felt timid about doing so" (Box 27, Yerkes Observatory).

105. Another way to express the discrepancy was suggested by Chamberlin: "If the matter of the solar system were expanded to some point beyond the orbit of Neptune in conformity to the laws of hydrodynamic equilibrium, and given the moment of momentum of the present solar system and allowed to contract by secular cooling, the centrifugal force in its equatorial portion would not become equal to the centrifugal force [and hence allow separation of a ring] until *after it had contracted far inside the orbit of Mercury*" (Letter to Moulton, 9 February 1900, in Letterbook XII, p. 189, C-UC (emphasis in original)).

106. Chamberlin, *op. cit.* (ref. 101), 71–72.

107. *Ibid.*, 72–73.

108. James E. Keeler, "The Crossley Reflector of the Lick Observatory", *Astrophysical journal*, xi (1900), 325–49 (p. 348).

109. T. C. Chamberlin and R. R. Moulton, "Certain Recent Attempts to Test the Nebular Hypothesis", *Science*, xii (1900), 201–8 (p. 208) (published 10 August).

110. T. C. Chamberlin and F. R. Moulton, "The Development of the Planetesimal Hypothesis", *Science*, xxx (1909), 642–5. See also "Preliminary Notes on the Consequences of Collisions between Larger and Smaller Nebulae as a Possible Mode of Origin of the Solar System" (Box 5, C-UC).

111. Henry S. Tropp, "Moulton, Forest Ray", *Dictionary of scientific biography*, ix (1974), 552–3. He had earlier remarked on the high speed of gases ejected in solar prominences; see "An Attempt to Frame a Working Hypothesis of the Cause of Glacial Periods on an Atmospheric Basis", *Journal of geology*, vii (1899), 561.

112. Chamberlin and Moulton, *op. cit.* (ref. 110), 644.

113. William Huggins, "The New Star in Auriga" (1892) and H. H. Turner, "The New Star in Gemini" (1903), both reprinted in *The Royal Institution library of science—astronomy*, ed. Bernard Lovell (New York, 1970), i, 378–89 and ii, 36–46. N. S. Shaler, *Outlines of the Earth's history* (New York, 1898), 42. A. W. Bickerton, "Some Recent Evidence in Favour of Impact", *Transactions of the New Zealand Institute*, xxvi (1894), 464–76 and plate LII, and "Explosion of Stars", *Popular astronomy*, xii (1904), 666–8. W. W. Campbell, "An Address on Astrophysics", *Popular science monthly*, lxvi (1905), 297–318 (p. 308). *Agnes Clerke*, *A popular history of astronomy during the nineteenth century* (4th ed., London, 1902), 397–8. Hector C. MacPherson, *A century's progress in astronomy* (Edinburgh, 1906), 195. R. L. Waterfield, *A hundred years of astronomy* (London, 1950), 186–7. W. H. S. Monck, "New Stars and Shooting-Stars", *Observatory*, viii (1885), 335–6. J. W. Gregory, *The making of the Earth* (London, 1912), 29. A. C. Clifford, "New Stars", *New Zealand journal of science and technology*, viii (1925), 1–7.

114. John Fiske, *The unseen world and other essays* (Boston, 1876), 11–14. Eugen Tornow, "Die Entstehung des Sonnensystems", *Weltall*, iii (1902), 69. J. Ellard Gore, "The Nebular Hypothesis", *Gentlemen's magazine*, ccxciii (1902), 178–85. Jacob E. Halm, "On Professor Seeliger's Theory of Temporary Stars", *Proceedings of the Royal Society of Edinburgh*, xxv (1905), 513–53, and "Some Suggestions on the Nebular Hypothesis", *ibid.*, 553–61. Max Wilhelm Meyer, *The Making of the World* (Chicago, 1906), 43–45. W. W. Campbell, "The Problems of Astrophysics", *Congress of Arts and Science, St. Louis, 1904* (Boston, 1906), iv, 446–69 (p. 459). Carl Snyder, *The world machine* (London, 1907), 421–3. Svante Arrhenius, *Worlds in the making* (New York, 1908), 152. A. W. Bickerton, [Report of his remarks at meeting on 28 December 1910], *Journal of the British Astronomical Association*, xxi (1911), 139–42, and "The New Astr onomy", *Knowledge*, n.s., viii (1911), 362–7. Henri Poincaré, *Leçons sur les Hypot hèses Cosmogoniques* (2nd ed., Paris, 1913), p. LXIII.

115. T. C. Chamberlin, "On the Possible Function of Disruptive Approach in the Formation of Meteorites, Comets, and Nebulae", *Astrophysical journal*, xiv (1901), 17–40 (p. 21).
116. For Roche's theory see ref. 65. In a memorandum dated 17 October 1901, Chamberlin noted that he had found somewhat similar ideas expressed in T. J. J. See's book, *Researches on the evolution of the stellar systems* (Lynn, Mass., 1896), i, 258 (Box 5, C-UC).
117. Nevertheless it is often stated that the Chamberlin-Moulton theory was first published in this paper.
118. E. O. Hovey, "Geology and Geography at the Denver Meeting of the American Association for the Advancement of Science", *Scientific American supplement* (21 September 1905), lii, 21504–5.
119. Letterbook XIV, 213, C-UC.
120. Chamberlin to Campbell, 18 October 1901 (Letterbook XIV, 254). Further letters to Campbell and H. D. Curtis, requesting photographs of nebulae, are in Letterbook XVII, 577, 690.
121. G. E. Hale, "Stellar Evolution in the Light of Recent Research", *Popular science monthly*, lx (1902), 291–313 (p. 292). This is also quoted at the end of a "Memorandum of Chamberlin's Studies on the Fundamental Doctrines of Geology" submitted to the Carnegie Institution of Washington to support Chamberlin's application for a research grant.

   Another astronomer thought that recent nova observations provided evidence for Chamberlin's theory of tidal disruption: Frank W. Very, "An Inquiry into the Cause of the Nebulosity around Nova Persei," *American journal of science*, (4) xvi (1903), 49–60 (p. 55).
122. W. H. Pickering, "Entdeckung eines neuen Saturnmondes", *Astronomischen Nachrichten*, clix (1899), 31–32, and "The Ninth Satellite of Saturn", *Harvard College Observatory annals*, liii (1905), 45–73.
123. A. C. D. Crommelin, "Phoebe, Saturn's Ninth Satellite", *Journal of the British Astronomical Association*, xv (1904), 32–35. One of Crommelin's colleagues immediately fulfilled his prophecy by suggesting a capture explanation: W. T. Lynn, "The Ninth Satellite of Saturn", *ibid.*, 35–36.
124. M. W. Meyer, *Die Welt der Planeten* (Stuttgart, 1910), 60. Th. Moreux, *Astronomy to-day* (London, 1926), 134–5.
125. W. H. Pickering, "The Rotation of Jupiter's Outer Satellites", *Astronomy and astrophysics*, xii (1893), 481–94; "Polar Inversion of the Planets and Satellites", *ibid.* 692–3; "Explanation of the Inclination of the Planetary Axes", *Astronomical journal*, xxii (1901), 56–57; "Direct and Retrograde Rotation of the Planets", *Astronomische Nachrichten*, clxiv (1904), 201–4, and "Planetary Inversion", *Publications of the Astronomy and Astrophysics Society of America*, read 1905 (pub. 1910), i, 250–1. F. J. M. Stratton, "Planetary Inversion", *Monthly notices of the Royal Astronomical Society*, lxvi (1906), 374–402. L. A. Redman, *Professor Montgomery's discoveries in celestial mechanics* (San Francisco, 1919). In reply to a criticism of Moulton, Pickering insisted that his theory is not the same as Kirkwood's (mentioned above, Section 2). See "Note on the Evolution of the Solar System", *Astrophysical journal*, xxii (1905), 354–5, with Moulton's reply and Pickering's rejoinder on following pages.
126. In his long article, "An Attempt to Frame a Working Hypothesis of the Cause of Glacial Periods on an Atmospheric Basis", *Journal of geology*, vii (1899), 545–84, 667–85, 751–87, T. C. Chamberlin made frequent use of the results presented by Arrhenius in "On the Influence of Carbonic Acid in the Air upon the Temperature of the Ground", *Philosophical magazine*, (5) xli (1896), 237–76. But later he protested to C. G. Abbot that he had never favoured Arrhenius's theory of glaciation, and had been misled by Arrhenius's results (Chamberlin to Abbot, 3 May 1913, in Secretarial Records of C. D. Walcott, record number 45, box 31, Smithsonian Institution Archives).
127. Svante Arrhenius, "Zur Kosmogonie", *Archives Néerlandaises des sciences exactes et naturelles*, (2) vi (1901), 862–73, and *Lehrbuch der Kosmischen Physik* (Leipzig, 1903), 221–30.
128. Svante Arrhenius, *Worlds in the making: The evolution of the universe* (New York, 1908), 148, 193–4, 206–10. His cosmogony is not even mentioned by many of his biographers, *e.g.* James Walker, "Arrhenius Memorial Lecture", *Journal of the Chemical Society* (1928), 1380–401; H. A. M. Snelders, "Arrhenius, Svante August", *Dictionary of scientific biography*, i (1970), 296–302. The account by his grandson makes it appear that his theory was monistic; see Gustav O. S. Arrhenius, "Svante Arrhenius' Contribution to Earth Science and Cosmology", *Levnadsteckningar över K. Svenska Vetenskapsakademiens ledamöter*, clvii (1962), 345–59.

For supporters of the Arrhenius theory see: F. W. Henkel, "The Birth and Death of Worlds", *Knowledge*, n.s., vi (1909), 6–8; W. Kaempffert, "The Life of a Star", *Outlook*, xcii (1909), 707–16; W. Balfour Musson, "Development in the Stellar Universe", *Journal of the Canadian Royal Astronomical Society*, iii (1909), 5–27; E. Becher, *Weltgebäude, Weltgesetz, Weltentwicklung* (Berlin, 1915), 284, 289; C. Snyder, *op. cit.* (ref. 3), 423; A. Amaftunsky, "Kosmogoniya", *Izvestiya Russkago Astronomischekaga Obshestva*, xv (1909), 135–44 (cited in *Astronomische Jahresbericht*, (1909)); Pierre Busco, *L'évolution de l'astronomie au XIXe siècle* (Paris, 1912), 69–70.

129. Percival Lowell, *The solar system* (Boston, 1903), 118–30. For earlier remarks favourable to the nebular hypothesis, see *Mars* (Boston, 1895), 4.

130. A. Lawrence Lowell, *Biography of Percival Lowell* (New York, 1935), 128; B. G. Marsden, "Lowell, Percival", *Dictionary of scientific biography*, viii (1973), 520–3.

131. Percival Lowell, "Mars as the Abode of Life", *Science*, xxx (1909), 338–40. The controversy between Lowell, See and Moulton is summarized by Norriss S. Hetherington, "The Simultaneous 'Discovery' of Internal Motions in Spiral Nebulae", *Journal for the history of astronomy*, vi (1975), 115–25. See also Lowell's refutation of the planetesimal hypothesis applied to satellite motions, in "Planets and their Satellite Systems", *Astronomische Nachrichten*, clxxxii (1909), 97–100. His own theory was presented in *The evolution of worlds* (New York, 1909), 24–30, 142–51, and in a popular article, "The Revelation of Evolution: A Thought and its Thinkers", *Atlantic Monthly*, civ (August 1909), 174–83. For its detailed application to individual planets see "The Genesis of the Planets", *Journal of the Royal Astronomical Society of Canada* (1916), 281–293.

132. William E. Rolston, "Planetology", *Nature*, lxxxiv (1910), 99–100. Chas. Lane Poor, review in *Science*, xxxi (1910), 506–7. Anonymous review in *Living age*, cclxv (11 June 1910), 699–700.

133. T. J. J. See, "Recent Discoveries Respecting the Origin of the Universe", *Atlantic monthly*, lxxx (1897), 484–92.

134. Agnes Clerke, *Modern cosmogonies* (London, 1905), 44. T. J. J. See, "Miss Clerke's Modern Cosmogonies", *Popular Astronomy*, xiv (1906), 313–14.

135. T. J. J. See, "Significance of the Spiral Nebulae", *Popular astronomy*, xiv (1906), 614–16.

136. T. J. J. See, "The Planar Arrangement of the Planetary System", *Nature*, lxxxi (1909), 275; "On the Cause of the Remarkable Circularity of the Orbits of the Planets and Satellites and on the Origin of the Planetary System", *Astronomische Nachrichten*, clxxx (1909), 184–94, and "The Laws of Cosmical Evolution and the Extension of the Solar System beyond Neptune", *Publications of the Astronomical Society of the Pacific* xxi (1909), 60–71.

137. T. J. J. See, "Origin of the Lunar Terrestrial System by Capture with further Considerations on the Theory of Satellites and on the Physical Cause which has Determined the Directions of the Rotations of the Planets about their Axes", *Astronomische Nachrichten*, clxxxi (1909), 365–86; "The Past History of the Earth as Inferred from the Mode of Formation of the Solar System", *Proceedings of the American Philosophical Society*, xlviii (1909), 119–25. A comprehensive exposition of See's theory may be found in his book *Researches on the evolution of the stellar systems*, ii: *The capture theory of cosmical evolution* (Lynn, Mass., 1910). Qualitatively similar theories had been proposed earlier by R. B. Taylor (1898, 1903) and A. Gareis (1901).

The following supported See's theory: N. W. Numford, "A Question on the Capture Theory", *Popular astronomy*, xx (1912), 228–32; J. C. Dean, "From Whence Came the Earth?", *Westminster review*, clxxx (1913), 635–44; Alfred Russel Wallace, letter to W. L. Webb, 28 October 1913, in See Papers, Library of Congress; F. W. Henkel, "New Theories of the Evolution of Stellar System", *Scientific progress in the twentieth century*, v (1910), 82–90 and "New Methods in Astronomy, Their Application to the Evolution of our System", *Knowledge*, vii (1910), 350–4; J. B. Kerfoot, *World to-day*, xxi (1912), 1665–76.

138. Kr. Birkeland, "Sur l'Origine des Planètes et de leurs Satellites", *Comptes rendus, Académie des Sciences, Paris*, clv (1912), 892–5, "The Origin of Worlds", *Scientific American supplement*, lxxvi (1913), 1, 8–9, 12, 20–21, 32.

139. T. C. Chamberlin, "The Origin of Ocean Basins on the Planetesimal Hypothesis", *Bulletin of the Geological Society of America*, xiv (1903), 548 (abstract); a similar account appeared on pp. 22665–6 of E. O. Hovey's report on "The Annual Meeting of the Geological Society of America, and Geology and Geography at the Convention of the American Association for the Advancement of Science", *Scientific American supplement*, lv (1903), 22646–8, 22665–7. See also *Science*, xvii (1903), 300–1.

140. The discussion was reported following the account of Chamberlin's talk, under the title "The Origin of Ocean Basins on the Planetessimal [sic] Hypothesis", *Science*, xvii (1903), 300–1. Chamberlin soon dropped the extra "s"; as he explained in a letter to H. L. Fairchild, 9 June 1903, "The analogy of 'infinitesimal' permits the omission of one 's', and this is in the line of simplification and progress" (Letterbook XVII, 269, C-UC).
141. Quoted from *Science*, xvii (1903), 301. For Merrill's later views on this subject see "The Composition of Stony Meteorites Compared with that of Terrestrial Igneous Rocks and Considered with Reference to their Efficacy in Worldmaking", *American journal of science*, (4) xxvii (1909), 469–74.
142. *Science*, xvii (1903), 301.
143. R. A. Millikan to T. C. Chamberlin, 23 December 1903 (Box 5, C-UC).
144. Copy in Box 4, folder 20, C-UC. A copy of Walcott's reply and other correspondence may be found in the file of Chamberlin correspondence at the Carnegie Institution of Washington (hereafter abbreviated CIW).
145. This is probably the undated "Memorandum of Chamberlin's Studies on the Fundamental Doctrines of Geology" at CIW (copy in Box 5, C-UC); however, that Memorandum contains a quotation from G. E. Hale's article in the February 1902 issue of *Popular science monthly* (see ref. 121), so one would have to assume that this article was available to Chamberlin before the nominal publication date in order to accept this identification.
146. "An inquiry into the fundamental problems of geology as set forth in detail in a communication to Trustee C. D. Walcott under date of Jan. 23d 1902 & other letters", Application for Grant in Aid of Research, 2 January 1902, in Chamberlin file at CIW (received 6 January 1903).
147. T. C. Chamberlin to C. D. Walcott, 10 January 1903, in Chamberlin file at CIW. The letter begins, "Yours of the 6th announcing that my application for a grant has been approved, is at hand."
148. *Ibid.*, 1. Correspondence on Hoskins's work for the project may be found in Letterbooks XVI and XVII, and in Addenda Box II, folder 4, C-UC. See also "Scheme for Work under the Carnegie Institution", 7 January 1903, in folder of miscellaneous notes and calculations, Box 5, Chamberlin papers at Chicago.
    Leander Miller Hoskins (1860–1937) had taught civil engineering and mechanics at Wisconsin from 1885 to 1892, and was professor of applied mathematics at Stanford from 1892 to 1925.
149. Correspondence with Slichter on this problem may be found in Letterbooks XIV, XVI and XVII, and Addenda Box III, folder 5, C-UC.
    Charles Sumner Slichter (1864–1946) was professor of applied mathematics at Wisconsin, with a special interest in the motion of underground water.
150. Lunn had been mentioned in Chamberlin's earlier letter to Walcott of 23 January 1902, as "a rising young mathematician of remarkable promise".
    Arthur Constant Lunn (1877–1949) taught applied mathematics at Chicago starting in 1903, promoted to professor in 1923.
151. Walcott to Chamberlin, 28 January 1902; Chamberlin to Walcott, 27 February 1903; Chamberlin to R. S. Woodward, 13 October 1908, all at CIW. "Proposition relative to the Grant of the Carnegie Institution for Investigations in the Fundamental Doctrines of Geology", apparently a memorandum to President Harper, copy in Box 4, folder 20, C-UC.
152. R. S. Woodward to T. C. Chamberlin, 8 March 1910, at CIW.
153. Chamberlin to Woodward, 14 January 1910; 11 February 1910. Woodward to Chamberlin, 21 January 1910, and 8 March 1910. (All at CIW.)
154. See below, ref. 170.
155. C. D. Walcott to T. C. Chamberlin, 25 January 1902; Chamberlin to Walcott, 28 January 1902; Walcott to Chamberlin, 12 March 1902 (on committee appointments). Chamberlin to Walcott, 14 June 1902 (on scientific journals). (All at CIW.)
156. R. S. Woodward to T. C. Chamberlin, 16 November 1906; Chamberlin to Woodward, 24 November 1906. For detailed discussion of the Carnegie Institution's change in policy see N. Reingold, "National Science Policy in a Private Foundation: The Carnegie Institution of Washington, 1902–1920". Reingold's interpretation is that Woodward wanted the Carnegie Institution to be not merely a "disbursing agency" but a real participant in research.
157. Chamberlin to Woodward, 6 September 1909 and 19 October 1909; Woodward to Chamberlin, 8 September 1909 and 21 October 1909 (on hiring Chamberlin's son

Rollin); F. R. Moulton to Woodward, 3 August 1905 (on hiring Moulton's brother). (All at CIW.)
158. Woodward to Chamberlin, 3 January 1910 and 21 January 1910; Chamberlin to Woodward, 14 January 1910 (the denial has "wrecked my plans" to recruit a new collaborator). (All at CIW.)
159. Chamberlin to Moulton, 12 January 1903 (Letterbook XVI, 404–6, C-UC).
160. Chamberlin to Moulton, 28 November 1903, in Letterbook XVII, 778; Moulton to Chamberlin, 20 December 1903, and 8 February 1904, in Box 5, folder "Correspondence with F. R. Moulton", C-UC.
161. Chamberlin to C. W. Hayes, 24 February 1904 and 14 March 1904, in Box 5, C-UC.
162. "Memorandum on Original Announcement of the Planetesimal Hypothesis", 4 October 1928, 1 (Box 5, C-UC).
163. Chamberlin to Newcomb, 17 March 1904; Chamberlin to Becker, 18 March 1904 (both in Box 5, C-UC); Chamberlin to Gilbert, 18 March 1904 (Letterbook XVIII, 102); Chamberlin to Newcomb, 18 March 1904 (Letterbook XVIII, 105). The original of the letter to Newcomb is in the Newcomb papers at the Library of Congress, container #19.
164. Chamberlin to Moulton, 28 April 1904 (Box 5, C-UC).
165. Chamberlin to Becker, 11 April 1904 (Box 5, C-UC).
166. G. F. Becker, "Kant as a Natural Philosopher", *American journal of science*, (4) v (1898), 97–112.
167. G. F. Becker, "Present Problems of Geophysics", *Science*, xx (1904), 545–56.
168. Chamberlin to Moulton, 28 April 1904, p. 2. See also Chamberlin to Gilbert, 11 April 1904.
169. Chamberlin to Moulton, 2 March 1904, in Box 5, C-UC; also Letterbook XVIII, 55.
170. T. C. Chamberlin, "Fundamental Problems of Geology", *Carnegie Institution yearbook for 1904*, 195–258 (pub. 1905). Similar remarks are found in the Chamberlin-Salisbury text (ref. 2), ii, 51, 55.
171. Chamberlin to Moulton, 16 May 1904 (Letterbook XVIII, 285).
172. Chamberlin, "Fundamental Problems"; Chamberlin and Salisbury, *op. cit.* (ref. 2), ii, 58.
173. Moulton to Chamberlin, 19 May 1904, in Box 5, C-UC.
174. Moulton to Chamberlin, 24 May 1904, in Box 5, C-UC.
175. Chamberlin to D. C. Gilman, 30 September 1904 (Chamberlin file, CIW).
176. Woodward to Wolcott, in Chamberlin file at CIW.
177. *Ibid.* George William Hill (1838–1914) was the leading mathematical astronomer in the United States at the time, equalled only by Simon Newcomb; he was appointed to the Rutherford chair of astronomy at Columbia (where Woodward was Dean of the College of Pure Science) but resigned because there were so few qualified students to teach.
178. "Fundamental Problems of Geology", *Yearbook No. 3*, 195–258. The fact that this is called the "Yearbook for 1904" has obscured the fact that Chamberlin's paper was not actually published until 1905.
179. F. R. Moulton, "On the Evolution of the Solar System", *Astrophysical journal*, xxii (1905), 165–81.
180. *Ibid.*, 168. The details of these calculations were never published. He mentioned the difficulty of the work in a letter to Woodward, 3 August 1905 (Chamberlin file at CIW). The results were said to be qualitatively favourable to the hypothesis in Moulton's "Report", *Carnegie Institution yearbook No. 4 for 1905* (1906), 186–90; see also letter from Chamberlin to Woodward, 28 October 1905, in Chamberlin file at CIW. In his report for 1906, Moulton stated that he had computed the orbits of material ejected from the Sun in 48 cases, following them for about five years each time; in most cases the disturbing star's closest approach to the Sun was 5 A.U. See "On the Probability of a Near Approach to Two Suns and on the Orbits of Material Ejected from them under the Stimulus of their Mutual Tidal Disturbances", *Carnegie Institution yearbook No. 5 for 1906* (1907), 168–9. But three years later Moulton reported that his work on this problem was not yet complete ("Inquiry into the Fundamental Problems of Geology", *Carnegie Institution Yearbook No. 8 for 1909* (1910), 225–6). I have not made a systematic search for possible later publications since Moulton himself told H. N. Russell that "in nearly 30 years I have written only three times on the Planetesimal Hypothesis, and in two of those three times the material appeared in my 'Introduction to Astronomy' " (Moulton to Russell, 6 June 1929, in Box 33, H. N. Russell papers, Princeton University Library).

181. J. Scheiner, "Ueber das Spectrum des Andromedanebula", *Astronomische Nachrichten*, cxlviii (1899), 325–8; *Astrophysical journal*, ix (1899), 149–50.
182. Moulton, "Evolution of the Solar System", 169.
183. P. 172.
184. *Ibid.*
185. Moulton, "An Attempt to Test the Nebular Hypothesis . . . " (ref. 97), 108.
186. The rotation periods for the planets in order of mass are: Jupiter $9^h 50^m 30^s$; Saturn $10^h 14^m$; Neptune 16^h; Uranus 11^h [retrograde]; Earth $23^h 56^m 4^s$; Venus 243^d [retrograde]; Pluto $6^d 9^h$; Mars $24^h 37^m 23^s$; Mercury 59^d.
187. P. 175. According to Newtonian gravitational theory the period of revolution of a satellite varies inversely as the square root of the mass of the primary.
188. See ref. 125.
189. H. H. Turner, as reported in "A Vital Change in our Theory of the Solar System", *Current literature*, xlvii (1909), 445–7. A. S. Eddington regarded the discovery of a retrograde 8th satellite of Jupiter as confirmation of Pickering's theory; see "Some Recent Results of Astronomical Research" (1909), in *The Royal Institution library of science—astronomy* (ref. 113), ii, 97–112.
190. P. 181.