THE DEVELOPMENT OF THE PLANETESIMAL HYPOTHESIS

When, in 1906, the planetesimal hypothesis had reached a stage of development sufficient to warrant its introduction as a working hypothesis into text-books of geology and astronomy, it seemed to its authors worth while to draw up and place on their private files a memorandum of the several stages of cosmogonic study that had led up to the hypothesis in the form it had then taken. It was not assumed that the hypothesis had reached a final form, much less that it was in any sense then proven or that could approach proof until after a long period of trial and the closest scrutiny. On the contrary, they were then engaged in further efforts to test its working qualities and to add to its details or to modify them. It, however, seemed worth while at that stage to make note of preceding steps of progress while fresh in mind for future reference if occasion should require. Such occasion seems now to have arisen.

In the introduction to the memorandum, by way of qualifying the statements of the individual parts taken, it was noted that the mutual studies of the authors had grown up so gradually and informally, their conferences had been so frequent and so free, and their relations so intimate that it was difficult to set down with accuracy the precise parts contributed by each, or the aid rendered each to the other in working these out. The memorandum was intended to indicate merely the main individual lines of work and the leading stages of progress. A quite accurate and detailed history could be worked out, if it were worth while, from the note-books of advanced students of the University of Chicago from 1892 onward, as they were familiar with the status these studies had reached at the times their lecture notes were taken. Several of these students made computations or rendered other aid sufficient to call for notice in the papers published, among whom were A. W. Whitney, H. L. Clarke, J. P. Goode, H. F. Bain, S. Weidman, C. F. Tolman, Jr., N. M. Fenneman, C. E. Siebenthal, R. T. Chamberlin and W. H. Emmons.

In the synopsis below, the memorandum of March 12, 1906, is followed in the main, but the abbreviated phrases and references have been rounded out or recast to make them more specific and the whole brought down to date.

I. Destruction (in the Main)

Line of Approach and First Step.—To find out what effects on geological climates might be assignable to changes in the constitution of the atmosphere, Chamberlin, in the middle nineties of the recent century, attempted to test, by means of the molecular velocities involved after the method of Johnstone Stoney, the probable limits to the extent of the atmospheres in early geological stages, particularly those conditioned by the molten and gaseous states of the early earth as then commonly postulated.

These tests were found to throw doubt on the common belief in the enormous extent of hot vaporous atmospheres supposed to prevail during the gaseous and molten states of the earth. The test was then carried back to the earth-moon ring postulated by the Laplacian hypothesis where its application seemed fatal to the hypothesis. Moulton aided in his test by preparing tables of parabolic velocities for the earth at various heights above its surface and at different rates of rotation. Dr. A. W. Whitney made computations relative to molecular velocities under varying temperatures and pressures. The results were set forth in a paper read by Chamberlin at the Toronto meeting of the British Association for the Advancement of Science, August 20, 1897, and more fully in the *Journal of Geology*, October-November, 1897, pp. 653–683.

Second Step.—The conclusion that the nebulous matter of the supposed earth-moon ring could not remain in a true gaseous state, *i. e.*, with the molecules in active collisional relations to one another, under the conditions postulated for the earth-moon ring under the Laplacian hypothesis, led Chamberlin to consider the alternative conception of molecules or particles revolving in independent orbits in planetoidal fashion. Condensation from this state had previously been held, generally if not universally, to give rise to *retrograde*

rotations, whereas most of the rotations of the solar system are direct. Among the more convenient references showing the general acceptance of this view are the following: D. Kirkwood, *Am. Jour. Sci.*, XXXVIII., Nov., 1864, pp. 1–2; A. Hinricks, *Am. Jour. Sci.*, XXXVII., 1864, pp. 48–52; D. Trowbridge, *Am. Jour. Sci.*, XXXIX., 1865, pp. 25–43; A. Clerke, "History of Astronomy during the Nineteenth Century," 1893, p. 383; H. Faye, "Sur l'Origine du Monde," 1896, pp. 138–140, 164–171, 270–281; C. A. Young, "General Astronomy," 1899, pp. 568–572; Sir Robt. Ball, "The Earth's Beginning," 1902, pp. 324–347; A. Clerke, "Modern Cosmogonies," 1905, pp. 26–42. It was therefore clear that if this deduction were valid it was fatal to all hypotheses of the planetesimal type; indeed its supposed validity was probably the reason why such hypotheses had not been entertained. This apparently fatal bar was removed by Chamberlin, who pointed out that in the case of bodies moving in elliptical orbits about a common center, collision can only take place when some part of the *perihelion* section of the *outer* orbit coincides with some part of the *aphelion* section of the *inner* orbit, and that *at the point of collision* the body in the outer orbit moves faster than the body in the inner orbit, though on the average the body in the larger orbit moves slower than the one in the smaller orbit, which general fact was made the basis of the previous adverse reasoning. The way was thus opened for the construction of a tenable hypothesis on the orbital basis, including the form later called *planetesimal.* This germ of constructive work on lines previously regarded as untenable was briefly stated in the paper read before the British Association for the Advancement of Science, Toronto meeting, August 20, 1897, and published in the *Journal of Geology,* October–November, 1897, p. 669.

Third Step.—The tenability of construction on an alternative line being thus assured, the skepticism regarding the old nebular and meteoroidal hypotheses was more freely entertained and led to a search for other tests, particularly those resting on grounds other than molecular activity. The discrepancy between the slow rotation of the sun at present and the rotation it should have if it had contracted from a gaseous spheroid filling the orbit of Mercury and having the equatorial velocity necessary to shed the Mercurial ring as postulated by the Laplacian hypothesis, first came to Chamberlin's attention and led to a conference, with Moulton, late in 1899, out of which grew the more systematic inspection of the dynamics of the solar system in which the chief work was done by Moulton.

Fourth Step.—By restoring theoretically, in conformity with the laws of gases, the nebulous stages of the Laplacian hypothesis, comparisons of the several moments of momenta of the spheroid at these stages with the moments of momenta of the equivalent parts of the existing system were made by Moulton with results that seemed fatal to the Laplacian hypothesis and to all other hypotheses which had a similar dynamic basis.[1] Several other tests of a dynamical character equally adverse to the Laplacian hypothesis were also set forth in this paper.

Although the restorations of the solar spheroids at the various nebulous stages were made on the basis of the known laws of distribution of gases, with liberal margins of safety, uncertainty as to the full trustworthiness of the extension of the laws of gases to bodies of such tenuity and at such temperatures was unavoidable. To cover doubts arising from this source, independent tests were made by Chamberlin on the basis of the ratios of the masses to the moments of momenta of the spheroids and of the separated rings, respectively, using the masses and the moments of momenta of the present derived bodies, thus avoiding the application of the laws of gases; and the results were found to be equally adverse to the Laplacian hypothesis.[2]

[1] "An Attempt to Test the Nebular Hypothesis by an Appeal to the Laws of Dynamics," by F. R. Moulton, *Astrophysical Journal*, March, 1909, pp. 103–130.

[2] "An Attempt to Test the Nebular Hypothesis by the Relations of Masses and Momenta," by T. C. Chamberlin, *Jour. Geol.*, Vol. VIII., January–February, 1909, pp. 58–73.

II. Constructive (in the main)

The preceding work was chiefly destructive, but there were three notable exceptions: (1) The opening of the way to construction on planetoidal lines; (2) the determination of rather rigorous criteria that must be met in forming a tenable hypothesis, viz., *the conditions must be such as to give low mass, high moment of momentum and irregular distribution of matter to the outer part of the system, and high mass, low moment of momentum and sphericity to the central part;* and (3) the recognition that spiral nebulæ offered the greatest probability of meeting these criteria and of having at the same time a planetoidal organization.[3] A summation of the leading points made in the destructive work, together with a statement of the constructive criteria above named and of the grounds for giving precedence to spiral nebulæ in the search for an origin of the solar system, was published in Science, August 10, 1900, by Chamberlin and Moulton jointly.

Fifth Step.—Considerable futile work was done, largely by Chamberlin, in trying out the possibilities of collision between nebulous bodies as a mode of origin of spiral nebulæ, but 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.

Sixth Step.—The effects of the differential attractions exerted by bodies on one another when they make close approaches were then studied by Chamberlin in the lines marked out by Roche, Maxwell and others, and found to be a promising field for hypothesis respecting the origin of meteorites, comets and nebulæ. This study included not merely the direct tidal effect on a passive body, following Roche, but also 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. It was shown that the contingency of close approach was much greater than that of collision, and that the results, (1) in the case of the disrupting of solid bodies, afforded a felicitous basis for explaining the erratic orbits of comets, the clustered fragments of the comet heads, and the angularity of the meteorites into which they are supposed to be finally dispersed; while (2) the explosive projections from suns under the influence of the passing body gave a reason for the two-armed feature of most spiral nebulæ—a neglected feature to which attention was specially called—for the spiral form, for the knots and haze, and at the same time offered a basis for inferring their dynamical state. These radical hypotheses were set forth in a paper entitled "On a Possible Function of Disruptive Approach in the Formation of Meteorites, Comets and Nebulæ," by T. C. Chamberlin, *Astrophys. Jour.*, Vol. XIV., July, 1901, pp. 17–40; also *Jour. Geol.*, Vol. IV., 1901, pp. 369–393.

Seventh Step.—With these conceptions of the origins and dynamical states of meteorites and spiral nebulæ as the bases of alternative hypotheses, a more critical study was made of the probabilities of origin of the solar system from *swarms* of meteorites of *heterogeneous* and *quasi-gaseous* organization, and, more radically, of the probabilities of the *origin of such swarms* either by concentration from a state of greater diffusion or by the dispersion of some previous body. Conditions favorable to the evolution of the solar system were not found, except when the meteoric organization took the planetesimal form.[4] Specifically, the conclusion reached was that the heterogeneous meteoritic state is "inherently moribund, passing into the gaseous state on the one hand, or into the planetesimal on the other, or, in the absence of assemblage, losing its constituents to existing suns and planets by capture one by one."[5]

[3] Chamberlin in *Journal of Geology*, VIII., January–February, 1900, pp. 72–73; Moulton, *Astrophysical Journal*, XI., March, 1900, p. 130.

[4] Chamberlin in Year Book No. 3, Carnegie Institution of Washington, 1904, pp. 195–208.

[5] *Ibid.*, p. 208.

Eighth Step.—Concurrently with these constructive attempts of Chamberlin with futile results except as based on planetoidal lines, Moulton attempted a critical review of all recorded cosmogonic hypotheses, but unforeseen conditions caused the temporary suspension of work and prevented a final treatment and publication of the assembled material.[6]

Ninth Step.—With (1) an open door for constructive work with nebulæ of planetoidal dynamics made available in 1897, with (2) the controlling criteria defined, and with (3) the limitations of tenable hypotheses narrowed by the futile work, the planetesimal hypothesis was gradually given shape and working form chiefly by Chamberlin in the absence of Moulton, as set forth in Year Book No. 3, Carnegie Institution, 1904, pp. 208–233; but this shaping of the hypothesis passed under the criticism of Moulton before publication. The spirit and purpose of this constructive work is thus stated, pp. 232–233:

> It has thus been my endeavor to develop the hypothesis into sufficient detail (1) to furnish a large number of points of contact with known phenomena and with recognized mechanical principles to facilitate testing its verity by those relations, if not now, at least in the early progress of investigation; (2) to furnish a basis for deducing the hypothetical stages of the earth that preceded its known history, and for drawing thence inferences as to the conditions of the interior which the earth inherited from the mode of its birth; and (3) to stimulate inquiry into the elements involved. In short, I have endeavored to give the hypothesis a working form under the conviction that so long as the complicated elements involved remain so imperfectly determined as at present its working value is its chief value.

Preliminary to this publication the essential features of the hypothesis had been discussed before several scientific societies and subjected to criticism. The hypothesis was also set forth by Moulton in a paper "On the Evolution of the Solar System," *Astrophys. Jour.*, October, 1905, pp. 165–181.

Later Steps.—The hypothesis was somewhat further elaborated and supplied with illustrations for text-book use by Chamberlin for the chapter on the Origin of the Earth in Chamberlin and Salisbury's "Geology," Vol. II., Chap. I., pp. 28–81, 1905, and by Moulton for his "Introduction to Astronomy," 1906, pp. 463–487.

Subsequent work in further testing, developing and applying the hypothesis has been in progress as set forth in Year Book No. 4, Carnegie Institution, 1905, pp. 171–173 (Chamberlin), and 186–190 (Moulton); Year Book No. 5, Carnegie Institution, 1906, pp. 165–172, and in later Year Books. More specifically and concretely, the continuation of investigation on lines growing out of the planetesimal hypothesis is shown by Publication No. 107, of the Carnegie Institution, entitled "The Tidal and Other Problems."

T. C. CHAMBERLIN
F. R. MOULTON

CHICAGO,
October 14, 1909

[6] Moulton in Year Book No. 3, Carnegie Institution, 1904, pp. 255–256.

AN ASSOCIATION OF AMERICAN CHEMICAL RESEARCH LABORATORIES

TO THE EDITOR OF SCIENCE: In connection with the second decennial celebration of Clark University, a special meeting was held on September 16 last "for the purpose of forming an Association of Chemical Research Laboratories, to systematically exchange chemicals urgently needed in research work." Many of your readers will doubtless be interested to know the outcome of that meeting.

Chemical research, especially organic research, in this country is greatly handicapped by the length of time it takes to import chemicals from Germany, when a need for them arises unexpectedly in course of an investigation. To quote a single opinion expressed at our September meeting, Professor Arthur Michael declared that his output of work, during the past twenty-five or thirty years, has been reduced fifty per cent. by this handicap.

Now, a chemical urgently needed in one laboratory is very often lying unused in some other laboratory. It would be gladly placed at the disposal of the investigator who hap-