DR. W. H. SHELDON, preceptor of philosophy at Princeton University, has been elected professor of philosophy at Dartmouth College.

DR. HARDEE CHAMBLISS, of the research staff of the General Chemical Company, New York, has accepted the professorship of chemistry in the Oklahoma Agricultural and Mechanical College at Stillwater, Okla.

MR. BENJAMIN F. LUTMAN, assistant in botany at the University of Wisconsin, has been appointed assistant botanist in the Agricultural College of the University of Vermont.

DR. J. ELIOT COIT, of the University of Arizona, has accepted the assistant professorship of pomology in the University of California.

IN the department of zoology at Northwestern University, Dr. E. H. Harper has been promoted to an assistant professorship and Charles S. Mead, Ph.D. (Columbia), has been appointed instructor in zoology.

PROFESSOR JOHN COX has retired after nineteen years as Macdonald professor of physics in McGill University and first director of the Macdonald Physics Building. Professor H. T. Barnes has been appointed director and Professor H. A. Wilson, F.R.S., has been appointed Macdonald professor of physics. Dr. H. L. Brown has been appointed assistant professor of physics, Mr. F. H. Day and Mr. W. R. Gillis, lecturers in physics, Mr. A. L. Dickieson, Mr. N. E. Wheeler and Mr. A. G. Hatcher, demonstrators in physics.

IN the Queen's University of Belfast appointments have been made as follows: professor of botany, Mr. D. T. Gwynne-Vaughan; lecturer in organic chemistry, Dr. A. W. Stewart; lecturer in physics, Dr. Robert Jack; lecturer in bio-chemistry, Dr. J. A. Milroy; lecturer in geology and geography, Dr. A. R. Dwerryhouse; lecturer on hygiene, Dr. W. James Wilson.

DR. G. S. WEST has been appointed to the chair of botany and vegetable physiology in the University of Birmingham, rendered vacant by the retirement of Professor Hillhouse.

DISCUSSION AND CORRESPONDENCE

REMARKS ON RECENT CONTRIBUTIONS TO COSMOGONY

TO THE EDITOR OF SCIENCE: In your issue of May 28 is a letter by T. J. J. See, ostensibly demanding "fair play and toleration" in the consideration of current contributions to science, but clearly written for the purpose of exploiting some of his own recent writings. In this letter, notwithstanding the implications of its caption, he takes occasion to characterize the work of Professor Chamberlin and myself as "inconsistent and purely destructive," and says:

> If Professor Blackwelder will study my last paper carefully, and the work now in press, when it appears, he will find that most of the recent speculations on cosmogony are not worth the paper they are written on; and yet some of them have been published by the *Astrophysical Journal* and the Carnegie Institution.

He also modestly states:

> It is only fair to say that no constructive results of consistent character had been reached on this subject till my own investigation was completed last year. . . . As I have worked on this subject uninterruptedly for twenty-five years, I am prepared to speak with some degree of authority.

Because of these extravagant pretensions and the fact that a majority of the readers of SCIENCE, being unfamiliar with the details of recent developments in this subject, will not credit any one with having the monumental nerve to put forward such claims without there being some basis for them, I beg the privilege of taking enough space to state briefly the facts relating to this matter.

The well-known nebular hypothesis was put forward briefly by Laplace, in 1796, at the end of a work on popular astronomy. Its simplicity and attractiveness, as well as the great name of its author, soon gained for it wide acceptance among scientific men. It satisfied those racial instincts for an explanation of the origin of things which gave rise to the cosmogonies of the ancients; and in stirring the emotions, the majestic sweep of events which

it described took the place of the heroic deeds celebrated in their epics. But its greatest value was in making, in the first half of the nineteenth century, a foundation for the development of geological theories respecting the age and evolution of the earth, and these theories, in turn, were important factors in Darwin's elaboration of his "Origin of Species."

The next important step in cosmogony was Helmholtz's contraction theory of the heat of the sun, published in 1854, which not only was not contradictory to the Laplacian theory, but was generally supposed to be a proof of its correctness.

In the latter half of the nineteenth century the Laplacian theory was supplemented by the consideration of some factors originally omitted, chiefly by Roche and Sir George Darwin, and some objections were urged against it, chiefly by Babinet and Faye. But the writings of practically all astronomers show that it was generally accepted without fundamental modifications. For example, Sir George Darwin in his classical researches on tidal evolution frankly stated that he accepted it in its main outlines; and in 1886 C. Wolf, of the Paris Observatory, reprinted in book form a series of articles appearing earlier in *Bulletin Astronomique,* Vols. I. and II., which clearly supported this theory. In the preface to this volume we read:

> Mon principal but, en écrivant ces articles, était de montrer que la théorie de Laplace répond encore aujourd'hui le mieux possible aux conditions que l'on est en droit d'exiger d'une hypothèse cosmogonique.

In the late nineties Professor Chamberlin in studying the earth's atmosphere, and particularly its origin and history, became skeptical of the soundness of the Laplacian theory; and simultaneously some of its weaknesses were forced on me while considering it in my classes in descriptive astronomy. Toward the end of 1899 we had several conferences on the question of its correctness, and as a result of these discussions we decided to test it, first as to its agreement with the facts established by observations, and secondly as to its self-consistency. The results of these inquiries are contained in a paper published by Professor Chamberlin in the *Journal of Geology,* February–March, 1900, and in one by myself in the *Astrophysical Journal,* March, 1900. It is well known that the conclusions reached in these papers seemed to us so adverse to the theory as to compel us to reject it as being no longer a satisfactory hypothesis; and since that time many astronomers have placed themselves on record as being in agreement with us.

Immediately after the publication of these papers constructive work was begun, chiefly by Professor Chamberlin. The first account of the new hypothesis which was developed was published by Professor Chamberlin in Year Book No. 3 of the Carnegie Institution, pp. 208–253 (1904), and another was published by myself in the *Astrophysical Journal,* Vol. 22, pp. 165–181 (1905). In Chamberlin and Salisbury's "Geology," Vol. 2, pp. 38–81 (1906), under the title of The Planetesimal Hypothesis, Professor Chamberlin gives an extensive account of the proposed theory. Some of the subheadings are: Subvarieties of the Hypothesis, The Hypothetical Origin of the Solar Nebula, The Contingencies of Stellar Collision, The Contingencies of Close Approach, The Special Consequences of Close Approach, The Acquisition of Rotatory Motion, The Result a Spiral Nebula, The Assigned Nebular Origin not Vital, The Evolution of the Nebula into Planets, The Part Played by Ellipticity of Orbit, The Evolution of Circularity, The Time Involved, The Bearing of the Mode of Accretion on the Direction of Planetary Rotation, The Spacing-out of the Planets, . . . He closes the chapter with the following summary:

> The planetesimal hypothesis thus assumes that the solar system was derived from a nebula of the most common type, the spiral, and that the matter of this parent nebula was in a finely divided solid or liquid state before aggregation, in harmony with the continuous spectra of spiral nebulæ. It regards the knots of the nebula as the nuclei of the future planets, and the nebulous

haze as matter to be added to the nuclei to form the planets. It assumes that both the knots and particles of the nebulous haze moved about the central mass in elliptical orbits of considerable, but not excessive, eccentricity. It postulates a simple mode of origin of the nebula connected with the not improbable event of a close approach of the ancestral sun to another large body, but the main hypothesis is not dependent on this postulate.

It assigns the gathering-in of the planetesimals to the crossing of the elliptical orbits in the course of their inevitable shiftings. Out of this process and its antecedents, it develops consistent views of the requisite distribution of mass and momentum, of the spacing-out of the planets, of their directions of rotation, of their variations of mass, of their varying densities, and of other peculiarities.

It deduces a relatively slow growth of the earth, with a rising internal temperature developed in the central parts and creeping outward. With such a mode of growth, the stages of the earth's early history necessarily depart widely from those postulated by the Laplacian and the meteoritic hypotheses. These stages now claim our attention.

In my "Introduction to Astronomy," pp. 463–487 (1906), I have discussed the same theory under the title of The Spiral Nebula Hypothesis. Some of the headings of the articles in this section are: Hypotheses Respecting the Antecedents of our Present System, A Possible Origin of Spiral Nebulas, The Development of the Solar System from a Spiral Nebula, The Origin of Planets, The Origin of Satellites, The Planes of the Planetary Orbits, Rotation and Equatorial Acceleration of the Sun, The Small Eccentricities of the Planetary Orbits, The Rotations of the Planets, The Eccentricities of the Satellite Orbits, The Moment of Momentum of the System, The Evolution of the Planets, The Age of the Solar System, The Future of the System. . . . The chapter is closed with the following summary:

The first word should be one of warning that the theory which has been sketched briefly should not be accepted as final. There are many points where quantitative results must be obtained and compared with our actual system. There may be many modifications of it possible and necessary. For example, the genesis of spiral nebulas may be different from that postulated above.

The hypothesis of an original spiral nebula is suggested by recent photographs of nebulas as well as by the system itself. The conditions which are supposed to have given rise to the spiral nebula seem most reasonable in view of the motions of the stars. The development of a spiral nebula by the near approach of two suns seems to be a necessary consequence, though this point needs further elaboration. The development of some such a system as ours from a small spiral nebula of the type considered seems to be inevitable. So far as the details have been worked out nothing directly contradictory to the theory, or even seriously questioning it, has been found, while it explains admirably all the main features of the system. It can be safely said that, at present, this hypothesis satisfies all the requirements of a successful theory much better than any previous one.

Since the publication of these books the work of elaborating and testing the theory has been carried forward by both Professor Chamberlin and myself, and a part of the results obtained have been published by the Carnegie Institution.

The alleged twenty-five years of uninterrupted work upon the evolution of the solar system by See have resulted only in the following papers so far as I am aware: (1) "Significance of the Spiral Nebulæ," *Popular Astronomy,* pp. 614–616 (December, 1906); (2) "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,* No. 4308 (February 24, 1909), the same paper printed in *Popular Astronomy,* May, 1909, and at least the substance of the same paper communicated by its author to the Chicago *Record-Herald* early in 1909.

In the paper in *Popular Astronomy,* written over the date October 23, 1906, See makes the following statements:

For a number of years the writer has given consideration to the probable nature of the spiral nebulæ, and their importance has been considerably increased by photographs obtained by Roberts and Keeler, and more recently at the Yerkes Observatory. Certain speculations have been in-

dulged in which implied that the spiral nebulæ are true nebulæ condensing into systems of stars. Though this premature and unauthorized line of thought has been extensively exploited, and even given place in one treatise on geology, it has always seemed to the writer quite unsound. I have consistently held that so far we do not know the true character of the spiral nebulæ, and this position is amply justified by the penetrating remarks of M. Poincaré. Whether the spiral nebulæ are other Milky Ways, as suggested by the illustrious French geometer, time alone can tell; and it may be several centuries before this question can be satisfactorily settled. Meanwhile the exploitation of the spiral form as typical of nebular development is certainly misleading, for, as Poincaré points out, there is no proof that these spirals are true gaseous nebulæ.

The speculations on spiral nebulæ have been decidedly overdone, and it is time to call a halt. There is not the slightest probability that our solar system was ever a part of a spiral nebula, and such a suggestion is simply misleading and mischievous. The great circularity of the planetary orbits shows the absurdity of such an hypothesis. . . . Least of all can we expect any light from the much exploited spiral nebulæ, which as M. Poincaré justly remarks, may be other galaxies. It is time, therefore, to drop such spirals from our text-books, or to candidly admit that we are quite in the dark as to their true significance.

In the last paper of See recently published in the *Astronomische Nachrichten* and several other places we read:

The solar system was formed from a spiral nebula, revolving and slowly coiling up under mechanical conditions which were essentially free from hydrostatic pressure. And spiral nebulæ themselves arise from the meeting of two or more streams of cosmical dust. The whole system of particles has a sensible moment of momentum about some axis, and thus it begins to whirl about a central point, and gives rise to a vortex. In the actual universe the spiral nebulæ are to be counted by the million, and it is evident that they all arise from the automatic winding up of streams of cosmical dust, under the attraction of their mutual gravitation. . . . When the nebula rotates and the coils wind up in such a way as to leave open spaces between the coils, or at least freedom from sensible hydrostatic pressure, the usual result is the development of a system made up of small bodies, such as the planets compared to the greatly preponderant sun, or the satellites compared to the much greater planetary masses which control their motions. In the solar system where the conditions are accurately known this is proved to have occurred; and it was repeated so many times always with uniform results giving a large central mass and small attendant bodies that the general law for this condition is clearly established.

Thus we see the variety of "consistent" conclusions recently reached by the twenty-five years of uninterrupted work on this subject.

At the end of this paper See admits its value in the following modest terms:

It has seemed advisable to call attention to the cause of the roundness of the orbits of the planets and satellites, because it appears likely that the criteria now introduced may go far towards clearing up the mystery which has always surrounded the origin of our solar system.

In See's paper there are only two points of divergence from the ideas fully developed by Professor Chamberlin and myself. The first is that spiral nebulas have their origin in "the meeting of two or more streams of cosmical dust." The second is that satellites are captured bodies. This latter view has been advanced by many amateurs and a few astronomers. It was considered in my writings quoted above, and rejected for what seemed to me to be good reasons. The resisting medium on which so much stress is laid is simply a special case of the collisions of *any* character considered by Professor Chamberlin and myself.

The quotations above are sufficient to remove the clouds which See's pretensions of long study of, and valuable contributions to, this subject might raise in the minds of those not particularly familiar with the history of recent developments in cosmogony. I wish to point out that notwithstanding the evidence furnished by his 1906 paper of his familiarity with our work, and in spite of the fact that at his request I furnished him reprints of my papers several months in advance of his recent publication, there is in it no direct or indirect reference to Professor

Chamberlin or myself. Ordinarily such conduct justifies the use of strong terms in characterizing it, but in the present case I believe astronomers and others who are familiar with the situation will fully agree with me that these aberrations are more deserving of pity than of censure. F. R. MOULTON

June 10, 1909

COMMUNICATING WITH MARS

TO THE EDITOR OF SCIENCE: In view of the recent proposals for opening communication with the planet Mars, as reproduced by the European press from American newspapers (with accompanying portraits), no truly patriotic American can fail to feel a thrill of pride and exultation at the thought that it his country that is solving this great cosmic problem. It is time to sound the alarm, however, for there are indications that an attempt will be made to rob us of the honor after all. A distinguished French astronomer has recently published a letter on the subject, in which, while giving a small measure of approval to the American projects, he broadly intimates that the last word has not been said. The Germans are keeping very quiet, but it is rumored that Count Zeppelin is thinking, and in commercial and manufacturing circles there is great though silent activity in the direction of trying to ascertain in advance just what articles now "made in Germany" are likely to be most in demand among the inhabitants of Mars when once communication is opened. Assuming that the planet is correctly named (and it has borne the name for hundreds of years without protest), the great Krupp establishment is looking for a practical monopoly of trade, and to meet the expected emergency it has taken options on all the land adjacent to the present planet. Their engineers are known to entertain the opinion that it will be a comparatively simple matter to send to Mars a 14-inch 70 foot gun, first, of course, hermetically sealing it in the aluminum cylinder. If it should not reach the exact spot where it is wanted it can readily be transported anywhere by canal boat.

Having all this information, which has only recently come to me, I have decided to protect American interests by making premature publication of my own scheme for signaling to our celestial neighbor, which, for efficiency, simplicity of arrangement and ease of operation altogether surpasses, I think, all will admit, anything hitherto before the public. It is well known, even among astronomers, that as the orbit of the earth lies between the sun and that of the planet Mars, the dark side of the earth must, at regular intervals and for considerable periods of time, be turned toward Mars.

A hole through the earth would, at this time, allow the passage of a beam of sunlight, the intelligent interruption of which could be made to appear as a series of signals, using the Morse (E. S.) code or any other that might be chosen.

That is all; the problem is solved in this simple way.

One can readily understand how the system might be also put in operation on the moon, if the lunatics would only bore a hole through which the sun might shine when the dark side of the moon was toward us and then arrange a device for cutting off this beam of light at will. For our immediate purpose of wigwagging to Mars such a hole must necessarily be several miles in diameter. Although some minor difficulties in the way of the execution of this plan remain to be overcome, many of the details are already settled, including the selection of the spot where such an opening might best be made in the interests of mankind generally. T. C. M.

DRESDEN, GERMANY,

May, 1909

P. S. I regret that I have no portrait to send with this.

"TYPHOID MARY"

MUCH has appeared in the press of late concerning the unfortunate woman who for two years past has been held a prisoner upon North Brothers Island by order of the board of health. On June 29 she appeared before Supreme Court Justice Giegerich on a writ of habeas corpus, sued out by her attorney to obtain her release. Judging from the evidence,