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ORDINARY GENERAL MEETING.*

PROFESSOR LIONEL BEALE, V.P., F.R.S., IN THE CHAIR.

The Minutes of the previous Meeting were read and confirmed.

The following paper was read by the Secretary, in the absence of the Author :—

THE NEBULAR AND PLANETESIMAL THEORIES OF THE EARTH'S ORIGIN. By WARREN UPHAM, M.A., F.G.S.Amer. (Hon. Corresponding Member.)

ASTRONOMY and geology, chemistry and physics, with their very useful arm or ally, spectroscopy, seek together to discover the origin and development of the earth and the moon, of the sun and his retinue of planets, and of the starry universe:

> "In the beginning how the heavens and earth
> Rose out of chaos."

While we are assured that they "declare the glory of God," and that "all things were made by Him," it has also been learned not less surely that He has worked by His established physical and chemical laws in the creation of suns and worlds. We may partially discern the laws, or methods of working, through which the Creator has made and upholds the myriads of stars and our relatively small, but yet vast, solar system; but beyond all that we know, as, for example, of the laws of gravitation, everywhere lies mystery which baffles our comprehension.

How all matter is influenced by all other matter and drawn toward it, how the earth began and came to its present condition, how the crystal or the plant or the animal grows, "great things and unsearchable, marvellous things without number," proclaim an omnipresent and omnipotent Creator and Ruler.

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* Monday, March 20th, 1905.

To learn continually more and more of His thoughts, as revealed in His works, is the highest reward of the student of nature; and increased powers of vision, whether with the telescope or the microscope, open ever-widening fields of knowledge and new problems to be solved. In every direction the search for truth reaches no limit; and in the themes of this paper, although much has been ascertained, infinitely more remains for inquiry.

The nebular hypothesis or theory may well be called the grandest generalization in all the range of the natural sciences. As most elaborately stated by the eminent astronomer and mathematician, Laplace, in his *Exposition du Système du Monde,* this theory traces the beginning and development of the solar system from an original gaseous nebula, an exceedingly tenuous and intensely heated cloud of matter, extending in a spheroidal form beyond the orbit of Neptune, the outermost planet. By its gravitation and resulting contraction, the nebula is supposed to have acquired a movement of rotation, with polar flattening. Whenever the outer equatorial belt of the revolving nebula attained a centrifugal force exceeding the attraction toward the central mass, a part would be left behind, either as a relatively small revolving nebulous body, or as a ring of such matter, somewhat like the rings of Saturn. Later the ring, if it was at first of that form, would be broken; and, finally, the detached mass would be gathered into a globe, which, in its condensation, would form satellites in the same manner as outer parts of the great central mass formed the successive planets.

Under this theory the principal features of our planetary system, implying unity of origin and development, find a consistent general explanation. Professor Charles A. Young has enumerated these features, which could only have originated by some long process of orderly evolution, as follows:—*

1. The orbits of the planets are all *nearly circular.*
2. They are all nearly *in one plane,* excepting considerable divergence of some of the little asteroids.
3. The revolution of all is *in the same direction.*
4. There is a curiously *regular progression of distances* between the planetary orbits.
5. There is a roughly regular *progression of density,* increasing both ways from Saturn.

* *Text-Book of General Astronomy,* 1893, p. 515.

6. The plane of the planets' rotation *nearly coincides with that of the orbits.*
7. The direction of the rotation *is the same as that of the orbital revolution,* excepting probably the two outermost planets.
8. The *plane of orbital revolution of the satellites* is nearly coincident with that of the planet's rotation.
9. The *direction* of the satellites' revolution also coincides with that of the planet's rotation.
10. The largest planets rotate most swiftly.

That these wonderfully harmonious relations of the planets to each other and to the sun, and of the satellites to the planets, could have originated by any fortuitous concourse of matter, like the visits of comets which may come from any part of the heavens, is utterly improbable. There is not one chance in millions for the order of the solar system to have come to pass without a systematic development; but the sublime theory of Laplace, in its main outlines, with modifications as required by further knowledge of astronomical and physical laws, or some other nebular theory, perhaps the one most fully reviewed in this paper, accounts for all this majestic unity of the Creator's plan in launching the earth and its associate planets to revolve around the enormously larger central sun.

Instead of an originally gaseous and very hot condition of the parent nebula, as supposed by Laplace, some prominent English physicists and astronomers have thought that in its earliest definable condition it consisted of meteorites, that is, particles and little masses of solid and cold matter. Sir Norman Lockyer, reasoning from his extensive investigations in spectrum analyses, states this view as follows* :—" Nebulæ are really swarms of meteorites or meteoritic dust in the celestial spaces. The meteorites are sparse, and the collisions among them bring about a rise of temperature sufficient to render luminous some of their chief constituents."

Besides the testimony of the spectroscope concerning the characters of the nebulæ, we may consider the rings of Saturn, which are very thin but have great areal extent, as probably a strong evidence of the meteoritic derivation of the p anet and the sun. Richard A. Proctor, after stating the physical

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* *The Meteoritic Hypothesis, a Statement of the Results of a Spectroscopic Inquiry into the Origin of Cosmical Systems*, 1890, p. 322.

impossibilities of the existence and permanence of these unique rings as either solid or liquid continuous bodies, wrote* :—

"The sole hypothesis remains that the rings are composed of flights of disconnected satellites, so small and so closely packed that, at the immense distance to which Saturn is removed, they appear to form a continuous mass."

In other words the Saturnian rings are made up of myriads of separately moving small masses, which are doubtless similar to the stony meteorites that fall rarely on the earth.

Again, the origin of the hundreds of asteroids, or minor planets, mostly no more than a few miles in diameter, but including several from 100 to perhaps about 300 miles in diameter, seems very readily explained under this modification of the nebular theory.

Professor Young well says† :—

"The *meteoric* theory of a nebula does not in the least invalidate, or even to any great extent modify, the reasoning of Laplace in respect to the development of suns and systems from a *gaseous* nebula. The old hypothesis has no quarrel with the new."

Another theory, which differs more widely from that of Laplace, has been very recently proposed by Professor T. C. Chamberlin, of the University of Chicago, who names it the Planetesimal Hypothesis. His studies in this direction have been in progress about five years, with publication of preliminary papers,‡ preparing the way for the new hypothesis; but its first somewhat detailed statement in print has appeared since the beginning of the present year.§ In this latest paper, Professor Chamberlin gives the following principal outlines of his researches for a new and more applicable nebular theory, especially having in view its relation to the origin of the earth.

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* *Saturn and its System*, second edition, revised, 1882, p. 135.

† *Text-Book of General Astronomy*, p. 526.

‡ "An Attempt to Test the Nebular Hypothesis by the Relations of Masses and Momenta," in the *Journal of Geology*, Chicago, vol. viii, pp. 58–73, Jan.–Feb., 1900. "On a possible Function of Disruptive Approach in the Formation of Meteorites, Comets, and Nebulæ, *Journal of Geology*, vol. ix, pp. 369–392, July–August, 1901.

§ "Fundamental Problems of Geology," in *Year Book*, No. 3, for 1904, of the Carnegie Institution of Washington, published in January, 1905, pp. 195–258.

"Under the typical form of the planetesimal hypothesis it is assumed that the parent nebula of the solar system consisted of innumerable small bodies, planetesimals [infinitesimal planetoids], revolving about a central gaseous mass, somewhat as the planets do to-day. The hypothesis, therefore, postulates no fundamental change in the system of dynamics after the nebula was once formed, but only an assemblage of the scattered material. . . .

"An inquiry into the possible modes by which the planetesimal condition might arise revealed several possible methods. Such condition might arise from a nebula that was originally gaseous. If, for example, it be supposed that the parent nebula was a gaseous spheroid, and that it detached material from its equatorial belt molecule by molecule, rather than by rings, as postulated by Laplace, these molecules would probably become planetesimals instead of members of a true gaseous body . . . There is reason to believe that this method would really be almost the only systematic one by which a gaseous spheroid of the Laplacian type would detach material from its equatorial belt. . . .

" . . . To develop the hypothesis as definitely and concretely as possible, I have further chosen a special case from among those that might possibly arise, the case in which the nebula is supposed to have arisen from the dispersion of a sun as a result of close approach to another large body. The case does not involve the origin of a star nor even the primary origin of the solar system, but rather its rejuvenation and the origin of a new family of planets . . . This particular sub-hypothesis was selected for first development (1) because it postulates as simple an event as it seems possible to assign as the source of so great results; (2) because that event seems very likely to have happened; (3) because the form of the nebula supposed to arise in this way is the most common form known, the spiral; and (4) because spectroscopic observations seem at present to support the constitution assigned this class of nebulæ . . .

"The continuous spectrum is interpreted to mean that their chief luminous material is in a liquid or solid state. . . . As the liquid condition is limited to a rather narrow range of temperature, and as this range is very different for different material, it is improbable that any large portion of a nebula is in this state, and the whole may be conveniently treated as though it were formed of solid matter, but matter in a finely divided condition. This last qualification seems necessary, for the volume of these nebulæ is often very great, and yet they appear to intercept but little light and give no signs of great attractive power.

"The prevailing form of these nebulæ is the *spiral*, as determined by the late Professor Keeler, and this form particularly characterizes the smaller nebulæ recently brought to knowledge by improved instruments and manipulative skill. Those newly discovered nebulæ are estimated to number at least ten times the whole number previously known. From the superior number of spiral nebulæ it is a safe inference that their peculiar forms represent some prevalent process in celestial dynamics. This is in itself a reason why research should turn to them, by preference, for the origin of the present solar system. . . .

"A notable and seemingly very significant feature of these nebulæ is the presence of *two dominant arms* that arise from diametrically opposite sides of the nucleus and curve concentrically away. No single arm-spiral of the watch-spring type has been found, so far as I am aware. There are often more than two arms in the outer part, and there is much irregularly dispersed matter, but even in the more scattered forms the dominance of two arms is discernible.

"A second feature of note is the presence of numerous *nebulous knots* or partial concentrations on the arms and more or less outside them. So, also, the more diffuse nebulous matter is unequally distributed, and in some of the forms, regarded as youngest, dark spots and lines emphasize the irregularity.

"All these features go to show that these forms are controlled, not by the support of part on part, as in a continuous body or in a mass of gas or even in a definite swarm of quasi-gaseous meteorites, but by some system of combined kinetic energy and gravity which *permits independence of parts*. It is, therefore, conceived that the innumerable solid or liquid particles which the continuous spectrum implies, revolve about the common center of gravity as though they were planetoidal bodies. If this were certainly known to be the case, these might well be called *planetesimal nebulæ*.

"It is clear from the tenuity of these nebulæ, as seen from the side of the spiral, that they are disk-like, and this is directly shown to be so when they are seen obliquely. In their disk-like shape these nebulæ conform to the mode of distribution of matter in the solar system. Within the area of their disks, also, the distribution is irregular, as it is in the solar system—a fact too much overlooked by reason of our predilection for symmetry, under the influence of the symmetrical Laplacian conception.

"All of the more familiar spiral nebulæ have dimensions that vastly transcend those of the solar system, and they cannot be taken as precise examples of the solar evolution. . . . It is to be hoped, however, that the present rapid progress in the perfection of instruments and of skill will soon bring within the reach of successful study some of the smaller spiral nebulæ that represent the solar system more nearly in mass and proportions.

"With this much of knowledge and of limitation of knowledge relative to existing nebulæ, the construction of a working hypothesis required not a little resort to supplementary deductive and hypothetical considerations. 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—a system perhaps in no very essential respect different from the present one . . .

"To this conception of an ancestral sun with an undefined antecedent history as a star, question will arise at once as to a sufficiency of energy for the sun's maintenance through such a prolonged history. . . . This objection is based on the assumption that the sun's heat and light are derived *almost wholly* from self-compression, as urged by Helmholtz. This self-compression has usually been computed on the basis of certain limiting assumptions, the validity of which is open to question. . . . The extraordinary energies displayed by radio-active substances are doubtless but an initial demonstration of immeasurable energies resident in other forms of matter and in the constitution of the sidereal system and competent for its maintenance for unassignable periods. . . .

" . . No appeal is here made to collisions as a source of the parent nebula of the solar system, but only to an approach of the ancestral sun to another large body, and this approach is not assumed to have been very close. . . .

"Our present sun shoots out protuberances to heights of many thousands of miles, at velocities ranging up to 300 miles per second and more. If it were not for the retarding influence of the immense solar atmosphere, some of these outshoots would doubtless project portions of themselves to the outer limits of the present system, and perhaps in some cases quite beyond it, for the observed velocities sometimes closely approach the controlling limit of the sun's gravity, if they do not actually reach it. . . . If with these potent forces thus nearly balanced the sun closely approaches another sun or body of like magnitude, suppose one several times the mass of the sun, since it is regarded as a small star—the gravity which restrains this enormous elastic power will be *relieved along the line of mutual attraction*, on the principle made familiar in the tides. At the same time the pressure transverse to this line of relief is increased. Such localized relief and intensification of pressure must, it is believed, result in protuberances of exceptional mass and high velocity. According to the well-known tidal principle, these exceptional protuberances would rise from opposite sides, and herein lies the assigned explanation of the prevalence of two diametrically opposite arms in the spiral nebulæ.

"Nothing remotely approaching a general dispersion of the ancestral sun seems to be required. The present planets and their satellites altogether amount to about one-seven-hundredth part of the mass of the system. Simply to supply the required planetary matter, the protuberances need include but this small fraction of the ancestral sun. However, some considerable part of the projected matter must probably have been gathered back into the sun, and some part may possibly have been projected beyond the control of the system. Making allowances for both these factors, the proportion of the sun's mass necessarily involved in the protuberances is still very small. Apparently 1 or 2 per cent. of the sun's mass would amply suffice. . . .

"The distal portions of the protuberances would obviously be formed from the superficial parts of the sun; while the later portions of the ejections forming the proximal parts of the arms would doubtless come mainly from lower depths, and hence would probably contain more molecules of high specific gravity. In this seems to lie a better basis for explaining the extraordinary lightness of the outer planets and the high specific gravities of the inner ones, than in the separation, from the extreme equatorial surface of a gaseous spheroid, of successive rings whose total mass only equaled one seven-hundredth part of the original nebula.

"It seems consistent with the conditions of the case to assume that the protuberances would consist of a succession of more or less irregular outbursts, as the ancestral sun in its swift whirl around the controlling star was more and more affected by the latter's differential attraction; and hence the protuberances would be directed in somewhat changing courses, and would be pulsatory in character, resulting in rather irregular and somewhat divided arms, and in a knotty distribution of the ejected matter along the arms. These knots must probably be more or less rotatory from inequalities of projection.

"It is thus conceived that a spiral nebula, having two dominant arms, opposite one another, each knotty from irregular pulsations, and rotatory, the knots probably also rotatory, and attended by subordinate knots and whirls, together with a general scattering of the larger part of the mass in irregular nebulous form, would arise from the simple event of a disruptive approach. . . .

"The problem of the luminescence of nebulæ is confessedly a puzzling one. There is little ground for assigning general incandescence to matter so obviously scattered and tenuous, and possessed of such an enormous radiating surface. The assignment of the light to the collision of meteorites, as done by Lockyer, encounters both dynamic and spectroscopic difficulties. The recent discoveries of the luminescent properties of radio-active matter and of its power to awaken luminescence in other matter offers some hope of a solution. . . .

"The solution of the problem may, however, lie along electrical lines. At present it seems more probable that the luminescence arises from some agency that acts at low temperatures, than that it is dependent on heat, and hence objections to a planetesimal organisation on the ground of low temperature do not seem to me to have much force. . .

"In attempting to follow the probable evolution of such a spiral nebula, three elements stand out conspicuously; (1) the central mass, obviously to become the sun; (2) the knots on the arms that are assumed to be the nuclei of the future planets and perhaps satellites; and (3) the diffuse nebulous matter to be added to the nuclei as material of growth. In the particular case of the solar nebula it is assumed (1) that the central mass was relatively very great; (2) that the knots were very irregular in size and placed at irregular distances from the center; and (3) that the nebulous portion was very small relative to the central mass and probably large relative to the knots.

" . . . Since all the planetesimals and planetary nuclei were revolving *in the same direction* about the solar mass, the collisions were all overtakes, and could have been violent only to the extent of their differences of orbital velocity, modified by their mutual attractions. These velocities are of a much lower order than the average velocities of meteoritic collisions. Many of the overtakes would obviously be due to differences of velocity barely sufficient to bring about an overtake. When the relative mildness of impact is considered in connection with the intervals between impacts at a given spot, the conviction can scarcely be avoided that *the surface temperature would not necessarily have been high.* It seems probable that it would have been moderate throughout most of the period of aggregation, and certainly so in the declining stages of infall. . . . .

"By graphical inspection of all probable cases, it may be seen that the possibilities of overtake favourable to forward rotation exceed those favourable to retrograde rotation. This holds true on the assumption of an equable distribution of planetesimals, which may fairly be assumed as an average fact, but not necessarily as always the fact; and hence the conclusion is not rigorous, and a backward rotation is not impossible. From the nature of the case, a varying rotation for the several planets is more probable than a nearly uniform one.

"It is also obvious that the impacts on the right and left sides of a growing nucleus, as well as those on the outer and inner sides, might be unequal, and hence *obliquity* of rotation of varying kinds and degrees might arise. As the solar system presents these variations, the method of accretion here postulated seems to lend itself happily to the requirements of the case.

" . . . A planetary nucleus gathers planetesimals that have orbits both smaller and larger than itself, and hence in effect it sweeps a space both outside and inside its own zone. The breadth of this space is dependent on the eccentricity of its own orbit and on the eccentricities of the orbits of the planetesimals it gathers in on either hand.

" . . . For the large planets that have dominated their collecting zones and presumably swept them thoroughly, the reductions of eccentricity are subequal. For the very small bodies that presumably grew but little, *the eccentricities remain large*, for the greater part. For example, the eccentricity of Mercury, the smallest of the planets, remains more than twice that of any other planet. Mars, the next smallest in size, comes next in eccentricity among the planets, while the asteroids, which probably grew but little, have high eccentricities, as a rule. . . .

"To bring out the geological bearings of the planetesimal hypothesis, I have given considerable time to a study of the probable stages of growth of the early earth, of the time and mode of introduction of the atmosphere and hydrosphere, and of the initiation of the great topographic features, together with the leading modern processes. . . .

"Following the postulates of the previous sketch, a nebular knot is assumed to have been the nucleus of the growing earth. . . Assuming that the nuclear mass was quite small, it is inferred that it was composed chiefly of matter of high molecular weight, since light molecules would be liable to escape because of their velocities. The nucleus is supposed to have been originally an assemblage of planetesimals grouped together by their mutual gravity, and to have passed gradually into a solid mass in connection with the capture of outside planetesimals. . . .

"As the solid nucleus thus formed may not have been massive enough to control a gaseous envelope in its earlier stages, a possible atmosphereless stage is to be recognized. Just how massive a planetary body must be to hold permanently an appreciable atmosphere is not accurately computable at present, because of the uncertain value of some of the factors involved. A fairly safe conclusion may perhaps be drawn from known celestial bodies. The moon . . . has no detectable atmosphere, nor has any smaller body, whether satellite or asteroid, so far as known. Mars . . . has an appreciable, but apparently quite limited, atmosphere. The limit between atmosphereless and atmosphere-bearing bodies probably lies between the two, *i.e.*, roundly between one-eightieth and one-tenth of the earth's mass. . . .

"When the growing earth reached a mass sufficient to control the flying molecules of atmospheric material, there were two sources from which these could be supplied for the accumulation of an atmosphere, an external and an internal one. . . .

"In the later stages of organisation, and thence down to the present time, the molecules discharged from all the bodies of the solar system were possible sources of atmospheric accretion. Of these the most important were probably volcanic, and similar discharges from the small bodies that could not hold gases permanently, and discharges from the sun by virtue of the enormous explosive and radiant energies that are there resident.

"As the planetesimals were gathered into the growing earth-nucleus they carried their occluded gases in with them, except as the superficial portion might be set free by the heat of impact. There was thus built into the growing earth atmospheric material. . . . .

"The gases chiefly occluded in meteorites and the crystalline rocks are hydrogen, carbon dioxide, and carbon monoxide in leading amounts, and marsh-gas and nitrogen in small quantities. It is assumed that the gases of the aggregated planetesimals, and hence those of the interior of the early earth, were of the same order of abundance. . . .

"In determining the actual proportions of the constituents of the early atmosphere, the abundance of the supply was probably less decisive than the power of the earth to hold the individual gases. As gravity gradually increased by the growth of the earth from an incompetent minimum, its power to control the heaviest molecules with the lowest velocities was acquired before its ability to hold the lighter ones of higher velocities. . .

"Carbon dioxide would be held some appreciable time before oxygen, and still longer before nitrogen, and all these a notable time before the vapor of water. The inference is that the initial atmosphere was very rich in carbon dioxide, for an abundant supply was correlated with a superior power of retention.

"The amount of oxygen in the early atmosphere is more uncertain, from doubt as to a competent source of supply . . . For the primitive atmosphere there is theoretical need for only enough oxygen to support the primitive plant life until it could supply itself, after which it would produce a surplus . . .

"After the earth acquired the power of holding water-vapor, the supply being abundant, accession doubtless went on for a time as fast as the capacity to hold increased.

"The problem of vulcanism assumes a quite new aspect under the planetesimal hypothesis, if very slow accretion without very high temperature be assumed. It has been taken for granted in the preceding statement that there was volcanic action. It is necessary, therefore, to consider how volcanic action may have arisen, and this involves the more radical question how the high internal temperatures of the earth may have arisen if the earth did not inherit its heat from a molten condition arising from a gaseous origin . . .

"The chief source of internal heat is assigned to the progressive condensation of the growing body as material was added to its surface. The amount of this condensational heat for the full-grown earth, computed on the best data now available, seems to be ample to meet all the requirements of the known geologic ages . . . That heat arising from condensation *solely* would reach the melting temperature of rock in a body one-twentieth of the earth's mass, seems more or less doubtful, but in a body one-tenth of the earth's mass the required conditions would probably be reached.

. . .

"Pressure itself is probably incompetent to melt rock substances that shrink in solidifying, but the high temperatures generated by pressure in the deep interior were constantly moving outward into horizons of lower pressures, where the melting-points were lower. As the computed temperature at the center of the adult earth is about 20,000° C., there would seem to be no lack of heat, in the later stages at least. The essence of the problem lies in its redistribution and in its selective action.

"The material of the interior was originally, by hypothesis, an intimate mixture of planetesimals of various kinds, with such gaseous material as they carried in or entrapped in the process of growth . . . The outward flow of heat in such a mixture must bring some parts to fusibility much before the melting-points of other parts were reached. Local spots of fusion must thus arise. To this fusion the entrapped and occluded gases may be presumed to have contributed and to have joined themselves to the fused masses, and to have aided in giving them fluidity . . .

"It is not necessary to the hypothesis to suppose that volcanic action was an essential preliminary to the acquisition of an atmosphere, for the initial atmosphere may have been supplied from external sources. The apparent vigor and the wide prevalence of volcanic action on the moon, if its pitted surface means vulcanism, as well as the glassy material found in meteorites, whose origin is referred preferably to small atmosphereless bodies, favors the view that the internal gases were given forth abundantly before the earth grew to a mass sufficient to hold them. If this were true, an ample source of atmospheric supply was ready and waiting when the earth first acquired sufficient gravity to clothe itself with a gaseous envelope.

"When the increasing water-vapor of the growing atmosphere reached the point of saturation, it is of course assumed to have taken the liquid form and become a contribution to the hydrosphere. . . ,

"If it be assumed that the earth's growing hydrosphere appeared at the surface when our planet had attained the mass of Mars, whose radius is about 2,100 miles, the subsequent growth would form a shell about 1,900 miles thick. It is not altogether certain that Mars bears water bodies on its surface, but the areas of greenish shades environed by a surface generally ruddy, the polar white caps ('snow caps') that come and go with the seasons, and the apparent occasional presence of clouds, not to appeal to the evidence of aqueous absorption lines in the spectrum reported by some good observers, but unconfirmed by others, lend some support to the opinion that water is present, though perhaps not in the form of definite water bodies. . . .

"Without attempting to fix the precise stage, it is not unreasonable to assume that surface waters had begun their accumulation upon the earth's exterior while yet it lay 1,500 to 1,800 miles below the present surface. The present difference between the radii of the oceanic basins and the radii of the continental platforms is scarcely 3 miles, on the average; so that if the continental segments be assumed to be in approximate hydrostatic equilibrium with the oceanic segments to-day, as seems highly probable, the selective weathering process brought about a difference in depression of only 1 mile in 500 or 600 miles, or about one-fifth of 1 per cent. . . .

"Not only is the evolution of the great abysmal basins and of the continental platforms thus assigned to a very simple and inevitable process, but there is therein laid the foundation for subsequent deformation of the abysmal and continental type.

". . . A theoretical scantiness of time for a prolonged evolution previous to the Cambrian period has been deduced from a molten earth, but this does not apply to the planetesimal hypothesis. The supposed limitation of the sun's thermal endurance would apply if the arguments could be trusted, but their foundation has been cut away by recent discoveries. It is not the least of the virtues of the planetesimal hypothesis that it opens the way to a study of the problem of the genesis and early evolution of life free from the duress of excessive time limits and of other theoretical hamperings, and leaves the solution to be sought untrammeled, except by the conditions inherent in the problem itself, which are surely grave enough.

"It is assumed that the conditions on which life is now dependent were prerequisites to its introduction. As already indicated, an atmosphere and hydrosphere sufficient to sustain life may have been acquired when the earth was about the size of Mars, or one-tenth grown. If, to be conservative, a preliminary growth of twice this amount be allowed, there still remains between this and the Cambrian record the growth of four-fifths of the mass of the earth. So far, therefore, as atmosphere and hydrosphere are concerned, life may have been introduced early in the history of the earth, and may have had a vast interval for development previous to the earliest legible record. There is another essential condition—a sufficiency, but not an excess, of heat and light. If the formation of the parent nebula involved only the outshooting of a small fraction of the ancestral sun, the solar supply of heat and light may not have been so seriously disturbed as to have fatally affected its availability to furnish what was necessary for life at any stage of the earth's growth. . .

" . . . There is little ground for apprehension that the infalling planetesimals would be seriously dangerous to the early forms of life, for in the first place the atmosphere must have been then, as now, an effective cushion, checking the speed of the planetesimals and partially dissipating them, and, in the second place, the early organisms were probably all aquatic and were further protected by their water covering. . . .

"So soon as plants and animals had come into action, all the great factors potential in the earth's physical evolution were in play.

"By hypothesis, volcanic action only began some time after the beginning of the earth's growth, for it was delayed (1) by the lack of sufficient compression in the central parts to give the requisite heat, and (2) by the time required for this central heat to move out to zones of less pressure, where it would suffice to melt the more fusible constituents. But, once begun, it is supposed to have gradually increased in actual and in relative importance until it reached its climax. This obviously came much later than the climax of growth, for it was dependent on the growth to give the increased compression from which arose the central heat on which the vulcanism depended. . . .

"The formations of this period of volcanic dominance, with very subordinate clastic accompaniment, are regarded as constituting the Archean complex, though perhaps only the later portions of the great volcanic series are represented by the *known* Archean."

To give a satisfactory statement of Professor Chamberlin's exceedingly interesting and elaborate theory has thus required very large quotation from his recent publication of it. Only by such direct presentation of his work in his own words could justice be done to this new nebular theory, to which this eminent glacialist was primarily led by his endeavours to explain the causes of the Ice Age, and of its several waxing and waning stages, by periodic changes in the content of carbon dioxide in the atmosphere. Having been an assistant under his direction on the United States Geological Survey during seven years in my work on the Glacial Lake Agassiz, it is with great pleasure and pride that I can claim for him and for America the distinguished honour of having developed this great theory of the origin of the earth. It will certainly introduce into geology and geophysics many new and fruitful methods of observation and research. Indeed, nearly all the great fields of theoretical geology now require renewed investigation, by which the planetesimal hypothesis shall be tested.

An earlier address by Professor Chamberlin, partially setting forth his studies in this direction, was given before the Geological Society of America, in Washington, D.C., on January 1, 1903, entitled "Origin of Ocean Basins on the

Planetesimal Hypothesis"; but only a very brief abstract or note of this address was published.*

From the oral statements in this and other unpublished addresses, Professor Herman L. Fairchild, Secretary of the Geological Society of America, presented on January 1st, 1904, at the sixteenth annual meeting of that society, an able discussion of the geologic bearings of the new hypothesis.†

The recent detailed publication of it, in *Year Book No. 3 of the Carnegie Institution*, from which I have so largely quoted, has no diagrams or other graphic illustrations; but such desirable aids for the more definite development of the subject, with ample treatment of its relations to geology, are intended to be published soon, in the second volume of a geological text-book by Professors T. C. Chamberlin and R. D. Salisbury, whose first volume of this work was issued early last year.‡

Chamberlin has contributed greatly to the establishment of an acceptable nebular theory, consistent with the known relations of the planets, their satellites, and the sun, by his derivation of the solar system from a spiral nebula, and by his indicating the probable mode of origin of such nebulæ, which abound by tens of thousands throughout the starry heavens, as discovered by the most powerful telescopes.

Both the meteoritic hypothesis of Lockyer and the planetesimal hypothesis of Chamberlin seem to me probably true in their regarding the nebulous matter from which planets and suns are made as having become mostly solid, though finely divided, and as very cold, being in almost absolutely cold and immensely extended space, previous to the condensation and segregation which formed it into worlds and stars.

During the accumulation of the planets and their satellites, much or perhaps nearly all of the nebulous matter forming them had remained, until thus gathered as great bodies, apparently in solid and cold molecules or in small masses brought together by their gravitative attraction, as seems reliably evidenced by the rings of Saturn and by the many little asteroids.

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* *Bulletin Geol. Soc. America*, vol. xiv, p. 548, March, 1904; and *Am. Geologist*, vol. xxxii, p. 14, July, 1903.

† "Geology under the Planetesimal Hypothesis of Earth-Origin," *Bulletin Geol. Soc. America*, vol. xv, pp. 243–266, published June 23, 1904; and *Am. Geologist*, vol. xxxiii, pp. 94–116, Feb., 1904.

‡ *Geology*. In two volumes. Vol. I. Geologic Processes and their Results, New York, Henry Holt and Co., 1904, pp. xix, 654.

Coming to the question whether the accumulation of so large a body as the earth took place without its becoming intensely hot and molten, somewhat like the sun, we have first the observations and theories of geology to aid in giving an answer, and these may be advantageously supplemented by the physiographic features of our satellite, the moon. It has been long held by geologists that the downward increase of heat in the earth's crust, present volcanoes, the widely distributed evidences of ancient volcanic action, and thermal metamorphism of great rock formations, indicate an internal temperature which must fuse any known rocks, unless they are prevented from this by overlying pressure. The new hypothesis of Chamberlin accounts for vulcanism, and for all that we know of the earth's internal heat, fully as well as the Laplacian hypothesis of condensation of an intensely hot gaseous nebula, while it better accords with the physical and dynamic relations of the planets and sun.

If our inquiry be turned to the moon, we see a most wonderful record, as it is generally regarded, of extinct volcanic action, implying a formerly very hot and probably almost wholly molten state of that globe, which has a little more than one-fourth the diameter of the earth. These two companion globes were doubtless accumulated similarly. The moon, after acquiring its present size, had multitudes of volcanoes which left round craters, or parts of their crater rims, of varying dimensions from those at the limit of telescopic vision up to one with a diameter of about 800 miles, or nearly four-fifths of the moon's radius. So great a lake or sea of molten rock, similar to the calderas of the Hawaiian volcanoes, but of vastly larger area, whose crater rim is partially preserved in the lunar Carpathian-Apennine-Caucasus chain of mountains, could only exist when much of the interior of the moon was melted. It seems possible and indeed probable, therefore, that the earth, whether formed as supposed by the old or the new nebular hypotheses, was nearly or quite all melted during a considerable part of the time of its accumulation. The planets undoubtedly tended in some degree toward the same intensely hot condition, which is reached by the sun and stars in the concentration of originally nebulous matter.

But another explanation of the origin of the very abundant small and large crateriform features of the moon has been advocated by G. K. Gilbert, of the United States Geological

Survey.* This very remarkable and ingenious explanation seems largely identical with the later planetesimal hypothesis of Chamberlin, so far as that hypothesis deals with the segregation of the originally nebulous matter to form planets and satellites. Mr. Gilbert writes:

" . . . It is my hypothesis that before our moon came into existence the earth was surrounded by a ring similar to the Saturnian ring; that the small bodies constituting this ring afterward gradually coalesced, gathering first around a large number of nuclei, and finally all uniting in a single sphere, the moon. Under this hypothesis the lunar craters are the scars produced by the collision of those minor aggregations, or moonlets, which last surrendered their individuality.

" . . . The introduction of the hypothesis of a Saturnian ring thus accomplishes much toward the reconciliation of the impact theory with the circular outline of the lunar craters. . . .

" In fine, the hypothesis of the Saturnian ring, by restricting the colliding bodies to a single plane, by substituting a low initial velocity and thus rendering the moon's attraction the dominant influence, and by introducing a system of directions controlling, and therefore adjusted to, the moon's rotation, relieves the meteoric theory of its most formidable difficulty. It also explains in a simple way the abundance of colliding bodies of a different order of magnitude from ordinary meteorites and aërolites. . . .

" The velocity of impact, depending chiefly on the moon's attraction, must be supposed to have increased gradually as the moon grew. In the closing stages of the process it did not vary greatly on either side of one and one-half miles per second, and the phenomena of the present surface may be discussed on the basis of that velocity. The energy due to that velocity would more than

* "The Moon's Face, a Study of the Origin of its Features," address as retiring President, delivered December 10, 1892, *Bulletin of the Philosophical Society of Washington, D.C.*, vol. xii, pp. 241–292, with one plate and 14 figures in the text; published April, 1893.

Referring to early suggestions of meteoric accumulation of the moon, and of other cosmic bodies, Mr. Gilbert said in this paper (1892): "I have discovered no published statement of meteoric theories more than twenty years old, but the idea is older and various obscure allusions indicate that it was earlier in print. Proctor makes a meteoric suggestion in 1873 (*The Moon*, p. 346), and advocates it in 1878 (*Belgravia*, vol. xxxvi, p. 153). A meteoric theory is said to be contained in *Die Physiognomie des Mondes*, by 'Asterios,' Nordlingen, 1879. A. Meydenbauer advances another in 'Sirius,' for February, 1882."

With these publications, compare *The Meteoritic Hypothesis*, 1890, by Lockyer, before cited, and a most important paper by Prof. George H. Darwin, "On the Mechanical Conditions of Swarms of Meteorites and on Theories of Cosmogony," *Phil. Trans. Royal Society*, 1888.

suffice . . . to melt the moonlet if it were composed of ordinary volcanic rock, and provided all of the energy were applied to the heating of the moonlet. Practically only a portion of it was thus applied; another portion produced heat in the contiguous tract of the moon's material; yet another was consumed in the deformation of moonlet and moon resulting in the crater, and another resulted in modifications of the moon's motions, changing its orbit, its orbital velocity, its axis, and its rotational velocity. The energy converted into heat might be regarded as the remainder after deducting all other effects, and the resulting temperatures would be further conditioned by the distribution of heat in the colliding masses.

"Since the area of the moon's surface directly struck by the moonlet is a function of the square of the diameter of the moonlet, while the energy applied to that area, being measured by the mass of the moonlet, is a function of the cube of its diameter, more energy would be applied to a unit of space in the case of large moonlets than in the case of small, and the temperatures caused by large moonlets would therefore be greater. To this relation I ascribe the restriction of inner plains, indicative of fusion, to the larger craters. . . .

"In the breaking up of the postulated pre-lunar ring there were at first many centers of aggregation—were the moon the only center, the scars of impact would all be small. So long as the masses were small the process of aggregation developed little heat, for the heat of impact depended almost wholly on velocities created by mutual attractions. That particular moonlet which became the nucleus of the moon may therefore be conceived as cold, or at least as sufficiently cool to be solid. As the moon's mass grew, the blows it received were progressively harder, and for a time their frequency also increased. The rate of heating probably reached and passed its maximum while the mass was materially less than now. During the whole period of growth the surface lost heat by radiation, but the process of growth cannot have been slow enough to permit the concurrent dissipation of all the impact heat. On the one hand, there should have been some storage of heat in the interior, and on the other hand, the stored heat can never have sufficed for the liquefaction of the nucleus. Toward the close of the process, when blows were hard but rare, liquefaction was a local and temporary surface phenomenon, but the general temperature of the surface was low. Impact heat, being evolved simultaneously in the surface and the subsurface, was dissipated more rapidly from the surface, so that there was a subsurface zone of relatively high temperature. The zone thus inferred deductively is also inferred inductively from the disparity of cavities and rims in the case of large craters; but, on the other hand, there is little evidence of the wrinkling which, theoretically, should result from the adjustment of a cold crust to a cooling nucleus. . . . It is therefore probable that the final shrinkage of nucleus was small, and the antecedent storage of heat correspondingly small. During the whole period of growth the body of the moon was cold."

After thus stating the hypothesis of Gilbert for the origin of the moon, in his own words, it is not needful to consider here in detail the numerous arguments which favour vulcanism, instead of impacts, as the cause of the moon's craters. The adoption of Gilbert's explanation of the physiography and development of the moon would go very far toward conclusive verification of the planetesimal hypothesis; but Chamberlin evidently thinks that volcanic origin of the lunar craters is more probable.

Gilbert considers the whole process of the moon's gathering its formerly scattered material to have been completed at least before the deposition of the earth's Paleozoic sediments, else they would here and there reveal evidences of collision of some of the portions of the previous ring matter, since these must have fallen not only on the moon but in like manner on the earth. Whether the craters of the moon resulted from meteoric aggregation or from vulcanism, the very steep and high mountains of the crater rims have doubtless remained through very long ages unaffected by agencies of erosion, because of the absence of atmosphere.

Geologic antiquity, as hitherto studied, falls far short of reaching back to the time of completion of the creation of these companion globes, the earth and its satellite, in nearly the same size and conditions which they have now. But in the new views opened by the hypotheses noticed in this paper the range of geologic inquiries and theories is extended almost inconceivably farther back, through the laying of "the foundation of the earth."

DISCUSSION.

Colonel MACKINLAY.—I understand Mr. Gilbert tells us on page 202, that what we call volcanoes in the moon are masses roughly comparable to the belt left on a wall when a snowball strikes it, and not volcanoes at all. I must confess it has always struck me as a very strange thing that the moon, which is so much smaller than the earth, is so much richer in volcanoes, and that they should be so very much larger than terrestrial ones. I never heard anyone give an explanation of this difference. I think it is very difficult to believe they can be volcanoes at all, and I am glad to think there are theories to account for the mass of rings on the moon's surface.

The SECRETARY.—Sir Robert Ball and myself and my son paid a visit several years ago to the Auvergne district of Central France, a district of recently extinct volcanoes, and he made that journey with the special purpose of observing the extinct volcanoes and their apparent similitude to those of the moon. I am sure Sir Robert Ball is a strong believer in the crater-like forms on the moon's surface as being volcanic. They are very deep depressions because the shadows are deep. The terrestrial ones are smaller than the moon's, but some of those in the Pacific Ocean, the great volcanic islands—are of enormous size—six or seven miles in diameter.

Mr. ROUSE.—It occurred to me that the impression made by a snowball upon another ball, or upon a wall for that matter, would not have been like that of the volcanic walls on the moon, because there would have been an inward slope as well as an outward, whilst they present the appearance of a perpendicular wall without. If any soft body is hurled against another there will be an inner slope of considerable deposit. There will be an inner very considerable slope greater than the outer.

Then it has also occurred to me that if the moon itself was in at all a soft condition, as we may suppose it was at that time, that there would be also a depression in the moon—not only a flat appearance which looks like the continued level of the moon inside the volcanic wall, but there would be a hollow.

The SECRETARY.—There is one difference between the extinct volcanoes of Auvergne and those of the moon. In Auvergne the lava flows break down the walls of the circle, which is generally formed of volcanic ash, but through which molten lava is coming up and filling the great bowl gradually up. It has broken down that rim in some places of least resistance, and then you have a stream flowing out for several miles, and so little covered with vegetation that you might think it was only a few years since they had ceased to flow.

Rev. JOHN TUCKWELL, M.R.A.S.—It will not be possible for us to spend time enough to discuss this nebular hypothesis to anything like its full extent.

There are great difficulties in the way of the acceptance of this new hypothesis when compared with the older hypothesis, more or less modified, of Laplace. Matter in its original condition was no doubt extremely attenuated, and in this extremely attenuated condition it hardly appears possible for us to believe that it was heated. The temperature of cosmic space I think is said to be something like 460° or 470° F. below zero. When we look upon such objects as comets, their tails, which consist of matter in an extremely attenuated condition, certainly cannot be regarded as a fire mist or anything of that kind. With regard to its motion the rotating and spiral nebulæ are certainly very suggestive of the original motion which resulted in the formation of the central sun and planets. I do not think it is necessary to the older hypothesis that we should suppose that the whole mass of the original nebulæ formed into one compact whole with a flattened surface. We may still accept it together with the suggestion made here, that various nuclei became formed. But when we go right back to their origin, and to the character of the original motion of the nebulæ upon either hypothesis, we come to that state of things when we are obliged to suppose the assertion of the Infinite Will, and the Infinite Wisdom of an Infinite Person. We know of no source whence force could originate except in will. Force may be transformed from one nature or condition to another, but force so far as we know could only originate in will; and thus in the origination of force there is no correlation between the power of the will—the assertion of the power of the will, and the effect produced. In the case of the nebular hypothesis, as we have been accustomed to think of it, you have evidence of the original gaseous condition of matter. This gaseous or nebulous condition may have preceded the granular state, if I may so call it, suggested by the hypothesis of Mr. Chamberlin. It is only necessary to apply a few simple laws of Nature to see at all events how development into subsequent conditions *might* have taken place from matter in its original and gaseous condition. I suppose in that original condition we must regard it as having been atomic. But whether these atoms were the atoms of one primitive substance or atoms possessing different qualities we do not know. By some means or other these atoms must have become combined into molecules, but by what means we do not know. That it did take place at some time or other must be taken for granted.

Then with regard to the formation of the planets out of the mass rotating around its central orb, which ultimately became our sun, we may perhaps blend the two hypotheses and suppose that various nuclei were formed which ultimately became planets. Then comes in the question of heat. When was heat evolved ? There are two ways by which it might have been produced. Heat may have been produced spontaneously by the closer contact of the original atoms or molecules of the planets, for everyone knows that the closer the atoms or molecules of any substance are driven together the greater the heat is which is evolved. But another means is possible. We have recently had evidence of the way in which a gaseous world can become suddenly ignited. Some two or three years ago there was a world observed, since known as *Nova Persei*, which suddenly became incandescent. How this took place we do not know. Sir Robert Ball suggested that it may have come into contact with some other planet or with some large meteorite, and that the impact produced ignition.

Professor LOBLEY.—The subject of this paper to-day is an illustration of the very great activity of scientific men on the other side of the Atlantic, and especially is this the case in the subject of astronomy. During the last two or three decades the American astronomers have achieved very great results. It seems to me that this in a great measure is due to the support given by the rich men of America, and it is an example to the rich men of this country, if we wish our country to maintain its place in the van of science.

There are very many points in this paper; it bristles with points suggestive of remarks. It cannot be adequately discussed in a short time, but may I venture one or two remarks about one or two points ?

I would like to refer to what has been said with regard to the theory of the rings on the moon being caused by impact. It seems a difficult thing to imagine that these were induced by a moonlet. Where has the moonlet got to ? The moonlet did not sink into the moon and there is no evidence of its presence. If it had sunk into the moon it would have left a hollow. Professor Hull has very well referred to the remarkable region of Central France in which you have a number of extinct volcanoes. There is another region in Europe which even more resembles the moon's surface, and that is the Phlegræan fields near Naples, where you have a number of craters quite resembling the moon's craters, but not on the extensive scale of the moon's surface, although a number of the craters on the moon's surface are small.

With respect to the large question of the nebular hypothesis I confess I must coincide with Mr. Tuckwell. It seems to me that Laplace's idea of a heated nebular mass is quite out of the question in cold regions of space. I quite conform to Mr. Chamberlin's theory of the mass being elemental, as it were, and that these atoms combined together would form molecular solids, and thus we get Mr. Chamberlin's original nebulæ. These would unite together and form a central nucleus, and attract more and more of those surrounding them, and thus rapidly grow.

With respect to the growth of the earth, as stated in this paper, I really cannot follow Mr. Upham. He speaks of primitive atmosphere when the earth was only half or less than half the size that it is. There was a gradual growth of the earth from that small mass to the greatness of a planet. In a nebula, such as he assumes, a great number of small bodies that formed one mass would be aggregated to a very considerable size. It could draw and unite others and it would grow to its maximum dimensions in a very short time. It would not require enormous ages for bodies one after another to come into it, and it would rapidly aggregate to itself all that was available for it, with the exception of any small masses which are coming in down to the present day.

With regard to the volcanic hypothesis I must say that something is wanting. The two writers seem to assume that the cause of volcanic heat is internal heat, the central heat of the globe. I have disputed that for a long time. It is practically impossible for volcanic lava to come from 30 miles below the surface. It is impossible for lava to penetrate through solid rocks for that distance; and lava is not due to the central heat of the globe.

There is no mention in this paper of rock-fusing temperature except this. This rock-fusing heat is induced by internal heat, but when it produces the chemical action—which again produces heat—you have a rock-fusing temperature obtained at a very short distance below the surface; and the lava comes from a very short distance below the surface, three or four miles at most.

I will conclude by stating that this is in my opinion an exceedingly important, because a most suggestive, paper, and will give, I think, an impetus to a great amount of thought on this very important subject.

Professor ORCHARD wished to express his entire agreement. I must say I thoroughly agree with what the author says on the first page, that the physical laws of nature are Divine methods of working, and with what he says on the second page, that "to learn continually more and more of God's thoughts as revealed in His works, is the highest reward of the student of nature." It reminds me of the words of Keble, "I thank Thee, O God, for letting me think Thy thoughts after Thee."

The SECRETARY.—I trust you will allow me to be the medium of conveying the thanks of the Institute to Mr. Upham, for this exceedingly important and interesting paper. He was a short time ago elected Honorary Member of the Institute, and in conveying that information to him, which was exceedingly gratifying, as he had long been what the French call *effectif* member, I coupled it with the provision that he should send us a paper for this Session, and notwithstanding that he has on his hands an enormous amount of work, he very kindly sent me the offer of two subjects, and this is the one which I selected. I am not at all sorry that it is the one I selected, and I shall be very pleased in sending him our warmest thanks.

As regards Professor Chamberlin's theory, while recognising its originality and interest, it seems to me to fail in giving a cause for the dominant forces of rotation and revolution by which the solar system is governed. Given the planetesimal conditions, we have to assume the force of gravitation in order that the "little planets" should congregate round centres of attractions; and in order to form planets rotating and revolving in space. There must have co-existed an impulse causing rotation round an axis, and revolution round a central sun; but there does not appear to be any explanation of the origin of these movements in Professor Chamberlin's theory. Nor do I feel disposed to accept the new theory for that of Laplace and Newton regarding the origin of our planet. The form of the earth (that of an oblate spheroid) is very suggestive of an originally molten condition from heat, and geological observations tend to support this view. The objection of

Mr. Tuckwell to the idea of highly heated matter revolving in the low temperature of space can scarcely be reconciled with the existence of the sun surrounded by space, and while agreeing with Professor Lobley that volcanic action does not originate at great depths below the earth's crust, there are zones of matter in a molten condition due to intense heat or otherwise, how could we account for the eruption of basaltic lavas (of several varieties it is true, but essentially similar in composition) at widely distant places over the whole globe for example, the British Isles, Central Europe, Sicily, India, America and Iceland?

COMMUNICATIONS.

From Rev. A. Irving, B.A., D.Sc. :—

Regretting my inability to be present at the reading of Mr. Warren Upham's paper on the "Nebular and Planetesimal Theories of the Earth's Origin," I beg to offer a few remarks as brief as possible thereupon.

Starting with the "protyle" (or prothyle) hypothesis of Sir William Crookes, F.R.S., I have preferred to regard the nebulous matter as entirely in its origin non-differentiated; while it is to the teaching of the "periodic" or natural system of the elements (now so well known to chemists) that we must look for light upon the genesis of the elements (so far as they are known) out of which our planet, with its four components, the barysphere, the lithosphere, the hydrosphere and the atmosphere, is made up. We thus suppose a stage at which the nebulæ consisted of matter in a state of elemental dissociation. By integration of the atomic matter further differentiation proceeded, gravitation came into play as a nucleus was formed with transformation of potential energy into heat, with its expansive force, and dissipation of that energy into space by radiation. These briefly—it is here submitted—are sufficient to account for the inorganic evolution of the globe, when we take into account the selective action of the chemical affinities of the atoms. From such general data I attempted to work out in the "eighties" an outline of the history and genesis of the present order of inorganic nature as that presents itself on our planet, in accordance with *evolutionary law.* This formed the fundamental idea of my graduation thesis for the Doctorate in Science, which was submitted to the University of London in 1888, and was published with considerable additions by Messrs. Longmans and Co., in 1889, under the title of *Chemical and Physical Studies in the Metamorphism of Rocks.* The conception, which I was thus able to form of the evolution of this globe, would seem therefore to have anticipated, by a decade or more, a good deal that Mr. Upham has brought forward in the latter part of his paper. I have returned to this subject of late, and have already in MS. a little work nearly ready for the press, in which stress is laid upon the confirmation given to my published views by the "spiral nebulæ" during the last three or four years. This flashed upon my mind, when I had the great pleasure of listening to Sir Robert Ball's splendid address to the Victoria Institute in 1903, and of seeing the photographs which on that occasion he threw upon the screen.

In the work, whose title is given above, will be found a discursus (pp. 22–24) on the results that would follow from the assumption of the following laws and principles:—

1. The law of universal attraction, and the specialised operation of this law in all cases of gravitation.
2. Elevation of temperature, when latent heat is set free either in the liquefaction of aeriform matter or in the solidification of liquids.
3. Transformation of potential energy due to chemical affinity into heat in chemical combination.
4. Dissipation of energy, as it is transformed into heat.
5. Transformation of energy into heat in all cases of impact.
6. Retardation of radiation by non-diathermanous gases anc vapours.
7. The enormous range of condensation-temperatures of th(known chemical elements from that of platinum, osmiun or ruthenium to that of hydrogen gas.

In the second appendix to the above work there appears also a discursus on the moon's surface, as throwing light upon th conditions of our planet in the pre-oceanic stage of its develop ment.

This has also been discussed more recently by Professor Suess o

Vienna in his little monograph, "Ueber den Mond" (*Sitzungsberichten der Kaiserlichen Akademie der Wissenschaften in Wien*). In his "Rede Lecture" before the University of Cambridge in 1893, Professor Bonney, F.R.S., has confirmed a good deal that was contained in my previous work; so also has much that is contained in Lord Kelvin's address to the Victoria Institute in 1897.

I am inclined, upon the whole, to look upon the Huronian phyllites and Grauwacke (as the late Roland D. Irving has described them)* as furnishing the record of the beginning of the hydrosphere; though, as I have pointed out in my book (pp. 54–55), traces of water may have been caught up in the formation, under great atmospheric pressure, of such basic minerals as hornblende, muscovite, etc., of the earlier crystalline schists, even in the pre-oceanic stage; an hypothesis, which has received experimental demonstration since from the splendid work of M. de Kroustchoff of St. Petersburg in the synthesis of such minerals (see *Nature*, vol. xliii, p. 545). With the glimpses we thus get through the æons of the past, we may well agree with the concluding remark of Mr. Upham's paper, when he says, "In the new views opened by the hypotheses noticed in this paper the range of geologic inquiries and theories is extended almost inconceivably further back, through the laying of the foundations of the earth." Only, as I could show more fully if space permitted, *those views have not quite the novelty which he seems to claim for them.* It is pertinent also to remark that Mr. Upham has done good service in bringing them forward in the way he has done, and thus driving another nail or two into the coffin of the Hutton-Playfair-Lyell Uniformitarian dogma. (See remarks by myself in the *Geol. Mag.* for June, 1892, with quotation from Lord Kelvin on "Dissipation of Energy.")

I see no reason for unsaying what I wrote in 1888, when I said, "The Archæan stage of the earth's history is seen to fall into a place in a natural order of development, and one more chapter is added to the history of the operation of the great Law of Evolution, which is written upon all created things. As the mists and clouds thus disperse, our intellectual vision begins to descry a boundary to geologic time, and the physical geologist begins to feel that over

* "Is there a Huronian Group?" (*Amer. Journ. of Science*, vol. xxxiv.)

this question he can join hands with the astronomer and the natural philosopher." (*Op. cit.* p. 97.)

Haeckel and his school may claim all that for their "Monism"; but I hope we may see that it is all included in that still higher monism which is involved in the theistic conception of creation contained in the Bible.

From Rev. J. RATE, M.A.:—My dear sir, I have read with interest Mr. Upham's paper on Laplace's Nebular Hypothesis. He says that R. A. Proctor asserts that there is an improbability of the existence and permanence of the rings of Saturn as either solid or liquid. This must have been written before the discovery of the dark inner transparent ring next to the body of the planet, seen by Laplace in his reflector, and by Dawes in his achromatic, and by Sir David Brewster in Lord Ross' great reflector. Sir David says, *Optics*, p. 499, "I have enjoyed the great privilege of seeing through this noble instrument the satellites and belts of Saturn, the old and *new* ring which is advancing with its crest of waters to the body of the planet." "Laplace has already discovered the transparency of the *new* ring of Saturn," Brewster's *Optics*, p. 500.

"We understand that this telescope" (Rev. M. Craig's achromatic) "exhibits satisfactorily the *new* ring of Saturn, which Laplace and Dawes have found to be transparent, as the body of Saturn is seen through it, but that the correction for spherical aberration in that of Mr. Craig's is not perfect, and that it is necessary to stop the central part of the object glass." Sir D. Brewster's *Optics*, pp. 507–8.

I myself spent a clear night in 1852 with Lord Ross at his great 6 feet (in diameter) reflector, of 57 feet focal length, in which I saw nebulæ which had never before been seen by mortal eye—except in that telescope, and, in his 3 feet reflector, of 26 feet focal length, I saw the planet Saturn with his rings.

I thank you much for sending me the proof of Mr. Upham's paper, and for your able fulfilment of the duties of Secretary to the Victoria Institute.