SCIENCE

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MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Professor J. McKeen Cattell, Garrison-on-Hudson N. Y.

*LORD KELVIN'S ADDRESS ON THE AGE OF THE EARTH AS AN ABODE FITTED FOR LIFE.**

I.

IN the early half of the century, when the more sober modes of interpreting geological data were struggling to displace the cataclysmic extravagances of more primitive times, it is not strange that there should have arisen, as a natural outgrowth of the contest, an ultra-uniformitarianism which demanded for the evolution of the earth an immeasurable lapse of time. It is not remarkable that individual geologists here and there, reacting impatiently against the restraints of stinted time-limits imposed on traditional grounds, should have inconsiderately cast aside all time limitations. It was not unnatural that the earlier uniformitarians, not yet fully emancipated from inherited impressions regarding the endurance of rocks and the immutability of the 'everlasting hills,' should have entertained extreme notions of the slowness of geological processes and have sought compensation in excessive postulates of time. Natural as these reactions from primitive restrictions were, a reaction from them in turn was inevitable. This reaction must have ensued, in the nature of the case, whensoever geologists came seriously to consider those special phenomena which point to

*This JOURNAL, May 12, pp. 665–674, and May 19, pp. 704–711.

limitations of time. But in the earlier part of the century geological attention was absorbed in the great phenomena that testify to the vastness of the earth's history. The time for the study of limitations had not come.

Nevertheless, however inevitab le must have been the ultimate recognition of limitations, it remains to be frankly and gratefully acknowledged that the contributions of Lord Kelvin, based on physical data, have been most powerful influences in hastening and guiding the reaction against the extravagant time-postulates of some of the earlier geologists. With little doubt, these contributions have been the most potent agency of the last three decades in restraining reckless drafts on the bank of time. Geology owes immeasurable obligation to this eminent physicist for the deep interest he has taken in its problems and for the profound impulse which his masterly computations and his trenchant criticisms have given to broader and sounder modes of inquiry.

At the same time, it must be recognized that any one line of reasoning, however logically and rigorously followed, is quite sure to lead astray if it starts from limited and uncertain premises. It is an easy error to press the implications of any single phase of the complex phenomena of geology until they shall become scarcely less misleading than the looser speculations which they seek to replace. A physical deduction which postulates an excessively short geological history may as easily lead to false views as did the reckless license of earlier times. Interpretations of geological and biological phenomena made under the duress of physical deductions, unless the duress be certainly known to be imperative, may delay the final attainment of the real truth scarcely less effectually than interpretations made on independent grounds in complete negligence of the testimony of physics. It is in the last degree important that physical deductions and speculations should be regarded as positive limitations only so far as they are strictly demonstrative. Falling short of demonstration, they are worthy to be regarded as moral limitations only so far as they approach moral certainty. In so far as they are drawn from doubtful assumptions, they are as obviously to be placed in the common category of speculations as are those tentative conceptions which are confessedly but the possible foreshadowings of truth. The fascinating impressiveness of rigorous mathematical analysis, with its atmosphere of precision and elegance, should not blind us to the defects of the premises that condition the whole process. There is, perhaps, no beguilement more insidious and dangerous than an elaborate and elegant mathematical process built upon unfortified premises.

Lord Kelvin's address is permeated with an air of retrospective triumph and a tone of prophetic assurance. The former is fairly warranted to the extent that his attack was directed against the ultra wing of the uniformitarian school of the earlier decades. It might be wholesome, however, to remember that there were other camps in Israel even then. There were ultra-conservatives in chronology as well as ultra-radicals. There were ultra-catastrophists as well as ultra-uniformitarians. Lord Kelvin's contributions have as signally failed to sustain the former as they have signally succeeded in overthrowing the latter. The great body of serious geologists have moved forward neither by the right flank nor by the left, but on median lines. These lines have lain, I think, rather in the field of a qualified uniformitarianism than in the field of catastrophism. Even the doctrine of special acceleration in early times, or at other times, has made only qualified progress toward universal acceptance. The body of competent geologists to-day are probably more nearly disciples of Hutton, Playfair and Lyell than of their opponents. But such is the freedom and the diversity of belief, of attitude and of method, among geologists that *as a class* they cannot be placed either here or there in the schools, nor could they thirty-five years ago.

But we are not primarily concerned with these matters of the schools and of the past. The address presses upon our attention matters of present interest and of profound importance. Referring to his former wide-ranged estimate of the time of the consolidation of the earth, Lord Kelvin says that "we now have good reason for judging that it was more than twenty and less than forty million years ago, and probably much nearer twenty than forty (This JOURNAL, May 12, p. 271), and he gives qualified approval to Clarence King's estimate of twenty-four million years. In the course of the address he speaks of 'strict limitations,' of 'sure assumption,' of 'certain truth,' and of 'no other possible alternative;' he speaks of 'one year after freezing,' and even of 'half an hour after the solidification'; he speaks of 'a crust of primeval granite,' of a depth of 'several centimeters,' and of other details of dimension and of time and of certitude so specifically and so confidently that it must encourage, in the average reader, the impression that the history of the earth is already passing into a precise science through the good offices of physical deduction. Is this really true? Can the uninstructed layman or the young geologist safely repose confidence in these or any other chronological conclusions as determinate? Can these definite statements, bearing so much the air of irrefutable truth, be allowed to pass without challenge? What is their real nature and their true degree of certitude when tested respecting their fundamental postulates and their basal assumptions?

With admirable frankness Lord Kelvin says (This JOURNAL, May 12, p. 672): "All these reckonings of the history of underground heat, the details of which I am sure you do not wish me to put before you at present, are founded on the very sure assumption that the material of our present solid earth all round its surface was at one time a white-hot liquid." It is here candidly revealed that the most essential factor in his reasonings rests ultimately upon an *assumption*, an assumption which, to be sure, he regards as 'very sure,' but still an assumption. The alternatives to this assumption are not considered. The method of multiple working hypotheses, which is peculiarly imperative when assumptions are involved, is quite ignored. I beg leave to challenge the certitude of this assumption of a white-hot liquid earth, current as it is among geologists, alike with astronomers and physicists. Though but an understudent of physics, I venture to challenge it on the basis of physical laws and physical antecedents.

By way of preface it may be remarked that the postulate of a white-hot liquid earth does not rest on any *conclusive* geological evidence, however generally it may be entertained as a probable hypothesis. Students of the oldest known rocks are not yet agreed that these are all igneous even. But granting that they may be all either igneous or pyroclastic, there is a wide logical gap between this admission and the postulate that they were all liquid *at one time* and enveloped the whole earth. Looking quite in the opposite direction is the testimony of the complex structure and intricate combination of rocks, diverse at once in chemical, mineralogical and structural characters, which the basement complex presents. The relations of the great batholite-like masses to the enveloping foliated rocks, and of analogous combinations of intrusive aspect, imply the presence of a portion of the basement complex in the already solid state when the remainder entered it in the liquid state. It would be a bold petrologist who would insist that it has been demonstrated that the basement complex is simply the molten envelope of the primitive earth solidified *in situ*, however much he might be disposed to entertain this view among his working hypotheses. It would be petrological hardihood to maintain that it was even a 'sure assumption.' Without denying that the basement complex may be the direct or the indirect offspring of a supposed molten state, no dogma of certitude is now admissible on geological grounds.

The hypothesis of a primitive molten earth is chiefly a deduction from the high internal temperature and from the nebular hypothesis. But it remains to be shown that the high internal temperature may not also be a sequence of an earth which grew up by meteoric accretion with sufficient slowness to remain essentially solid at all stages. An attempt has recently been made to show that a highly-heated state of the interior of the earth would have resulted from the self-compression of the mass during its accretion.† The methods of reasoning employed in this attempt were identical with those of Helmholtz relative to the heat of the sun, save that they were applied to a solid body. The computations of Mr. Moulton seem to indicate that gravitative concentration may have been an adequate cause of internal heat. In addition to this the thermal effect of molecular change and tidal kneading require recognition. Until these agencies are rigorously tested and found wanting, inferences based on the alternative hypothesis can scarcely be the ground of sure assumption. The irregular distribution of internal heat is more notably in harmony with the hypothesis of internal compressive generation than with that which makes it a residuum of a molten state whose temperature should be approximately uniform. If this irregularity be assigned to volcanic action it must be remembered that vulcanism is itself a part of the irregularity and adds to the burden of explication. Both hypotheses ultimately appeal to the same source, the gravitative descent of the earth's substance. Their differences lie in the modes of action assumed respectively, and these modes are determined by the antecedent conditions of aggregation. Has it been demonstrated that these antecedent conditions were of the one kind and not of the other?

Lord Kelvin obviously assumes a nebulous state of the earth as the controlling antecedent condition. It is not quite clear whether he adopts the complete gaseous theory of Laplace, including the earth-moon gaseous ring, or not. Apparently, however, he has not adopted the gaseous earth-moon ring, but has substituted therefor a meteoroidal ancestry for the earth, for he says (p. 706): "Considering the almost certain truth that the earth was built up of meteorites falling together, we may follow in imagination the whole process of shrinking from gaseous nebula to liquid lava and metals, and solidification of liquid from central regions outwards." A little farther on he speaks of "the gaseous nebula which at one time constituted the matter of our present earth." Without feeling quite certain that I am not in error, I interpret these sentences to mean that the matter of the earth was in a meteoroidal condition just previous to its falling together, and that it passed into the gaseous condition as a result of the heat of impact, and that from thence it shrank into the liquid and later into the solid state. If this be correct it would be interesting to learn on what grounds the older hypothesis of a nebulous ring, once regarded as a quite sure assumption, has been abandoned, and

* A Group of Hypotheses bearing on Climatic Changes. *Jour. Geol.*, Vol. V., No. 7, Oct.-Nov., 1897, p. 670.

whether the reasons for that abandonment do not bear adversely also on this modified phase of the gaseous hypothesis. The strongest objection recently urged against the Laplacean gaseous ring is the apparent inability of the feeble gravity of such a ring to overcome the high molecular velocities of its lighter constituents at the high temperatures necessary to maintain the refractory material of the earth in a gaseous condition.* In addition to this radical objection to the gaseous earth-moon ring, there is the extreme probability that, if formed, it would cool below the temperature of volatilization of rock substance before it would concentrate into a globe.

The studies to which reference has just been made seemed to show that even in the globular form it is doubtful if the earth could be volatilized without the dissociation of its water and the loss of its hydrogen by molecular projection away from the earth. The inquiry seemed even to raise a doubt whether the vapor of water, as such, or the atmospheric gases could be retained at the temperature of rock volatilization; indeed, it seemed that the oceanic and atmospheric constituents might even be in jeopardy at the temperature of white-hot lava. Without insisting that these molecular inquiries are demonstrative—for they only profess to be preliminary—they seem, at least, to justify the radical inquiry whether the hypothesis that the earth was once a gaseous nebula can be entertained with any confidence, in the light of modern molecular physics. As an abstract proposition in physics addressed to physicists would Lord Kelvin feel free to assert that the water now on the surface of the earth would be retained within its gravitative control if the earth were heated so that its rock substance was volatilized? May I be pardoned for inquiring whether Lord Kelvin has not joined the company of geologists and neglected some of the physical considerations that bear pertinently on the problem in hand?

But passing this point, and striking hands with Lord Kelvin in assuming "the almost certain truth that the earth was built up of meteorites falling together," what imperative reason is there for inferring a gaseous or even a white-hot liquid condition as a result? It goes without saying that the energy of impact of the falling meteorites would be sufficient, under assumable conditions, to give rise to the liquid condition and much more, but the *actual* condition that would be assumed by the earth would be dependent wholly on *the rate at which the meteorites fell in*. If they fell in simultaneously from assumable distances an intensely hot condition may be predicated with all the confidence of logical certitude. If they fell at as great intervals as they do to-day a low surface temperature may be predicated with equal certainty. If they fell in at some intermediate rate an intermediate thermal state of the surface must be postulated. No physical deduction can be more firm than that the temperature of the surface of the earth would be rigorously dependent on the *rate of infall* so far as the influence of infall alone is concerned. Before a white-hot condition can be regarded as a safe assumption it must be shown that the meteoroids would necessarily fall together *at a highly rapid rate;* otherwise the heat of individual impacts would be lost concurrently, as is now the case, and would not lead to general high temperature.

Now, has Lord Kelvin, or any other of our great teachers in physics or in astronomy, followed out to a final conclusion, by the rigorous processes of mathematics, the method and rate of aggregation of a multitude of meteorites into a planet, so as to be

* A Group of Hypotheses bearing on Climatic Changes. *Jour. Geol.*, Vol. V., No. 7, Oct.-Nov., 1897, pp. 658-668.

able to authoritatively instruct us as to the rapidity at which the ingathering would take place? Can the problem be solved at present with any such close approximation to precison as to determine whether a liquid or a gaseous state would or would not ensue? I assume that the most probable hypothesis relative to the distribution and movements of the meteorites is one that assumes that they consisted of a swarm or belt revolving about the sun in the general neighborhood of the present orbit of the earth; in other words, some form of meteoroidal substitute for the gaseous ring of the Laplacean hypothesis. The hypothesis may, doubtless, diverge much in detail, and, indeed, in some very important factors, but I assume that no radical departure from this can be entertained without endangering the peculiar relations of the earth to the rest of the solar system and the harmonious relations of the whole; without, in other words, jeopardizing the consanguinity of the planets. If a distribution of meteorites bearing any close resemblance to the Saturnian rings, the foster parents of the nebular hypothesis, be assumed, a definite problem is presented for determination. If the rings of Saturn, which are quite certainly formed of discrete solid matter, were to be enlarged so that they should lie outside Roche's limit, and so escape the sphere of specially intense tidal strain which will permit no aggregation, what reason is there to think that they would gather together precipitately? Does the tidal influence, which, within Roche's limit, is able to tear a satellite to pieces, cease instantly outside the limit and give place to a precipitate tendency to come clashing together? On the contrary, is it not difficult to demonstrate, by rigorous processes, even the method by which the meteorites will aggregate, much less their rate, or even to demonstrate that, apart from extraneous causes, they will fall together at all. Is not the presumption in such a case favorable to a slow rather than to a rapid aggregation? If a distribution like the meteoroidal swarms that are associated with the comets of the solar system be assumed, a definite problem is set concerning which some appeal to observation is possible. Here the observed tendency is toward dispersion rather than aggregation. In either of these assumptions, or in any other assumption, the problem involves the balance between gravitative forces, revolutionary forces and tidal forces, and the gravitative forces are not simply those between the meteorites mutually, but those between the meteorites and the central solar body and the exterior planetary bodies, a complex of no mean intricacy. Is it certain that these forces would be so related to each other as to produce a swift ingathering of the whole swarm or belt, or, on the other hand, an ingathering prolonged through a considerable period? If the latter be the case (and, in the absence of demonstration, is it unreasonable to think it quite as probable as the opposite) are there any imperative grounds for assuming that a liquid state of the earth would result? Until the rate of aggregation is worked out fully and rigorously are there any moral prohibitions, strict or otherwise, to a free interpretation of geologic and biologic evidence on its own grounds? Is not the assumption of a white-hot liquid earth still quite as much on trial as any chronological inferences of the biologist or geologist?

It, of course, remains to be seen whether the alternative hypothesis of an earth grown up slowly in a cold state, or in some state less hot than that assumed in the address, would afford any relief from the limitations of time urged upon us. At first thought it would, perhaps, seem that this alternative would but intensify the limitations. Since the argument for a short history is based on the degree to which the earth is cooled, an original cold state should but hasten the present status. But this neglects an essential factor. The question really hinges on the proportion of *potential energy convertible into heat* which remained within the earth when full grown. There is no great difference between the alternative hypotheses so far as the amount of sensible heat at the beginning of the habitable stage is concerned. For, on the one hand, the white-hot earth must have become relatively cool on the exterior before life could begin, and, on the other, it is necessary to assume a sufficiency of internal heat coming from impact and internal compression, or other changes, to produce the igneous and crystalline phenomena which the lowest rocks present. The superficial and sub-superficial temperatures in the two cases could not, therefore, have been widely different.

So far as the temperatures of the deep interior are concerned there is only recourse to hypothesis. It is probable that there would be a notable rise of temperature toward the center of the earth in either case. In a persistently liquid earth this high central temperature would be lost through convection, but if central crystallization took place at an early stage through pressure, much of the high central heat might be retained. In a meteor-built earth, solid from the beginning, very much less convectional loss would be suffered, and the central temperature would probably correspond somewhat closely to the density. The probabilities, therefore, seem somewhat to favor a higher thermal gradient toward the center in the case of the solid meteor-built earth.

But if we turn to the consideration of potential energy, there is a notable difference between the two hypothetical earths. In the liquid earth, the material must be presumed to have arranged itself according to its specific gravity, and, therefore, to have adopted a nearly complete adjustment to gravitative demands; in other words, to have exhausted, as nearly as possible, its potential energy, *i. e.*, its 'energy of position.' On the other hand, in an earth built up by the accretion of meteorites without free readjustment there must have been initially a heterogeneous arrangement of the heavier and lighter material throughout the whole body of the earth, except only so far as the partial liquefaction and the very slow, plastic, viscous and diffusive rearrangement of the material permitted an incipient adjustment to gravitative demands. A large amount of potential energy was, therefore, restrained, for the time being, from passing into sensible thermal energy. This potential energy thus restrained is supposed to have gradually become converted into heat as local liquefaction and viscous, molecular and massive movements permitted the sinking of the heavier material and the rise of the lighter material. This slow conversion of potential energy into sensible heat is thought to give to the slow-accretion earth a very distinct superiority over the hot liquid earth when the combined sum of sensible and potential heat is considered. The theoretical difference is capable of approximate computation, and Mr. F. R. Moulton has kindly undertaken to make the computation in a simplified hypothetical case which may give some impression of the possible order of magnitude of this factor. For the purposes of the computation the earth was assumed to have been composed of 40 % of metal with a normal surface specific gravity of 7 and 60 % of rock with a normal surface specific gravity of 3. These combined would give an earth whose average specific gravity would be only 4.6. The real specific gravity (5.6) is supposed to have been obtained by compression which would amount to about 18 %. Very likely the proportion of metal is put too high and the effect of compression too low, but the purpose of the computation is only to show the theoretical possibilities of the case. The metal is supposed to have been originally scattered uniformly through the rock material in meteoric fashion, and to have gathered thence to the center, forcing the rock material outwards so far as necessary. The heat produced, Mr. Moulton found to be sufficient to raise the temperature of the whole earth (specific heat taken at .2) more than 3,000° C. The magnitude of this result is sufficient to require the careful consideration of the potential element unless the whole hypothesis can be set aside. It is large enough to cast the gravest doubt on any conclusion based on the rate of a supposed decline of internal temperature. Complete readjustment of the interior matter, however, is not postulated under the slow-accretion hypothesis. It is only assumed that a slow readjustment has been in progress throughout the geological ages and still is in progress, and that this has changed a certain amount of potential energy into sensible heat and that this heat has contributed to the maintenance of the internal temperature of the earth.

But there are in addition, incidental factors which enter effectively into the case. The gravitative readjustment of the heterogeneous interior material is presumed to have taken place by the descent of the metallic and other heavier materials toward the center and the reciprocal ascent of lighter materials from the central region toward the surface, this being accomplished in various ways, the most declared of which has its superficial manifestation in volcanic action. Now, this process of vertical transfer, beside developing heat in proportion to the work done, as above indicated, also incidentally brings the hotter material of the interior toward the surface and thus increases the subsurficial temperature. It is a species of slow convection. This convection is in no radical sense different from that which is supposed to have taken place in the liquid earth, save that it was delayed so that the heat is available within the life era of the earth, instead of being brought to the surface and dissipated in the prezoic hot stage, when it was a barrier to the existence of life instead of an aid.

Again, in the liquid earth there were the best imaginable conditions for the intermixture of the earth constitutents and for the formation of such chemical and mineral combinations as best accorded with the high pressures of the interior. In the heterogeneous solid earth, on the other hand, such combinations were restrained and delayed and have been able to take place only slowly throughout the secular intermingling of the internal material. It, therefore, hypothetically follows that throughout geological ages, as the internal material was able slowly to readjust itself, new chemical and mineral combinations become possible. These combinations would be controlled by the high pressure in the interest of maximum density, and of hypothetically possible mineral combinations, only those would form which gave the higher density.* Thus a slow process of recrystallization in the interest of greater density would be in progress throughout the ages. This denser crystallization would set free heat. It would furthermore permit the shrinkage of the whole mass and the consequent intensification of its self-gravitation and this would in turn result in further development of heat. This large possible shrinkage meets the demands of geological phenomena at a point where the liquid earth has been felt to conspicuously fail. The losses of heat from the earth, as computed by Lord Kelvin and other authorities, and the shrinkage resulting therefrom have long been held to be quite incompetent to produce the observed inequalities. Their incompetence is now

* Professor C. R. Van Hise has worked this out elaborately in manuscript not yet published.

very generally admitted by careful students. Lord Kelvin also admits this, by implication, when he says (sec. 31, p. 706) "If the shoaling of the lava ocean up to the surface had taken place everywhere at the same time, the whole surface of the consistent solid would be the dead level of the liquid lava all around, just before its depth became zero. On this supposition there seems no possibility that our present day continents could have risen to their present heights, and that the surface of the solid in its other parts could have sunk down to their present ocean depths, during the twenty or twenty-five million years which may have passed since the *consistentior status* began or during any time however long."

In addition to this recognized quantitative deficiency, the present writer has been led to question its qualitative adaptability. The phenomena of mountain wrinkling and of plateau formation, as well as the still greater phenomena of continental platforms and abysmal basins, seem to demand a more *deep-seated* agency than that which is supplied by superficial loss of heat. This proposition demands a more explicit statement than is appropriate to this place, but it must be passed by with this mere allusion. It would seem obvious, however, that an earth of heterogeneous constitution, progressively reorganizing itself, would give larger possibilities of internal shrinkage, and that this shrinkage must be deep-seated as well as superficial. In these two particulars it holds out the hope of furnishing an adequate explanation for the deformation of the earth where the hypothesis of a liquid earth seems thus far to have failed.

But the essential question here is the possibility of sustained internal temperature. It is urged that the heterogeneous, solid-built earth is superior to the liquid earth in the following particulars: (1) It retains a notable percentage of the original potential energy of the dispersed matter, while in the liquid earth this was converted into sensible heat and lost in prezoic times; (2) it retains the conditions for a slow convection of the interior material, bringing interior heat to the surface, a function which was exhausted by the liquid earth in the freer convection of its primitive molten state; (3) it retains larger possibilities of molecular rearrangement of the matter and of the formation of new minerals of superior density, whereas the liquid earth permitted this adjustment in the prezoic stages. In short, in at least these three important particulars, the slow-built meteoric earth delayed the exercise of thermal agencies until the life era and gradually brought them into play when they were serviceable in the prolongation of the life history, whereas the liquid earth exhausted these possibilities at a time of excessive conversion of energy into heat and thus squandered its energies when they were not only of no service to the life history of the earth, but delayed its inauguration until their excesses were spent.

Let it not be supposed for a moment that I claim that the alternative hypothesis of a slow-grown earth is substantiated. It must yet pass the fiery ordeal of radical criticism at all points, but it is the logical sequence of the proposition that a swarm of meteorites revolving about the sun in independent individual orbits and having any probable form of dispersion would aggregate slowly rather than precipitately. If the astronomers and mathematicians can demonstrate that the aggregation must necessarily have been so rapid as to crowd the transformed energy of the impacts into a period much too limited to permit the radiation away of the larger part of the heat concurrently, the hypothesis will have to be set aside, and we shall be compelled to follow the deductions from the white-hot liquid earth, or find other alternatives.

But I think I do not err in assuming that mathematical computations, so far as they can approach a solution of the exceedingly complex problem, are at least quite as favorable to a slow as to a rapid aggregation. If this be so, the problem of internal temperature must be attacked on the lines of this hypothesis as well as those of the common hypothesis before any safe conclusion can be drawn from it respecting the age of the earth.

Another basis upon which the address urges the limitation of the earth's history is found in tidal friction. The limitations assigned on this basis are not, however, very restrictive. The argument is closed as follows: "Taking into account all uncertainties, whether in respect to Adams' estimate of the ratio of frictional retardation of the earth's rotary speed, or to the conditions as to the rigidity of the earth once consolidated, we may safely conclude that the earth was certainly not solid 5,000 million years ago, and was probably not solid 1,000 million years ago" (p. 670) and in a foot-note it is added: "It is probable that the date of consolidation is considerably more recent than 1,000 million years ago."

The foundations of any argument involving the relations of the moon to the earth are very infirm. In the first place, no hypothesis respecting the moon's mode of origin, or of the time in the history of the earth when it became aggregated and came into effective possession of its tidal function, can claim even a remote approach to substantiation. There is not only no substantiated theory of the origin of the moon, but there can scarcely be said to be even a good working hypothesis, for the radical reason that the hypotheses offered will not *work*. George Darwin, who has probably studied the subject more assiduously and more profoundly than any other investigator, ancient or recent, strongly expresses the situation when he says, in his recent work on 'The Tides,' (p. 360) "The origin and earliest history of the moon must always remain highly speculative, and it seems fruitless to formulate exact theories on the subject." The annular theory of Laplace encounters in their maximum intensity the objections which arise from the application of the modern doctrine of molecular velocities. The gravitative control of an attenuated ring having the mass of the moon over its constituent material must have been exceedingly low, while the high temperature necessary to sustain the refractory material of the moon in a gaseous condition must have rendered the molecular velocities very high, so that no material except that of very high atomic weight and consequent low molecular velocity could be presumed to have been retained. But the specific gravity of the moon (3.4) seems a fatal objection to the assumption that it is composed wholly of material of very high atomic weight. Besides, it is difficult to understand how the high temperature of a ring of such attenuation could have been maintained during the time necessary for its concentration. This was less difficult when it was assumed, as formerly, that the temperature of the sun at that time was excessively high, as was also that of the earth. But modern inquiry seems decidedly opposed to the assumption of excessively high temperatures at that stage. On the contrary, it has recently been urged from different quarters that the early temperature of the sun's surface must have been much lower than at present, and this is also implied in certain statements of the address (p. 711, Sec. 43). There are also grounds for grave question as to the high temperature of the earth, as has already been indicated. Under the revised forms of the nebular hypothesis there seems no substantial reason for supposing that if the matter of the moon was once distributed in a ring about the earth, it could maintain the gaseous condition throughout the stages of its condensation. The hypothesis therefore rests upon exceedingly doubtful premises and upon exceedingly questionable deductions from these doubtful premises.

The fission hypothesis of George Darwin has recently replaced it in favor, but the above quotation implies that even its founder does not now rest much confidence in it. The objections to the theory are several and grave. In the first place, the theory of the fission of a celestial body by high rotation, as worked out independently by Darwin and Poincaré, requires that the separated bodies should not be very greatly different in mass, *i. e.*, the smaller body should not be less than one-third the mass of the larger. But the mass of the moon is but $\frac{1}{80}$ of that of the earth, and hence it lies far outside the computed limits of applicability of the fission process.

Another difficulty lies in the effect of tidal strain itself. George Darwin, in his recent work on 'The Tides' (p. 259), assigns 11,000 miles from the center of the earth as Roche's limit. This leaves a tract of 7,000 miles above the terrestrial surface within which the earth's tidal force would be so great as to tear the moon to fragments, and, perhaps, scatter these into the form of a ring. The rings of Saturn are supposed to illustrate this form of intense tidal action. The escape of the moon, even presuming it to have been separated from the earth would, therefore, have been jeopardized by its transformation into a meteoroidal ring or swarm. If the fragments, after having been torn apart, were still sufficiently affected by a minute tide to be carried away from the earth in a slow spiral, the time occupied in passing outward beyond Roche's limit must have been protracted ; and, after their escape from it into a zone where conditions not hostile to aggregation might, perhaps, have been afforded, there must probably have been another protracted period before the aggregation of the moon would have been sufficiently advanced to give it appreciable tidal effect upon the earth. It remains, therefore, to be determined, if this hypothesis is followed, at what stage in the evolution of the moon it was sufficiently concentrated to assume effective tidal functions. This is a question also applicable to the aggregation of the moon under the Laplacean hypothesis, if it be modified so as to conform to the demands of modern scientific probability. It also applies to any hypothesis which postulates aggregation from a dispersed condition. In any case, it seems necessary to determine when the moon became full grown before it is possible to assign a positive date for the commencement of effective tidal action. It would appear that such action might be developed gradually as the material of the moon became aggregated. During such gradual assumption of the tidal function the reaction between the moon and the earth must have been of a feeble sort, and a recomputation of its amount based on a series of hypotheses which shall cover the whole ground of legitimate speculation would seem necessary before any satisfactory conclusions can be reached.

It may be urged that the computations of George Darwin following, in backward steps, by the masterly application of mathematical analysis, the stages of the earth-moon relationship give a firmer ground for conclusions. In a qualified degree this must be conceded. But it is to be remarked, in the first place, that the mathematics becomes indecisive before the origin of the moon is reached, which may signify that this is not the true line of approach to the origin of the moon, or that there is some error or defect in the assumptions. It would seem to be obvious, however, that if the tidal function was the result of a slow aggregation which began at an indeterminate stage in the earth's existence the numerical results of a computation based on a full-grown moon may need radical revision.

Furthermore, the agencies which are assumed to have accelerated the rotation of the earth in its earlier history must not be neglected. If they may safely be assumed to have been competent to give the earth a rotary speed sufficient to detach from itself the matter of the moon, as is postulated in the Laplacean and the fission hypotheses in common, the same agencies, if more evenly distributed in time, might prolong the period of acceleration so that it should be coincident with that of tidal retardation and offset it in any degree that falls within the legitimate limits of assumption. We encounter here again, in another form, a deduction from the assumption of a very rapid concentration of the matter ingathered to form the earth and moon, and the consequent exhaustion of its energy in an early stage. If, however, the concentration were less rapid and less complete in the early history of the earth, as is postulated by the accretion theory, as herein entertained, acceleration might be far less advanced in the earliest stages and be greater in the later stages. Hence the retarding effects of tidal friction may have been more effectually antagonized by the shrinkage of the earth during the progress of geological history. Mr. Moulton has computed the effects of the internal change of metal and rock material, assumed in a hypothetical case on a previous page, on the speed of rotation of the earth, and found that it would accelerate the then-current rate, whatever it was, about one-fifth. If, therefore, the delayed central concentration left some notable part of the acceleration to be gained during the period of geological history, and if, at the same time, a slow aggregation of the moon delayed its effectual tidal influence upon the earth and the reciprocal influence of the earth upon it, the whole history may be notably affected in the direction at once of less maximum speed and of less retardation, *i. e.*, of more near approach to uniformity.

If we turn to the geological data that bear on the question of former high rotation and subsequent retardation we find ample support for profound skepticism regarding the applicability of the tidal argument. As pointed out by Lord Kelvin, if the rotation of the earth were once notably greater than at present it should have resulted in an oblateness of the spheriod such that the equatorial regions would now be all dry land, unless the body of the earth were deformed to correspond to the slackening rotation in an almost perfect manner. But there is not the slightest evidence in the configuration of the earth of such an equatorial land tract. The equatorial belt is notably oceanic rather than otherwise. Reciprocally, there should have been, with the gradual slackening of the earth's rotation, an accumulation of the oceanic waters about the poles, but there is no geological evidence of such an accumulation in any appreciable degree. In the Arctic regions, as exemplified in Greenland, Spitzbergen and the Arctic islands of America, there are ancient shallow water deposits which lie both above and below the present oceanic level. These deposits range throughout the Paleozoic and represent in some less degree both the Mesozoic and Cenozoic eras. The nature of these shallow-water deposits is such that they cannot have been formed at great depths below the oceanic surface, so that, with the allowance of a few hundred feet, it is possible to locate the ancient horizons relative to the crust of the earth, at most or all of these periods. From these it may be inferred with great confidence that the ancient ocean surface n the Arctic regions was in numerous stages of Paleozoic, Mesozoic and Cenozoic eras not notably different from that of to-day. The facts even justify the seemingly extravagant statement that at several stages in geological history, early and late, the surface of the ancient ocean did not vary a foot from that of the present, since it must have passed both above and below the present horizon repeatedly during the earth's history. Geological evidence, therefore, interpreted on its own legitimate basis, seems to lend no appreciable support to any theory that postulates a high speed of rotation for the early earth, or a low speed of rotation for the present earth, unless that hypothesis is correlated with the assumption of an almost perfect adjustability of the form of the earth to the changing rotation, in which case the argument of Lord Kelvin set forth on p. 670 stands confessedly for naught.

If we postulate a slow accretion of the earth and of the moon alike, the whole subject of the former speed of rotation of the earth and the relations of the earth to the moon take on a new aspect and invite investigation along the lines of new working hypotheses. Can it be shown that it is absolutely necessary that the aggregating meteoroids gave to the earth an exceedingly high rotation at the outset? Is not this assumption of high rotation merely an offspring of the nebular hypothesis? If the moon were aggregated slowly and came into tidal functions at a late stage, and at a distance from the earth's center quite unknown, may not all its relations to the earth have developed on much more conservative lines than those worked out by Darwin and at the same time preserve those apparently significant relations to the movements of the two bodies to which Darwin has so strongly appealed in support of his hypothesis of the history of the two bodies? In other words, without challenging the validity of Darwin's most beautiful investigation in the essentials of its method, may not a change in the premises deducible from an equally legitimate hypothesis of the original condition of the two bodies lead to results in equally satisfactory accord with the existing relations of the two bodies?

At any rate, as remarked at the outset, the time-limits assigned on tidal grounds are not very restrictive, even on the assumptions made, and when they shall be worked out on revised data in accord with the newer hypotheses they may, perhaps, even be found to favor the longevity of the earth and become one of the arguments in support of it.

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(*To be concluded.*)

PERSPECTIVE ILLUSIONS FROM THE USE OF MYOPIC GLASSES.

THE phenomena to be described occurred during the first days' use of myopic glasses, and may be grouped under the following heads:

a. There was an apparent diminution in size of moving objects—persons, animals, street cars—as compared with buildings, natural scenery, and, in general, with the elements of the background of the visual field. Here the total visual fields of the normal and of the myopic eye are equally extensive; there are the same number of projection points in each. Over this background, in the case of a myopic individual, there is distributed a relatively small number of distinct and at the same time interesting or important objects. When the near-sighted person puts on powerful glasses the number of such important and interesting distinct objects thrown upon this background is vastly increased; it is crowded with a multitude of persons, animals, trees, buildings, and the like. There are here two sets of factors whose interpretation in terms of perspective point in divergent directions. Multiplicity of objects in the visual field