nomic Section on the "Economic and Sociologic Relations of the Canadian States and the United States, prospectively considered," roused considerable criticism. The meeting adjourned to meet next year on the third Wednesday in August at Indianapolis. The officers of the meeting will be as follows: president, Professor George L. Goodale, Harvard University; vice-presidents, A, Mathematics and Astronomy, S. C. Chandler, Cambridge, Mass.; B, Physics, Cleveland Abbe, Washington; C, Chemistry, R. B. Warder, Washington; D, Mechanical Science and Engineering, James E. Denton, Hoboken, N. J.; E, Geology and Geography, John C. Branner, Little Rock, Ark.; F, Biology, C. S. Minot, Boston, Mass.; H, Anthropology, Frank Baker, Washington; I, Economic Science and Statistics, J. Richards Dodge, Washington; permanent secretary, F. W. Putnam, Cambridge, Mass., office, Salem, Mass.; general secretary, H. Carrington Bolton, of New York; secretary of the council, James Loudon, Toronto; secretaries of the sections, A, Wooster A. Beman, Ann Arbor, Mich.; B, W. Le Conte Stevens, Brooklyn, N. Y.; C, W. A. Noyes, Terre Haute, Ind.; D, M. E. Cooley, Ann Arbor, Mich.; E, Samuel Calvin, Iowa City, Iowa; E, John M. Coulter, Crawfordsville, Ind.; H, Joseph Jastrow, Madison, Wis.; I, S. Dana Horton, Pomeroy, Ohio; treasurer, William Lilly, of Mauch Chunk, Penn.; auditors, Henry Wheatland, Salem, Mass.; Thomas Meehan, Philadelphia.

THE MATHEMATICAL THEORIES OF THE EARTH.[1]

THE name of this section, which, by your courtesy, it is my duty to address to-day, implies a community of interest among astronomers and mathematicians. This community of interest is not difficult to explain. We can of course imagine a considerable body of astronomical facts quite independent of mathematics. We can also imagine a much larger body of mathematical facts quite independent of and isolated from astronomy. But we never think of astronomy in the large sense without recognizing its dependence on mathematics, and we never think of mathematics as a whole without considering its capital applications in astronomy.

Of all the subjects and objects of common interest to us the earth will easily rank first. The earth furnishes us with a stable foundation for instrumental work and a fixed line of reference, whereby it is possible to make out the orderly arrangement and procession of our solar system and to gain some inkling of other systems which lie within telescopic range. The earth furnishes us with a most attractive store of real problems: its shape, its size, its mass, its precession and nutation, its internal heat, its earthquakes and volcanoes, and its origin and destiny, are to be classed with the leading questions for astronomical and mathematical research. We must of course recognize the claims of our friends the geologists to that indefinable something called the earth's crust, but, considered in its entirety and in its relations to similar bodies of the universe, the earth has long been the special province of astronomers and mathematicians. Since the times of Galileo and Kepler and Copernicus it has supplied a perennial stimulus to observation and investigation, and it promises to tax the resources of the ablest observers and analysts for some centuries to come. The mere mention of the names of Newton, Bradley, d'Alembert, Laplace, Fourier, Gauss, and Bessel calls to mind not only a long list of inventions and discoveries, but the most important parts of mathematical literature. In its dynamical and physical aspects the earth was to them the principal object of research, and the thoroughness and completeness of their contributions toward an explanation of the "system of the world" are still a source of wonder and admiration to all who take the trouble to examine their works.

A detailed discussion of the known properties of the earth and of the hypotheses concerning the unknown properties, is no fit task for a summer afternoon: the intricacies and delicacies of the subject are suitable only for another season and a special audience. But it has seemed that a somewhat popular review of the state of our mathematical knowledge of the earth might not be without interest to those already familiar with the complex details, and might also help to increase that general interest in science, the promotion of which is one of the most important functions of this association.

As we look back through the light of modern analysis, it seems strange that the successors of Newton, who took up the problem of the shape of the earth, should have divided into hostile camps over the question whether our planet is elongated or flattened at the poles. They agreed in the opinion that the earth is a spheroid, but they debated, investigated, and observed for nearly half a century before deciding that the spheroid is oblate rather than oblong. This was a critical question, and its decision marks perhaps the most important epoch in the history of the figure of the earth. The Newtonian view of the oblate form found its ablest supporters in Huyghens, Maupertuis, and Clairaut, while the erroneous view was maintained with great vigor by the justly distinguished Cassinian school of astronomers. Unfortunately for the Cassinians, defective measures of a meridional arc in France gave color to the false theory and furnished one of the most conspicuous instances of the deterring effect of an incorrect observation. As you well know, the point was definitely settled by Maupertuis's measurement of the Lapland arc. For this achievement his name has become famous in literature as well as in science, for his friend Voltaire congratulated him on having "flattened the poles and the Cassinis," and Carlyle has honored him with the title of "Earth-flattener."

Since the settlement of the question of the form, progress towards a knowledge of the size of the earth has been consistent and steady, until now it may be said that there are few objects with which we have to deal whose dimensions are so well known as the dimensions of the earth. But this is a popular statement, and like most such, needs to be explained in order not to be misunderstood. Both the size and shape of the earth are defined by the lengths of its equatorial and polar axes; and, knowing the fact of the oblate spheroidal form, the lengths of the axes may be found within narrow limits from simple measurements conducted on the surface, quite independently of any knowledge of the interior constitution of the earth. It is evident in fact, without recourse to mathematical details, that the length of any arc, as a degree of latitude or longitude, on the earth's surface, must depend on the lengths of those axes. Conversely, it is plain that the measurement of such an arc on the surface and the determination of its geographical position, constitute an indirect measurement of the axes. Hence it has happened that scientific as distinguished from practical geodesy has been concerned chiefly with such linear and astronomical measurements, and the zeal with which this work has been pursued is attested by triangulations on every continent.

Passing over the earlier determinations as of historical interest only, all of the really trustworthy approximations to the lengths of the axes have been made within the half century just passed. The first to appear of these approximations were the well-founded values of Airy, published in 1830. These, however, were almost wholly overshadowed and supplanted eleven years later by the values of Bessel, whose spheroid came to occupy a most conspicuous place in geodesy for more than a quarter of a century. Knowing as we now do that Bessel's values were considerably in error, it seems not a little remarkable that they should have been so long accepted without serious question. One obvious reason is found in the fact that a considerable lapse of time was essential for the accumulation of new data, but two other possible reasons of a different character are worthy of notice, because they are interesting and instructive whether specially applied to this particular case or not. It seems not improbable that the close agreement of the values of Airy and Bessel, computed independently and by different methods, — the greatest discrepancy being about one hundred and fifty feet, — may have been incautiously interpreted as a confirmation of Bessel's dimensions, and hence led to their too ready adoption. It seems also not improbable that the weight of Bessel's great name may have been too closely associated in the minds of his followers with the weight of his observations and results. The sanction of eminent authority, especially if there is added to it the stamp of an official seal, is sometimes a serious obstacle to real progress. We cannot do less than accord to Bessel the first place among the

[1] Address before the Section of Mathematics and Astronomy of the American Association for the Advancement of Science, at Toronto, Ont., Aug. 28-Sept. 3, by R. S. Woodward, vice-president of the section.

astronomers and geodesists of his day, but this is no adequate justification for the exaggerated estimate long entertained of the precision of the elements of his spheroid.

The next step in the approximation was the important one of Clarke in 1866. His new values showed an increase over Bessel's of about half a mile in the equatorial semi-axis and about three-tenths of a mile in the polar semi-axis. Since 1886, General Clarke has kept pace with the accumulating data, and given us so many different elements for our spheroid that it is necessary to affix a date to any of his values we may use. The later values, however, differ but slightly from the earlier ones, so that the spheroid of 1866, which has come to be pretty generally adopted, seems likely to enjoy a justly greater celebrity than that of its immediate predecessor. The probable error of the axes of this spheroid is not much greater than the hundred thousandth part, and it is not likely that new data will change their lengths by more than a few hundred feet.

In the present state of science, therefore, it may be said that the first order of approximation to the form and dimensions of the earth has been successfully attained. The question which follows naturally and immediately is, how much further can the approximation be carried? The answer to this question is not yet written, and the indications are not favorable for its speedy announcement. The first approximation, as we have seen, requires no knowledge of the interior density and arrangement of the earth's mass; it proceeds on the simple assumption that the sea surface is closely spheroidal. The second approximation, if it be more than a mere interpolation formula, requires a knowledge of both the density and arrangement of the constituents of the earth's mass, and especially of that part called the crust. "All astronomy," says Laplace, "rests on the stability of the earth's axis of rotation." In a similar sense we may say all geodesy rests on the direction of the plumb-line. The simple hypothesis of a spheroidal form assumes that the plumb-line is everywhere coincident with the normal to the spheroid, or that the surface of the spheroid coincides with the level of the sea. But this is not quite correct. The plumb line is not in general coincident with the normal, and the actual sea level or geoid must be imagined to be an irregular surface lying partly above and partly below the ideal spheroidal surface. The deviations, it is true, are relatively small, but they are in general much greater than the unavoidable errors of observation, and they are the exact numerical expression of our ignorance in this branch of geodesy. It is well known, of course, that deflections of the plumb-line can sometimes be accounted for by visible masses, but on the whole it must be admitted that we possess only the vaguest notions of their cause and a most inadequate knowledge of distribution and extent.

What is true of plumb-line deflections is about equally true of the deviations of the intensity of gravity from what may be called the spheroidal type. Given a closely spheroidal form of the sea level and it follows from the law of gravitation, as a first approximation, without any knowledge of the distribution of the earth's mass, that the increase of gravity varies as the square of the sine of the latitude in passing from the equator to the poles. This is the remarkable theorem of Stokes, and it enables us to determine the form or ellipticity of the earth, by means of pendulum observations alone. It must be admitted, however, that the values for the ellipticity recently obtained in this way by the highest authorities, Clarke and Helmert, are far from satisfactory, whether we regard them in the light of their discrepancy or in the light of the different methods of computing them. In general terms we may say that the difficulty in the way of the use of pendulum observations still hinges on the treatment of local anomalies and on the question of reduction to sea level. At present, the case is one concerning which the doctors agree neither in their diagnosis nor in their remedies.

Turning attention now from the surface, towards the interior, what can be said of the earth's mass as a whole, of its laws of distribution, and of the pressures that exist at great depth? Two facts, namely, the mean density and the surface density, are roughly known; and a third fact, namely, the precession constant, or the ratio of the difference of the two principal moments of inertia to the greater of them, is known with something like precision. These facts lie within the domain of observation, and require only the law of gravitation for their verification. Certain inferences also from these facts and others have long been and still are held to be hardly less cogent and trustworthy, but before stating them, it will be well to recall briefly the progress of opinion concerning this general subject during the past century and a half.

The conception of the earth as having been primitively fluid was the prevailing one among mathematicians before Clairaut published his "Théorie de la Figure de la Terre" in 1743. By the aid of this conception Clairaut proved the celebrated theorem which bears his name, and probably no idea in the mechanics of the earth has been more suggestive and fruitful. It was the central idea in the elaborate investigations of Laplace, and received at his hands a development which his successors have found it about equally difficult to displace or to improve. From the idea of fluidity spring naturally the hydrostatical notions of pressure and level surfaces, or the arrangement of fluid masses in strata of uniform density. Hence follows, also, the notion of continuity of increase in density from the surface towards the centre of the earth. All of the principal mechanical properties and effects of the earth's mass, viz., the ellipticity, the surface density, the mean density, the precession constant, and the lunar inequalities, were correlated by Laplace in a single hypothesis, involving only one assumption in addition to that of original fluidity and the law of gravitation. This assumption relates to the compressibility of matter, and asserts that the ratio of the increment of pressure to the increment of density is proportional to the density. Many interesting and striking conclusions follow readily from this hypothesis, but the most interesting and important are those relative to density and pressure, especially the latter, whose dominance as a factor in the mechanics of celestial masses seems destined to survive whether the hypothesis stands or falls. The hypothesis requires that while the density increases slowly from something less than 3 at the surface to about 11 at the centre of the earth, the pressure within the mass increases rapidly below the surface, reaching a value surpassing the crushing strength of steel at the depth of a few miles, and amounting at the centre to no less than three million atmospheres. The inferences, then, as distinguished from the facts, are that the mass of the earth is very nearly symmetrically disposed about its centre of gravity, that pressure and density except near the surface are mutually dependent, and that the earth in reaching this stage has passed through the fluid or quasi-fluid state.

Later writers have suggested other hypotheses for a continuous distribution of the earth's mass, but none of them can be said to rival the hypothesis of Laplace. Their defects lie either in not postulating a direct connection between density and pressure or in postulating a connection which implies extreme or impossible values for these and other mechanical properties of the mass.

It is clear from the positiveness of his language in frequent allusions to this conception of the earth, that Laplace was deeply impressed with its essential correctness. "Observations," he says, "prove incontestably that the densities of the strata [*couches*] of the terrestrial spheroid increase from the surface to the centre;" and "the regularity with which the observed variation in length of a seconds pendulum follows the law of the squares of the sines of the latitudes, proves that the strata are arranged symmetrically about the centre of gravity of the earth." The more recent investigations of Stokes, to which allusion has already been made, forbid our entertaining anything like so confident an opinion of the earth's primitive fluidity or of a symmetrical and continuous arrangement of its strata. But, though it must be said that the sufficiency of Laplace's arguments has been seriously impugned, we can hardly think the probability of the correctness of his conclusions has been proportionately diminished.

Suppose, however, that we reject the idea of original fluidity. Would not a rotating mass of the size of the earth assume finally the same aspects and properties presented by our planet? Would not pressure and centrifugal force suffice to bring about a central condensation and a symmetrical arrrangement of strata similar at least to that required by the Laplacian hypothesis? Categorical answers to these questions cannot be given. But whatever may have been the antecedent condition of the earth's mass, the conclusion seems unavoidable that at no great depth the pressure is sufficient to break down the structural characteristics of all known substances, and hence to produce viscous flow whenever and wherever the stress difference exceeds a certain limit, which cannot be large in comparison with the pressure. Purely observational evidence also of a highly affirmative kind in support of this conclusion, is afforded by the remarkable results of Tresca's experiments on the flow of solids and by the abundant proofs in geology of the plastic movements and viscous flow of rocks. With such views and facts in mind, the fluid stage, considered indispensable by Laplace, does not appear necessary to the evolution of a planet, even if it reach the extreme refinement of a close fulfilment of some such mathematical law as that of his hypotheses. If, as is here assumed, pressure be the dominant factor in such large masses, the attainment of a stable distribution would be simply a question of time. The fluid mass might take on its normal form in a few days or a few months, whereas the viscous mass might require a few thousand or a few million years.

Some physicists and mathematicians, on the other hand, reject both the idea of the existence of great pressures within the earth's mass, and the notion of an approach to continuity in the distribution of density. As representing this side of the question, the views ot the late M. Roche, who wrote much on the constitution of the earth, are worthy of consideration. He tells us that the very magnitude of the central pressure computed on the hypothesis of fluidity is itself a peremptory objection to that hypothesis. According to his conception, the strata of the earth from the centre outwards are substantially self-supporting and unyielding. It does not appear, however, that he had submitted this conception to the test of numbers, for a simple calculation will show that no materials of which we have any knowledge would sustain the stress in such shells or domes. If the crust of the earth were self-supporting, its crushing strength would have to be about thirty times that of the best cast steel or five to one thousand times that of granite. The views of Roche on the distribution of terrestrial densities appear equally extreme. He prefers to consider the mass as made up of two distinct parts, an outer shell or crust whose thickness is about one-sixth of the earth's radius, and a solid nucleus having little or no central condensation. The nucleus is conceived to be purely metallic, and to have about the same density as iron. To account for geological phenomena, he postulates a zone of fusion separating the crust from the nucleus. The whole hypothesis is consistently worked out in conformity with the requirements of ellipticity, the superficial density, the mean density, and precession; so that to one who can divest his mind of the notion that pressure and continuity are important factors in the mechanics of such masses, the picture which Roche draws of the constitution of our planet will present nothing incongruous.

In a field so little explored and so inaccessible, though hedged about as we have seen by certain sharply limiting conditions, there is room for a wide range of opinion and for great freedom in the play of hypothesis; and although the preponderance of evidence appears to be in favor of a terrestrial mass in which the reign of pressure is well-nigh absolute, we should not be surprised a few decades or centuries hence to find many of our notions on this subject radically defective.

If the problem of the constitution and distribution of the earth's mass is yet an obscure and difficult one after two centuries of observation and investigation, can we report any greater degree of success in the treatment of that still older problem of the earth's internal heat, of its origin and effects? Concerning phenomena always so impressive and often so terribly destructive as those intimately connected with the terrestrial store of heat, it is natural that there should be a considerable variety of opinion. The consensus of such opinion, however, has long been in favor of the hypothesis that heat is the active cause of many and a potent factor in most of the grander phenomena which geologists assign to the earth's crust; and the prevailing interpretation of these phenomena is based on the assumption that our planet is a cooling sphere whose outer shell or crust is constantly cracked and crumpled in adjusting itself to the shrinking nucleus.

The conception that the earth was originally an intensely heated and molten mass appears to have first taken something like definite form in the minds of Leibnitz and Descartes. But neither of these philosophers was armed with the necessary mathematical equipment to subject this conception to the test of numerical calculation. Indeed it was not fashionable in their day, any more than it is with some philosophers in ours, to undertake the drudgery of applying the machinery of analysis to the details of an hypothesis. Nearly a century elapsed before an order of intellects capable of dealing with this class of questions appeared. It was reserved for Joseph Fourier to lay the foundation and build a great part of the superstructure of our modern theory of heat diffusion, his avowed desire being to solve the great problem of terrestrial heat. "The question of terrestrial temperatures," he says, "has always appeared to us one of the grandest objects of cosmological studies, and we have had it constantly in view in establishing the mathematical theory of heat." This ambition, however, was only partly realized. Probably Fourier underestimated the difficulties of his problem, for his most ingenious and industrious successors in the same field have made little progress beyond the limits he attained. But the work he left is a perennial index to his genius. Though quite inadequately appreciated by his contemporaries, the "Analytical Theory of Heat," which appeared in 1820, is now conceded to be one of the epoch-making books. Indeed, to one who has caught the spirit of the extraordinary analysis which Fourier developed and illustrated by numerous applications in this treatise, it is evident that he opened a field whose resources are still far from being exhausted. A little later Poisson took up the same class of questions and published another great work on the mathematical theory of heat. Poisson narrowly missed being the foremost mathematician of his day. In originality, in wealth of mathematical resources, and in breadth of grasp of physical principles, he was the peer of the ablest of his contemporaries. In lucidity of exposition it would be enough to say that he was a Frenchman, but he seems to have excelled in this peculiarly national trait. His contributions to the theory of heat have been somewhat overshadowed in recent times by the earlier and perhaps more brilliant researches of Fourier, but no student can afford to take up that enticing though difficult theory without the aid of Poisson as well as Fourier.

It is natural, therefore, that we should inquire what opinions these great masters in the mathematics of heat diffusion held concerning the earth's store of heat. I say "opinions," for, unhappily, this whole subject is still so largely a matter of opinion that in discussing it one may not inappropriately adopt the famous caution of Marcus Aurelius, — "Remember that all is opinion." It does not appear that Fourier reached any definite conclusion on this question, though he seems to have favored the view that the earth in cooling from an earlier state of incandescence reached finally, through convection, a condition in which there was a uniform distribution of heat throughout its mass. This is the *consistentior status* of Leibnitz, and it begins with the formation of the earth's crust if not with the consolidation of the entire mass. It thus affords an initial distribution of heat and an epoch from which analysis may start, and the problem for the mathematician is to assign the subsequent distribution of heat and the resulting mechanical effects. But no great amount of reflection is necessary to convince one that the analysis cannot proceed without making a few more assumptions. The assumptions which involve the least difficulty, and which for this reason partly have met with most favor, are that the conductivity and thermal capacity of the entire mass remain constant, and that the heat conducted to the surface of the earth passes off by the combined process of radiation, convection, and conduction, without producing any sensible effect on surrounding space. These or similar assumptions must be made before the application of theory can begin. In addition, two data are essential to numerical calculations, namely, the diffusivity, or the ratio of the conductivity of the mass to its thermal capacity, and the initial uniform temperature. The first of these can be observed, approximately at least; the second can only be estimated at present. With respect to these important points which must be considered after the adoption of the *consistentior status*, the writings of Fourier afford little light. He was content, perhaps, to invent and develop the exquisite analysis requisite to the treatment of such problems.

Poisson wrote much on the whole subject of terrestrial temperatures, and carefully considered most of the troublesome details which lay between his theory and its application. While he admitted the nebular hypothesis and an initial fluid state of the earth, he rejected the notion that the observed increase of underground temperature is due to a primitive store of heat. If the earth was originally fluid by reason of its heat, a supposition which Poisson regarded quite gratuitous, he conceived that it must cool and consolidate from the centre outwards; so that according to this view the crust of our planet arrived at a condition of stability only after the supply of heat had been exhausted. But Poisson was not at a loss to account for the observed temperature gradient in the earth's crust. Always fertile in hypotheses, he advanced the idea that there exist, by reason of interstellar radiations, great variations in the temperature of space, some vast regions being comparatively cool and others intensely hot, and that the present store of terrestrial heat was acquired by a journey of the solar system through one of the hotter regions. "Such is," he says, "in my opinion, the true cause of the augmentation of temperature which occurs as we descend below the surface of the globe." This hypothesis was the result of Poisson's mature reflection, and as such is well worthy of attention. The notion that there exist hot foci in space was advanced also in another form in 1852 by Rankine, in his interesting speculation on the re-concentration of energy. But whatever we may think of the hypothesis as a whole, it does not appear to be adequate to the case of the earth unless we suppose the epoch of transit through the hot region exceedingly remote and the temperature of that region exceedingly high. The continuity of geological and paleontological phenomena is much better satisfied by the Leibnitzian view of an earth long subject to comparatively constant surface conditions but still active with the energy of its primitive heat.

Notwithstanding the indefatigable and admirable labors of Fourier and Poisson in this field, it must be admitted that they accomplished little more than the preparation of the machinery with which their successors have sought and are still seeking to reap the harvest. The difficulties which lay in their way were not mathematical but physical. Had they been able to make out the true conditions of the earth's store of heat, they would undoubtedly have reached a high grade of perfection in the treatment of the problem. The theory as they left it was much in advance of observation, and the labors of their successors have therefore necessarily been directed largely towards the determination of the thermal properties of the earth's crust and mass.

Of those who in the present generation have contributed to our knowledge and stimulated the investigation of this subject, it is hardly necessary to say that we owe most to Sir William Thomson. He has made the question of terrestrial temperatures highly attractive and instructive to astronomers and mathematicians, and not less warmly interesting to geologists and paleontologists. Whether we are prepared to accept his conclusions or not, we must all acknowledge our indebtedness to the contributions of his master hand in this field as well as in most other fields of terrestrial physics. The contribution of special interest to us in this connection is his remarkable memoir on the secular cooling of the earth. In this memoir he adopts the simple hypothesis of a solid sphere whose thermal properties remain invariable while it cools by conduction from an initial state of uniform temperature, and draws therefrom certain striking limitations on geologic time. Many geologists were startled by these limitations, and geologic thought and opinion have since been widely influenced by them. It will be of interest, therefore, to state a little more fully and clearly the grounds from which his arguments proceed. Conceive a sphere having a uniform temperature initially, to cool in a medium which instantly dissipates all heat brought by conduction to its surface, thus keeping the surface at a constant temperature. Suppose we have given the initial excess of the sphere's temperature over that of the medium. Suppose also that the capacity of the mass of the sphere for diffusion of heat is known, and known to remain invariable during the process of cooling. This capacity is called diffusivity, and is a constant which can be observed. Then from these data the distribution of temperature at any future time can be assigned, and hence also the rate of temperature increase, or the temperature gradient, from the surface towards the centre of the sphere can be computed. It is tolerably certain that the heat conducted from the interior to the surface of the earth does not set up any reaction which in any sensible degree retards the process of cooling. It escapes so freely that, for practical purposes, we may say it is instantly dissipated. Hence if we can assume that the earth had a specified uniform temperature at the initial epoch, and can assume its diffusivity to remain constant, the whole history of cooling is known as soon as we determine the diffusivity and the temperature gradient at any point. Now Sir William Thomson determined a value for the diffusivity from measurements of the seasonal variations of underground temperatures, and numerous observations of the increase of temperature with depth below the earth's surface gave an average value for the temperature gradient. From these elements and from an assumed initial temperature of 7,000°, he infers that geologic time is limited to something between twenty million and four hundred million years. He says: "We must allow very wide limits in such an estimate as I have attempted to make; but I think we may with much probability say that the consolidation cannot have taken place less than twenty million years ago, or we should have more underground heat than we actually have, nor more than four hundred million years ago, or we should not have so much as the least observed underground increment of temperature. That is to say, I conclude that Leibnitz's epoch of emergence of the *consistentior status* was probably between those dates." These conclusions were announced twenty-seven years ago, and were republished without modification in 1883.

Recently, also, Professor Tait, reasoning from the same basis, has insisted with equal confidence on cutting down the upper limit of geologic time to some such figures as ten million or fifteen million years. As mathematicians and astronomers, we must all confess to a deep interest in these conclusions and the hypothesis from which they flow. They are very important if true. But what are the probabilities? Having been at some pains to look into this matter, I feel bound to state that, although the hypothesis appears to be the best which can be formulated at present, the odds are against its correctness. Its weak links are the unverified assumptions of an initial uniform temperature and a constant diffusivity. Very likely these are approximations, but of what order we cannot decide. Furthermore, if we accept the hypothesis the odds appear to be against the present attainment of trustworthy numerical results, since the data for calculation obtained mostly from observations on continental areas are far too meagre to give satisfactory average values for the entire mass of the earth. In short, this phase of the case seems to stand about where it did twenty years ago, when Huxley warned us that the perfection of our mathematical mill is no guaranty of the quality of the grist, adding that, "as the grandest mill will not extract wheat-flour from peas-cods, so pages of formulæ will not get a definite result out of loose data."

When we pass from the restricted domain of quantitative results concerning geologic time to the freer domain of qualitative results of a general character, the contractional theory of the earth may be said to still lead all others, though it seems destined to require more or less modification, if not to be relegated to a place of secondary importance. Old as is the notion that the great surface irregularities of the earth are but the outward evidence of a crumpling crust, it is only recently that this notion has been subjected to mathematical analysis on any thing like a rational basis. About three years ago Mr. T. Mellard Reade announced the doctrine that the earth's crust, from the joint effect of its heat and gravitation, should behave in a way somewhat analagous to a bent beam, and should possess at a certain depth a "level of no strain," corresponding to the neutral surface in a beam. Above the level of no strain, according to this doctrine, the strata will be subjected to compression, and will undergo crumpling, while below that level the tendency of the strata to crack and part is overcome by pressure which produces what Reade calls "compressive extension," thus keeping the nucleus compact and continuous. A little later the same idea was worked out independently by Mr. Charles Davison, and it has since received elaborate mathematical treatment at the hands of Darwin, Fisher, and others. The doctrine requires for its application a competent theory of cooling, and hence cannot be depended on at present to give anything better than a general idea of the mechanics of crumpling and a rough estimate of the magnitudes of the resulting effects. Using Thomson's hypothesis, it appears that the stratum of no strain moves downward from the surface of the earth at a nearly constant rate during the earlier stages of cooling, but more slowly during later stages. Its depth is independent of the initial temperature of the earth; and if we adopt Thomson's value of the diffusivity, it will be about two and a third miles below the surface in a hundred million years from the beginning of cooling, and a little more than fourteen miles below the surface in seven hundred million years. The most important inference from this theory is that the geological effects of secular cooling will be confined for a very long time to a comparatively thin crust. Thus, if the earth is a hundred million years old, crumpling should not extend much deeper than two miles. A test to which the theory has been subjected, and one which some consider crucial against it, is the volumetric amount of crumpling shown by the earth at the present time. This is a difficult quantity to estimate, but it appears to be much greater than the theory alone can account for.

The opponents of the contractional theory of the earth, believing it quantitatively insufficient, have recently revived and elaborated an idea first suggested by Babbage and Herschel in explanation of the greater folds and movements of the crust. This idea figures the crust as being in a state bordering on hydrostatic equilibrium, which cannot be greatly disturbed without a readjustment and consequent movement of the masses involved. According to this view, the transfer of any considerable load from one area to another is followed sooner or later by a depression over the loaded area and a corresponding elevation over the unloaded one; and in a general way it is inferred that the elevation of continental areas tends to keep pace with erosion. The process by which this balance is maintained has been called "isostacy," and the crust is said to be in an isostatic state. The dynamics of the superficial strata with the attendant phenomena of folding and faulting, are thus referred to gravitation alone, or to gravitation and whatever opposing force the rigidity of the strata may offer. In a mathematical sense, however, the theory of isostacy is in a less satisfactory state than the theory of contraction. As yet we can see only that isostacy is an efficient cause if once set in action; but how it is started and to what extent it is adequate remains to be determined. Moreover, isostacy alone does not seem to meet the requirements of geological continuity, for it tends rapidly towards stable equilibrium, and the crust ought therefore to reach a state of repose early in geologic time. But there is no evidence that such a state has been attained, and but little if any evidence of diminished activity in crustal movements during recent geologic time. Hence we infer that isostacy is competent only on the supposition that it is kept in action by some other cause tending constantly to disturb the equilibrium which would otherwise result. Such a cause is found in secular contraction, and it is not improbable that these two seemingly divergent theories are really supplementary.

Closely related to the questions of secular contraction and the mechanics of crust movements are those vexed questions of earthquakes, volcanism, the liquidity or solidity of the interior, and the rigidity of the earth's mass as a whole,— all questions of the greatest interest but still lingering on the battle-fields of scientific opinion. Many of the "thrice slain" combatants in these contests would fain risk being slain again; and whether our foundation be liquid or solid, or to speak more precisely, whether the earth may not be at once highly plastic under the action of long continued forces and highly rigid under the action of periodic forces of short period, it is pretty certain that some years must elapse before the arguments will be convincing to all concerned. The difficulties appear to be due principally to our profound ignorance of the properties of matter subject to the joint action of great pressure and great heat. The conditions which exist a few miles beneath the surface of the earth are quite beyond the reach of laboratory tests as hitherto developed, but it is not clear how our knowledge is to be improved without resort to experiments of a scale in some degree comparable with the facts to be explained. In the mean time, therefore, we may expect to go on theorizing, adding to the long list of dead theories which mark the progress of scientific thought, with the hope of attaining the truth not so much by direct discovery as by the laborious process of eliminating error.

When we take a more comprehensive view of the problems presented by the earth, and look for light on their solution in theories of cosmogony, the difficulties which beset us are no less numerous and formidable than those encountered along special lines of attack. Much progress has recently been made, however, in the elaboration of such theories. Roche, Darwin, and others have done much to remove the nebulosity of Laplace's nebular hypothesis. Poincaré and Darwin have gone far towards bridging the gaps which have long rendered the theory of rotating fluid masses incomplete. Pioncaré has in fact shown us how a homogeneous rotating mass might, through loss of heat and consequent contraction, pass from the spheroidal form to the Jacobian ellipsoidal form, and thence, by reason of its increasing speed of rotation, separate into two unequal masses. Darwin, starting with a swarm of meteorites and gravitation as a basis, has reached many interesting and instructive results in the endeavor to trace out the laws of evolution of a planetary system. But notwithstanding the splendid researches of these and other investigators in this field, it must be said that the real case of the solar system, of the earth and moon, still defies analysis; and that the mechanics of the segregation of a planet from the sun or of a satellite from a planet, if such an event has ever happened, or of the mechanics of the evolution of a solar system from a swarm of meteorites, are still far from being clearly made out.

Time does not permit me to make anything but the briefest allusion to the comparatively new science of mathematical meteorology, with its already considerable list of well-defined theories pressing for acceptance or rejection. Nor need I say more with reference to those older mathematical questions of the tides and terrestrial magnetism than that they are still unsettled. These and many other questions, old and new, might serve equally well to illustrate the principal fact this address has been designed to emphasize, namely, that the mathematical theories of the earth already advanced and elaborated are by no means complete, and that no mathematical Alexander need yet pine for other worlds to conquer.

Speculations concerning the course and progress of science are usually untrustworthy if not altogether fallacious. But, being delegated for the hour to speak to and for mathematicians and astronomers, it may be permissible to offer, in closing, a single suggestion, which will perhaps help us to orient ourselves aright in our various fields of research. If the curve of scientific progress in any domain of thought could be drawn, there is every reason to believe that it would exhibit considerable irregularities. There would be marked maxima and minima in its general tendency towards the limit of perfect knowledge; and it seems not improbable that the curve would show throughout some portions of its length a more or less definitely periodic succession of maxima and minima. Races and communities as well as individuals, the armies in pursuit of truth as well as those in pursuit of plunder, have their periods of culminating activity and their periods of placid repose. It is a curious fact that the history of the mathematical theories of the earth presents some such periodicity. We have the marked maximum of the epoch of Newton near the end of the seventeenth century, with the equally marked maximum of the epoch of Laplace near the end of the eighteenth century; and, judging from the recent revival of geodesy and astronomy in Europe, and from the well-nigh general activity in mathematical and geological research. we may hope if not expect that the end of the present century will signalize a similar epoch of productive activity. The minima periods which followed the epochs of Newton and Laplace are less definitely marked but not less noteworthy and instructive. They were not periods of placid repose; to find such one must go back into the night of the middle ages; but they were periods of greatly diminished energy, periods during which those who kept alive the spirit of investigation were almost as conspicuous for their isolation as for their distinguished abilities. Many causes, of course, contributed to produce these minima periods, and it would be an interesting study in philosophic history to trace out the tendency and effect of each cause. It is desired here, however, to call attention to only one cause which contributed to the somewhat general apathy of the periods mentioned, and which always threatens to dampen the ardor of research immediately after the attainment of any marked success or advance. I refer to the impression of contentment with and acquiescence in the results of science, which seems to find easy access to trained as well as untrained minds before an investigation is half completed or even fairly begun. That some such tacit persuasion of the completeness of the knowledge of the earth has at times pervaded scientific thought, there can be no doubt. This was notably the case during the period which followed the remarkable epoch of Laplace. The profound impression of the sufficiency of the brilliant discoveries and advances of that epoch is aptly described by Carlyle in the half humerous, half sarcastic language of Sartor Resartus. "Our Theory of Gravitation," he says, "is as good as perfect: Lagrange, it is well known, has proved that the Planetary System, on this scheme, will endure forever; Laplace, still more cunningly, even guesses that it could not have been made on any other scheme. Whereby, at least, our nautical Logbooks can be better kept; and water transport of all kinds has grown more commodious. Of Geology and Geognosy we know enough: what with the labors of our Werners and Huttons, what with the ardent genius of their disciples, it has come about that now, to many a Royal Society, the creation of a World is little more mysterious than the cooking of a dumpling; concerning which last, indeed, there have been minds to whom the question *How the apples were got in*, presented difficulties." This was written nearly sixty years ago, about the time that the sage of Ecclefechan abandoned his mathematics and astronomy for literature to become the seer of Chelsea, but the force of its irony is still applicable, for we have yet to learn, essentially, "how the apples were got in," and what kind they are.

As to the future, we can only guess, less or more vaguely, from our experience in the past and from our knowledge of present needs. Though the dawn of that future is certainly not heralded by rosy tints of over-confidence amongst those acquainted with the difficulties to be overcome, the prospect, on the whole, has never been more promising. The converging lights of many lines of investigation are now brought to bear on the problems presented by our planet. There is ample reason to suppose that our day will witness a fair average of those happy accidents in science which lead to the discovery of new principles and new methods. We have much to expect from the elaborate machinery and perfected methods of the older and more exact sciences of measuring and weighing — astronomy, geodesy, physics, and chemistry. We have more to expect, perhaps, from geology and meteorology, with their vast accumulations of facts not yet fully correlated. Much, also, may be anticipated from that new astronomy which looks for the secrets of the earth's origin and history in nebulous masses or in swarms of meteorites. We have the encouraging stimulus of a very general and rapidly growing popular concern in the objects of our inquiries, and the freest avenues for the dissemination of new information; so that we may easily gain the advantage of a concentration of energy without centralization of personal interests. To those, therefore, who can bring the prerequisites of endless patience and unflagging industry, who can bear alike the remorseless discipline of repeated failure and the prosperity of partial success, the field is as wide and as inviting as it ever was to a Newton, or a Laplace.

AMONG THE PUBLISHERS.

"TERMINAL facilities of New York" is the title of the supplement feature in *Harper's Weekly* for Aug. 31. The article is from the pen of Mr. G. T. Ferris, and the illustrations, of which there are thirteen, were drawn by Messrs. Schell and Hogan.

— Following the article on the late Miss Laura Bridgman, in the August *St. Nicholas*, the number for September contains an account of "Helen Keller," the young girl also deaf, dumb and blind, whose rapid advance in her studies was described in *Science* a year ago. The sketch is by Florence Howe Hall, a daughter of Dr. Howe, and contains portraits of the child, of her teacher, a facsimile letter from the little girl herself to Mrs. Hall, and other illustrations. In the same number Lieutenant Hamilton gives a sketch of the modern method of defending coasts or harbors, and shows how necessary such defences have become as a consequence of the development of the world's navies.

— The September number of the *Political Science Quarterly* contains a critical estimate of the work of Thorold Rogers, by Professor W. J. Ashley of Toronto University; a demonstration of the "radical unfairness" of representation in Connecticut under the town-rule system, by Clarence Deming of New Haven; a discussion of farm mortgages, by an Illinois farmer, W. F. Mappin; a strong attack upon the policy of the general land office as regards the "indemnity lands" granted to the railroads, by Fred. Perry Powers of Washington, D.C.; a statistical paper upon Italian immigration, by Hon. Eugene Schuyler; the first of two papers upon the materials for English legal history, by Professor F. W. Maitland, Downing professor of law at Cambridge University, England; and the usual number of book reviews.

INDUSTRIAL NOTES.

Electrical Apparatus Abroad.

INFORMATION has reached us that the Sprague Electric Railway and Motor Company has recently closed quite a large contract for electric street railway apparatus with the principal street railway company of Florence, Italy, for the equipment of their line. This apparatus includes overhead system of the regular Sprague type, ten complete car equipments using two thirteen horse-power motors on each car, and station equipment complete.

This will be the first installation of American street railway apparatus abroad, where the progress in electric railway science has been very slow. The present method of running street cars in Florence is partly by animal power and partly by small steam dummies; and it is thought that the electric cars which combine the safety of the horse-car with the speed of the steam-car, and are much cheaper to operate than either, will have a large field to fill. It is said that this equipment is only a small portion of a very large equipment which will be ordered by this company, and if the result proves successful, it is thought that very many other Italian cities will adopt electricity for their street cars.

Electricity at Cleveland.

Cleveland can now be called properly the electric city of the West. In a short time there will be over a hundred electric street-cars running over the principal streets of Cleveland, besides a large number of stationary electric motors in use in a great many varied industries throughout the city.

The history of the East Cleveland Street Railway Company, which was the first in Cleveland to adopt electricity on its line, is an instance of the success and satisfaction which electric street railway cars are giving in every city where they have been installed. The first equipment of this company was installed by the Sprague Company about nine months ago, and included overhead line, station equipment, and sixteen electric motor cars. The proposition to install this line met with a great deal of opposition in Cleveland.

The electric line was to cover some of the most important and principal business and residential streets in Cleveland, but the equipment was finally installed; and after it had been put in operation, the citizens of Cleveland discovered that the neat iron poles and overhead erection were hardly noticeable, while the rapid transit afforded by the street cars was something vastly superior to the former slow service given, when the cars were drawn by animal power.

There have altogether been five separate orders given by the East Cleveland Company for electric car apparatus. The second order was for four additional cars, the third for eight additional cars, the fourth for eighteen additional cars, and a recent order placed with the Sprague Company by its agent, Mr. C. W. Foote, for thirty additional motors, making seventy-six motor cars to be operated on this one line.

Besides this road, there are two others in Cleveland; the Broadway and Newburgh, and the Brooklyn Avenue roads also operated by electricity. There is nothing which speaks more highly for any kind of apparatus than the indorsement by its users, and there is no indorsement more convincing than the continued addition to an original equipment. The results, therefore, at Cleveland prove conclusively the good results and satisfaction given by electric apparatus when applied to street railways, and cannot be too commendatory of the style of motors used.