SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

FRIDAY, OCTOBER 28, 1904.

CONTENTS:

MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

PRESENT PROBLEMS OF GEOPHYSICS.*

ADVANCES in science are seldom made without a view to the solution of specific, concrete problems, even when the results of investigation possess the widest generality. The history of science is full of instances of the fruitfulness of researches the immediate purposes of which were narrowly defined. Geophysics is only that portion of general physics, including under that term physical chemistry, which is applicable to the elucidation of the past history and present condition of the earth. It is thus a very definite branch of applied science, the exigencies of which call for the solution of a group of related problems. These, however, possess great interest apart from their application to the globe, while for the most part they offer very serious experimental and theoretical difficulties. Had they been easy, they might have been solved long ago, for many of these problems have been propounded and more or less discussed from the birth of modern science to the present day. Their difficulty, not lack of recognition of their importance, has postponed their solution.

The main purpose of this paper is to deal with the order in which it would be expedient to investigate the questions embraced under the head of geophysics, but a brief and incomplete enumeration of the problems from a geological standpoint will serve to lend a coherency and a human

* Address delivered at the International Congress of Arts and Science, at St. Louis, before the Geophysical Section of Department 12, on September 21, 1904.

interest to the subject which it would otherwise lack.

Physical geology begins with the solar nebula and the genesis of the earth-moon system. The harmonies of the solar system compelled the immortal Kant and the ever-living Laplace to seek the origin of the planets, the sun and the other stars in heterogeneous nebulas which they supposed to have condensed about one or several nuclei. Every attempt to devise an essentially different hypothesis has failed, and every history of the globe which begins after the birth of the planet is unsatisfying. In the drama of the universe there must have been pre-nebular scenes, but of these we have as yet no inkling. The nebular hypothesis, as its authors propounded it, explains the similarity in the composition of the members of the solar system which is indicated by the analysis of meteorites and by the spectroscope, though the facts thus revealed were unknown to Kant and Laplace. It is also compatible with and accounts for the heterogeneity in the composition of the earth manifested in the actual asymmetric distribution of oceans, mountain ranges and anomalies of gravitational force, as well as in the curiously local occurrence of certain ores (such as those of tin and mercury) and in the predominance of certain alkalies among the rocks over wide areas.

This heterogeneity, however, is of a small order of magnitude. The general dependence of gravity on latitude, the nearly spheroidal shape of the earth and other phenomena show that the distribution of density is nearly symmetrical, while the divergence of the spheroid from the figure characteristic of a fluid of the same mean density and mass as the earth demonstrates that the interior layers of equal density are oblate. These and similar facts are consistent with and are strong evidence for the hypothesis that the globe has been fused at least to a considerable depth from the growing surface of the gathering nebulous mass. Nevertheless, Houghton, and more recently Professor Chamberlin, have supposed that the accretion of nebulous matter was so slow that the heat of impact did not suffice to produce fusion. The hypothesis of superficial fusion is not incompatible with the minor heterogeneity pointed out above; for the laws of diffusion in viscous fluids give proof that sensibly perfect homogeneity could not be produced even in 50,000,000 years throughout a body of liquid originally heterogeneous and possessing a tenth of the mass of the earth. On the other hand, there is no known ground other than mere convenience for supposing an original homogeneity either of the nebula or of the earth.

The problem of the distribution of density in the earth is one of the most important in all geophysics. It is as significant for geodesy and terrestrial magnetism as for geology. That Laplace's empirical law represents it approximately is generally acknowledged, but it appears substantially certain that this is merely an approximation without theoretical value. Only extended researches, however, can replace it by one better founded.

The solidity of the earth is now very generally accepted, though Descartes's hypothesis of its fluidity, invented to satisfy his erroneous theory of vortices, died hard. Lord Kelvin showed from tidal phenomena that the effective rigidity of the earth is about that of a continuous globe of steel. Professor Newcomb pointed out that the Chandlerian nutation leads to the same conclusion and an almost identical value of the modulus of rigidity, and Professor George H. Darwin demonstrated that, if the earth is a viscous liquid, its viscosity must be some 20,000 times as great as that of hard brittle pitch near the freezing point of water. From the point of view of modern physical chemistry and in consideration of Professor Arrhenius's opinions, the matter requires further consideration. In particular it is most important to know whether the earth is substantially a crystalline solid or an amorphous substance, for many modern physical chemists consider amorphous matter as liquid. This opinion is far from being established, however, and recent experiments by Mr. Spring show that mere deformation at ordinary temperatures, attended by only a very small absorption of energy, suffices to convert crystalline metals into substances exhibiting characteristics of amorphous bodies. Since Nordenskiöld's great discovery of large masses of terrestrial iron, or, rather, nickel steel, in Greenland, and the wide distribution since proved for similar metal imbedded in igneous rocks, a great amount of evidence has accumulated that a large part of the earth is composed of material indistinguishable from that of metallic meteorites. Meteoric iron is of course a highly crystalline material.

It is a very striking fact that the mean rigidity of the earth is about that of steel, for the only substance likely to occur in extensive continuous masses and displaying such rigidity at ordinary temperatures and pressures is steel itself. Nevertheless, the conclusion can not yet be drawn from the resistance to deformation displayed by the earth, that it is chiefly composed of steel. Elastic resistance is known to be a function both of pressure and of temperature, and until this function has been determined by theory and experiment, the bearing of the evaluation of rigidity by tidal action can not be ascertained.

Having shown the earth to be a solid globe, Lord Kelvin calculated its age from one of Fourier's theorems, assuming for purposes of computation an initial temperature of 7000° F. (nearly 3900° C.) and that the thermal diffusivity of the earth is that of average rock. These assumptions, with the observation that the temperature near the surface of the earth increases at the rate of 1° F. for every 50 feet of depth, lead to an age of 98,000,000 years; but on account of the uncertainty as to conductivities and specific heats in the interior, the conclusion drawn by Lord Kelvin was only that the time elapsed since the inception of cooling is between 20 and 400 million years.

Clarence King subsequently took a further important step on the basis of data determined at his request by Professor Carl Barus on the volume changes which take place in diabase during congelation, and on the effects of pressure in modifying the melting and solidifying points. Assuming that the earth can never have had a crust floating on a liquid layer of inferior density, computation leads him to 24 million years as the maximum period for the time since superficial consolidation was effected, provided that the superficial temperature gradient and conductivity are correctly determined.

These researches, together with Helmholtz's investigation on the age of the solar system, which is incomplete for lack of knowledge of the distribution of density in the sun, have had a restraining influence on the estimates drawn from sedimentation by geologists. Many and perhaps most geologists now regard something less than 100 million years as sufficient for the development of geological phenomena. Yet the subject can not be regarded as settled until our knowledge of conductivities is more complete. An iron nucleus, for example, would imply greater conductivity of the interior and a higher age for the earth than that computed by King, though probably well within the range explicitly allowed by Lord Kelvin in view of the uncertainty of this datum.

The researches of Kelvin and Darwin, supplementing those of Kant and others, have left no doubt that the moon was formerly closer to the earth than it now is, and that the rotation of the latter was more rapid, involving a greater ellipticity of the meridian than it now shows. In a fluid or Cartesian earth the change of figure might have produced little effect on the structure of the planet. If the earth is chiefly a mass of crystalline nickel steel, it is very possible that such a change in the figure of equilibrium might rupture it. Since the epoch at which the earth rotated in 5 hours 30 minutes, the polar axis must have elongated by several per cent., most of it before the time of rotation was reduced to 11 hours.* Were the earth chiefly composed of forged steel, such elongation might be produced by plastic deformation; but meteoric iron is rather comparable with cast iron, or better still, with relatively brittle, unforged cement steel, and might break.

Now it is an indubitable fact that a majority of the outlines of the great oceanic basins and of the chief tectonic lines of the globe, lie nearly on great circles tangent to the Arctic Ocean and to the Antarctic continent.† These lines, or most of them, are of extremely high geological age, their main features having found expression as early as the oldest known fossils and in some cases still earlier. It appears to me very possible that these fundamental ruptures of the globe were due to the change of figure attendant upon diminution of the earth's period of rotation. Their symmetrical disposition with reference to the polar axis is unquestionable, as well as the fact that they penetrate to great depths. They must be due to some tremendous force acting axially, which actually altered the ellipticity of the meridian, since these fissures could not have been formed without modifying the shape of the globe, and the only known disturbance of this description is the change of figure referred to. On the other hand, were the earth homogeneous, such ruptures would be expected to have as envelopes small circles in latitude 45° instead of at about latitude 70°. But since the earth is not homogeneous, this discordance does not invalidate the suggestion.

Be this as it may, upheavals, subsidences and attendant contractions have been in progress throughout the whole of historical geology or the period within which fossils afford a guide to the succession of strata. The so-called contractional theory has shown itself wholly inadequate to account for the amount of deformation traceable in the rocks of the globe, nor has the extravasation of igneous rock been sufficient to account for the phenomena. To me the earth appears to be a somewhat imperfect heat engine in which the escape of thermal energy is attended by the conversion of a part of the supply into the vast amount of molar energy manifested in the upthrust and crumpling of continents. The subject will probably turn out to be accessible mathematically after certain experimental determinations have been made, and I shall return to it presently.

Orogeny or mountain building is a mere

* Compare Thompson and Tait, 'Nat. Phil.,' § 772, where the rotational period and eccentricity are given for a fluid of the mass of the earth and possessing its mean density. When the period is 5h. 30m., this table gives the data for computing that the polar axis has a length equal to 0.95 of the length which it has when the period is a siderial day. For rotation in 10h. 57m. the polar axis is 0.99 times that for a day.

† In 1857 Professor R. Owen, of Tennessee, and, independently, Benjamin Peirce, called attention to the tangency of the coast lines to the polar circles (not to the coast lines of the arctic sea and the antarctic continent), each attributing the facts to the influence of the sun. In the first 'Yearbook' of the Carnegie Institution I failed to refer to these publications.

detail of the more general subject of upheaval and subsidence, but it exhibits problems of great complexity both from the experimental and from the theoretical points of view. There is no question that unit strains are often reached or even surpassed in contorted strata and in belts of slate, but the theories of elasticity and plasticity as yet developed are inadequate to deal with these strains in complex cases. An investigation on finite elastic and plastic strain is now under way in my laboratory and has made gratifying progress thus far; but this is not the place for detailed results. Something also has been done in the way of working out homogeneous finite strains in rocks, so that the general nature of joints, faults and systems of fissures and the mechanism of faulting is now fairly clear. The theory of slaty cleavage is a subject of dispute in which I have taken part. Few colleagues appear to agree with me that this cleavage is due to weakening of cohesion on planes of maximum slide, but I am not hopeless that my view will make its way to favor in time.

Seismology is a vast subject by itself, but one almost totally lacking in theoretical foundation. Seismological observations should afford the means of exploring the elastic properties of the earth throughout its interior, but the theory of the vibrations of a spheroid like the earth is not yet worked out. Meantime observations are being accumulated, but it can be foreseen that these will contribute little to elucidation until they include the vertical components of the vibrations as well as the horizontal ones. In other words, we must know the angle at which the wave emerges from the surface as well as its azimuth. The causes and conditions of earthquakes afford a separate topic of great interest. That some of them are of volcanic origin is evident; others appear to be due to paroxysmal faulting, yet there is very possibly a common underlying cause.

On no subject are opinions more divergent than concerning the origin and mechanism of volcanoes. To the ancients they were the mouths of the river Phlegethon. To those who adhere to the Cartesian doctrine they are communications with the liquid interior of the earth. Most geologists think of them as connected with hypogeal reservoirs of melted matter subsisting for indefinitely long periods of time. Finally it is conceivable that the lava may be extruded as soon as the melted mass has accumulated in sufficient quantity, somewhat as water may break through an obstructing dam after its depth reaches a certain value. The continual movements of the rocks show that they must be to some extent in a state of elastic strain, so that a given cubic mile of rock resists surrounding pressure in virtue both of its rigidity and of its compressibility. If that cubic mile becomes liquid, its rigidity is gone and the change of shape of surrounding masses may aid in its expulsion. Of course imprisoned gases, especially the 'juvenile waters' of Professor Suess, may also play a very important part in expulsion. But the more I have studied the matter, the less probable it seems to me that considerable bodies of melted lava can remain quiet for long periods of time in the depths of the earth. The influences tending to their expulsion would seem to be at a maximum immediately after the fusion of enough material to supply an eruption.

Relief of pressure is often invoked to explain fusion of lava, but it is not a wholly satisfactory cause. If a deep crack were to form, the rock at the bottom might melt indeed, but, as the crack filled, the pressure and the solidity of the source would be restored. To me, Mallet's hypothesis is more satisfactory, so far as the explanation of fusion is concerned. Only those who have studied the minute evidence of mechanical action in mountain ranges can appreciate the evidence they present of stupendous dissipation of energy. This has not indeed been enough to fuse the rocks, but it is hard to conceive that it is always insufficient to furnish the latent heat of fusion to rocks already close to their melting point under the prevailing pressure. From this point of view, vulcanism is a feature of orogenic movement and it is to be looked for where relative motions are concentrated in zones so narrow that the local dissipation of energy is relatively intense. It is also possible that percolating waters, by reducing the melting points of rocks, sometimes bring about fusion without change of temperature. Such an hypothesis might fit the volcanoes of the Hawaiian islands where there is no known faulting in progress.

The physics of magmatic solutions is a great subject which is experimentally almost untouched, although a vast amount of geological speculation has been based upon assumed properties of magmas. It is only within a few months that even satisfactory melting-point determinations of those most important rock-forming minerals, the lime-soda feldspars, have been made. The feldspars are only one series of isomorphous mineral mixtures. Their study is fundamental and must be followed by that of the remaining class, *i. e.*, the eutectics. These, in my opinion, will lead to a rational classification of igneous rocks, themselves mixtures and incapable of logical description except in terms of standard mixtures, the eutectics.

It appears to me highly probable, for many reasons, that the magmas of the granular rocks are not liquids but stiff emulsions, comparable with modeling clay, the solid constituents (perhaps free oxides) being merely moistened with magmatic liquids. Such masses behave mechanically like soft solids; they display some rigidity and in them diffusion is reduced to a vanishing quantity. They may be ruptured and the (aplitic or pegmatitic) liquid portion may then seep into the cracks. Such a magma might be forced into minute fissures, as is the case when clay is molded to terra cotta articles and yet it would support permanently, on its upper surface, rocks of superior density. Only in such a magma can I comprehend the simultaneous growth of crystals of various minerals; for in a liquid not exactly eutectic, the formation of crystals must follow a definite order. Again, if banded gneisses and gabbros had been fluid, the bands would show evidences of diffusion which as a rule are absent or barely traceable in these rocks.

The relations between consanguineous massive rocks have occupied a large part of the attention of geologists for many years. At one time it was supposed that homogeneous liquid magmas might split up into two or more homogeneous magmas by processes of molecular flow due to differences of osmotic pressure. This process was called the differentiation of magmas. It has been shown, however, that these processes are so much slower even than heat diffusion, that they can not be efficient beyond distances of a few centimeters. For this reason, Mr. Teall,* who first suggested the application of the Soret process to account for differentiation, Professor Brögger† and others, have abandoned the hypothesis of differentiation on a considerable scale by molecular flow. Nevertheless, observations on laccolites and other occurrences leave no doubt that a single magma may solidify to different though consan-

* *Proc. Geol. Soc. London*, Vol. 57, 1901, p. lxxxv.

† 'Eruptivgesteine der Kristianiagebietes,' part III., p. 339.

guineous rocks. If the separation is not molecular, it is self-evident that it must be molar. The only molar currents readily conceivable in a body of magma are convection currents, and these, or even an equivalent mechanical stirring, would necessarily lead to fractional crystallization, a familiar process known even to the pupils of Aristotle, and which is almost unavoidable when mixed solutions solidify. This process is one of precipitation and is absolutely distinct from the differentiation (or more properly, segregation) of rock magmas, in which a single liquid is supposed to separate into two or more distinct liquids. The general conditions of the order of precipitation during fractional crystallization in accordance with the phase rule are by no means beyond the reach of discussion, and the able investigations of Messrs. J. H. L. Vogt and J. Morozewicz have a direct bearing on this subject.

A mystery which will assume greater importance as the accessible supply of coal diminishes is the origin of petroleum. There is much to be said in favor of the unpopular hypothesis of Mendeleef, supported by experiments on cast iron, that liquid hydrocarbons are due to the decomposition of the iron carbides of the terrestrial nucleus. Such vast accumulations of oil as exist on the Caspian and in the Caucasus seem incompatible with the hypothesis of animal or vegetable origin, although oils belonging to the same series as do the petroleums have been produced in the laboratory from organic materials. On the other hand, some meteorites contain hydrocarbons (which may themselves be due to the alteration of iron carbides) and there are geologists who infer that the petroleum may be derived from the mass of the earth itself.* If the origin of the oil is not animal or vegetable, the supply is very likely inexhaustible. More extended study of the connection between volcanic phenomena and the origin of asphaltic and other hydrocarbons is a desideratum.

* See H. L. R. Fairchild, *Bull. Geol. Soc. Amer.*, Vol. 15, 1904, p. 253.

Ore deposits themselves form the branch of geology which was earliest cultivated and which will never lose its interest so long as mankind remains gainful. Yet much remains to be done by experiment for the theory and practice of mining geology. The mechanism of the secondary enrichment of ores, particularly those of copper, detected by Mr. S. F. Emmons and enlarged upon by Mr. W. H. Weed, is being studied experimentally in the laboratories of the U. S. Geological Survey. A feature deserving careful experimental study is the osmotic separation of ores from their solutions by the wall rock. Many minutiæ of occurrence suggested that the walls of veins often act as a species of diaphragm or molecular filter and have a dialytic action on the ore solutions.* The origin of the ores themselves is still very obscure and will hardly be elucidated until more is known of the earth's interior. Sometimes they seem to be derived from adjacent rocks; in other cases conditions suggest that the rocks and the veins derive their metallic content from a common deep-seated source. Here, as in several other connections, Professor Suess's theory of 'juvenile waters' is very suggestive. It is held that many of the great iron deposits are due to magmatic separation. Deposition of lead ores by replacement of calcite is a known process, but takes place under unknown conditions. In some cases replacement of rock by ores appears to me to be alleged without sufficient proof. Pseudomorphosis is the only adequate test of replacement.

Erosion appears to be a subject which is capable of more exact treatment than it has received. Weathering and abrasion

* 'Min. Resources of the U. S. for 1892,' p. 156.

proceed with a rapidity which increases with the surface exposed per unit of volume.* Hence these processes lead to minimum surfaces. Therefore also the mathematics of erosion is essentially identical with that of capillarity.

Geological climates are as interesting to astrophysicists as to meteorologists and geophysicists. Messrs. Langley and Abbot appear to have evidences of recent variations in solar emanation. If these have been considerable in the course of the period of historical geology, light should be thrown upon them by the paleontology of the tropics. Variations in the composition of the atmosphere must have been very influential in determining both the mean temperature of the earth's surface and the distribution of temperature; but so also is the distribution of water. No theory of the glacial period seems generally accepted. Croll's theory is discredited. I have shown to my own satisfaction that the astronomical conditions most favorable to glaciation are high obliquity and low eccentricity of the earth's orbit,† but can not claim any extensive following. If I am right, it should be possible to obtain a definite measure of geological time in years as soon as the astronomers have completed the theory of secular variations in the planetary system so far as to be able to assign the lapse of time between successive recurrences of low eccentricity and high obliquity.

A most interesting observation, which promises much light on the past history of the globe, is that lavas and strata indurated by lavas retain the polarity characteristic of the locality in which they cooled.‡ The time may come when this will lead to determinations of the relative age of lavas, the duration of periods of eruption and possibly even absolute determinations of date.

Geology has long, and with some justice, labored under the reproach of inexactitude. As has been illustrated in the preceding pages, the science is still in the qualitative stage and almost wholly lacks the precision of astronomy. Even its most ardent students have seldom succeeded in ascertaining the quantitative relations between effects and operative causes and have been perforce content to indicate tendencies. Thus geological doctrine is far too much a matter of opinion, but this is hardly the fault of the areal geologist. The country must be mapped both for economic reasons and to accumulate a knowledge of the facts to be explained. Working hypotheses the field geologist must have, or he could not prepare his map; and he is only responsible for living up to the standard of knowledge of his time. He is continually face to face with phenomena for which physics and chemistry should account, though they have not yet done so, and must accept seeming probabilities where certainty is unattainable. So, too, Kepler's predecessors recorded facts and guessed at generalizations as best they might.

The physics of extreme conditions still awaits satisfactory exploration. The geologist turns to the physicist for help and in most cases meets with the reply: We can not tell. Astrophysics is in much the same situation. Astronomers know as little of the distribution of density in the stars or planets as do geologists. Real knowledge of the physics and chemistry of high temperatures would be as welcome to them as to us. After all, physical geology is the astrophysics of this, the only accessible planet. Geodesy, too, and terrestrial magnetism are waiting for the solution of geophysical problems. How much might be done, Lord Kelvin and Mr. George H.

* U. S. Geol. Survey, Mon. XIII., 1888, p. 68.

† *Amer. Jour. Sci.*, Vol. 48, 1894, p. 95.

‡ Brunhes and David. *Comptes Rendus*, Vol. 133, 1901, p. 153.

Darwin have shown; but there are many problems too broad and too laborious to be solved by individual effort, and these are as essential to the rounding out of the science of physics as they are to the development of geology and astrophysics.

In the brief review which precedes, I have endeavored to show that the history of the earth bristles with problems, few of them completely solved, though in many cases we have some inkling of the solution. This sketch has been drawn for the purpose of considering the strategy of a campaign against the series of well intrenched positions occupied by our great enemy, the unknown.

Generalizing the results of the sketch presented, it is easy to see that nearly all the problems suggested involve investigation of the properties of solids, or of liquids, or of the transition from one phase to the other. It is the business of the experimental physicist to establish linear relations; it is the occupation of the mathematical physicist to draw logical inferences from these relations. Each will have plenty to do in a methodical study of geophysics.

There can be no doubt that the character of the earth's interior and the physical laws which there prevail constitute the most fundamental object of geological and geophysical research, while the results of successful investigation would be immediately applicable at least to the moon and Mars. No one questions that enormous pressures and very high temperatures exist near the earth's center, while the quality of matter which constitutes the interior can not be satisfactorily determined until we know how substances would behave under extreme pressures and at temperatures approaching 2000° C. There is every reason to suppose that under purely cubical compression, dense, undeformed solids are perfectly elastic. Hence the basal problem of geophysics is to find the law of elastic compressibility. This can not be accomplished by direct means, but the task is, nevertheless, as pointed out above, not a hopeless one, and has been taken in hand. Should success be achieved, researches will follow on the variation of elasticity with temperature. This feature of the investigation will present very great experimental and theoretical difficulties, but there is no good reason to despair of success.

When the law of resistance of solid bodies becomes known as a function of both temperature and pressure, even for isotropic substances with only two moduluses of elasticity, the way will be opened to various important investigations, largely mathematical in character. It is true that thoroughly isotropic bodies are seldom met with, yet geological masses must, nevertheless, often approach closely to this ideal. Many of the most important rocks are chiefly composed of triclinic feldspars, which, indeed, occur about as abundantly as all other minerals found at the surface of the earth put together. A triclinic feldspar crystal rejoices in the full possible number of elastic moduluses, 21. Yet a large spherical mass of small, fortuitously oriented feldspars will behave to external forces of given intensity and direction in the same way, no matter how the sphere may be turned about its center, and will, therefore, act as an isotropic body. This fact is enough to show that an infinite variety of intimate molecular structures are compatible with molar isotropy.

Thus a knowledge of isotropic elasticity will suffice as a basis for testing reasonable hypotheses of the constitution of the earth's interior, taking into account its known rigidity and density. Still greater light can be thrown on this subject by including in the investigation the moon and Mars; for their masses and dimensions are known and there seems every probability that they are composed of the same materials as the earth, though in different proportions. If a given hypothesis as to the chief constituents satisfies the known conditions of all three planets, it will doubtless find acceptance. Such a result would open the way to fresh advances in geodesy and terrestrial magnetism, and cast backward through the vista of time a ray of light on the nebular hypothesis.

Again, when the law of elasticity and the approximate constitution of the globe are known, it will be possible to work out a satisfactory theory of the simpler modes of vibration in a terrestrial sphere, and then seismological observations can be applied to determining more precisely the intrinsic elastic moduluses of the earth along the paths of earthquake waves.

It will also be practicable to examine critically the possible rupture of the globe as a consequence of change of figure and to study intelligently the simpler cases of the crumpling of strata, fissuring and other problems in the mechanics of orogeny.

The science of elasticity has had a very disappointing history. Simple as is the assumption *ut tensio, sic vis,* the attempt to solve even such seemingly elementary problems as the flexure of a uniformly loaded rectangular bar leads to insoluble equations; so that the science has been relatively unfruitful. It remains to be seen whether a truer relation between load and strain will not simplify formulas and increase the applicability of algebra to concrete cases.

From an astrophysical point of view the dialytic action of mineral septa is unimportant, but it is very interesting in its bearing on metamorphism and ore deposition, and may readily contribute to economic technology.

The relations of viscosity to the diffusion of matter have not yet been elucidated even for ordinary temperatures. This subject is one of much importance in connection with the genesis of rock species, and of course it should be studied at 10° before undertaking researches at 1000°.

High temperature work is essential even to the investigation of the elastic problem and it is almost a virgin field. Even thermometry is very imperfect above the melting point of gold, though it is destined soon to become exact at least as high as 2000°, a range which will probably suffice for geophysics. But we are also in almost total ignorance of the extent to which the laws of physics, studied at ordinary temperatures, prevail at one or two thousand degrees. One of the less difficult problems of this group is that of thermal conductivity and specific heat of solid bodies at high temperatures. For the principal metals this is already known as far as 100°, but not for rocks or minerals. It would be especially desirable to have such determinations for granite, basalt and andesite, the last representing the average composition of the accessible part of the lithosphere.

It seems to me that when the thermal diffusivities are known for these rocks, over a range of a thousand degrees, the question of upheaval and subsidence can be attacked with a good prospect of success. A cooling sphere is conceivable in which the distribution of thermal diffusivity is such that the flow of heat would be 'steady,' in Fourier's sense, and thus accompanied by no superficial deformation. With any other distribution of diffusivities, deformation would occur, and the globe would act as an imperfect heat engine, the work done being that of upheaval or subsidence. Now when the assuredly variable value of diffusivity for the materials of the globe is known, the mathematical conditions for steady flow can be worked out, and if these are not consistent with the facts of the globe, a *vera causa* for upheaval will have been found, which may lead to further and more detailed conclusions. It should also either elucidate or simplify the subject of the fusion of magmas and their eruptive expulsion.

The data for constitution and thermal diffusivity will readily be applicable to the problem of the earth's age and will yield a corrected value of the probable lapse of time since the initiation of the *consistentior status* of the Protogæa.

The most difficult field in geophysics is the study of solutions at high temperatures. This is largely because both methods and apparatus require to be invented. When work of this kind was undertaken in the laboratory of the Geological Survey, three years since, no furnace existed in which pure anorthite could be melted and a trustworthy determination of the temperature of fusion made. For the study of aqueo-igneous fusion, which must, of course, be performed at considerable pressures, extremely elaborate preparation is necessary; indeed, all attempts hitherto made in this direction have been only very partially successful.

Were it not that the number of important rock-forming minerals is small, the study of igneous solutions for geophysical purposes would be an almost hopeless task. The feldspars, the pyroxenes, the amphiboles and the micas appear to form isomorphous series and must be studied as such. They, with quartz, make up nearly 93 per cent. of the igneous rocks, nepheline, olivine, leucite, apatite, magnetite and titanium minerals substantially completing the list which enter into these rocks in sensible proportions. After the melting points of the minerals have been determined and their isomorphism has been studied, the most important research to be undertaken is that on their eutectic mixtures. Other features, however, must receive attention, such as their latent heat, ionization, viscosity and diffusivity. Immensely interesting will be the study of melts into which hydroxyl enters as a component and which may turn out to be emulsions rather than solutions. Such researches will constitute a most substantial addition to physical science and, as pointed out above, offer a good prospect for the rational classification of rocks.

Enough has been said to show how closely geophysical researches interlock. Researches at high temperatures must accompany investigations at common temperatures, physics must be supplemented by physical chemistry, mathematical ability of the highest order must be called upon at every step to elucidate difficulties and to draw inferences capable of being again submitted to inquiry, and some geological knowledge, too, is requisite to appreciate the bearing of results and to indicate the questions of importance. No human being has the length of days, the strength, the skill or the knowledge needful to undertake, without help, the investigation of geophysics as a whole. Only a few of the topics touched upon in the earlier pages of this essay are independent of cooperation; for instance, the astronomical conditions favorable to glaciation and perhaps the application of the mathematics of capillarity to the problem of erosion. On the other hand, the list of geophysical problems requiring cooperation could be almost indefinitely extended even now, and will be supplemented when the most pressing questions approach their answers.

Organization increases efficiency in scientific work as much as in technical pursuits, though it has seldom been attempted. Instances in point are the U. S. Geological Survey, the Reichsanstalt and astronomical surveys of the sky. Geophysics, then, is too difficult a subject to be dealt with excepting by a well organized staff, working on a definite plan resembling that indicated above. The tastes and convenience of individuals must give way to the methodical advancement of knowledge along such lines that the work of each investigator shall be of the utmost assistance to the progress of the rest.

Work in geophysics is already in progress in this country, thanks to the appreciative sympathy of Director Walcott, of the Geological Survey and the liberality of the Carnegie Institution, by members of my staff and in part under my direction. Messrs. A. L. Day and E. T. Allen have made an excellent series of determinations of the melting points of the triclinic feldspars and studied their other thermal properties. They are now preparing to make experiments in aqueo-igneous fusion. Mr. C. E. Van Orstrand has made a novel application of the theory of functions to elastic problems and has reduced several series of important observations on elastic strains for comparison with theory. Dr. J. R. Benton is occupied in experimental investigation of elastic strains in various substances. The men engaged in these researches are able and devoted to their work, but they are too few in number, and they are required to make determinations of the most delicate character in an office building standing in the busiest portion of Washington, where the walls are in a state of incessant tremor and where there is no suggestion of uniformity of temperature. Under such circumstances the results of observation can not be of the most refined character and must be obtained at great expense of time and effort.

Most of the great physicists of the world have expressed their interest in geophysics and their belief that the time is ripe for its investigation. Geologists are eager for its results, but no government can undertake investigations so remote from industry as this. I do not think I can more fitly conclude this paper than by quoting a resolution introduced by Mr. S. F. Emmons at Vienna a year ago. It was passed by acclamation by the Geological Congress, after a ringing speech by Professor Suess, and it expresses my own views most accurately.

EMMONS'S RESOLUTION.

"It is a well-known fact that many of the fundamental problems of geology, for example those concerning uplift and subsidence, mountain-making, vulcanology, the deformation and metamorphism of rocks and the genesis of ore deposits, can not be discussed satisfactorily because of the insufficiency of chemical and physical investigations directed to their solution. Thus, the theory of large strains, either in wholly elastic or in plastic bodies, has never been elucidated; while both chemistry and physics at temperatures above a red heat are almost virgin fields.

"Not only geology but pure physics, chemistry and astronomy would greatly benefit by successful researches in these directions. Such researches, however, are of extreme difficulty. They would require great and long sustained expenditure as well as the organized cooperation of a corps of investigators. No existing university seems to be in a position to prosecute such researches on an adequate scale.

"It is, therefore, in the judgment of the Council of the Congrès Géologique International, a matter of the utmost importance to the entire scientific world that some institution should found a well-equipped geophysical laboratory for the study of problems of geology involving further researches in chemistry and physics."

GEORGE F. BECKER.