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PROBLEMS AND PROGRESS ON THE GEOLOGICO-SEISMOLOGICAL FRONTIER[1]

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THE recent organization by the American Geophysical Union of two cooperative committees of national scope, the Special Committee on the Geophysical and Geological Study of Ocean Basins, under the chairmanship of Professor R. M. Field, and the Special Committee on the Geophysical and Geological Study of Continents, under Professor W. T. Thom, points unmistakably to a growing realization of the convergence and interdependence of geology and geophysics. And what is said of geophysics in general is particularly true of seismology. Hence the speaker thought it would be of interest to this joint meeting of geologists and seismologists to take our stand as it were on an eminence and survey the wide stretch of frontier between the two sciences and attempt to chart the progress of the two converging groups of explorers. There are times when even the formulation of a problem from two different points of view is an advance toward its solution. In any case it is well to take stock of fact and probability, of established theory and working hypotheses and to attempt to locate ourselves with some precision on the map of research.

In the brief compass of an address it is obviously impossible to qualify one's statements adequately; but if some of my statements seem to you so incomplete

[1] Address of the retiring vice-president and chairman of the Section of Geology and Geography of the American Association for the Advancement of Science, delivered at a joint session of the section and the Seismological Society of America, St. Louis, December 30, 1935.

as to be misleading or so categorical as to seem dogmatic or so one-sided as to appear unfair, I hope you will challenge them vigorously.

Geologists and seismologists are on exactly the same footing when looking into the depths of the earth. Both must observe from the surface. The difference lies partly in the methods and instruments with which they carry on their exploration, but partly also, and perhaps principally, in the frame of mind and the background of specialized training with which they approach the same problems. The geologist is trained to observe facts as they present themselves in the field and to balance them against alternative hypotheses; but he does so in a somewhat qualitative manner, for he depends largely on cumulative probabilities for his final decisions. His background usually will be mineralogical and petrographical; it will be physiographical, stratigraphical and frequently paleontological; but it will seldom be physical and mathematical. On the other hand, the geophysicist, generally speaking, whether he is working in pure seismology or in the applied and economic phases of the subject, will nearly always have come into the field with a background of physics and mathematics. This rigorous training makes him psychologically dissatisfied with observations and hypotheses in which there are too many variables. He wishes to isolate the phenomena and to devise instruments for the precise measurement of each. Indeed, he is restless until he can apply instrumental methods and can formulate a mathematical theory that will enable him quantitatively to correlate his observations and to predict further relationships. As fundamentally a physicist, he instinctively seeks to reduce field methods to such simplicity and precision that he can substitute a closed system in his laboratory and reproduce his results at will. Now it seems to me that it is the interplay of these radically different outlooks and of the consequently divergent methods that has proven most fruitful in unraveling some of the apparently hopeless tangles of earth structure.

The most accessible part of our earth is its outer crust; yet this very outer crust bristles with unsolved problems. Geologists have been accustomed to speak of the zone of fracture near the surface of the earth and of the underlying zone of flow. Now it has been found that the depth of first yielding in most destructive earthquakes is not, as might be expected, near the top of the zone of fracture but is at or somewhat below its base. The *normal* depth of focus of earthquakes or depth of first significant radiation of earthquake waves seems to be between ten and fifteen kilometers. Does this first sudden failure take place by faulting properly so-called? Branca, Conrad, Krumbach and Oldham[2] have suggested that thermodynamic causes may be at work even in ordinary earthquakes and that geologic faulting may not tell the whole story. On the other hand Wood[3] presented evidence that volcanic earthquakes as well as volcanic eruptivity itself may be largely controlled by tectonic factors. The problem is complicated by the necessity of explaining both the truly plutonic earthquakes to which we shall refer later in this address and also the large-scale surface faulting that has clearly been caused by certain great earthquakes, such as those of central California (1906), Owens Valley (1872), New Zealand (1855), Tango, Japan (1927), Pleasant Valley (1915) and many others. Furthermore, calculations have been made by Nakano, Byerly, Hasegawa and Gräfe[4] that seem to indicate the possibility, at least in certain favorable cases, of determining from the direction of motion at the beginning of certain phases in the seismograms of earthquakes recorded at distant points not only the fact that faulting occurred at the origin but even the orientation of the fault plane and the direction of slip. It seems clear, then, that the problem of earthquake causes is more of an open question than ever.

Closely related to the origin of earthquakes is the structure of the earth's crust. The interest of geologists and seismologists alike converges on seismic measurements at the surface of the earth where observations and interpretations can be checked by the examination of actual rock exposures and by the drilling and coring of wells. It is in the outer region of the earth's crust that those triumphs of seismic prospecting have been scored which are so numerous and so well known to you as need no description here. But these very successes were generally based on empirical and often on *ad hoc* solutions of the problems that arose in the process. What is, for example, the true significance of the so-called "weathered layer"? A much more scientific approach to the same problems than seemed to be possible in commercial work was naturally made in such independent investigations as those of Gutenberg, Wood and Buwalda in various parts of California,[5] Reich,[6] Barsch,[7] Korte[8] and

2 W. Branca, *Sitzungsberichte der K. Preussischen Akademie der Wissenschaften, Mathematisch-physikalische Klasse*, 28: 380–399, 1917; V. Conrad, *Gerlands Beiträge zur Geophysik*, 20: 276, 1928; G. Krumbach, *ibid.*, 30: 361–364, 1931; R. D. Oldham, *Jour. Geol.*, 34: 385–398, 1926.

3 Harry O. Wood, *Bull. Nat. Research Council*, No. 90, pp. 9–31.

4 H. Nakano, *Seismological Bulletin of the Central Meteorological Observatory of Japan*, 1: 92–120, 1923; P. Byerly, *Am. Jour. Sci.*, 16: 232–236, 1928; M. Hasegawa, *Gerlands Beitrage zur Geophysik*, 27: 102–128, 1930; H. Gräfe, *Zeits. für Geophysik*, 9: 31–43, 1933.

5 B. Gutenberg, H. O. Wood and J. R. Buwalda. *Bulle-*

others in Germany, Gutenberg in Wyoming, Ewing on the Atlantic continental shelf, and others. Seismic prospecting methods are of most use, of course, in regions where the successive formations are poorly exposed but have strongly contrasting elastic properties.

Important as are the thickness, character and structural attitudes of the sedimentary layers from the standpoint of geology and of applied geophysics, these surface formations shrink into insignificance in comparison with the so-called granitic and basaltic layers in their effects on the transmission of earthquake waves. Why this should be true becomes clear when we reflect that a structure must be large or at least of the same order of size in comparison with the length of a particular wave or group of waves in order to produce reflection or refraction. Now the velocity of longitudinal elastic waves in dense limestone and in granite is found to be five to six kilometers a second and the periods of the same type of earthquake waves are one to twenty seconds. Hence, the wave-length of these earthquake waves, which is given by the product of the period and the velocity, will vary from five or six kilometers to more than one hundred kilometers. The wave-lengths that are used in seismic prospecting are incomparably smaller, ranging in general from a few millimeters to several meters. The waves from larger commercial blasts are often of intermediate length.

Therefore, it is obvious that the relatively long waves radiated by natural earthquakes are only suitable for the study of the coarser crustal structure or large scale layering and not for determining the fine structure or the folding and faulting and the succession of sedimentary formations near the surface. This must be clear because the only means a seismologist has of characterizing a layer is by the velocity with which it transmits elastic waves; and the only way in which he can locate the boundaries between layers is to observe the phenomena of reflection, refraction and diffraction of elastic waves which occur only when the conditions are such that the boundaries form relative discontinuities; that is, when these boundaries mark *relatively sudden* changes in the velocity-depth ratio for a train of elastic waves of given wave-length. On the other hand, the energy required to penetrate the entire crust and give useful records of reflection and refraction by structures at depths greater than a very few kilometers is so enormous that only natural earthquakes can be relied on to furnish it. If the material composing the layer can be considered isotropic, so that its properties are independent of direction, then the thirty-six elastic constants of the general case reduce to two; and the types of possible body waves reduce also to two—the longitudinal type, in which there are propagated only alternations of irrotational change of volume, and the transverse type, which involves only equivoluminal shear. There are two ways in which the velocity of these two kinds of elastic waves in a particular type of rock can be determined. The one is a direct measurement, as, for example, by observing the vibrations from an artificial source, such as a blast of explosives, by means of a seismograph placed at a suitable distance on an exposure of the rock in place. The other method involves calculation from the elastic constants and the density of a specimen of the rock as measured in the laboratory. As is well known, the square of the wave velocity is equal to the ratio of the respective elasticity to the density of the rock. In the past it was always found that velocities measured in the field were greater than the velocities calculated in the laboratory, so that there arose an hypothetical distinction between static and dynamic constants based on the obvious differences between the application of slight forces for a very short time in the passage of an earthquake wave and the heavy static loading of the specimen in laboratory measurements.[9] This distinction was shown to be correct when the magnetostrictive vibration method was used by Muzzey and others,[10] and when an even more satisfactory electrostatic vibration method was devised and applied by Ide[11] to the same cylindrical rock specimens as had been used by Zisman. The elastic constants so determined gave velocities which agreed quite well with the field determinations. Nevertheless, the identification of completely concealed rocks by means of velocities alone without further evidence is so precarious as to be rarely justified; for, apart from the fact that radically different rock types such as limestone and granite may transmit elastic waves with the same velocity, it is reasonable to suppose with Daly[12] that the mineral composition of a given rock type will accommodate itself to the high pressures that exist in the deeper portions of the earth's crust. Hence it is only in the loosest possible sense of the word that we can speak, as do Jeffreys and others,

tin of the Seismological Society of America, 22: 185–246, 1932.

6 H. Reich, Jahrbuch der Preussischen Geologischen Landesanstalt, 42, 1921.

7 O. Barsch, Jahrbuch der Preussischen Geologischen Landesanstalt zu Berlin, 49, 1928.

8 W. Korte, *Zeits. für Geophysik*, 7: 57–68, 1930.

9 L. H. Adams and E. D. Williamson, *Jour. Franklin Inst.*, 195: 526–527, 1923; L. D. Leet and W. M. Ewing, *Physics*, 2: 168–169, 1932; W. A. Zisman, *Proc. Nat. Acad. Sci.*, 19: 680 and 653, 1933; L. D. Leet, *Physics*, 4: 375–385, 1933.

10 D. S. Muzzey, *Phys. R.v.*, 36: 935, 1930; Giebe and Blechschmidt, *Annalen der Physik*, 18: 417–457, 1933.

11 J. M. Ide, *Rev. Sci. Instruments*, 6: 296–298, 1935.

12 R. A. Daly, "Igneous Rocks and the Depths of the Earth," p. 179, McGraw-Hill, 1935.

of a layer characterized by a given velocity as a granitic layer or a basaltic layer. The important point in the seismological evidence is not to be looked for in the assignment of rock types but rather in the fact that the deeper crust really seems to be composed of shells separated by rapid transitions or discontinuities and that the velocity increases from one shell to the next lower. A second contribution that is very valuable because exceedingly difficult consists in the more detailed working out of the crustal structure in individual regions. In Southern California, Gutenberg[13] found the structure very complex; so that, according to him, the crust there would seem to consist of at least four distinct shells of thickness 14, 11, 6 and 8 kilometers, respectively. In contrast with this 39-kilometer crust in Southern California, Byerly[14] finds but three layers in Central California and a total thickness of 31 kilometers. In further contrast, Hodgson[15] found but one crustal layer and a total thickness of only 16 kilometers in that central portion of the main island of Japan which is characterized by both normal and plutonic earthquakes. As evidence accumulates in regard to crustal structure the impression given is one of increasing complexity, almost of confusion. Before any certain conclusions can be drawn we must have a much greater body of evidence for a large number of regions. Such study can be prosecuted with great promise of fruit where there are a number of stations close together equipped with sensitive seismographs and recording with an open-time scale; and then only when earthquakes occur within the proper range of distance.

At the base of the crust there seems to be everywhere a major discontinuity, which has been named after the elder Mohorovičić, and below this discontinuity there does not appear, at present at least, to be convincing evidence for any other until a depth of about 970 to 1,000 kilometers is reached. What constitutes this great shell, or stone mantle, as it was called by Wiechert?[16] Is it a mass of crystalline ultrabasic rock or is it a glass? This much seems to be certain, that it transmits both types of earthquake waves with relatively high velocity and that this velocity on the whole, at least, increases rapidly with depth. This fact attests that it possesses a high degree of incompressibility and of rigidity for stresses that are applied for short intervals of time. It does not prove that it has the property of resistance to flow under long-continued strain. This latter property is called strength in counter-distinction to rigidity. Also, it is usually assumed that this shell is horizontally homogeneous and that the velocity of earthquake waves varies only with depth; but evidence has been presented by Dahm[17] which seems to indicate that the velocity at any given depth in the mantle varies slightly from region to region, and hence that there are regional differences of structure which extend far below the supposed level of isostatic compensation. Furthermore, this mysterious, intriguing mantle is the seat of plutonic earthquakes which have occurred at a wide variety of depths down to 700 kilometers and are frequently violent. In some of the deepest of these earthquakes the total energy suddenly released is enormous. This energy must have been stored as potential energy in some form or other. Two possibilities suggest themselves: the energy might have been stored chemically or it might have been stored as potential energy of elastic strain. The term chemical energy would naturally be taken in a wide sense to include latent energy of various types such as that of solution, melting, vaporization and of some forms of crystallization. It is conceivable that there might be aggregates at these great depths existing under forms that are in stable equilibrium only under other conditions of pressure and temperature than those proper to that level. It would not be unreasonable to suppose that this instability could progress to such a degree of unbalance that when the change of state or other reaction required to restore equilibrium occurred, it would take place with explosive violence. At least the work of Bridgman and analogies from the chemical laboratory would seem to point that way. If this were the explanation it would seem reasonable —almost necessary—to suppose either a recrystallization from an initial crystalline state or a change from a non-crystalline to a crystalline state. Granting that all thermodynamic and chemical difficulties could be removed, it is difficult to see how a purely explosive phenomenon propagated in all directions from a small nucleus could produce the enormous shear waves which we frequently observe in these earthquakes. In fact, there are some deep earthquakes in which almost all the energy seems to be in the shear waves. If, on the other hand, these enormous quantities of energy are stored in the form of potential energy of elastic strain two consequences would seem to follow: The first is that the energy must be stored partly and in some cases almost exclusively in shearing strain. The second consequence is that the medium itself must have

[13] B. Gutenberg, *Gerlands Beiträge zür Geophysik*, 35: 6–50, 1932.

[14] P. Byerly, Bull. Seismol. Soc. Amer., 25: 223–246, 1935.

[15] E. A. Hodgson, Bull. Seismol. Soc. Amer., 22: 270–287, 1932.

[16] E. Wiechert, "Nachrichten der K. Gesellschaft der Wissenschaften zu Göttingen, Math.-phys. Klasse," p. 427, 1907.

[17] C. G. Dahm, Papers presented at the Ottawa meeting of the Eastern Section of the Seismological Society of America, May 27–28, 1935, and at the St. Louis meeting of the Seismological Society of America, December 30–31, 1935.

sufficient strength to allow the building up of such shear. Furthermore, it is difficult to imagine a mechanism which could introduce this shearing strain at depths presumed to be so far below the supposed level of isostatic compensation, unless it be recrystallization or some other change of state. Now we must suppose that at these depths changes of temperature and pressure can take place very slowly indeed. If the recrystallization or other change of state were to take place slowly but were to advance asymmetrically it is conceivable that the surrounding matter would be thereby distorted and that this distortion might progress to an extent that would involve the storage of the required quantity of energy in the form of potential energy of shearing strain before it would cause plastic failure, elastic rebound and the radiation of earthquake waves. Therefore the occurrence of violent earthquakes at depths of hundreds of kilometers within the mantle of the earth seems to suggest two ideas: *crystallinity* and *strength,* which are completely at variance with some otherwise very intriguing and plausible geological hypotheses such as that advanced by Daly.[18] Whether the facts can be explained on a purely thermodynamic basis, as proposed by De Lury,[19] or whether the deep-seated inhomogeneity suggested by Dahm's results points to the megadiastrophic ideas of T. C. Chamberlin,[20] it would be too early as yet to conjecture.

It seems to be fairly well established that the shell within which the velocity of earthquake waves increases rapidly and in which deep earthquakes occur is separated at its base from the underlying transition shell, in which the velocity varies but little with depth, by a second order discontinuity or thin shell of such rapidly changing properties as to reflect a part of the wave energy that falls upon it. To this discontinuity, which lies at a depth of 970–1000 km, the name of Repetti has been given because he[21] was the first clearly to demonstrate its existence and its depth by identification on the seismographic records of reflections produced by it and by the discovery of cusps in the functional curves derived from the observed times of arrival of earthquake waves affected by it. It is true that many years before a "peculiarity" at that depth was suggested by Geiger,[22] but no corresponding discontinuity appears in his published curves. Gutenberg and Richter[23] say: "This is actually the same discontinuity which was first discussed by Wiechert; the corresponding rays emerge at about the same distance as found by him. The decreased depth is due to the general improvement in observed travel times." But I think it will be clear to all who read the statements of Wiechert[24] that he is speaking of the boundary of a central metallic core and that his discontinuity is the one which was later shown by Gutenberg himself to lie at a greater depth than Wiechert had supposed. Thus, for example, Wiechert said in 1907: "Rather, it must be absolutely assumed that there are in the deep interior of the earth substances of greater specific gravity than on the earth's surface. It certainly seems natural to think that the stone mantle on which we live envelops a metallic core. We see at once where to look for its boundary; obviously at that depth of 1500 km, where so striking a change appears in the influence of the earth's substance on earthquake waves."[24] And in 1910 he wrote: "I was forced to see in the sudden change of the velocity-depth relation at 1500 km the boundary between the stone mantle and the metallic core which had been deduced from astronomical observations. Zoeppritz and I could find no indications of a discontinuity within the stone mantle, such as would have corresponded to the travel time curves of Milne and Benndorf."[22] The distance of emergence on the earth's surface of the rays concerned in Wiechert's supposed 1500 km discontinuity was 49°, which is quite different from the 33° found by Repetti, Witte and Dahm. The difficulties of investigation increase with depth, and much remains to be done before we shall have a satisfactory picture of the transition into the earth's core. The well-known phenomena of the shadow zone seem not to be nearly so simple as had been supposed.[25] The results of Dahm's[26] comparison of the depth of reflections with that attained by the grazing rays seem to indicate that there intervenes between the overlying material and the core proper a shell somewhat more than 200 km in thickness that plays an essential part in the production of the shadow zone.

While excellent contributions have been made toward the solution of the problem of the core, those who are working in the field would be the first to admit that their methods are indirect and their conclusions very tentative. No one has as yet discovered a direct method of measuring the velocities of seismic waves at any particular point or level within the core. The velocities and travel times that have been given, for example, by Gutenberg[27] were based on methods of

18 R. A. Daly, "Igneous Rocks and the Depths of the Earth," pp. 173–213, McGraw-Hill, 1933; *Jour. Wash. Acad. Sci.,* 25: 389–399, 1935.

19 J. S. De Lury, *Jour. Geol.,* 43: 759–764, 1935.

20 T. C. Chamberlin, *Jour. Geol.,* 19: 391–415, 1931.

21 W. C. Repetti, "New Values for Some of the Discontinuities in the Earth," 17 pp., Manila, 1930.

22 E. Wiechert, *Physikalische Zeits.,* 11: 298, 1910.

23 B. Gutenberg and C. F. Richter, *Gerlands Beiträge zur Geophysik,* 45: 347, 1935.

24 E. Wiechert, *Physikalische Zeits.,* 9: 46, 1907.

25 J. B. Macelwane, *Bull. Amer. Phys. Soc.,* 1: 4, 1925; H. Jung, *Nachrichten d. Gesellschaft d. Wiss. zu Göttingen,* Math.-Phys. Kl. 1933: 42–80.

26 C. G. Dahm, Dissertation, St. Louis, 1934.

trial and error by which he set up twenty-eight alternative hypotheses and chose the one which seemed to him to accord best with the observed total travel times of outstanding phases. But he did not publish any demonstration that his solution was either unique or the best attainable from the data. Not until a direct method of attack on the core problem is found can we begin to discuss with confidence the distribution of velocities within the core or the probable characteristics of the material composing it. There seems to be evidence for the transmission of shear waves through the core, but this evidence raises new problems which call for direct attack. The transmission of shear waves would prove effective rigidity and consequent solidity in the popular sense, but would not prove solidity in the technical sense of the phase theory. In fact, the conditions at the center of the core would seem to transcend all possibilities of direct human experience. The temperature is completely unknown and the pressure is so enormous as to stagger the imagination. Unknown states of matter are not excluded.

In conclusion, it would seem that the entire picture of positive geological results attained by seismological methods, of the new problems thus realized and formulated, and of the better understanding and readiness to cooperate which is evidenced in the ranks of geologists and of seismologists, is a most encouraging one and foreshadows a new and exceedingly interesting era of geologico-seismological research.

OBITUARY

JAMES HARTLEY ASHWORTH

JAMES H. ASHWORTH, professor of natural history in the University of Edinburgh, died suddenly at his home in Edinburgh in the night of February 3–4, 1936. He had attended a meeting of the Royal Society of Edinburgh, of which he was general secretary, the preceding afternoon, at which meeting Professor H. S. Jennings, of Johns Hopkins University, who is this year George Eastman professor at Oxford, gave an address. In the evening he was present at a dinner in honor of Professor Jennings, who was a guest at his home. He retired for the night, weary but with no premonition of death; in the morning he was found cold and still.

Professor Ashworth was 62 years old. He was born in Lancashire, educated at Manchester University; and after he had taken the degree of D.Sc. at London, he was for four years lecturer and demonstrator in zoology at Manchester. For the past thirty-six years he had been associated with the University of Edinburgh, first as lecturer in invertebrate zoology, then as professor of zoology and for the past nine years as professor of natural history and administrative head of the department. His publications include memoirs on many groups of invertebrate animals, and the accuracy and value of his researches brought him important honors and awards, among them the Keith Medal of the Royal Society of Edinburgh; he was president of the Royal Physical Society of Edinburgh, a fellow of the Royal Society of London and member of its council. But it is probable that his most important contributions to science were in the fields of instruction, organization and administration. He was a conscientious and inspiring teacher and a particularly able organizer and wise executive. When he came to his professorship in the university he found the department of zoology inadequately organized and badly housed. He at once set about making improvements, with the result that his department is now of outstanding excellence. In 1923 a generous donor gave £20,000 toward the development of the department of zoology, in 1925 the trustees of the Carnegie Trust for the Universities of Scotland set aside £18,000 for this purpose, and at Christmas, 1926, the International Education Board of the Rockefeller Foundation appropriated £74,000 to carry out this project. The University Court in accepting these generous gifts "placed on record their sincere appreciation of the part which Professor Ashworth had taken in making possible this great development, and their recognition that the provision of funds adequate for the undertaking had been due in large measure to his personality and unsparing efforts." Sketch plans of the building were prepared by Professor Ashworth; construction was begun in June, 1927, and the building, which cost £80,000 and is one of the most complete laboratories of zoology in the world, was formally opened by His Royal Highness Prince George on May 15, 1929.

Professor Ashworth's interests were not limited to his own institution, but he was always ready to lend a helping hand to others. He was a trustee of the Bermuda Biological Station for Research and had been active in recent months in undertaking to raise funds in Great Britain for that institution. He was widely known in this country, where he had visited and lectured at universities and scientific institutions from the Atlantic to the Pacific. The gracious and charming personalities of Professor and Mrs. Ashworth are remembered by a host of American friends who lament his untimely death.

E. G. CONKLIN

[27] B. Gutenberg, "Nachrichten der K. Gesellschaft der Wissenschaften zu Göttingen, Math.-physikalische Klasse," pp. 28–29, 1914.