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# SEISMOLOGY FROM A MATHEMATICAL VIEW-POINT[1]

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THE high honor attaching to the office of vice-president of the American Association for the Advancement of Science demands a worthy effort on an occasion like the present. Very properly a general consideration of some field within the scope of mathematics may come under review, and all the more will this be appropriate for a section of the American Association if it links itself to some other department of science. I am therefore returning to an old-time love of mine and considering to-day the advance that has been made in seismology within, let us say, the last thirty years.

At first sight the phenomena to be studied seem hopelessly confused; the ground effects near the origin of the earthquake show buffeting blows from all directions; earthquake records, even those of the same earthquake, often look widely different; and the layman wonders what can be made of these happenings as a science. It is, however, the glory of the human mind that it can select, classify, analyze, and can thus bring order out of chaos.

The universe is a system, a unit,
Only in the mind of man.

[The speaker described (1) the evident mechanism of earthquakes consisting of a gradually increasing strain and subsequent fracture of the rock structure; (2) the transmission of the shock, the longitudinal and transverse wave through the earth and a wave of greater amplitude over the surface; (3) various reflected and refracted waves; and (4) methods for obtaining information about velocities below the surface,

[1] Address of the retiring vice-president and chairman of the Section on Mathematics, American Association for the Advancement of Science, Richmond, December 28, 1938. The address was illustrated by a large number of slides.

the depth of the earthquake focus and of various discontinuities below the earth's surface.][2]

Wiechert[3] had by 1907 determined from the three components the angle of emergence of $P$ and $S$ waves where they meet the surface of the earth as a table giving the sine of this angle compared with $\Delta$, the epicentral distance, and had plotted the velocity at the vertex (the deepest point of the path) against the depth of the vertex. He had thus prepared the way for the theoretical treatment made by Herglotz in 1907 and accepted even now as the method for determining the paths of the waves through the earth's interior. Seismologists have always assumed Snell's law that in passing from one medium to another of a different density the angles of incidence and refraction are connected with the velocities in the two media by the relation $\sin i_1/\sin i_2 = v_1/v_2$. Fermat's principle that a wave travels from one point to another in the least possible time, and an assumed law of dependence of velocity on depth, will determine the path, the travel-time in terms of epicentral distance, etc. Happily Herglotz recognized that conversely, with a few natural assumptions such as an increase of velocity with depth, the data furnished by a travel-time curve give a unique solution for the paths of the waves and for such incidental relations as that between the maximum velocity and the depth of the vertex. For those sufficiently conversant with mathematics, it may be said that Herglotz[4] in February, 1907, recognized Benndorf's[5] formulation of May, 1906, as an integral equation of the form

$$\Delta = \frac{2R}{V_\Delta}\int_{x=a}^{x=b} \frac{d \log r}{(x-a)^{1/2}},$$

$R$ earth's radius; $r$ radius to a point of the path; $x = 1/V_r^2$; $a = 1/V_\Delta^2$; $b = 1/\bar{v}^2$; $v_r$ vel. at the point of the path; $V_r = \frac{v_r R}{r}$; $\bar{v}$ vel. of wave as it emerges at earth's surface; $V_\Delta$ component of $\bar{v}$ along earth's surface. Herglotz gave as its solution:

$$\log R/r_v = -\frac{1}{2\pi R}\int_b^x \frac{V_\Delta \Delta da}{(a-x)^{1/2}},$$

$r_v$ radius of vertex; and Wiechert[6] in 1910 simplified it thus:

$$\log R/r_v = \frac{1}{\pi R}\int_0^\Delta q d\Delta,$$

$\cosh q = V_r/V_\Delta$.

The computation, which by this formula gives the radius of the vertex of any path in terms of the epicentral distance, as well as the velocity at the vertex, depends essentially on an estimation of the slope of an accurate travel-time curve at various points and on a graphical integration. While the method now used is precisely that of 1910, the improvement in the reliability of records and in technical ability to interpret these records has enabled seismologists within the past ten years to utilize a greatly increased mass of records and to obtain results far more reliable than formerly.

Numerous ingenious methods have been developed for the determination of deep foci. These all depend on the fact that the travel-time curve of any phase varies somewhat with the depth of the focus, as Scrase[7] in 1931 indicated in his $P$ and $S$ curves drawn for several different focal depths. Brunner[8] draws the $P$ curve for an earthquake occurring at the surface, a curve slightly lower than this for the corresponding $P$ wave if the depth of the focus were 100 km, a curve slightly higher than the first for the wave $pP$ reflected from the earth near the epicenter for a focal depth of 100 km, with similar pairs of curves for 200 km, 300 km, . . . 700 km and a similar set of curves for $S$ and some other phases. If then, as in the illustration which was shown, the $pP$, $S$ and $sS$ phases occur later than the $P$ phase by 1 min. 15 sec., 10 min. 18 sec., and 12 min. 43 sec., these are marked on a straight strip of paper according to the time scale and the strip is fitted uniquely to the Brunner chart, indicating in this special case an epicentral distance of 90°, a depth of approximately 340 km and the additional fact that the earthquake originated 12 min. 24 sec. before the time of arrival of the $P$ phase. Depths up to 700 km have been determined for both light and heavy earthquakes.

From the information available in 1926 Daly[9] inferred that heavy shocks originate at depths less than 25 miles and that the occasional shock at depths greater than 40 miles may mean that the material below that depth is of the nature of hot glass and not crystalline rock. Such conclusions as to the composition of the earth have been based on a questionable extrapolation; but these conclusions have been gradually revised because of data that give much fuller information as to the depth of strong and weak shocks, and they can now be modified much more reliably by reason of such heavy-pressure work as that of Bridgman through the attainment of laboratory pressures of 50,000 atmospheres or approximately 50,000 kg/cm$^2$ corresponding to a depth of perhaps 160 km if we make a reasonable assumption of density.

The dependability of records from good instruments is unquestioned, as is evidenced by almost identical

[2] For discussion of these points see, *e.g.*, Macelwane, "Theoretical Seismology," New York, 1936.

[3] E. Wiechert, Gött. Nachr., math.-phys. Kl., 1907.

[4] G. Herglotz, *Phys. Zeitschr.*, 8: 145–147, 1907.

[5] H. Benndorf, *Mitt. d. Erdbeben Komm.*, 29, 1905; 31, 1906; *Sitzungsber.*, 114, 1905; 115, 1906.

[6] E. Wiechert, *Phys. Zeitschr.*, 11: 302, 1910.

[7] F. J. Scrase, *Proc. Roy. Soc.*, A, 132, 1931.

[8] G. J. Brunner, "Earthquake Notes," Eastern Sec., Seism. Soc. of Amer., 1934.

[9] R. A. Daly, "Our Mobile Earth," pp. 116, 119, New York, 1926.

seismograms of the same shock on instruments of similar instrumental constants used at the same station or of similar instruments in the same general region. There has been a decided gain in the clearness of records and in the greater control over a wider range of magnification.

This discussion divides itself as between theory and practice; and it becomes manifest at once that the applicability of seismological theory depends on the advance made in observational instruments and methods and in their trustworthiness.

## Seismometers

It is probably well known that a seismometer consists of a so-called "steady mass" suspended delicately through a flat spring or a pivot on a framework which is rigidly attached to the ground, preferably to solid rock. It is suspended so as to move freely, say, east and west; a compressional wave coming from the west will displace the ground and framework toward the east, but the steady mass will lag on account of its inertia and will therefore appear to move toward the west, its relative motion being magnified by mechanical means and recorded on a roll of smoked paper as in the Milne and Wiechert types, or magnified by galvanometric means and recorded photographically as in the more recent types. To furnish a record at a really complete station it is necessary to have three instruments to register the vertical, the north-south and the east-west motions, respectively, as well as to record waves of both short and long periods.

(The speaker here described the leading types, contrasting the older and newer instruments.)

Much intensive work has been done in California the past six years in the study of near-by earthquakes by means of "accelerometers," *i.e.,* instruments which are designed to register not displacements or velocities but accelerations. Lack of time prevents any discussion of this study, which is so important in relation to construction that shall withstand earthquake shocks and in furnishing data for the mathematical treatment of disturbances close to the epicenter. Nor can attention be given here to the great progress being made in the seismological exploration for oil deposits, or to the study of microseismic movements which obscure the records in varying degrees and which have yet to be correlated successfully with meteorological or other phenomena, even after thirty-five years of research.

## Interpretation of Records

An amazing amount of highly creditable investigation has been given during the past thirty years to the mapping out of various "phases" of earthquake waves. This involves the determination of the exact time at which a sharp change in the record takes place, and exceedingly critical examination and comparison of many records of the same earthquake and of similar earthquakes so as to establish a definite continuity in the progress of this suspected phase as the epicentral distance increases. The travel-time curves of such phases and the accumulated evidence of velocities at varying depths below the earth's surface throw light on the probable path through the earth. In particular, the very complete analysis by Jeffreys[10] in forming his travel-time tables shows a high grade of mathematical work, as measured by the very small standard errors of his results; its dependability is limited only by his data, since he, like others, made an arbitrary but judicious choice from the available earthquake records. To be sure, seismologists, like our national economists to-day, sometimes form consistent and well-authenticated theories, which fail, however, to agree with those of other equally qualified experts; nevertheless, there is a gratifying agreement as to the real existence and travel-time of the more important phases, along with a great improvement in the time control and accuracy of time determinations.

## An Appraisal of the Field

Numerous attempts have been made to apply harmonic analysis to the study of seismograms either on account of the quasi-periodic character of the curves or on an assumption of periodic motion of the earth for a short interval. (Slides were shown of two complex curves not entirely dissimilar: one of a seismogram imitated quite closely years ago by Professor D. C. Miller by the combination of thirty harmonic curves, another as the combination of ten harmonic curves, also by Professor Miller; the second was, however, not a seismogram but the note of a clarinet!) The fallacy in this sort of analysis is that the theory of harmonic or Fourier series analysis resolves a curve uniquely into harmonic constituents only where there is a periodic motion, ever so complicated, perhaps, but with a definite and sustained period. About all that has been done along this line is for an expert seismographer to sketch a curve through a complicated seismogram which evidently shows a wave of longer period and of large amplitude, superposed on which are smaller waves of shorter period. The instant reaction of the mathematician to this procedure, however, is that if such a curve is legitimate in practical interpretation; *i.e.,* if there is actually a wave of this character, some mathematical theory should be developed which will correspond to this postulated reality. While several have presented criteria for the existence of such a periodic wave, the science of seismology is still very much undeveloped at this point.

One line of evidence on which any theoretical inter-

[10] H. Jeffreys, *Beitr. d. Geophysik,* 1936; Mo. Notices, Roy. Astr. Soc., Geophys. Suppl., 4, 1937–1938.

pretation must be based is the amount of displacement of the ground at any station and hence its frequency or possible periodicity, its amplitude, etc., as inferred from the records of its three components. These curves are quite unlike. The usual formula for a damped pendulum whose support is disturbed by a blow, or by a sustained motion of the earth, furnishes theoretically the relation between the displacement on the seismograph record and the ground displacement, both for mechanical and for galvanometric registration. Formulas for these two leading types of seismometers are exhibited here:

Equation of mechanical photographic seismometer:

$$V\frac{d^2x}{dt^2}=\frac{d^2a}{dt^2}+2\varepsilon\frac{da}{dt}+\left(\frac{2\pi}{T_0}\right)^2 a.$$

$x$ is ground displacement; $a$ is record displacement; $\varepsilon$ is damping factor; $T_0$ is natural period of pendulum; $V$ is magnification for very short waves. The solution is given by

$$Vx=a+2\varepsilon\int_{t_0}^{t} a\,dt+\left(\frac{2\pi}{T_0}\right)^2\int_{t_0}^{t} dt\int_{t_0}^{t} a\,dt.$$

From this equation it follows that a seismometer measures displacements, velocities or accelerations of the ground motion according as the period of a wave is much less than $T_0$, intermediate in value, or much greater than $T_0$. Both graphs and nomograms are available for finding the magnification, whether the record indicates an obvious period or only an impulse.

Equations of galvanometric seismometer:

$$K\frac{d^2\phi}{dt^2}+D\frac{d\phi}{dt}+U\phi=ML\frac{d^2x}{dt^2}-Gi$$

$$k\frac{d^2\theta}{dt^2}+d\frac{d\theta}{dt}+u\theta=gi$$

$x$ = ground displacement; $\phi$ = angular displacement of steady mass; $\theta$ = angular displacement of galvanometer coil; $i$ = current intensity. From these is derived a rather complicated expression for magnification.

Since any disturbance is ordinarily damped out on the record within half a vibration, it is theoretically possible, on each of the three components, to take account of the manner in which magnification depends on the period of the wave at any instant and to draw a record of the actual displacement of the ground through mechanical or graphical integration and thus by comparing the three components to know, for example, whether the wave at any moment is longitudinal, transverse or screw-like, whether it occurs in a horizontal or vertical or oblique plane or in no plane, whether it is a periodic motion continuing for several seconds or for a few minutes, *i.e.*, whether the earth in any limited region has a so-called "proper motion," or whether, on the other hand, there appears to be no analyzable motion aside from the leading phases already mentioned. Here is where, in my judgment, a proper advance has not been made in the past thirty years. Even conceding that promising trials have been made by Gutenberg, Sharpe and others, we must have in mind that Wiechert began this as early as February, 1901. There should even at the present day be closer agreement and greater dependability in the calculation of ground displacement and ground motions. For example, Sharpe interprets the same record by two different methods as giving at one instant displacement measurements so diverse as 93 μ and 63 μ at a distance of 72°.6, 62.4 μ and 30.1 μ at 77°.5, etc., although he has much closer agreement from 80° on. Moreover, within the past two or three years doubt has been thrown[11] on the formulas used in the Galitzin-Wilip and other types, the observed magnification being in some instances 50 per cent. larger than the theoretical, probably because of the reaction of the coil of the galvanometer on the motion of the steady mass. If a more critical study than has yet been made of this whole class of trials throws doubt on the validity or applicability of the formulas taken from mathematical physics, it is of prime importance that scholars highly qualified in applied mathematics should correct the older methods or develop new and correct ones. It is also undoubtedly true, as more than one careful observer has said, that too much of routine station work is unreliable except for the good indication of times of arrival of the various phases. Only 20 per cent. of the records which Sharpe[12] collected were usable for ground motion study, and a similar criticism was made in 1936 by Gutenberg and Richter.[13] Much more than is now the case should instruments be kept in adjustment, the records and the instrumental constants be uniformly well taken so that onsets, amplitudes, periods may be obtained reliably.

The first $P$ phase and its immediate successors are more readily studied than the $S$ or $L$ phases because they are less overlaid and hidden by waves already on the record. There are, however, many accurate records where the $S$ phases enter as measurable impulses and where consequently fairly definite conclusions should have been made as to the nature of these waves. It appears to me, after a careful study, that the most that has been accomplished in the past thirty years is agreement that the $S$ phases are approximately transverse, usually polarized, *i.e.*, vibrating in a plane, but that this is only a rough first approximation. For example, to suggest the complexity of the situation, Macelwane shows an $S$ phase which appears first as a westward impulse, then as a southward impulse and then (approximately nine seconds later) as an upward impulse with

11 Wenner and McComb, *Bull. Seism. Soc. of Amer.*, 26, 1936.

12 J. A. Sharpe, *Bull. Seism. Soc. of Amer.*, 25, 1935.

13 Gutenberg and Richter, *Beitr. d. Geophysik*, 47, 1936.

a similar condition in the next two or three half-oscillations; hence this must be more nearly a helical motion, the energy vector pointing first upward, then eastward, etc.

It is worth while noting that several seismologists have collected and analyzed data which convince them that the core of the earth does transmit transverse waves, a conclusion which, if well founded, will modify interpretations as to the earth's interior.

With regard to the *L* or surface waves, it is only fair, in a critical review such as this, to say that these are the most difficult to measure for the reasons that much is happening on the record already, that surface waves are less uniform due to the heterogeneity of the geological surface structure and that the trace frequently runs off the record or onto adjacent lines of the record. In the same breath I feel impelled to say that remarkable progress has been made during these years in the deciphering of complex records and in the recognition of various *L* phases, and that more might have been expected in determining the nature of these waves.

Rayleigh long ago showed the mathematical possibility of surface waves whose intensity decreased rapidly with the distance below the surface, a sort of skin effect, the energy of which decreased inversely as the square, rather than the cube, of the epicentral distance. But in this type of wave a particle on the surface would vibrate with a retrograde movement in an ellipse elongated *vertically* in a plane passing through the path of the wave while the observed motion is much more commonly in an ellipse elongated *horizontally* and has a strong component at right angles to the path. A second type of surface wave, the Love wave, has a sound mathematical basis, but serves only incompletely to correspond to the waves which it was supposed to represent. Whether other types mathematically possible can be found that will conform to the observed types or whether some more general, and unfortunately more complicated, theory must be used, there is here a large field for the highest kind of research. I think I am correct in saying that, except for Uller's[14] theory and perhaps Sezawa's or Gutenberg and Richter's, very little of worth has been done on the theory of earthquake waves in the past thirty years. My own prediction is that the simple elasticity theory in use almost unchanged for the past fifty years of the history of seismology must be modified along the line of what in optics is called the theory of dispersion or possibly by the use of a theory somewhat like that now employed in atomic theory. Gutenberg said in 1935: "Recent investigations have shown that in most regions—perhaps everywhere—we do not have one homogeneous surface layer, but a few with slightly different properties, and that, besides, the thickness of these layers and even their properties are different in different regions. . . . It seems to be probable that differences found by different authors are due more to the use of different earthquakes than to differences in the methods used by them." In some details we must expect each earthquake to give its own characteristic record and even each station to have its own individual record of each earthquake. Much attention should be given in the immediate future to comparing the records of specific stations with the accepted "average" curves so as to identity as much as possible the variant characteristics of each station, in the hope of ascertaining the variations in local structure.

One greatly contested question is whether the period of seismic waves is independent of distance or whether, on the contrary, the irregular waves tend to smooth out, just as when an object is thrown into water. Gutenberg, Galitzin and others have in the past adduced evidence that there is no change of period with distance, but in 1923 Macelwane[15] from the records of 66 stations pointed out that both short and long *P* waves tend to approach a uniform prevailing period. Gutenberg in 1934–35 found that for all types of curves the prevailing period at a station increases as the station distance increases. In my opinion, there is a sufficiently large supply of trustworthy records so that this could be definitely decided by two or three experts, criticizing each other's findings. The classical theory of elasticity takes no account of possible change of period. But this is a question of prime importance in seismological theory; if there be such a progressive change, due to viscosity or other cause, some able mathematical physicist must develop a suitable theory.

If, as I believe after a consideration of the evidence concerning waves through the deeper parts of the earth's interior, we include in our study what are undoubtedly variations in their behavior at these depths also, and if we accept the usual postulate that a shock is transmitted from a focus to a distant point in the least possible time, we must conclude that the major portion of an impulse, as measured by intensity, amplitude, acceleration or other mathematical characteristics, will go from one point to another along such paths as are now accepted, but that, because of variations in structure along these paths, a minor part will be scattered over paths which differ slightly from the main path and will differ more or less from the main phase in time and direction of arrival and will show smaller amplitudes and intensity and slight differences in phase, period, etc. Thus a phase will appear on a record as a rather sharp change or impulse, but surrounded by small variations which, so to speak, cluster somewhat irregularly about the main impulse. It is

[14] K. Uller, *Beitr. d. Geophysik*, 18, 1927 and later vols.

[15] J. B. Macelwane, *Bull. Seism. Soc. of Amer.*, 13, 1923.

analogous to the appearance of a point of light which appears through clear glass as a point but through slightly ground glass as somewhat diffused owing to the small irregularities caused by the grinding. Whether these variations will be amenable to such successful treatment as, for example, the statistical study of travel-time curves by Jeffreys, must be answered according to our states of mind, ranging, in Gutenberg's facetious phrase, from his own optimism to Macelwane's pessimism!

The whole subject of seismology is complex, somewhat as the field of economic or sociological phenomena; it grows out of and depends on a variety of superposed causes and elements and is therefore especially difficult of analysis. In its applications to seismology, mathematics must examine not merely its validity but its sufficiency, for in this field its sufficiency *is* the measure of its validity. In closing, we can only join with the Countess in "All's Well that Ends Well":

> Will your answer serve fit to all questions?
>
> . . . . . . . . . . . . . . . . .
>
> It must be an answer of most monstrous size that must fit all demands.

# OBITUARY

## CALVIN BLACKMAN BRIDGES

THE death of Calvin Blackman Bridges on December 27, 1938, is a serious less to genetics and also a personal loss to his many friends. Taking part from the beginning in the *Drosophila* investigations that started at Columbia University about 1910, he became, after obtaining his doctorate, a member of the small group supported by a grant from the Carnegie Institution of Washington. He was still a member of the staff at the time of his death. During these twenty-five years Bridges made a long series of contributions that won him wide recognition as an outstanding genetic investigator.

He was born in 1889, at Schuyler Falls, New York State, and his early years were passed near Plattsburg, N. Y. Beginning as an undergraduate at Columbia he was my private assistant from 1910 to 1915, and fellow 1915–16, taking his Ph.D. in 1916. As stated above he was a member of the "Carnegie Group" 1915–38. In 1936 he was elected to the National Academy of Sciences.

A bare list of the titles of his papers from 1913 to 1938 would give some idea of the nature of the many contributions he has made. His paper on non-disjunction has become a classic; it adduced convincing evidence that chromosome movements furnish the mechanism of heredity. This evidence rested both on observational work and genetic experiment. What seemed at first an exception to accepted genetic interpretations turned out a brilliant confirmation of them —the exception that proved the rule.

Bridges' early discovery (1917) that certain genetic data could be interpreted as due to deficiencies in the chromosome-construction has led in recent years to a factual demonstration of such deficiencies. In some of his latest work (1937–38) he made use of this discovery in the interpretation of overlapping deficiencies to demonstrate the characteristics of certain mutant types. It would be hard to find in the history of genetic research a more convincing demonstration of the combination of factual evidence and masterly interpretation of it. As early as 1919 Bridges described "duplication" as a chromosomal aberration, and here, as in his other work, his conclusions rested not on guessing or vague hypotheses but on experimental proof. Much later he also reported the occurrence of "repeats" in the normal chromosome which will have to be seriously considered in future interpretations of certain types of genetic behavior.

His work on sex determination was a brilliant venture into a more theoretical field, although here, too, it is important to observe that there was no idle flight of speculation but an adherence to actual evidence based on his own thoroughgoing observations. His interpretation of the effects of tetraploidy, triploidy, haploidy on the constitution of the individual is an outstanding contribution to the theory of sex determination in such forms as *Drosophila,* where the outcome is not complicated by the presence of sex hormones in the conventional use of this expression. This work led him to a theory of gene balance that applies not only to problems of sex determination but more broadly to gene balance involving the physiology of phenotypic expression. His interpretations of balance in sex determination in particular inclined him to believe that it is unwise, *i.e.,* not in accord with the evidence at hand, to look for a male-producing and a female-producing gene, this being too naïve a way of expressing the facts, which are more probably due to balance of many kinds of genes more or less widely distributed in the chromosomes. This does not mean that some genes may not be more influential than others in regulating the development of one or the other sex, which may well be the case, but the search for genes concerned only with sex has up to the present not been successful.

In recent years Bridges has spent much time in revising the genetic maps which are the standard ones wherever *Drosophila* is used. His work here was more than a routine job, for he devised ingenious methods to meet some of the statistical problems involved. The