SEISMOLOGY

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§1. INTRODUCTION

The modern study of seismology falls into three main divisions. The first concerns the local effects of earthquakes, especially their destructive effect, which connects the subject with engineering, and the observed displacements of the Earth's surface, which connect it with structural geology. As a scientific study it is almost as old as astronomy, instruments for estimating the direction and intensity of the movement having been known to the ancient Chinese. The second division concerns the effects of earthquakes outside the region of destruction. These take the form of elastic waves, some of which travel through the interior of the earth and some over its surface, and are recorded by suitable instruments. These waves give us the most detailed information about the internal structure of the Earth that we have. The third concerns artificial earthquakes. The methods devised for the analysis of the motion due to small earthquakes, accurately recorded at short distances, are applicable to the waves sent out when a buried charge of high explosive is released. They have been used to determine the structure of the upper layers, mainly sedimentary, and especially to locate buried anticlines in oil-bearing rocks and hence to indicate the best places to make borings.

Popular interest in seismology concerns only the first division, and even there its distribution is strangely erratic. The occasional reports of earthquakes in the Press give an impression that earthquakes are much less frequent than they actually are. An indication of their actual frequency is given by the International Seismological Summary, which collects the information from most of the existing observatories. The condition for including an earthquake in the Summary is roughly that it should be recorded by the observatories in more than one country, and on an average about 600 are included per year. This excludes earthquakes, specially prevalent in Japan and California, that are too small to be recorded except locally; these may reach thousands per year.

Modern developments are almost entirely concerned with the applications of the theory of elastic waves. The prediction that an elastic solid should be able to transmit two types of wave through its substance was first made by Poisson in 1829. One of these is similar to the sound wave in a liquid or gas; when it passes a particle, that particle vibrates in the direction of travel of the wave. Such a wave is called longitudinal or compressional, the latter term being used because the passage of the wave is associated with changes of volume. The second type can occur only in a solid; when it passes a particle, the particle is displaced at right angles to the direction of travel of the wave. It is often called transverse or distortional. The usual practice is to denote the longitudinal waves by P (for primary, because they have the greater speed) and the transverse ones by S, for secondary. Turner suggested that P and S might be taken to
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indicate push and shake, though P is as often as not a pull. P and S can travel in any direction. Their velocities \( \alpha \) and \( \beta \) are given, in terms of the elastic properties and density of the material, by

\[
\alpha^2 = (\lambda + 2 \mu)/\rho; \quad \beta^2 = \mu/\rho,
\]

where \( \rho \) is the density, \( \mu \) the rigidity, and \( \lambda + \frac{2}{3} \mu \) the bulk-modulus or the reciprocal of the compressibility. For the type of solid considered by Poisson, \( \lambda = \mu \) and \( \alpha = \beta \sqrt{3}/3 \). These relations are roughly correct for rocks.

It was first recognized by Rayleigh that another type of wave can travel over the surface with a velocity rather less than \( \beta \), the amplitude diminishing with depth and becoming inappreciable at depths more than a few wave-lengths, as for water waves on deep water. Another type of surface wave, found by observation, was explained by Love in 1911.* It is a transverse wave that can exist if the velocity of S increases with depth. In spite of their apparent similarity, surface waves and water waves are theoretically quite different. Water waves are controlled by gravity. The surface waves of earthquakes are elastic waves, and gravity has little or no effect on them.

The theory of the propagation of elastic waves is similar to that of other wave motions, the following properties being specially important. (i) S shows polarization, as for light waves; displacements in two directions in the wave front and perpendicular to each other are propagated independently, with the same velocity. It is usual to take one of these directions to be horizontal and the other to be in the plane containing the ray and the centre of the Earth, and to denote the components by SH and SV. (ii) Times of travel, rays and wave fronts are related according to the principle of stationary time, and in general the distribution of energy over the wave front is approximately the same as if the energy within a given cone of rays travelled without influence from neighbouring cones. (iii) When an SH wave meets a free surface it is wholly reflected as SH; when one meets a horizontal interface it follows the usual law of refraction, but there is also a reflected wave, reflexion becoming total at a critical angle. The behaviour of P and SV is more complicated because each requires two boundary conditions at a free surface and four at an interface, instead of one and two respectively as in the cases of sound and light. When either meets a free surface, reflected waves of both types are produced; when either meets a horizontal interface it produces refracted waves and reflected waves of both types. There are critical angles in both cases, and some of the derived movements, when the angle of incidence is high, are not waves travelling away from the interface. (iv) If a transition between two media is spread uniformly, or nearly so, through a region several wave-lengths in extent, waves reaching it are almost completely transmitted in the original type, or, if this is forbidden by the law of refraction, they are totally reflected as in atmospheric mirage. (v) The wave velocities of bodily waves are practically independent of the wave-length. This is not true of surface waves, and considerable dispersion occurs.

A disturbance that is zero up to a definite instant is called a pulse or a phase.

A wave may be reflected several times at the outer surface. The history of each is indicated by the notation. Thus PP is a wave that starts as P and is

* This, and other earlier references, appear in The Earth (Jeffreys, 1929).
once reflected as P on the way to the observatory. PS is one that starts as P and is reflected as S. PPP has been twice reflected, remaining of P type throughout, and similarly for other phases.

The systematic recording of earthquake waves began in the later nineteenth century, especially in Japan and Italy. The earlier instruments were not very satisfactory. The ideal would be to record the displacement of the ground in each direction against the time, but this is mechanically impossible. A fair approximation to it over a range of periods is possible, and has been achieved in many ways. The most important property in a recording instrument is a quick response to any sudden change of the velocity of the ground, so that times of arrival of the different phases can be read accurately. The next is a quick recovery after a displacement, so that the instrument will be ready to record later movements. These require the instrument to have a suitable natural period and damping. It is best for the natural period to be comparable with that of the first swings; about 7s is convenient, and damping should be enough to make the second swing after an impulse small compared with the first. Shorter periods, such as 1s or 2s, are found useful for some purposes. Some of the earlier instruments had little damping and small magnification, and it is probably for this reason that it was so long before the theoretical predictions of Poisson and Rayleigh were verified; P, S and the surface waves were first clearly distinguished, in the records of an Indian earthquake, by R. D. Oldham in 1900.

The times of P and S did not increase with distance as fast as they would if the velocities were the same at all depths. This implies that the velocities increase with depth, which implies in turn that the rays are curved upwards. But when the times of travel between points on the surface of a sphere are known, it is a soluble problem to find what distribution of velocity with depth will give them. This problem was solved formally by Herglotz, Bateman and Wiechert. It remained to find the travel times; this could be done only by collection and analysis of large numbers of observations. Oldham made a beginning, but his tables were superseded by those of K. Zöppritz (1907). Many other workers attempted to improve on Zöppritz's tables, notably A. and S. Mohorovičić and B. Gutenberg. Substantial differences remained outstanding, and it is only within the last few years that it has been possible to say that the travel times of P and S are known within the ordinary inaccuracy of reading.

The place where the earthquake actually occurs is called the focus, and the point on the surface vertically above it is the epicentre. The opposite point of the surface is called the anticentre. The distance from the epicentre to an observing station is usually calculated as an angle. Until recently it was sufficiently accurate for seismological purposes to treat the Earth as a sphere, and the distance was taken as the angle between the verticals at the epicentre and the station. It has now been found better to take it as the angle \( \Delta \) subtended at the centre of the Earth by the line joining the epicentre and the station; for a spherical Earth the two would be identical, but modern discussions have reached such an accuracy that the difference must be taken into account. The need for the distinction was pointed out independently by Gutenberg and Richter and by Comrie. The two distances are known respectively as the geographic and geocentric distances.
Oldham noticed in 1906 that, while P reaches a distance of 100° in about 14 m, it takes about 20 m to reach 180°. He found that the difference was too large to be attributed to distance alone; it was necessary that there should be a drop of velocity at the greater depths. The consequences of this were worked out, partly by Oldham and very fully by Gutenberg (1914). The velocity of P at short distances was about 8 km./sec., rising to about 13 km./sec. at a depth of about 0·4 of the radius, but to fit the time at 180° it must then drop to about 9 km./sec. The result would be that a ray reaching this depth would be sharply refracted downwards, and there would be a shadow zone. The nearer edge of the shadow was located by Gutenberg, by study of individual seismograms, at about 103°, and the further edge at about 143° where P reappeared with an enormous amplitude, falling off rapidly at greater distances. The phenomenon can be imitated optically by filling a spherical flask with water and placing a pinhole source of light and a photographic plate on opposite sides of it at a distance of about twice the radius. The ratio of the velocities of light in air and water is about the same as that of the velocities of P just outside and within the core. The rays of light are cut off by the plate before they reach the focus, but there is a caustic surface surrounded by a shadow. The plate shows, therefore, a very bright ring of light, corresponding to the large intensity about 143°; outside this is shadow; inside it there is illumination, but less intense.

The P wave that has passed through the central core is now usually denoted by PKP, K by itself being used for the phase inside the core. Oldham’s discovery by itself would not determine whether the boundary of the core is a discontinuity or a continuous transition, but it has been shown by other methods that the former is correct. Gutenberg proceeded to an extensive calculation of the times of other phases that should exist if the boundary is a discontinuity. A P or SV wave striking an internal discontinuity would give rise to two reflected and two refracted waves, one P and one SV of each. The reflected P and S waves are known as PcP and ScS (the c being needed to distinguish them from the surface reflexions PP and SS). But an S wave striking the core should give rise to P and S waves in the core, if the core is solid. These would be broken up afresh on striking the core boundary again, giving a great variety of phases, which should reach the surface at different times and distances. The most important of these is SKS, which is of S type in both passages through the outer shell, but of P type in the core. Gutenberg identified this phase and several other core phases; but all that he found had been of P type in the core. He also tried to find waves that had been of S type in the core, assuming the ratio of velocities to be about the same as in the shell (about 1·8), but failed. Several other workers, notably Macelwane (1930) and Bastings (1935), have claimed to detect such waves, but the results are conflicting. Some observations seem to refer to SKKS, some to SKSP, some to PPS, and some to the rather complicated wave ScSPKP. (The last, as Gutenberg has pointed out, has a caustic like PKP, and, therefore, is specially liable to be mistaken for S through the core. Its existence was first pointed out, and approximate times were first given for it, by Gutenberg and Richter (1934, p. 123).) Others are doubtfully readings of genuine phases.
at all. There is no evidence that the core can transmit S waves, and if it cannot it is liquid. We shall see later that there is other and more direct evidence in favour of this suggestion.

At first the most prominent phase that undergoes reflexion at the core boundary was SKKS (reflected, of course, on the inside). Its existence provided good evidence that the core boundary was a discontinuity. The simplest reflexions, PcP and ScS, were not clearly recognized for some time. This was probably because they were mixed up with the train of surface waves; at any rate the first published readings of them seem to have been in a paper on a deep-focus earthquake by F. J. Scrase (1933), in which surface waves were small, and Gutenberg and Richter have recently found them readable in normal earthquakes on the records made by short-period instruments. Their existence is decisive evidence that the core boundary is a discontinuity.

A possibility that does not seem to have been discussed is that PKP might really be of S type in the core and that the true P wave in the core is faster, so that the search should really have been for waves that travel in the core at about 15 km./sec. This, however, is impossible for two reasons. A wave at normal incidence would give zero amplitude for the wave of the other type. Hence PKP at the anticylce, having undergone two such transformations, would have zero amplitude. This is not so. Also, on this hypothesis, a wave that has been of P type all the way would still be expected at the anticylce, but much earlier than PKP. This is not observed. Gutenberg’s assumption that PKP is of P type all through is, therefore, the only possible one.

§ 3. NEAR EARTHQUAKES

At distances up to 700 km. the records of an earthquake are strikingly different from those at larger distances. The oscillations are much more rapid, a single swing and recovery taking about 1 or 2 as against 4 to 7 at larger distances. To get useful information it is necessary to study rather small earthquakes, since the disturbances at short distances in large earthquakes are too violent for accurate reading. It was discovered in 1909 by A. Mohorovičić that the records of an earthquake in Croatia showed two P and two S phases, travelling with different velocities. He inferred that there was an upper layer in the crust where the velocities were substantially less than below it. A fracture within this layer would send out P and S waves; these would in part travel directly to the surface, and in part would be refracted into the lower layer, travel along in this, and be refracted up again when they again strike the interface. This might have been expected from geological considerations, since most igneous rocks fall into two main types, according as they contain free silica or not, and the former appear to have come from the smaller depths. The direct waves are denoted by Pg and Sg, P and S by themselves being reserved for waves that have been in the lower layer. At very short distances (say up to 150 km.) P arrives after Pg and S after Sg, which are considerably larger, and P and S are unreadable; but at greater distances they precede Pg and Sg respectively and can be read. At these short distances the travel times are practically linear functions of the distance, but the constant terms differ on account of the time lost by P and S in the inclined parts of their paths, and lead to information relevant to the thickness
of the upper layer. Gutenberg (1915) made a very thorough examination of the records of two earthquakes in South Germany, which occurred in 1911 and 1913, and obtained similar results from a much larger amount of observational material. V. Conrad (1923) found another traceable P pulse, which appeared to have travelled in an intermediate layer; this was also found by the present writer (1927) in the records of the Jersey earthquake of 1926, and the corresponding S pulse was also found. This pair are denoted by P* and S*.

Unfortunately most of the analyses for near earthquakes are completely vitiated by inadequate statistical treatment. It is obvious that if the focus is at a depth \( h \) and the horizontal distance is \( x \), the time of Pg would be \( (x^2 + h^2)^{1/2} / c \), where \( c \) is the velocity, assumed uniform. If the velocity varies with depth, the formula is more complicated. But inspection of the data shows that a linear law fits the data very well, within departures of the order of a second. To establish the possibility of finding \( h \) or the variation of velocity with depth from the data it would be necessary to make a least-squares solution and show that the estimates of these quantities considerably exceed their standard errors. In practice continental workers have proceeded by a number of different methods, largely graphical, and given estimates of about 35 km. for the focal depth and 55 km. for the depth down to the top of the lower layer, usually with stated uncertainties of a few kilometres. These continue to be quoted in text-books even though their baselessness has been repeatedly pointed out. This is not the place to give the objections in detail, but it may be mentioned that if the times of Pg and P had been calculated from the solutions given, to give a proper final check, it would have been found that the whole of the calculated P times were about 4s later than the observed times that they are alleged to be based on. In one case I found a complete disagreement between the observations of a pulse in an intermediate layer and the solution given; it turned out afterwards that the author had obtained his solution graphically and miscounted the large squares. But that solution has been republished in a text-book without reference to my corrected solution. Perhaps the phenomenon of good observations being maltreated in the process of reduction so as to give a completely worthless result is not unknown in other branches of physics.

Least-square determinations of the velocities of Pg, Sg, P and S in numerous near earthquakes are in satisfactory agreement. The velocities in the intermediate layer are much more difficult to determine. Conrad's observations of P* in the Schwadorf earthquake agreed with mine in the Jersey earthquake. His earlier results for the Tauern earthquake gave a significantly smaller velocity, but some of the readings are capable of other interpretations (Jeffreys, 1937 b).

Gutenberg (1932) finds two different intermediate layer P waves in California, neither agreeing with any European determination. Byerly and Wilson (1935), however, working in the same region, get different results again. The problem here is one of difficulty of reading. Pg and Sg are large sharp movements, and are habitually read at the stations themselves in their routine measurement. But between P and Pg, and between S and Sg, the record shows a continual oscillation of fluctuating amplitude. It is difficult to be sure which of the increases in amplitude are sufficiently sudden to be regarded as definite phases, and there is a possibility that personal opinion may have played a part. On the other hand, in many Japanese earthquakes long series of routine readings, sometimes at as
many as twenty stations, are in good agreement with the times of S* calculated from the Jersey earthquake velocity. There seem to be systematic differences between regions, but these Japanese data seem to show that the readings for intermediate-layer waves correspond to genuine phases and are not accidental.

Determinations of the focal depth and the thicknesses of the upper and intermediate layers from near earthquakes are difficult. Focal depth could be shown only by an upward curvature at short distances in the time-curve for Pg; the departure from a straight line would hardly be appreciable except at the nearest station, and is comparable with the uncertainty of observation. Schmerwitz (1938; cf. also Jeffreys, 1939d) has made a statistical study of a number of recent earthquakes in the Alps and South Germany, and finds in several cases that the estimate of $h^2$ would be negative. This is just what would be expected if $h$ was too small to be determined from the data; if we try in such conditions to determine it, it is as likely as not that a least-squares solution for $h^2$ will give a negative estimate. All that we can say is that if Pg exists the focus is certainly in the upper layer, and if linear forms are fitted to the times of Pg and P*, or Sg and S*, the constant terms will differ by amounts proportional to the vertical distance travelled by P* or S* in the upper layer. Thus we get extreme cases according as the focus is supposed to be at the surface or at the base of the upper layer; the thickness calculated in the latter case will be twice that in the former, and the truth must lie between them. From such considerations it appears that the thickness of the upper layer in Europe is between 19 and 33 km. That of the intermediate layer is found to be 9 ± 3 km. (Jeffreys, 1937b, p. 210). But there are some outstanding difficulties in the interpretation of the data; the constant terms in the times of the various pulses do not agree with any hypothesis as closely as the consistency of a single series of observations would lead us to expect.

§ 4 SURFACE WAVES

The dispersion of Love and Rayleigh waves provides useful additional information about the structure of the upper layers. On account of the fact that they do not spread downwards, they lose less amplitude with distance than the bodily waves do, and at large distances they form the most prominent feature of the record. Their prevailing periods are 10⁶ to 30⁶. On a uniform solid the velocity of Rayleigh waves would be the same for all periods, and Love waves would not exist at all. The two types can be distinguished by the fact that in Rayleigh waves the displacement of a particle is partly vertical and partly in the plane of travel; in Love waves the displacement is at right angles to the plane of travel. The distinction is similar to that between P and SV on the one hand and SH on the other. Consequently if we had all three components of the motion the two types could be separated completely by a rather laborious analysis. But this can be avoided because the vertical component shows only Rayleigh waves, and if we choose a station such that the waves come to it from nearly north or nearly west, one horizontal component will show the Rayleigh waves and the other the Love waves.

The seismograms actually show on each component a long train of approximately simple harmonic waves, varying slowly in amplitude and period. In other words the surface waves show dispersion. This was explained by Love
as due to the heterogeneity of the crust. If the velocity of S increases with depth, Love found that for short waves of both types the wave velocity would be determined mainly by that of S at small depths, and conversely. The existence of dispersion in the surface waves is therefore evidence for a layered crust, confirming the near earthquake evidence. The conditions of observation make it impossible to follow an individual wave, but we can find the rate of travel of each particular period. This is the group-velocity associated with that period, and if we work out the variation of group-velocity with period according to various possible structures and compare the results with observation we can considerably limit the range of structure permitted. Most of the work on these lines has been done by Stoneley (1928, 35, 37). The observational difficulty is much less than for near earthquake studies, but the labour of computation is far greater. This method will probably be considerably extended in future. So far it has been usual to assume a particular ratio of the thicknesses of the upper and intermediate layers and get an absolute determination by matching the group-velocities for a particular period; but greater detail would be possible.

So far the surface waves provide our only method of investigating the ocean floor at small depths. They have the great advantage that they can travel for any distance and do not require a large number of stations near the epicentre. The maximum velocity appears to be about the same as in the continents, but a given fraction of it occurs at a smaller period, indicating that the upper layer is thinner (Byerly, 1930; Bullen, 1939). Little is known at present about the velocities under the ocean for short periods, and such information would be of great value.

§ 5. THE INTERNATIONAL SEISMOLOGICAL SUMMARY

There are approximately 500 seismological observatories in the world. Many of these are purely seismological, but others carry on seismology in association with astronomy or meteorology. The observer at each station reads the most prominent phases on his record of each earthquake. Little use, however, can be made of a single record in comparison with what can be got from the comparison of a large number. When the announcement of a large earthquake appears in the newspapers, with a statement of the position of the epicentre given by a single station, it has usually been obtained as follows. The interval between P and S increases with distance, and hence the observed interval gives an estimate of the distance of the epicentre from the station. The ratio of the extents of the first P movements on the horizontal components determines the direction, with an ambiguity of sign. This cannot be resolved without further information, since the motion may be either inwards or outwards from the epicentre, and the observer sometimes takes the more seismic region as the most likely. Information from one more station will settle the matter, and so will the vertical component if it is recorded; for if the displacement is outwards the vertical displacement will be upwards, and conversely. Such determinations are only preliminary and cannot be accurate because the displacement is difficult to measure. Errors of 10° are quite normal. Accurate determinations need the comparison of widely separated stations.
The systematic collection of the world’s information was begun by John Milne and carried on by H. H. Turner at Oxford, under the auspices of the British Association. In 1922 the scheme was approved by the International Seismological Association, which contributed a large share of the cost. Up to Turner’s death in 1930 he was assisted by J. S. Hughes and Miss E. F. Bellamy, who have continued the work under the supervision of Prof. H. H. Plaskett. Most stations send their readings to Oxford, and when the data for an earthquake have all arrived, its position and time are worked out. The results, including a comparison of each observed time with the calculated time, are published quarterly in the International Seismological Summary.* The solutions were originally based on the intervals between P and S at the stations, but it has now been found better to use the times of P alone. No attempt is made to get the best possible solution from the whole of the data, since it would take too long; a solution is made from the time of P at a few stations, and the residuals at all stations are available for a more accurate determination if one is required.

§ 6. RECENT WORK ON TRANSMISSION TIMES

The Zöppritz tables of P and S, with supplementary tables computed by Turner, were used for solution and comparison in this work for earthquakes up to the end of 1929 (published in 1933). Turner, however, published an important paper in 1926, in which he classified the residuals for 1918–22 by distance, and found that they varied systematically. The means varied with distance by about 20s for P and 30s for S. About the same time P. Byerly (1926) gave a set of times based on his special study of the Montana earthquake of 1925 June 28. I compared these with the corrections found by Turner and found very close agreement (Jeffreys, 1928). It appeared at this stage that these corrections were not likely to be far wrong. In two later papers (1931 b) I extended Turner’s work to the earthquakes of 1923 to March 1927, and again got similar corrections, which are embodied in my 1932 tables, published by the British Association Seismological Committee. The apparent standard errors of these times were mostly under a second, but there were grounds for mistrusting them. The corrections were simple means of the residuals (apart from a correction to take account of departure from the normal law of error) against the Zöppritz times; but the epicentres and times of occurrence had been found so as to give the best possible agreement with the latter, and in different earthquakes the observations were differently distributed in distance. Thus part of the variation of the errors in the tables would have been converted into errors of the epicentre and times of occurrence, and would not be revealed by a mere classification of the residuals. I called attention to this feature at this stage (1931 b, p. 337), but some critics overlooked the warning. The Zöppritz times being taken as a first approximation, my 1932 ones were a second approximation, but it would be necessary to complete the process before the apparent accuracy could be claimed as genuine.

At this stage I obtained the valuable assistance of K. E. Bullen. It was necessary (Jeffreys and Bullen, 1935) to reduce afresh all the earthquakes used

* Referred to hereafter as the I.S.S.
for P and S, determining corrected epicentres and times of occurrence so as to
give the best possible fit with the 1932 tables, and then to classify the new residuals.
This showed that further corrections, reaching about 5s for P and 9s for S, were
still needed. Applying these, we got a new set of tables and repeated the process
till there was no further change. S was not found useful in determining epi-
centres at this stage, and the solutions for the individual earthquakes rest on P
alone. Other phases were not treated until the solution for P was complete.

For S there were several troubles. Up to about 20°, the S residuals were
spread over about 20s without any convincing concentration of frequency, and
our final solution was based very largely on analogy with the times of P in this
range, combined with knowledge of the velocity of S at short distances derived
from special studies by Conrad and myself. This scatter of the S residuals
remains unexplained. From 25° to 83° it was found that the S residuals for a
given earthquake varied fairly smoothly, but in comparison of different earth-
quakes there was a complication, which we called the Z phenomenon. S behaved
as if its time of origin differed from that of P, and not always by the same amount.
The time of origin found from P being \( t_0 \), the average S residual, taking the same
\( t_0 \), might be anything from \(-12s\) to \(+8s\). This was noticed first by Lehmann
and Plett (1932) in their studies of the Peru and Marianne Islands earthquakes
of 1928 July 18 and 1930 October 24. Bullen and I had to decide which of these
should be regarded as a typical earthquake, and chose 1928 July 18, on the ground
that the early S on 1930 October 24 might be due to focal depth. This, as it
turned out, was wrong.

Beyond 83° SKS precedes S. It had given Turner much trouble. The
practice at most stations was to read P and L first, L being the beginning of the
surface waves. The interval between these gives a rough estimate of the distance,
and hence of the time when S should arrive, within a minute or two. But beyond
83° an observer reading the first sharp movement about that time would inevitably
read SKS for S. The result was that the I.S.S. contained hardly any readings
of S at these distances, nearly all being SKS. A proper criterion for the separa-
tion was given by Lehmann and Plett. SKS, having been of P type in the core,
is wholly of SV type; but S at any distance includes SH. If a station has records
of both horizontal components, they give the total horizontal displacement at
each instant. Until S arrives this is in the same direction as for P, but the arrival
of S is indicated by an abrupt change both in amount and in direction. Also the
interval between P and SKS from 83° to 105° is nearly constant at about 108 40s.
It appeared, therefore, that if an observer finds an interval of about this amount
between P and S he should suspect that the apparent S is really SKS, and should
look for the true S after it. Since then the number of genuine S readings at large
distances has greatly increased.

SKS resembled S in showing the Z phenomenon; it and SKKS were reduced
as far as possible to the same origin time as S.

For PKP the residuals showed a violently asymmetrical law of error. The
normal law of error does not hold for any phase. The residuals usually show a
concentration about a mode, this by itself suggesting a standard error of 2s or 3s;
but there is a long range on both sides within which the numbers of residuals
fall off very slowly up to about 10s. A mean and a mean square error found from
the residuals in the usual way would mean hardly anything. The explanation lies chiefly in a difficulty of identification. After a phase has passed, the ground does not return to rest, but continues to vibrate irregularly, usually with a diminishing amplitude, until the next phase arrives. If different observers agree on what to read they generally agree to 1s or 2s, but an observer reading his records without reference to the other stations is liable to take some part of the irregular movement as a genuine phase, and this is shown by a large residual. Even P does not usually start from rest, since there are perpetual vibrations of the ground, known as microseisms; these lead to uncertainties about the position of the true beginning of P on the record and to large residuals of either sign. There is also occasional trouble from large clock errors. This was dealt with by detecting the offending stations and afterwards ignoring them. The result is that we have to deal with a modified law of error, with a sharp peak superposed on a nearly uniform background. I found that this can be dealt with without prohibitive trouble by the method of "uniform reduction", in which the residuals are grouped at 1s intervals and counted; then a constant is subtracted from the number in each interval, including the central ones, so as to leave the central group isolated, and a mean and standard error are found from the latter as reduced. Later work (1936) showed that this is a close approximation to the most accurate treatment, which supplies a weight to be attached to each observation as a function of the residual. This weight is approximately the probability that that particular observation is normal.

The usual methods, such as rejecting observations while treating those retained at full weight, are completely unsatisfactory. The decision about which observation to reject and which to retain is arbitrary, and may displace the mean by the full amount of its apparent standard error. If the study of the distribution of the residuals shows that the normal law of error is not followed, the correct procedure is to acknowledge the fact and devise a method of reduction suitable to the actual law. This is not prohibitively difficult in practice—certainly nothing like as difficult as making the observations (Jeffreys, 1938b, 39a).

For PKP there were a few early readings as for P, then a sharp rise to a maximum within a range of about 2s, but then the readings dropped off very slowly over about 20s. Bullen and I made a provisional solution, but without much confidence. The only other useful phase recorded with appreciable frequency in the I.S.S. was SKKS. The residuals for this were very scattered, but a solution had to be made because SKS and SKKS together make it possible to compute times of ScS without further assumption, and hence to infer times of other core waves to serve as standards of comparison.

As far as possible we relied on the reports in the I.S.S. for observational data. Numerous special studies of individual earthquakes had been made, but their authors usually appeared to disagree, and it was not possible to say how far the differences were due to some personal equation, whether some correlation between errors due to the method of reading might have led to an appearance of excessive accuracy, or whether there was some genuine difference between earthquakes. Using the I.S.S. we knew that all records for each earthquake were read by different observers, so that the personal equation could legitimately be treated as random error, and would be included in the estimate of uncertainty.
derived from the scatter of the residuals. This method had the further advantage that the two most laborious parts of the work, the actual reading of the records and the preliminary calculation of an approximate epicentre and distances, had already been done for each earthquake by the stations and the Oxford staff. There were questions that could be settled only by special study, but this method indicated which these were.

This consideration is particularly important because a beginner in seismology, faced with an actual record, is often unable to distinguish at all between definite phases and irregular oscillation. This may lead to an attitude of complete scepticism. The reading of seismograms is an art that requires experience. Even experienced observers sometimes disagree; but when a large number of them agree we can be sure that they are right. In near earthquakes Pg and Sg are frequently read in routine observations, and the results of special studies may be considered confirmed; but P* and S* are seldom read, except for what appears to be S* in Japan, and to that extent it may be doubted whether they are genuine. Special studies serve a useful purpose in settling doubtful points, but the devising of adequate checks is more difficult than with routine observations.

The tables published by Jeffreys and Bullen (1935) were adopted in place of the Zöppritz tables for the I.S.S. reductions from 1930 January. The work since then has consisted mainly of tracing and eliminating systematic errors. The first of these was the effect of the ellipticity of the Earth. Gutenberg and Richter had suggested a method of allowing for it, but we were not satisfied that this was the best, and other outstanding errors seem likely to be larger, and their treatment could not be postponed until the calculation of the ellipticity effect was done in detail. The trouble was that the layers of equal velocity within the Earth would also be elliptical, and this would affect the times of transmission even between points at the same distance from the centre and from each other. Since Bullen's return to New Zealand at the end of 1933 he has calculated the correction for all the relevant phases—a most laborious piece of work—and allowance has been made for it. The final conclusion is simple (Bullen, 1936, 37 a, and later papers in same volume). If geocentric latitudes are used, the effect of ellipticity is allowed for within about 0.2 by adding a term proportional to the sum of the heights of the epicentre and the station above the sphere of equal volume; the other factor is tabulated by distance for each phase. The form of the correction is more complicated if geographic latitudes are used. Bullen has dealt with this by providing a comprehensive table of triple entry for converting geographic to geocentric distances (1938). The difference can reach 0.4, and in extreme cases an epicentre may be wrong by a quantity of this order if geographic distances are used in finding it. Incidentally this correction accounted for a number of cases where an earthquake in a medium latitude had given a set of very consistent negative residuals of −3s or −4s on the other side of the equator; when geocentric latitudes were used these fell into line. The correction of the P table was simple because the earthquakes used fell into a few separate geographical groups: South-Eastern Europe and Western Asia; North and Central America; Japan; South America; and Pacific Ocean. For earthquakes of one group the ellipticity correction would be nearly the same, and its values for a mean epicentre could be applied at the end of the work. The corrected times for the groups
could then be combined, keeping the previous standard errors, since the correction was a known quantity. Thus the P table was reduced to a mean sphere (Jeffreys, 1937c).

A further correction was needed for P at large distances (over 90°) and PKP, on account of the prevalence of late readings due to weakness. The trouble is that if they are small the observer usually waits till he is sure of them before he makes his reading, and the result is that the readings are systematically late. Up to about 90° there is little trouble because the law of error is nearly symmetrical. The difficult ranges were treated by using only the best stations with instruments for recording vertical movement—the latter because P at large distances and PKP rise steeply and are most prominent on the vertical. I found that this selection gave symmetrical distributions of residuals, with about the same standard error and background effect as for P at moderate distances, and the effect of skewness was satisfactorily eliminated. The greatest change from the Jeffreys and Bullen (J.B.) times due to correction of these two systematic errors was -2s.7.

For S and SKS the most satisfactory procedure was to start again, using geocentric distances throughout. The residuals were much more scattered in some earthquakes than in others, and in some cases seemed to fall into two or more distinct series separated by several seconds. A summary based on observations from two series would be representative of neither. At this stage, therefore, attention was confined to earthquakes with long series of S and SKS readings, consistent enough to indicate that nearly all stations over a long range of distance had read the same thing (Jeffreys, 1939b, 39c). To get a sufficient number of SKS observations at large distances it was necessary to use a number of earthquakes in the southern hemisphere (Jeffreys, 1938a); it has been recorded up to 145°. When these data were combined it was found that the J.B. times of S beyond 80° and of SKS were about 12s too long. This had already been inferred from some deep-focus earthquakes (Jeffreys, 1935a), which will be discussed in a moment. On applying Pearson's $\chi^2$ test to the mean residuals of these earthquakes over 5° ranges of distance it was found that they were as consistent as their apparent standard errors would suggest, so that the correctness of the revised tables is adequately checked.

§7. DEEP-FOCUS EARTHQUAKES

These were discovered by Turner in 1922, using the I.S.S. data. His method of determining a preliminary epicentre was to draw an arc about the point of a globe that represented each station with radius equal to the distance corresponding to the interval from P to S. These arcs usually intersected near one point and gave a rough epicentre. But he found that in some cases the arcs failed to meet by several degrees. The data could, however, be reconciled fairly well by supposing P and S to have come from a focus at a considerable depth. When this happened, if PKP was also observed, it was found to be systematically early. A criticism of Turner's method was that the discrepancies in time were of the same order of magnitude as the errors known to exist, though not then well determined, in the Zöppritz tables, and that the possibility that they arose from errors of the comparison tables could not be excluded.
K. Wadati (1928) studied the arrivals of P and S in Japan in local earthquakes, also relying principally on the interval between P and S. This provides an estimate of the distance from the focus. In a deep-focus earthquake it would be larger at the epicentre than in a shallow one, but would not increase so fast with distance from the epicentre. Wadati found substantial differences between earthquakes in this respect. Further, it happened several times that both he and Turner found about the same focal depth for an earthquake by their different methods. His tables were much more accurate than those of Zöppritz, but it appeared possible that some of the upper-layer waves might have been mistaken for P and S. Consequently both methods, though suggestive, were not quite convincing.

Decisive evidence was obtained by Stoneley (1931) and Scrase (1931). By a reciprocal theorem in the theory of small oscillations, a normal mode cannot be excited by an impulse at one of its nodes. Now a surface wave of given period is a normal mode, and effectively the whole region more than a wavelength deep is composed of nodes. Hence a deep-seated disturbance should give rise to no surface waves of lengths less than the focal depth and of appreciable amplitude. The theory was worked out in some detail by Lamb (1904) and Nakano. Now in the alleged deep-focus earthquakes the I.S.S. gave numerous readings in the column devoted to surface waves, which provided further reason for doubt. But Stoneley found that these readings were much earlier than in shallow earthquakes, the differences reaching 10 minutes. They agreed well with the times expected for various S movements (SS, SSS, etc.) reflected at the outer surface, which would exist for any depth of focus. When original records were inspected, the true surface waves were untraceable at the proper time for them. It appeared therefore that the deep foci did exist. Stoneley's work provided the first unambiguous evidence.

Scrase followed up an earlier suggestion of G. W. Walker. If the focus was in the outer surface, PP would be reflected midway between the focus and the observer. But if the focus is at some depth two phases of PP type are possible, one reflected about distance $\frac{1}{2}\Delta$, as in a shallow earthquake, but the other reflected close to the epicentre. The latter is actually the more familiar in the analogous case in optics, since it corresponds to the rays that form the image in a concave mirror. The phase reflected at half the distance is much less familiar in the optical case because the mirror seldom includes a sufficient amount of the sphere. Scrase denoted the near reflexion by pP, keeping PP for the distant one, and similarly he denoted the other possible near reflexions by sP, pS and sS. He found, on examining the Kew records of a number of deep shocks, that these reflexions were present and very clear. Many stations had read them for themselves and reported them as unidentified additional readings. The intervals between P and pP or sP, and between S and sS, should vary slowly with distance; this also was verified. The verification of these near reflexions provided another crucial test of the hypothesis of deep foci, and there was no further occasion to doubt their reality. Stoneley's test and Scrase's, it must be noticed, do not depend on having accurate times of transmission as a preliminary, and are independent of the possibility of errors of the types that cast doubt on the methods of Turner and Wadati.
When accurate times of transmission were available I found it possible to make Turner's method much more sensitive. An approximate calculation of the effects of focal depth made it possible to find focal depths from P alone. In the five earthquakes chosen (1935 a), if the focus was assumed shallow, the residuals of P varied systematically with distance by 14 to 60, however the epicentre might be chosen, but with allowance for depth the standard deviation was reduced to about 2, which is as good an agreement as is usually found for a normal earthquake. Analogous results were found for S; in fact S appears to be better observed in many deep earthquakes than in normal ones.

It is convenient to record focal depth below the top of the lower layer as a fraction of R, the radius of that layer. The first work that indicated to me (1935 a) that there was something seriously wrong with the J.B. times of S at large distances and SKS was on the Afghanistan deep-focus earthquake of 1929 February 1. A long series of European stations had read S, and many North American ones SKS. The former gave small residuals, the latter very consistent ones of about -12. The only possible explanation seemed to be that the J.B. times of SKS, and, therefore, of S at large distances (with which it had been accurately compared in the process of identification), were about 12 too long. This was confirmed by three other well-observed shocks, the deep ones of 1928 March 29 and 1931 February 20, studied by Stechschulte and Scrase, and the Marianne Islands one of 1930 October 24, studied by Lehmann and Plett. We had actually used Lehmann and Plett's readings in the latter, but they all referred to great distances, and the comparison with intermediate distances became possible only when the data had appeared in the I.S.S. Scrase had actually remarked that $dt/dA$ for S beyond about 70 in his study (1933) was less than my 1932 times gave, but the J.B. values are larger than my 1932 ones. I was not sure then that the difference was not due to some error in estimating the allowance for focal depth, but later work has fully confirmed it.

Deep-focus earthquakes have also been useful in solving the problem of the times of S up to 20 to 25. S can be observed at these distances in normal earthquakes if attention is paid to the change of direction of the movement, but it is followed by several large movements, one or other of which is read by most stations. No satisfactory explanation has been given of these movements. It is, however, clear that they are not S. In deep-focus earthquakes, however, the S residuals at these distances show a pronounced concentration, with only the usual amount of background effect. A study of ten such earthquakes in and near Japan, combined with the velocity at short distances found from European near earthquakes, has led to what appears to be a highly satisfactory determination of the times of S up to 20.

Another question settled by deep-focus earthquakes is whether the interfaces postulated in near earthquake study are true discontinuities or continuous transitions. Near earthquakes by themselves can only establish the existence of approximately uniform layers, and say little about the transitions. But if there are discontinuities, the usual pP, reflected at the outside, should be preceded by a smaller reflexion from each interface. Direct examination of seismograms of deep shocks showed no such reflexions, and, therefore, the transitions must be continuous (Jeffreys, 1935 a). It is, however, still necessary to use sharp transi-
tions in any model used for calculation, because we have still no means of estimating how thick the zones of transition are, and an over-estimate of their thicknesses would be as bad as treating them as zero, and more difficult to correct.

If we adopt a standard structure of the upper layers we can find the focal depth from the observations of P and S at different distances. But given this depth we can also calculate the times of pP and sS. These are more sensitive to the thickness of the upper layers than P and S are, because they have passed through them three times instead of once. Consequently the observations of pP and sS provide an additional equation of condition for the thicknesses of the upper layers. By combining this with the surface-wave data I get 15 ± 3 km for the upper layer, 18 ± 4 km for the intermediate one. With these values it is possible to find times corresponding to a surface focus, which is the most convenient standard of comparison.

The core reflexions PcP and ScS are much easier to read in deep shocks than in normal ones, chiefly on account of the smallness of the surface waves. The readings of PcP are still rather scattered, presumably because a P wave striking the core is largely transmitted. ScS is quite satisfactory, and leads to the best determination of the radius of the core.

§ 8. THE 20° DISCONTINUITY

Byerly's work on the Montana earthquake and my earlier work showed that \( \frac{d^2t}{d\Delta^2} \) for P is very large about distance 20°. The same had already been noticed by F. Neumann, and emerged again in the earlier stages of my work with Bullen. At short distances \( \frac{dt}{d\Delta} \) is about 14s.3/1°, decreases gradually to 12s.3/1° between 18° and 19°, and then drops to 10s.4/1° between 20° and 21°. At larger distances it falls off steadily, reaching 4s.4/1° beyond 100°. These facts indicate that between the depths reached by the rays that emerge at 19° and 20° there is a great increase of the velocity of P. There might be a new discontinuity, which was supported by some observations made by Lehmann (1934). She found what appeared to be a second P in records between 20° and 24°, just as Pg in near earthquakes continues to be readable in near earthquakes at distances where it is preceded by P. Her readings, however, are a few seconds too late to lie on a smooth continuation of the P curve up to 19°. The large amplitudes of P about 20° also suggest a strong curvature of the time-curve and, therefore, a rapid continuous increase of velocity with depth above and below the discontinuity, if there is one. The question was, therefore, not whether there are two regions, within each of which the velocity varies nearly uniformly with depth, and separated by a sharp discontinuity, but whether the increase is or is not sufficiently rapid to make the distance reached by a ray less for some rays than for some others that have begun by descending less steeply. If it is not, the time-curve is a smooth curve with a strong curvature about 20°; if it is, the curve has a loop terminating at each end in a cusp, and for distances within this range there will be three P's. The oscillations that follow P everywhere make it impossible to identify these with confidence. Consequently the question could not be decided by means of normal earthquakes alone. It was tested by means of the Japanese deep shocks (Jeffreys, 1939 b). Separate calculations of
P times were made for different focal depths, one assuming everywhere finite curvature of the time curves, the other assuming a sharp discontinuity. They differed by amounts up to 1°-4 for the depths of these earthquakes. If the difference between the times of the first P on the two hypotheses is $x$, and the true time differs from those on the hypothesis of discontinuity by $ax$, $a$ could be estimated from the data, and was found to be $+0.17 \pm 0.21$. This amounted to a disproof of the first hypothesis, which would correspond to $a = 1$. Nevertheless the second is not altogether satisfactory because it does not explain the large amplitudes. Finally, a continuous distribution of velocity was adopted, the variation over a limited range being rapid enough to give a triplication of P at the corresponding distances. In the extreme case of a true discontinuity, the receding branch would reduce to the pulse reflected at it; this was sought but not found. In deep shocks the first arrivals of P and S at some distances corresponded to parts of rays that did not give first arrivals in normal shocks, and consequently part of the range where P and S are triplicated could be reconstructed from the data for the deep shocks. Some arbitrariness remained, but at any rate the solution reconciled the whole of the data.

§ 9. THE INNER CORE

On the simplest theory, corresponding to geometrical optics, there would be no P or PKP between 105° and 142°. Actually a feeble extension of P from 105° exists, and has been traced as far as 150°. This is due to diffraction around the core. It is quite different in appearance from P proper, having no sharp beginning. From PKP at 142°, again, an extension is traceable back to about 110°. This was originally interpreted as also due to diffraction, in this case near a caustic. Lehmann (1936) and Gutenberg and Richter (1938,39), however, rejected this explanation, because it did not account for the short periods and large amplitudes to be found at these distances. I found (1939e), by applying Airy's classical theory of diffraction near a caustic, that periods of the order of 18 should be traceable back to about 139°, and those of 108 to about 128°. Actually periods of 18, associated with large amplitudes, are observed between 123° and 125°. The diffraction theory was, therefore, impossible, and it was necessary to construct one of refraction. But to get a branch of such length requires very exceptional conditions. A reflected branch would always be associated with an earlier refracted one. The deep earthquake of 1932 January 9 gave a fine series of observations. It was found that the time-curve is nearly straight, and there was considerable difficulty in framing a hypothesis that would give the observed length. A sharp increase of velocity with depth, well within the core, would give the longest reflected branch with any ordinary hypothesis, but calculation showed that it could not be long enough unless, in addition, the velocity decreased with increase of depth for some distance outside the discontinuity. Rays grazing what we must call the inner core could be reflected; those striking it sufficiently steeply to penetrate would give a refracted branch giving slightly smaller times of travel, for equal distances, than the reflected wave. On this hypothesis the data could be fitted (Jeffreys, 1939f).
§10. THE VELOCITY DISTRIBUTION

If the times of a pulse satisfying the principle of stationary time are known over a range of distance starting from 0°, the distribution of velocity can be found by a series of straightforward numerical integrations. Theoretically this remains true even in cases of strong refraction leading to a triplication of the pulse over some range of distance, but in practice the upper branches are unobservable, and some hypothesis is needed to reconstruct them. For a shadow zone the pulse does not exist at all over a range of distance, and the method fails. It also fails, in theory, if the times are not known over a range near the origin, but in that case they can usually be inferred without difficulty. It can be shown that if the velocity has a continuous derivative with regard to the depth for small depths, the times of arrival have the form \( a\Delta - \beta \Delta^3 + \ldots \), and if we can find distances where the time is known, and this formula agrees with them, the formula can be used to infer times for all shorter distances.

If a pulse undergoes reflection or discontinuous refraction at any stage we can make direct use of the principle of stationary time. To take a simple example, that of PS, let the point of reflection be at distance \( \Delta_1 \) as seen from the centre of the Earth and the time of P at it be \( T_1 \). Then the angular distance travelled as S is \( \Delta - \Delta_1 \), and S travels this distance in time \( T_2 \). The total time is then \( T_1 + T_2 \), and for it to be stationary for small variations of \( \Delta_1 \), for fixed \( \Delta \), we must have

\[
\frac{\partial T_1}{\partial \Delta_1} = \frac{\partial T_2}{\partial (\Delta - \Delta_1)} = \frac{\partial (T_1 + T_2)}{\partial \Delta}.
\]

To construct a table for PS, therefore, all we have to do is to inspect the tables of P and S to find distances where the values of \( \partial T/\partial \Delta \) are equal; and then, by adding the distances and times, we get a distance for PS and the time corresponding to it. Repeating the process for different values of \( \partial T/\partial \Delta \) we can construct the complete table for PS. In practice an error in \( \Delta_1 \) for given \( \Delta \), makes only a second-order error in the sum \( T_1 + T_2 \), and great accuracy in matching the gradients is not necessary; tabular entries at 1° intervals are quite enough. Calculated times to 0s.1 are necessary to prevent accumulation of rounding-off errors; the actual standard errors are mostly of 0s.3 to 0s.5 for the well-observed phases.

Conversely, if we had the times of P and PS, but not of S, we could infer those of S by matching gradients and subtracting distances and times.

This process can be used to infer times within the core. If we have times of ScS and SKS with the same \( \partial t/\partial \Delta \), the time spent by SKS within the outer shell is that of ScS, and by subtraction we get a corresponding time and distance for the wave in the core alone. Similarly we can infer times of K from those of PcP and PKP; the range of \( \Delta \) in the core is not the same in the two cases, the first case giving the smaller distances and the second the larger, with a slight overlap. The distances for K do not come down to zero, but some of them are small enough for a cubic formula to be fitted to the times, and thus the times can be calculated for short distances between points on the surface of the core. The removal of the effects of an outer layer in this way may be called "stripping". It is applied in succession to remove the effects of the upper and intermediate layers, then the
outer shell, and finally the outer part of the core, leaving the inner core, which, as far as we know, contains no further discontinuity.

Typical velocities in km./sec. found in this way (1939e, 39f) are as follows:

| Layer                | P   | S   |
|----------------------|-----|-----|
| Upper layer          | 5-58| 3-36|
| Intermediate layer   | 6-50| 3-74|
| Lower layer (top, \(r=0.94R\)) | 7-747| 4-35|
| \(r=0.93R\)         | 8-97| 4-96|
| \(r=0.96R\)         | 9-50| 5-23|
| Core: \(r=0.548R\)   | 8-10|     |
| \(r=0.230R\)        | 10-37|  |
| Inner core: \(r=0.20R\) | 11-16|  |
| Centre: \(r=0.00R\)  | 11-31|  |

It is convenient to use \(R\), the radius of the base of the intermediate layer, as the standard of length. For deep-focus earthquakes the depth below the base of the intermediate layer is given as a fraction of \(R\).

In Gutenberg's original method the depth of the core was found as the greatest depth reached by \(P\) before the shadow begins. This was not very satisfactory because the shadow is not sharp, and the determination requires the slope of an empirical curve at its very end, always a matter of considerable uncertainty, but at that time no other method was possible. We can now determine the velocities of \(P\) and \(S\), and extrapolating them slightly we can compute times of \(PcP\) and \(ScS\) for trial depths of the core; then comparison with the observed times gives an estimate of the depth of the core. The radius of the core is found to be \((0.5480 \pm 0.0004)R = 3473 \pm 2.5\) km. With this datum the times of \(PcP\) and \(ScS\) are computed, and are used in determination of times in the core.

On any admissible hypothesis, a reflected pulse from the outside of the inner core follows too quickly after the refracted one to be read separately, and the determination of the radius is less definite.

The final stage is the computation of the time of various compound phases, especially those that have undergone one or more reflexions at the outer surface or the core boundary. This has been done (Jeffreys, 1939f; Bullen, 1939b) not only for a surface focus but for depths at intervals of \(0.01R\) to \(0.12R\). The greatest focal depth known so far is about \(0.093R\) (originally reported as \(0.11R\), but this turned out to be too great when more observations became available (Jeffreys, 1941)). A large number of compound phases are readable, but in general with less accuracy than the main phases \(P\), \(S\), \(PcP\), \(ScS\), \(PKP\), \(SKS\), and the deep-focus phases \(pP\), \(sS\).

In a long series of papers over about the same period as mine and Bullen's, Gutenberg and Richter have made independent determinations of the times of the principal phases and the velocity distributions. On the whole their results agree well with ours, and we were often enabled to settle doubtful points quickly by consultation with them.

§11. IDENTIFICATION OF MATERIALS AND DENSITY DISTRIBUTION

Much experimental information is available about the mechanical properties of rocks at pressures equivalent to those existing at depths up to 60 km. This
range covers the depths relevant to near earthquakes. The most satisfactory method of comparison seems to be to use the equation

\[ a^2 - \frac{4}{3} \beta^2 = \frac{\lambda + \frac{4}{3} \mu}{\rho} = \frac{k}{\rho}. \]

The quantity on the left is determinate from the velocities alone, and for any material tested the right side is the ratio of the bulk-modulus to the density. The bulk-modulus is the easiest elastic constant to measure at high pressure, and in particular has a negligible time-effect. It was originally expected from geological considerations that the upper layer would be granite and the lower "basalt", the latter for this purpose including the crystalline forms dolerite and gabbro. Sues's terms "sal" (altered to "sial" by Wegener) and "sima" were originally intended to allow somewhat greater latitude of interpretation, but in practice have come to be used in the restricted senses. The following table compares measurements by L. H. Adams, E. D. Williamson and R. E. Gibson at the Geophysical Laboratory, Washington, with the seismological data.

| Layer     | \( k/\rho \) (km./sec.)² | \( \rho \) (at 2 \times 10^9 dynes/cm²) |
|-----------|---------------------------|----------------------------------------|
| Upper     | 16.1                      | 18.3-19.5                              |
| Intermediate | 23.6                  | 22.8-24.3                              |
| Lower     | 34.8                      | 27.8-28.2                              |
|           | Tachylyte              | 24.2                                   |
|           | Dunite                  | 35.2-38.3                              |

The order of increasing \( k/\rho \) in the crystalline rocks tested is that of increasing density and that of decreasing silicon and alkali content. Tachylyte is the glassy form of basalt. No rock tested gives a higher value of \( k/\rho \) than dunite, which is nearly pure olivine (Mg, Fe)\(_2\)SiO\(_4\). This value agrees very well with the seismological value for the lower layer. The seismological value for the upper layer is rather low for a typical granite; perhaps the most likely interpretation is that the upper layer contains more free silica than the average granite, or it might contain some of the glassy form, obsidian. No layer agrees reasonably well with gabbro, though the intermediate layer would agree with either tachylyte or diorite. According to modern petrological theories, igneous rocks are considerably altered in composition by partial crystallization in rising from their original levels to accessible places. Then we should have to regard the typical surface granite and basalt as having come from rather low down in the upper and intermediate layers respectively, possibly from the transition zones. This is reasonable also because in each layer the temperature is nearest the melting point at the bottom. On account of the close correlation between \( k/\rho \) and \( \rho \), if we are concerned with properties that involve only the density, the latter is well determined for each layer, since all ordinary rocks that give the same \( k/\rho \) have nearly the same density.

Two other data relevant to the distribution of density are the mean density and the moment of inertia. The latter is very accurately known for the following reason. If \( C \) is the moment of inertia about the polar axis, \( A \) that about an axis in the equator, the rate of the precession of the equinoxes contains a factor \((C - A)/C\), the other factors being very accurately known. If \( E \) is the Earth's mass and \( a \) its equatorial radius, the distribution of gravity with latitude and three measurable perturbations of the moon depend on \((C - A)/Ma^2\). The ratio of
these numbers determines $C/Ma^2$, which is $0.3345 \pm 0.0012$. For a homogeneous sphere this ratio would be 0.4, so that we can infer that the Earth has a considerable central condensation. Various distributions of density have been chosen to fit these (or equivalent) data. A more natural one was due to Wiechert in 1897, who guessed that the Earth might consist of a rocky shell and a metallic core, and supposed both uniform in density. Such a constitution involves three unknowns, the two densities $\rho_0$, $\rho_1$, and the radius of the core, $a\alpha$, one of which can therefore be chosen arbitrarily. He took the plausible value 3·20 for $\rho_0$, and got $\rho_1 = 8.22$, $\alpha = 0.78$ (with a slightly different mean density from that now accepted). But seismology shows no discontinuity anywhere near the boundary of Wiechert's core. If on the other hand we take the seismological value $\alpha = 0.545$, we get $\rho_0 = 4.22$, $\rho_1 = 12.32$. The explanation of the discrepancy between $\rho_0$ and the density of dunite is simple; each layer of the Earth is compressed by the weight of the material above it, and, therefore, even a layer uniform in composition would have a density increasing with depth. A rough comparison showed that if the material of the shell had density 3·2 gm./cm$^3$ at atmospheric pressure, the actual mean density of the shell would be in the neighbourhood of 4·2. Similarly if we regard the core as liquid, the velocity of longitudinal waves in it gives $k/\rho$ directly; assuming a mean density of 12 gm./cm$^3$ at the actual pressure, and then supposing this pressure removed, I found (1926) that the density would sink to about 8 gm./cm$^3$, about the density of iron. Consequently Wiechert's principle was valid, and the rough agreement between his densities and those of dunite and iron persisted; his radius for the core was wrong because he had not allowed for compression. He wrote, of course, before $P$ and $S$ had been identified, much less PKP, so that he had no geophysical data for estimating the compression.

The hypothesis that the core is liquid is strongly suggested by the failure to identify $S$ waves in it. This does not quite settle the matter, because they might exist but be strongly damped. There is, however, much confirmatory evidence. Kelvin showed long ago that the tidal yielding of the earth indicated a rigidity comparable with that of steel. But the rigidity of dunite at atmospheric pressure is about two-thirds of that of steel, and it follows from the increases of the density and the velocity of $S$ with depth that at the base of the shell the rigidity is about three times that of steel. A calculation (Jeffreys, 1926) showed that if the core was taken as liquid we could get a good agreement with the tidal yielding; if it was taken as having a rigidity comparable with its bulk-modulus the tidal yielding would be far too small. The core therefore certainly behaves as liquid under stresses with periods of the order of 12 hours. Even this does not quite close the matter, because a solid might have such imperfection of elasticity that it would behave as liquid to tidal forces but as solid to stresses with periods of a few seconds. This not very hopeful suggestion, however, is hard to reconcile with the size of SKS. Many cases of refraction at interfaces between solids have been worked out, and one peculiarity is common to all so long as the velocities of $P$ and $S$ are in about the same ratio; an incident $P$ gives a very small transmitted $S$, and conversely. But SKS has undergone two such transformations and yet is comparable in size with $S$. It seems, therefore, that any escape from the conclusion that the core is liquid would have to be wildly artificial.
We have now much more detailed information about the distribution of density from the work of Bullen (1936, 37b, 40 a-41). This started as a step in the work on the ellipticity correction in seismology. The ellipticities of internal surfaces of equal density were required, and a preliminary stage was the determination of the density-distribution itself. This requires the solution of the differential equations

\[
\frac{dp}{d\rho} = \frac{k}{\rho} = \alpha^2 - \frac{1}{3}\beta^2;
\]

\[
\frac{dp}{dr} = -g(r)\rho,
\]

where \( r \) is the distance from the centre, \( p \) the pressure, and \( g(r) \) the local intensity of gravity, itself depending on the values of \( \rho \) for smaller values of \( r \). These can be solved numerically, the seismological values of \( \alpha \) and \( \beta \) being used for each \( r \). After allowing for the outer layers, Bullen proceeded inwards to the core boundary; but he found that any density just within the core that would lead to the correct mass would give too small a moment of inertia. This approach, in fact, had assumed only one adjustable constant, and another was still needed. But there was a possibility of another discontinuity of density, namely at the 20\(^\circ\) discontinuity. He took this to be a sharp change at a depth of 350 km., and found that with a jump of density as well as of the earthquake wave-velocities he could get an agreement. I found a little later that if the change is treated as a true discontinuity its depth is about 480 km.; with this modification the jump of density is from 3.68 to 4.20 gm./cm\(^3\). If the pressure was removed from the lower material its density would be about 3.8 and the velocity of a P wave in it about 8.4 km./sec.

After inspection of the data for some likely materials I found (1937 b) that the only one with about the right properties was MgO. A most interesting alternative suggestion was made by J. D. Bernal. The olivine crystal is a rhombic lattice of oxygen atoms with the silicon and metallic atoms in a somewhat asymmetrical pattern. At high pressures there is a possibility of a complete rearrangement, suggested by the corresponding germanium compound Mg\(_2\)GeO\(_4\), which has been made by Goldschmidt, and is isomorphous with Mg\(_2\)SiO\(_4\). Ge has larger atoms than Si, and if there is any question of a rearrangement under high pressure the Ge atoms would obstruct the further compression of the oxygen lattice at smaller compressions than Si atoms would. Now a denser form of Mg\(_2\)GeO\(_4\) exists at ordinary pressures, its form being cubic. It is therefore plausible that at high pressures olivine may change over from a rhombic to a cubic form with a sudden increase of density, and that this is the explanation of the 20\(^\circ\) discontinuity.

The pressure at a depth of 480 km.* has not yet been imitated in the laboratory (Bridgman has attained that at a depth of about 150 km.), but a striking confirmation is found by comparison with the Moon (Jeffreys, 1937 b). If the material between the 20\(^\circ\) discontinuity and the core is a new material it occupies about 80 per cent of the volume, and we should expect it to compose also most of the Moon. But the Moon's mean density is only 3.334 ± 0.003 gm./cm\(^3\), prac-

* 16 \times 10^{10} \text{ dynes/cm}^2
tically that of ordinary olivine. If it had the composition of the Earth's shell, on the hypothesis of a new material, the density would be about 3.8 gm./cm³. Consequently this hypothesis meets with formidable difficulties if we are to suppose any sort of analogy between the Earth and the Moon. On the other hand the pressure at a depth of 480 km. in the Earth is not reached at all in the Moon and, if the 20° discontinuity is a pressure change, the corresponding increase of density will not be present in the Moon, and the mean density of the Moon is explained without any further hypothesis. This check strongly confirms Bernal's suggestion that the material below the 20° discontinuity is cubic olivine. Further, if the Moon is uniform in composition (apart from a slight correction for the outer layers) we can calculate what the density of the main material would be at zero pressure; it would be 3.29 gm./cm³, in perfect agreement with the standard specimen of dunite used by Adams and Gibson.

The material is presumably not pure, so that there would be no ground for surprise if the discontinuity is smudged out to some extent; the evidence that it is a rapid transition rather than a sharp one does not affect the main argument. Bullen has taken the matter considerably further in later papers (1942a, 42b).

The inner core remains a problem. Its volume is so small that any reasonable density for it would hardly affect the rest of the results.

From analogy with meteorites, Goldschmidt had inferred that a layer of metallic sulphides should surround the core. This would be liquid and would probably be immiscible with liquid metal. Consequently there should be independent reflexions at its top and bottom and ScS should be double. This is not so. I also found serious difficulties in fitting the times of K at short distances on this hypothesis (1936). It appears that if a sulphide layer exists it cannot be much more than a tarnish.

§ 12. OUTSTANDING PROBLEMS

It became clear at an early stage of the revision of the tables that the chief need if any substantial progress was to be made was for better methods of combining observations than had hitherto been used. Consequently the seismological work has been carried on simultaneously with investigations into the principles of scientific method and the development of probability theory (Jeffreys, 1939). Considerable advances had been made within the present century, especially by Karl Pearson and R. A. Fisher, mostly for biological purposes, but seismology was found to present a number of problems that had not been treated (not counting the treatment of serious departure from the normal law of error, the principles of which had been given by Fisher). The most frequent of these was perhaps that of smoothing an empirical table when the true values were not expected to agree with any pre-assigned mathematical function; the observed values would necessarily show some irregularity on account of observational error, and the question was how far this could be smoothed out without at the same time smoothing out some genuine irregularity. The solution of the problem should have much more general application. The principles of significance tests have had to be considerably extended because, whereas biologists have usually been able to design their experiments so as to
make a comparatively simple analysis of the results adequate, such design is impossible in seismology, and approximations have had to be developed. These may be expected to be useful in such matters as the revision of the fundamental constants of physics. Much more, however, remains to be done.

It was found that many of the differences that had been found previously disappeared on statistical examination, being found to be reasonably attributable to known random errors. Some remained; some of these have been explained, but not all. The Z phenomenon, an apparent variation in the times of S and SKS relative to P when different earthquakes were compared, gave some trouble at the start, but most of it is now explained. The correction for ellipticity accounted for several instances of it, and multiplicity for many others. Turner had found a large number of earthquakes in which the S-P interval for given distance was larger than in the normal earthquake, and attributed these to foci higher than normal. This led to the conclusion that the normal earthquake has a focal depth of at least 100 km.; but this was contradicted by the Montana earthquake, which certainly had an upper-layer focus and gave transmission times agreeing with Turner’s average. The phenomenon was ultimately explained by Stoneley (1939). Some earthquakes are double or triple, the original one being followed by one or more larger ones after a few seconds to several minutes. An observer will naturally read the P of the first shock, since this is the first displacement on his record. But the most conspicuous S will be one of the later ones, so that the interval between the shocks is included in the observed interval between P and S. In practice many observers read two or more P’s and two or more S’s, and on comparison it is found that the readings of P and S can each be analysed into two series, separated by intervals independent of distance. There are, however, more readings of the first P and the second S, and if attention is confined to these, the phenomenon found by Turner will arise. Tillotson, by study of the additional readings reported in the I.S.S., showed that they could be explained in accordance with Stoneley’s suggestion. This accounted for the other cases of positive Z that were known. A few cases of negative Z were probably due to focal depth, the near stations having been too few to enable it to be detected from P alone.

Even after the bulk of the Z variation had been explained in these ways, it was found that a little survived in deep-focus earthquakes. By a suitable choice of epicentre and focal depth, it was possible to fit the times of P at all distances, pP and sS, S at short distances and SKS within their apparent uncertainties; but the mean residual of S from 20° to 90° fluctuated from earthquake to earthquake as if it had a standard error of about 1.5 superposed on the random error. No explanation has been suggested of how such a variation, affecting only part of the range of one phase, could arise. There is a similar fluctuation in the difference of the constant terms in the times of the near earthquake phases (Jeffreys, 1937 d); for this several explanations have been suggested, but none appears satisfactory. There are small but apparently significant differences in the times of P for earthquakes in different regions, the largest being in North America where they are about 3 s longer about 20°, in comparison with short distances, than in Europe and Japan (Jeffreys, 1940). It would hardly have been expected that the Earth’s materials would have exactly the same properties everywhere; it is rather remarkable that they are as uniform as they are.
The greatest outstanding difficulty is one that possibly escapes attention because it is so familiar; it is nothing less than the general appearance of the seismogram itself. According to Lamb's theory (1904) for a uniform crust, $P$, $S$ and the Rayleigh waves from a sudden shock would all consist of single swings. The actual movement is a continual irregular oscillation. We know why the Rayleigh waves and surface waves of Love's type show oscillation; it is due to dispersion. But no cause of dispersion has been suggested that is capable of converting a sudden $P$ or $S$ into a long train (Jeffreys, 1931a, 31c). The most hopeful at one time appeared to be internal reflexion in the upper layers, but this failed when reflexions at their interfaces were sought, but not found, in deep-focus earthquakes. If the original disturbance at the focus was oscillatory it would explain oscillations in $P$ and $S$; but the duration of the oscillation would be the same at all distances, whereas it increases with distance. This applies, in particular, to the suggestion, sometimes made, that the oscillation is due to non-linearity in the stress-strain relations near the focus.

A possibly related fact is that oscillation of the ground in a near earthquake continues long after the time that the slowest surface wave should take to arrive.

There is evidence that all seismic waves show damping as they advance. This is required in particular to explain why the upper and intermediate-layer phases become too small to be read at distances of 8° at most. The most likely explanation is scattering (Jeffreys, 1937d, p. 220). If a layer is not quite uniform the rays will be bent irregularly and a sharp wave-front will be blunted. It is found that the time of most rapid increase of displacement will still be proportional to the distance, and its velocity will represent the average velocity in the material. There will be no sharp beginning, but a small diffuse movement will precede the main shock; and it will be the main shock that obeys the fundamental rules of transmission. This seems to explain a curious anomaly found in near-earthquake studies. When a cubic formula is fitted to the times of $P$ up to 20°, it gives $dt/d\Delta$ at short distances corresponding to a velocity of about 7.8 km./sec., which agrees with the value derived from most near-earthquake observations up to about 1930. Since then many studies have given velocities of 8.0 to 8.2 km./sec., which would not agree with the times up to 20°. Conrad, in his study of the Schwadorf earthquake, read two separate $P$'s at the beginning of the records of some stations, the faster giving $8.10 \pm 0.09$ km./sec., but it was feeble, and not read at the majority of the stations; while the later and larger one corresponded to a velocity of $7.85 \pm 0.04$ km./sec. The natural interpretation is that the second is the main pulse and the earlier a precursor due to scattering, and recorded only on instruments with very high magnifications. If so these are not an unmixed blessing unless their user is aware of the danger.

Another peculiarity is in the amplitudes at short distances. These decrease with distance, very roughly like $1/\Delta$, but definitely not so fast as $1/\Delta^2$. For smooth interfaces, theory indicates that the amplitude of $P_g$ should decrease like $1/\Delta^2$ and $P$ should increase slowly. There seems to be some possibility that theory and observation may be reconciled here if the interfaces are irregular, giving a diffusive effect like that of ground glass.

The travel time of any compound phase is stationary for small variations of the path, but not necessarily a minimum for all. It is easy to see that the time of
PP in a homogeneous sphere would be a maximum for variations of the point of reflection in the plane of the path. Attempts were made to derive empirical times for all predicted phases, but it was found that there was a qualitative difference between those whose times were true minima and those whose times were merely stationary. For the former, namely P, PKP, S, SKS, PcP, ScS, pP and sS, the distributions of residuals showed pronounced humps corresponding to a standard error of 1s to 3s, with the usual background effect. The latter, typified by PP, PS, SS, SKKS and SKSP, give distributions scattered over 20s or more with no pronounced concentration, and were finally abandoned; their published times are calculated, the observed times being scattered on both sides of the calculated ones. Several attempts were made to construct a theory of a pulse of this class, but the mathematical difficulties were very great, and finally I adopted a suggestion of Mr. F. G. Friedlander, who pointed out that in the expansion of a cylindrical pulse the disturbance from the rear of the cylinder has the same property. An analytical solution of this problem was known, and there was no great difficulty in approximating to it. The result was that the disturbance should be very large, but not sharp; it should begin a little before the theoretical time and subside gradually after it (Jeffreys, 1942a). Consequently we have at least a clue to, though perhaps not a complete explanation of, the unsatisfactory character of these phases.

The large scatter of the readings of S in normal earthquakes up to 20° is still unexplained; it is clear that the large movements read as S by most observers are not S, but we still do not know what they are.

§13. LOCAL EFFECTS OF EARTHQUAKES

The instrumental recording of earthquake waves helps appreciably towards understanding what goes on near the epicentre of a large earthquake. The original movement is a fracture, in many cases a slip on a known fault. The records show definitely that the motion produced is a vibration, beginning with Pg and followed by Sg, the interval at short distances being a few seconds. Sg in general is larger than Pg and, since the SH component is usually the larger, may be nearly at right angles to it. It follows that buildings that withstand Pg may be wrecked by Sg, and if so they will fall in a completely different direction. In some earthquakes movement from different directions has been detected from the fall of buildings, and it has been inferred that there were really two earthquakes in different places; actually the phenomenon is just what we should expect a single earthquake to produce.

At greater distances the change in the direction of the movement can be felt; I noticed it at Cambridge in the North Sea earthquake of 1931 June 7.

Observers have reported that they saw lights during an earthquake, and there is a frequently quoted report that a man saw the waves travelling across country "faster than he could walk, but not so fast as he could run" (cf. Heck, 1936; Davison, 1936.) The velocity of Sg is about 3·3 km./sec., and the train of waves following it dies down so that the tail travels roughly at 1·5 km./sec. This observer should clearly have entered for the Olympic Games. But what we do know is that an observer in such conditions is undergoing horizontal and vertical accelerations applied to his feet, and must be making muscular efforts to keep his
balance. It would be a problem for an experimental psychologist to say whether in these conditions he would be likely to imagine that he sees a wave-like motion of the ground or to "see stars". A characteristic "earthquake noise" is also reported, somewhat resembling thunder, and the nature of this also needs investigation.

Pillars have often been found to have rotated during an earthquake. This has been attributed to rotation of the ground, but the prevailing wave-lengths are of the order of some kilometres. It is more likely that the rotation is due to the point of support, when detachment is imminent, not being vertically below the centre of mass.

It appears that an acceleration of 0.1g is enough to produce almost complete destruction of buildings. Several non-quantitative scales of intensity have been produced, and are useful in the approximate location of epicentres; they have been calibrated quantitatively by Sieberg (1932), and a scale for estimating the total energy of an earthquake from instrumental data is given by Richter (1935).

The energy liberated by a major earthquake (one recorded at distances of 80° or more) is of the order of $10^{21}$ ergs. The small Hereford earthquake of 1926 liberated $5 \times 10^{16}$ ergs and the Jersey earthquake of the same year $10^{19}$ ergs. The total displacement of the ground during a large earthquake may reach several metres; this may really be accumulated during several fractures at intervals over a few minutes.

A large earthquake is often preceded by foreshocks, and is nearly always followed by a series of aftershocks, diminishing in frequency and intensity. The former are probably slips on a small part of the fault-face where adhesion is relatively weak, the main shock coming when the stresses have grown sufficiently to produce general fracture. The aftershocks are readjustments following the redistribution of the general stress made by the main shock. There is good reason to suppose that the huge displacements shown in major faults (which may reach thousands of feet) are the result of many successive earthquakes at considerable intervals of time (Jeffreys, 1942b).

The disturbance felt in deep-focus earthquakes is more widely distributed than in shallow ones and less intense at the epicentre, for the same total energy, and this property often provides the first indication of focal depth.

The existence of deep foci is a serious difficulty for any of the hypotheses that require continuous flow at great depths to play a constructive part in geological phenomena. The tendency of continuous flow is always to reduce the local stress-differences; it therefore makes it more difficult to accumulate the large stresses shown to exist by deep earthquakes. It is possible that continuous flow does occur; but if it does, any discontinuous changes are in spite of it and not because of it. I have always preferred to work on the hypothesis that the materials behave as perfectly elastic until the stress-difference reaches a definite non-zero value, and then break suddenly. If this hypothesis cannot explain the facts, then, a fortiori, no hypothesis involving continuous flow can do so.


§14. SEISMIC SURVEY

The methods of the study of near earthquakes have been extensively applied to the investigation of the sedimentary layers. The stimulus probably came from the Oppau explosion of 1921, which was well recorded by seismographs to a distance of about 350 km. The waves are generated by firing a charge of high explosive, which must be buried to prevent nearly all the energy from going into an air-wave. If this is done there is hardly any change in the form of the surface afterwards. The position of the focus is known, and the time of the explosion can be recorded directly on the seismograms, so that the time of transit of each pulse is measured directly; thus one of the chief difficulties of the study of natural earthquakes, the need to determine the epicentre and time of origin from the times of arrival themselves, does not arise. Further, the only way of making sure of getting a record of the natural earthquake is to keep the seismograph running all the time; to do this, and keep the apparatus manageable in size, limits the speed of the paper to about 1 mm./sec., and usually much less. But in recording an artificial earthquake we are free to start the drum whenever we like, and consequently can run it much faster. It is therefore as easy to read the records to 0.001 as to 1 in a natural earthquake, and it becomes possible to determine not only the mean depths of layers but variations about the mean. On the other hand the energy available is small compared with that in a natural earthquake. It would take several tons of explosive to match even the small Hereford earthquake, and the larger the charge used the more deeply it must be buried to prevent it from blowing out and wasting most of its energy in the air. It is probable that in some large quarry blasts Pg has been recorded, but the waves in the lower layer have never been.* In practice, therefore, the method is usually confined to observations up to distances of the order of 10 km. and used for the investigation of the rocks at comparatively small depths.

As the original disturbance is almost wholly outward, P movements in artificial earthquakes are much larger than S ones, contrary to what is found in natural earthquakes. S from explosions has hardly been convincingly recorded, though Rayleigh waves are produced, and their beginning looks rather like S.

The principle is that if two points are at distance $x$ apart, $z$ is the depth, $v$ the velocity of a pulse at the greatest depth reached, and $c$ the velocity at depth $z$, the time of transmission is

$$T = x/v + \int \left( \frac{1}{c^2} - \frac{1}{v^2} \right) |dz|,$$

accurate to quantities of the second order in the slope of any interface. The quantity under the integral sign has been called the delay-depth coefficient by A. W. Lee. If we denote it by $\eta$, and the depths of the first interface below the beginning and end of the path are $H_1$, $H_2$,

$$T = x/v + \eta(H_1 + H_2).$$

If several seismographs are arranged in a straight line, $v$ and $H_1$ remain constant, but $H_2$ varies if the interface is not parallel to the outer surface. If two explosions

* Since this was written I have found that the Burton-on-Trent explosion of 1944 Nov. 27 gave a P, recorded at Puy de Dôme, and Sg was well recorded.
are made at distance \( l \) apart, at places where the depths are \( H_0, H_0' \), and the seismographs are placed along the straight line connecting them at distances \( x_1, x_2 \ldots \) from the first, the time at \( x_n \) for the first explosion will be
\[
T_n = \frac{x_n}{v} + \eta (H_0 + H_n)
\]
and for the second
\[
T'_n = \frac{l - x_n}{v} + \eta (H_0' + H_n).
\]

The difference of these is independent of \( H_n \), and if times are known for several values of \( x_n \) we can find \( v \); then \( \eta \) is calculated, and the sums \( T_n + T'_n \) vary only on account of the variation of \( H_n \), which can therefore be found (Jeffreys, 1935 b).

A direct wave in the upper layer of sediments is sometimes unobservable over a long distance if that layer is chalk or clay; damping is strong unless the rock is hard or under considerable pressure. It is usual to start from a place where the stratum to be mapped either appears at the surface or has its depth found from a boring (and without such a check the geological interpretation will remain uncertain even if the form is known), but, given a starting point, the method can be used to follow the top of a given stratum for considerable distances.

Extensions are obviously possible to find the forms of several superposed layers.

The method has been much used in oil prospecting, especially in the United States and Persia, since the oil collects in the anticlines in a limestone or a salt dome. The highest point in the oil-bearing layer is located and provides the most promising place for a boring.

The most thorough published description of the method is by Bullard, Gaskell, Harland and Kerr Grant (1940), who used it for following the top of the Palaeozoic rocks through Eastern England; several Mesozoic strata were followed at the same time, and the results were checked by the existing borings. The agreement at these points was satisfactory, and the result is a substantial contribution to geology. Another application is to the investigation of the structure of the continental shelf; this was begun in the Eastern United States by M. Ewing (1937), and applied to the mouth of the English Channel by Bullard and Gaskell (1941). This work was unfortunately interrupted by the outbreak of war, but the preliminary results look extremely promising.

Reflxions have also been used, especially in California.

§ 15. ACKNOWLEDGMENT

By the kind permission of the Council of the Royal Astronomical Society, I have included in this Report a considerable part of the Report on Seismological Tables from the Annual Report of that Society for 1939.

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