The Cause and Character of Earthquakes.

The study of earthquakes having always been recognized as one of the departments of Geology, no excuse is needed for devoting my address, on this occasion, to a review of the present state of our knowledge of the cause and character of earthquakes, using that word in the restricted, and original, sense of the disturbance of the ground which is sensible to human feelings, which causes alarm and destruction, and is properly that seism of the ancient Greeks, from which our modern term s e i s m o l o g y is derived. This explanation is necessary, for, of late years, seismology has been extended to the study of a phenomenon of different character, the long-distance records of disturbances, only to be detected by very sensitive instruments of special construction; in some cases these are clearly connected with great earthquakes—as the word is here used—and by inference have been presumed to be so in all cases, even when there is no independent evidence of the earthquake proper. The records, regarded as records of the progressive enfeeblement of the larger disturbance of the true earthquake, would represent the cryptoseism, or unfelt earthquake, and be correctly described in the observatory records as earthquakes. That they are correctly so described is indisputable, if the word is taken in its literal interpretation as a quaking, however feeble, of the earth; but, if the implication is added that they have the same origin as the greater disturbance, the correctness of the description becomes doubtful. In presenting to you, some dozen years ago, the results of a study of the records of the Californian earthquake of 1906, I pointed out that, although the immediate origin of the earthquake proper might be traced to occurrences which took place in the outermost parts of the Earth's crust, these were but the secondary result of a deep-seated origin, or bathyseism, which gave rise, at the same time, to the disturbance which was recorded at long distances by suitable instruments. Later work and research has more and more confirmed both the correctness of this interpretation and the conclusion that the proximate cause of great and destructive earthquakes is distinct from that of the long-distance records, although the two origins are connected with each other as effect and cause.

In the present state of our ignorance of the nature of the bathyseism, it is difficult to give a clear and precise definition of the connexion between it and the earthquake proper: the subject is an interesting one, and a review of the evidence, together with the deductions which can be drawn from it, would fill the time available; but it is not my intention to do more than to attempt, by analogy, to illustrate and explain the nature of the connexion of the bathyseism with its two independent results.

Not many years have passed since, in the south-eastern corner of England, we heard what were known as the guns of Flanders; and the description was correct. The sound—it was more a sensation than a sound—which was heard in Kent and Sussex was undoubtedly produced by the report of great guns, by the explosion, that is, of the charge in the gun itself; but, had the explosion done no more than give rise to the sound-waves which travelled far in every direction, it would have little troubled the enemy. Simultaneously, however, with the production of the report, and by the same explosion, a projectile was sent flying through the air, and, after a trajectory of some miles, itself exploded, causing the damage which was the purpose of its despatch. The effect of this second explosion was severe but local, and at a short distance away neither sound nor shock was sensible.

Here we have a very complete analogy, the explosion of the gun represents the bathyseism; the report and sound-waves, travelling afar, correspond to the disturbance which, propagated through the substance of the Earth, gives rise to the long-distance records; the explosion of the shell to those dislocations in the outer crust which produce the destructive earthquake; and the trajectory to the connexion, of which the character is as yet unknown, between the bathyseism and the surface-shock.

If this interpretation be accepted, it becomes evident that the distant records represent something which is distinct from the earthquake, as originally understood, and that the study of them, with the deductions drawn from that study, have little or no bearing on the problems of geology, as we usually limit the scope of that science. It is otherwise with the earthquake proper; originating in, and affecting, the outermost crust of the Earth, it has long, and rightly, been regarded as one of the departments of geology, both as regards cause and character, and it is with this aspect of the subject alone that I shall deal.

The character of earthquakes is known to an extent sufficient for my purpose; they are elastic waves, transmitted through the

substance of the Earth, not, as was once supposed, merely waves of elastic compression, but of most complicated character, and in all but a small minority of cases nothing but this vibratory movement, the orchesis, can be recognized. Occasionally, however, and only in the case of some earthquakes of destructive violence, there is also a bodily and permanent displacement of the solid ground, and this mass, or molar, movement has been distinguished as the mochleusis of the earthquake, as distinct from the elastic displacement, accompanied by return to the original position, which constitutes the orchesis. Now, the elastic waves can only be initiated by some sudden impulse or disturbance, such as might be produced by the fracture of rock; and as, in those earthquakes where mochleusis can be recognized, there is usually evidence of sudden movement along some pre-existent fault-plane, or of rending and fissuring of the solid rock, faulting or fracturing has come to be regarded as the cause from which the vibratory disturbance originates.

This conclusion is supported by the fact that the proximate origin of the shock can almost always be placed at a moderate depth from the surface. It is, unfortunately, impossible to give any precise figures, for none of the methods which have been suggested for determining the depth of the origin can be trusted, some because they depend on assumptions which the progress of knowledge has shown to be erroneous, others because they demand data which cannot be supplied with the requisite precision, if at all; but there is another way in which some idea of the depth of origin may be reached, based on the fact that there is almost invariably a well-defined area of maximum intensity of shock, surrounded by regions of diminishing intensity, as the distance from the central area increases. Since the violence of the disturbance will decrease with the increase of distance from the origin, it follows that the nearer the origin lies to the surface, the more closely does the variation of surface-distance from the epicentre approximate to the variation in actual distance from the origin; hence it is evident that the rate of variation of intensity of the disturbance will give some notion of the depth of the origin. In this way, quite apart from any numerical estimates which have been made, it becomes clear that, excluding a small minority of earthquakes which will be referred to later, the origin lies at a very moderate depth below the surface, probably seldom over 10 miles, and usually less. This places the origin within the limits of be a certain increment which will lead to fracture, earthquake, and partial relief; then with a further increment the process will be repeated, and so we reach the concept of a mean strain-interval for each shock, which may be regarded as constant, on the average, for any given region, provided that the average is taken over long enough a period. If, then, we divide the mean increment of strain in a unit period of time by the mean number of shocks occurring in the same period, we obtain a fraction which represents the mean stress-interval for each shock, a fraction which should remain constant in the region under consideration; and from this it results that any variation in the rate of growth of strain must be accompanied by a corresponding variation in the frequency of earthquakes. We have, then, four quantities so inter-related with each other that, if three of them were known, the fourth can be determined. Two of these, namely, the mean frequency and the variation from that mean in any chosen period, can be obtained from observation; and, if the variation from the mean rate of growth of strain is also known for the selected portion of the whole period, that mean rate, which is the object of search can be obtained by a simple rule-of-three sum.[1]

The frequency of earthquakes is known to be subject to great variation from time to time, and this variation indicates a corresponding change in the rate of growth of strain. In the main, this change may, and probably must, be attributed to causes acting within the Earth, and directly related to the changes or processes by which the strain is produced; but it is evident that, if there were any external cause, which acted periodically and alternately in increase and decrease of the rate of growth of strain, and if it were possible to disentangle the variations due to this from those due to other causes, we should possess the means of framing a numerical estimate of the general rate of growth of strain.

One such cause of periodic variation is to be found in the tide-producing stresses set up by the sun and the moon. It is true that many attempts have been made at different times to detect some connexion between the frequency of earthquakes and the position of the moon, and that no such connexion has yet been established, but these attempts have all been based on very imperfect records. In time it may, perhaps, be possible to apply to an earthquake record that method of harmonic analysis which has proved so fertile in the case of the ocean tides, but the day is long distant when a record of sufficient completeness will be available. Meanwhile, there are some simpler relations, of which a discussion is feasible, and the most promising of these seems to depend on the fact that the downward pressure is greatest at the time when the attracting body is on the horizon, and least when it is on the meridian. If, then, we divide an earthquake record into two groups, one containing all shocks which occur within six hours before a meridian passage, and the other all that happened within six hours after, the first of the two groups will cover a period during which the downward pressure is, on the average, increasing, while the other will cover a period during which it is decreasing. As the amount of the change so introduced is known, with sufficient accuracy for the present purpose, and as it must, on the hypothesis being used, influence the frequency of earthquakes, it follows that we have here a method which should enable us to make an estimate of the rate of growth of the strain to which fracture is due.

Although simple in principle, the method is difficult in application. To begin with, a record is required, of sufficient extent and continuity to give a trustworthy average, not merely of the general frequency, but also of the frequency in each of the two sections into which it is divided; and this means that the record must contain at least two thousand shocks and ought to contain double that number or more. Then it must be reasonably accurate as to times and complete as to occurrences, or at least must be fairly uniform in its incompleteness over the whole period investigated. There are not many records which fulfil these primary requirements, but there is another even more important. In all records there is a noticeable variation in frequency at different times of the day; moreover, the nature of this diurnal variation has been found to vary in different regions, but appears to be constant and characteristic in each region over the period of record. The cause of this periodicity may be reasonably attributed to some effect, meteorological or other, connected with the daily course of the sun; but its nature, no less than its variability,

[1] The argument may be put in a different form, simpler and more easily intelligible to some. If S represent the mean increment of strain in a given period and N the mean number of earthquakes recorded in the same period, then S/N is the fraction representing the mean strain-interval corresponding to an earthquake. If the variation of the growth of strain in any particular period is represented by v and that of the number of earthquakes by d, we get the equation

$$S \div N = (S \pm v) \div (N \pm d)$$

where

$$d = v/S$$

whence we obtain the simple ratio

$$d : N :: v : S.$$

shows that it cannot be attributed to gravitational attraction. It is only, therefore, by a conversion of the record from solar to lunar times that the influences of these other effects can be eliminated, and the gravitational attraction of the moon be detected and estimated, and, for the satisfactory application of this method, it is necessary that the record should cover a complete lunar cycle, or a period of nineteen years. There are only two records extant, and available, which fulfil this requirement, and of these the Italian is not only the most complete and accurate, but is the only one to which the conversion into lunar times has been applied.

A summary of the figures obtained has been published in our Quarterly Journal,[1] and from this we find that in the six hours preceding a meridian passage there were 3337 shocks, and in the six hours succeeding only 3270, giving a mean departure, from the general average for six lunar hours, of 33·5, or almost exactly 1 per cent. of the mean. So small a variation from perfect equality is well within the limit of what might reasonably be expected, if it were purely fortuitous and the stresses set up by the attraction of the moon had no effect whatever. This point will be returned to later on, but it will be useful to see what conclusions may be drawn if the variation is accepted as real, and due to the cause under consideration. The first of these is that the main stress, to which the strain is due, is of a compressive nature, consequent on an increase of downward pressure, or a removal of support from below. The second is that the vertical component of the general increase of strain amounts to just 100 times the variation of the corresponding component of the gravitational stress set up by the moon. It has been established by mathematicians that the maximum upward stress set up by the moon, at the points on the surface of the Earth where it is in the zenith or nadir, amounts to 1/8,450,000 of the Earth's force of gravity, and where it is on the horizon there is a downward stress of just one half of this: the total variation of downward pressure is, therefore, equivalent to a change of about 1/5,630,000 of that due to terrestrial gravitation, as between the points and times when the moon is on the horizon, or at the zenith or nadir. But the moon could never be directly overhead in any part of Italy, and a computation of the mean range of variation, over the whole period and the whole area concerned, reduces this fraction to about 1/9,400,000. As the

[1] Vol. lxxvii (1921) p. 2.

general increase of stress is 100 times this figure we get the result that the rate of increase of strain is equivalent to that which would be produced by an increase of downward pressure, or a corresponding reduction in the support of the crust, at the rate of 1/94,000 of that due to gravity, in each period of six hours.

It has long been established that the strength of the crust is far from being able to withstand the crushing strains which would be set up by removal of support from below, and the estimates, which have been independently made by different investigators, concur in putting the limit of the removal of support, which would result in crushing, where an area comparable with Italy is concerned, at not more than about 1/400 of the force of gravity. If this fraction is divided by that obtained in the previous paragraph we get the result that, starting from a condition of no strain, fracture would come about after an interval of 235 periods of six hours, or not quite 59 days. The calculation, therefore, indicates that the rate of growth of strain in Italy has been, on the average, such that the breaking point would be reached in about two months from start, with a wide variation on either side. Some other relations between the frequency of earthquakes and the diurnal variation of the tidal stresses might be, and have been, investigated; none of them seem so appropriate as that which has been detailed, and all give fairly confirmatory results, the longest period indicated, as required for reaching the breaking strain, being just about a year.

It must not be supposed that value can be attached to the precise figures. As is invariably the case, in all calculations regarding physics of the Earth, many considerations are involved of a very uncertain nature; but the reasoning does show that the increase of strain must have taken place at such a rate that the breaking point was reached in a period measurable at most by months. They prove conclusively that the period could not have been of such length as to be measurable by years or decades, for, had this been the case, the disparity dealt with would have been much greater than that actually found.

The same conclusion may be reached in another way. The stress-difference required to produce fracture in average hard rocks, as they are met with at the surface, is round about 1,000,000 grammes per centimetre square, and, allowing for the greater strength at depth which is indicated by the experiments of Prof. Adams and the computations of Prof. Burrell, we may put the breaking strength of the Earth's crust at about double of this, so that, in order to reach this point in one year from starting, the strain would have to increase at the rate of about 1400 grammes per centimetre square in each quarter of a day. According to the late Sir George Darwin the stress-differences set up by the moon in the latitude of Italy would amount to about 20 grammes per centimetre square in an incompressible Earth, and in a compressible Earth with an incompressible crust—a condition much more akin to what we have reason to suppose is the reality—the stress-differences would be many times this figure [1]; but even the lower amount is nearly $1\frac{1}{2}$ per cent. of the growth required to reach breaking point in one year, it would be close on 15 per cent. if the period is increased to ten years, and, with anything approaching this proportion, a periodicity would result, which could not have escaped detection before now.

The figures, therefore, give us a lower limit of the rate of growth of strain, it must have been something faster than that needed to reach the breaking point in one year from starting, if the differences on which the argument is based are real. But are they real? The actual amount of difference, barely 1 per cent. of the mean, is so small that it may well be fortuitous, and the true interpretation may be that the gravitational stresses, and the stress-differences produced by them, have no effect whatever in determining the time of occurrence of an earthquake. If this be so, then the rate of growth of strain becomes infinite, and each earthquake becomes the result of a rapid development of strain, akin to an explosion in its suddenness.

The truth may lie anywhere and must lie somewhere between these extremes, and so we reach the conclusion that there is no support for the commonly-accepted notion of a continuous, slow growth of strain, extending over years, decades, or even centuries, before the breaking point is reached. On the contrary, it appears that the cause of earthquakes is a r a p i d growth of strain. This strain cannot be developed without some deformation, but the magnitude of this has no relation to the frequency or magnitude of the earthquake; if change of form is slow and prolonged, relief may be provided by gradual yielding, if rapid, a very small amount of distortion may lead to fracture, and on the extent, form, and position of this fracture will depend the character of the resulting earthquake.

This study of the growth of strain leads on to the question, which is the really important one in its bearing on geology, of how the strain is produced. It can hardly be the result of those tectonic processes which result in folding, for these must necessarily be slow in their action; the change of form involved in the bending of solid rock from its original shape into complicated folds, without breach of continuity, can only have been a slow one, and, as we have seen, the deformation which produces earthquakes must be a rapid one. With faults the case is different; many earthquakes are known to have been accompanied by movement along pre-existing fault-planes, in others the origin evidently agrees in position with known faults, and in all of these the distribution of the intensity of disturbance is closely correlated with the faults, being greatest in proximity to them and decreasing as the distance becomes greater. So much is indisputable, yet, despite a general acceptance of the explanation, that the earthquake was a result of the same process as that which gave rise to the formation of the fault, it must be recognized that the proof is not logically complete, for it might be that the cause and processes which gave rise to the earthquake were wholly different from, and independent of, that which produced the fault, the only connexion being that the weakness, resulting from the fault-fracture, served to localize the yielding, and so controlled the distribution and intensity of the earthquake. In a study of the Californian earthquake of 1906, where the greatest intensity of disturbance ranged along the line of the San Andreas fault, and was accompanied by considerable displacement and distortion of the surface along the fault-line, I was able to show that the ultimate cause of the earthquake was quite distinct from that which produced the fault, and that the fault was not the cause of the earthquake, nor the earthquake an incident in the formation of the fault.

In support of the supposition that earthquakes are not produced by, or at any rate are not necessarily the product of, the tectonic processes which have given rise to the displacements in faults, may be instanced the fact that in some cases of minor earthquakes, where it has been possible to fix the position of the epicentre with a close approach to definiteness, it has been found that surface examination gives no indication of the presence of a fault. This, however, is not conclusive, for there might have been a deep-seated, incipient, fault which had not yet extended to the surface, and so could not be recognized by geological survey.

Much more weighty and suggestive evidence is to be derived

[1] Sir G. H. Darwin, Scientific Papers, vol. ii (1908) p. 502; and Phil. Trans.

from the great earthquakes which have been studied in detail. Reference has already been made to the Californian earthquake of 1906, and the conclusion drawn in that case is more fully exemplified by the Indian earthquake of 1897. Here there was no single leading fault and zone of maximum intensity of shock, but a complicated network of lines of extreme destructiveness, ramifying over an area not much different from that of England, and extending right across a series of great tectonic features, across the great monocline of the southern face of the Assam range, across that range itself, across the alluvial plain of the Brahmaputra Valley, the great boundary-faults of the Himalayas, and probably even across the main axis of elevation of the range.

A still more instructive instance is the Charleston earthquake of 1886. Here, in a region as devoid of any great structural feature, either of folding or faulting, and as little subject to earthquakes, as could be found in our own country, there suddenly occurred a great earthquake, of destructive violence in the central area and felt over an area measuring about 1500 miles across. It was an earthquake of first-class magnitude, whether we regard the maximum violence of shock or the extent of area affected, yet there is nothing in the structure of the surface-rocks to suggest that its origin was due to any tectonic process, and equally nothing which could lead to its classification as volcanic; and, if we accept the conclusions of Col. Harböe, regarding the character and extent of earthquake origins, the absence of any connexion, between the origin of the earthquake and the tectonics of the surface-rocks, becomes absolute, for, according to this interpretation, the origin becomes almost co-extensive with the seismic area, and the diminution of violence in the outer portions is not solely due to enfeeblement, resulting from the elastic propagation of the earthquake wave, but very largely to a diminution in magnitude of the originating impulse.

Whether this explanation be accepted or not, it must be conceded that, as regards the two earthquakes particularly referred to, of Charleston in 1886 and India in 1897, Col. Harböe's conclusions are not only supported by the particular facts on which they were based, but are in better accord with a number of peculiarities in the local variation of violence of the shock, and of other phenomena recorded, than is the current notion of a central focus of comparatively restricted dimensions. It accords also with those great earthquakes which, like the Calabrian earthquakes of the present century, had more than one centre of maximum intensity, connected by regions of less violence of shock.

These are some of the considerations which have led me to believe that Col. Harböe's interpretation is, in the main, well founded, and if it be true that earthquakes of great extent are due to systems of fracture, or analogous disturbance, ramifying over, and practically co-extensive with, the seismic areas of the earthquakes, that is, over areas of which the dimensions in any direction may be measured in hundreds of miles, it becomes more than ever necessary to recognize that the earthquake origins cannot be the result of processes and displacements, recorded, and indicated, by the tectonics of the surface-rocks. The real and ultimate origin must be more deep-seated, and involve either a displacement of, or a change of volume in, the material underlying the outer crust.

This is no occasion to enter into detail, and so I have merely indicated the general character of the studies which have gradually forced me to the conclusion that great earthquakes, and also to a great extent those lesser ones which are commonly classed as tectonic, do not owe their origin to the tectonics of the outer crust, but to processes and changes which take place in the material below it. What these processes may be we cannot know, with the certainty which comes from direct observation, for such knowledge as we think we have comes from inference, deduction, and, to some extent, simple assumption; but suggestions have been made which possess a considerable degree of probability. Among these, and especially apposite to present considerations, may be placed Dr. L. L. Fermor's studies of the changes in mineral aggregation which may take place in the solidification of a magma; he has suggested that the determining factor in deciding the form in which the rock finally solidifies, is the inter-relation of pressure and temperature, and has shown that the change of volume, consequent on the change from one mode to another, may amount to over 20 per cent. in extreme cases. Mr. W. H. Goodchild has also studied the subject from another point of view, and suggested that some of the changes, especially the separation of metallic sulphides, take place with great, even explosive, rapidity.

I may point out that we have, within our common everyday experience, familiar analogies to those changes which are presumed to take place in the material below the outer crust. Every time that we fire a gun, the impact of the hammer starts a change, by which the chemical elements, forming the material of the charge, pass from one mode of combination to another in which they occupy a vastly greater space, and in so doing give rise to the pressure by which the projectile, whether ball or shot, is propelled.

The familiar lecture experiments of supersaturated solutions, which remain liquid until some disturbance, or the introduction of foreign matter, causes a rapid solidification, accompanied by a change of bulk, offer another analogy; and a third group of possible changes is represented by those allotropic alterations with which we are familiar, of which the alteration of aragonite into calcite may be quoted as an example.

It is not improbable that, in the material beneath the outer crust, changes, more or less analogous to one or other of these types, are taking place, some slow and gradual, others more rapid and sudden, but all accompanied by a greater or less change of bulk, either of increase or decrease; and, if this be accepted, we find an explanation, not only of the forms and origin of earthquakes, but of many other phenomena, which are difficult of explanation on any hypothesis of contraction and compression alone. On the one hand, slow movements of elevation such as that of the northern Scandinavian region may be attributed to slow and gradual change involving the whole bulk of large masses, the lesser earthquakes may be due to more rapid changes in smaller portions, the greater to transformations involving a larger bulk of material, and possibly a more abrupt change of combination and density; while the greatest earthquakes, of first-class magnitude, result from similar changes involving a large bulk of material, the difference between the origin of small and great earthquakes being analogous to the difference in the effect of the explosion in a shot-gun, and that of the Vimy mines, or the recent havoc at Oppau.

To elaborate these considerations forms no part of my purpose; enough has been said to show that, even in our very fragmentary knowledge of what goes on within the substance of the Earth, we have means of explaining and interpreting the greater part of the facts known to us regarding the character of earthquakes. I shall, therefore, end my address by summing up the conclusions which have been put forward, as to origin and cause. These are, first, that earthquakes are not due to any slow acting process of secular duration, but to a rapid, possibly instantaneous, development of strain, a conclusion which I believe to be true of the greater part, at least, of those earthquakes usually classed as tectonic, and of all those of great magnitude; and, secondly, that the development of strain is not the result of processes which have produced the tectonic structures recognized by surface observation, but to changes and displacements in the matter which lies below the cooled and solid outer crust.

February 22nd, 1922.

Prof. A. C. Seward, Sc.D., F.R.S., President, in the Chair.

Irene Helen Lowe, M.Sc., Egremont, Manorgate Road, Kingston Hill (Surrey), and Trevor Hughes Stonehouse, Lawnswood House, Hill Grove Crescent, Kidderminster, were elected Fellows of the Society.

The List of Donations to the Library was read.

The following communications were read:—

1. 'Description of a New Plesiosaur from the Weald Clay of Berwick (Sussex).' By Charles William Andrews, B.A., D.Sc., F.R.S., F.G.S.

2. 'The Carboniferous Rocks of the Deer-Lake District of Newfoundland.' By Thomas Landell-Mills, F.G.S., Arthur Smith Woodward, LL.D., F.R.S., Pres.L.S., F.G.S., and Albert Gilligan, D.Sc., B.Sc., F.G.S.

Specimens, microscope-slides, lantern-slides, and maps were exhibited in illustration of the above-mentioned papers.

March 8th, 1922.

Mr. R. D. Oldham, F.R.S., Vice-President, in the Chair.

Cecil Stevenson Garnett, 25 Crompton Street, Derby; George Johnston, Keloil, Kelham, Newark (Nottinghamshire); William Russ, B.Sc., Assistant Geologist, Geological Survey of Northern Nigeria; and Cecil Edgar Tilley, B.Sc., A.I.C., Emmanuel College, Cambridge, were elected Fellows of the Society.

The List of Donations to the Library was read.

Dr. A. Smith Woodward described certain photographs (natural size) of *Desmostylus* teeth from the Lower Miocene Sandstone of Southern Vancouver Island (B.C.) exhibited by Ira E. Cornwall, F.G.S.

The exhibitor wrote that these *Desmostylus* teeth are slightly different from any found in either California or Japan, as they show a well-developed cingulum. They may be from an older species than *Desmostylus hesperus*, as recent research has shown that the formation in which they were found is at least Lower