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CONTENTS

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THE ELASTIC-REBOUND THEORY OF EARTHQUAKES*

BY
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Earthquake shocks have apparently occurred in all geologic ages, in the past as well as in the present. This is shown by the presence in the rocks of sandstone dykes, which have been explained as the filling up of earthquake cracks from above or below by loose sand, which afterwards became consolidated; and by the presence of faults, which is, in itself, evidence that shocks occurred when they were formed; for whenever there is a movement on a fault surface, it is always accompanied by an earthquake.

It is very easy to picture to ourselves the terrorizing effect of an earthquake shock to an uncivilized people; but the lack of records in prehistoric times, and even later among uncivilized people, accounts for the fact that the history of many serious and disastrous earthquakes has been wholly lost. Indeed, with the exception of a few countries, such as Japan and China, it is only within the most recent times that a complete record of the heavy shocks occurring in civilized regions has been kept; and, without doubt, many serious shocks in less known parts of the world's surface, even within the last two or three hundred years, have entirely escaped our notice. At the present time fairly complete lists of even the weaker shocks are kept.

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* First of the Hitchcock Lectures delivered at the University of Cailfornia in the Spring of 1911. These lectures, with additions, will be published as a book.

Naturally enough great efforts were made to explain the causes of earthquakes. The very crude notions in ancient times, and among uncivilized people, have suggested the most extraordinary ideas. Among the semi-civilized and barbarous races we find the idea was prevalent that an animal of some kind existed below the ground whose movements caused earthquakes; the different races selected different animals, according to their tastes. In his interesting book on "Seismology," Professor Milne writes: "In Japan it was supposed that there existed beneath the ground a large earth-spider, or *jishin mushi*, which later in history became a cat-fish. . . . In Mongolia, the earth-shaker is a subterranean hog; in India, it is a mole; the Mussulmans picture it an elephant; in the Celebes there is a world-supporting hog; while in North America the subterranean creature is a tortoise." There were other futile attempts to explain earthquake phenomena during the period before the development of science. Science, as has been said, is merely systemized knowledge, and it is only by the use of scientific methods, that is, by a careful examination and comparison of the phenomena with known facts and principles, and by a systematic record of the results, that we can gradually approach, step by step, to a truer and more accurate knowledge of the facts.

It is hardly necessary for me to point out to this audience, the majority of whom were present at the time of the great earthquake of April 18, 1906, the general effects of a great earthquake and, moreover, they have been described in sufficient detail in several well-written books. I shall endeavor in these lectures to put before you, as clearly as possible, a conception of what actually takes place at the time of a tectonic earthquake, the nature and purposes of the instrumental observations at distant stations, the methods of study, the problems awaiting solution, and the revelations regarding the interior of the earth which earthquake study, up to the present time, has made.

Let me first, however, call your attention to the essential phenomena of earthquakes. It has long been recognized that earthquakes were due to a rapid to-and-fro motion of the earth, and that these vibrations were propagated from a center of disturbance. It has also been recognized for some time that these vibrations must be of the nature of waves, elastic and not gravitational waves; and the question which presents itself to us is: what produces these waves? Their origin lies, of course, in the region of greatest disturbance, but its exact position and the causes which produce the disturbance are not so easily discovered. Many theories have been advanced. The earlier ideas, suggested largely by the outbursts of volcanoes, were that the earth was a fluid mass surrounded by a thin liquid crust floating upon it, and that movements of the fluid interior caused earthquakes in the crust. These vague notions have been rendered somewhat more precise by Pilar, who, in his book on "Abyssodynamik" in 1881, supposed that the crust was broken into sections by cracks which were inclined and not vertical, and that those blocks which had a broader base and contracted upwards were raised, and the intervening blocks lowered, under the action of gravity and the pressure of the fluid interior, whenever the disturbance allowed this readjustment, and in this way earthquakes were produced. The idea that volcanic outbursts were always accompanied by earthquakes and that generally the regions of the earth where volcanoes were common were also regions of many earthquakes, led to the belief that these were but two phases of the same phenomena, and that earthquakes themselves were due to explosions in the fluid interior. The downfall of rock in caverns was, one hundred years or so ago, looked upon as the most important cause of earthquakes, but we shall see that this cause is insufficient to bring about strong earthquakes and indeed it has gradually received less and less support. The ideas that many earthquakes are independent of volcanic action and that they are due to movements of some kind in the crust of the earth became stronger, and in 1850 C. F. Naumann divided earthquakes into two classes, the *volcanic* and the *tectonic;* the first being due to volcanic explosions and the second to movements in the rock-mass. The importance of this latter cause has become more and more apparent, so that now we feel quite certain that all the really great earthquakes are of the tectonic class, and that earthquakes connected with volcanic outbursts are of comparatively little importance. It is always found that volcanic earthquakes, although they may be very violent in the immediate neighborhood of their origin, die out at a very short distance from it, and, indeed, this is one criterion actually used to determine in which class a particular shock should be put.

Researches in science are like explorations in an unknown country. The careful explorer maps his route and determines carefully his position with respect to known places from which he starts; he gradually maps the country he traverses, determining the positions of the mountain ranges, the courses of the rivers and so on. He may from some vantage ground obtain a glimpse of distant regions and of broad rivers, and he may make a fairly good guess as to what part these rivers play in the drainage of the country; but this is merely a guess, and he must proceed step by step, laying down his position and the positions of the topographic features in order to get a true and exact knowledge of the interior, and a correct conception of the courses of these rivers. So it is in science. At certain moments we form conceptions of phenomena which may, or may not, be correct. We must not be satisfied with this, but by patient reasoning, by careful observation and, where possible, by experiments, we must gradually weed out the error and prove the truth and thus establish a new starting point for future advances. It is by such methods and only by such methods that we can increase our knowledge of earthquakes.

Let us then examine what occurs at the time of earthquakes and by a careful comparison with known physical laws, try to discover the actual process of events. We cannot do better than to take for our example the great California earthquake of April 18, 1906, which occurred a little after five o'clock in the morning, and produced such disastrous results. Great praise is due to the energy of the scientific men of the Pacific Coast, who promptly induced the Governor of the State of California to appoint a commission to investigate the earthquake, the necessary funds being supplied by the liberality of the Carnegie Institution of Washington. The great mass of facts collected, together with a discussion of them, are contained in the report of the Commission, which was published by the Carnegie Institution. It is impossible and, indeed unnecessary, to repeat all these interesting observations and it will only be necessary to summarize some of the most important which lead directly to a clearer conception of the forces which produced the shock.

A few days before the shock Professor Branner's students had been working in the region of the San Andreas fault and after the shock they quickly realized that there had been a new movement on this line. Reports from other places along the same fault showed that displacements had also occurred there, and the more thorough exploration of the whole region brought out the fact that at the time of the earthquake there had been a slip on the San Andreas fault over a distance of 270 miles, in which the two sides of the fault had been displaced relatively to each other by amounts varying from a maximum of twenty-one feet to an unascertained minimum, but which, of course, must have disappeared entirely at the ends of the fault. The movement was practically horizontal and although it is probable that there was some vertical component, varying in different parts of the fault, this has not been made out with satisfactory accuracy. The discovery of this dislocation emphasized the horizontal slip on faults, a fact which, although not unknown before, had not received its merited attention. Text-books on geology practically considered only movements in the vertical plane at right angles to the fault, and gave methods of determining the vertical throw, but none to determine the horizontal movement. The horizontal displacements on the San Andreas fault were proved, without question, by the dislocations and offsets in roads, fences and pipes, where they crossed the line of the fault. Very naturally the cause of the earthquake was ascribed to this sudden movement and we shall see later that it accounts for the disturbance and supplies an abundant amount of energy to explain all the effects produced.

The field observations showed very clearly the dislocations and offsets at the fault line itself, but they were not competent to show how far from the fault the actual displacement extended. That the displacement gradually became less and less, as the distance from the fault-line increased, seemed probable, because a thorough exploration of the region failed to discover any other lines with offsets similar to those along the San Andreas fault.

and therefore it was presumable that blocks of the earth's crust were not shifted as a whole. Fortunately accurate geodetic surveys had been made throughout this region many years ago, and the Commission appealed to Mr. O. H. Tittman, Superintendent of the United States Coast and Geodetic Survey, to repeat the surveys and to determine the true displacements of the various stations of the surveys. Mr. Tittman realized the importance of this, and the work was carried out under the immediate direction of Messrs. Hayford and Baldwin. The results were published in detail in the report of the Commission, and we shall merely summarize them here.

The earlier surveys can be divided into two groups: I, those made between 1851 and 1866; II, those between 1874 and 1892. The survey after the earthquake (III) was made in 1906–7. The surveys extend from Mt. Diablo, thirty-three miles east of the fault-line, to the Farallon Lighthouse, twenty-three miles west of it, as shown in the map, figure 1.

All the surveys are connected through the common points Mt. Diablo and Mocha on the inner Coast Range, and the line between them, which is practically parallel with the fault-line. Dr. Hayford has shown that this line either has not moved at all, or has moved parallel with itself without changing its length; for astronomical observations at the times of the surveys show differences in its direction of only a small fraction of a second of arc—that is, within the limits of error of the surveys; and the distances of various points from it measured at right angles to its direction do not show a systematic change depending on their distances, which proves that it has substantially preserved its length without change. Although there is no evidence that this line has moved as a whole, evidence that it has not is also lacking; but fortunately for our purposes this is unimportant, as we are considering only relative displacements.

Survey II covers very well the region north of San Francisco, and in combination with survey III brings out clearly the displacement of a number of stations between the dates of these surveys. Without going into details the following table summarizes the displacements (which are practically parallel with the fault-line) undergone by the best determined points.

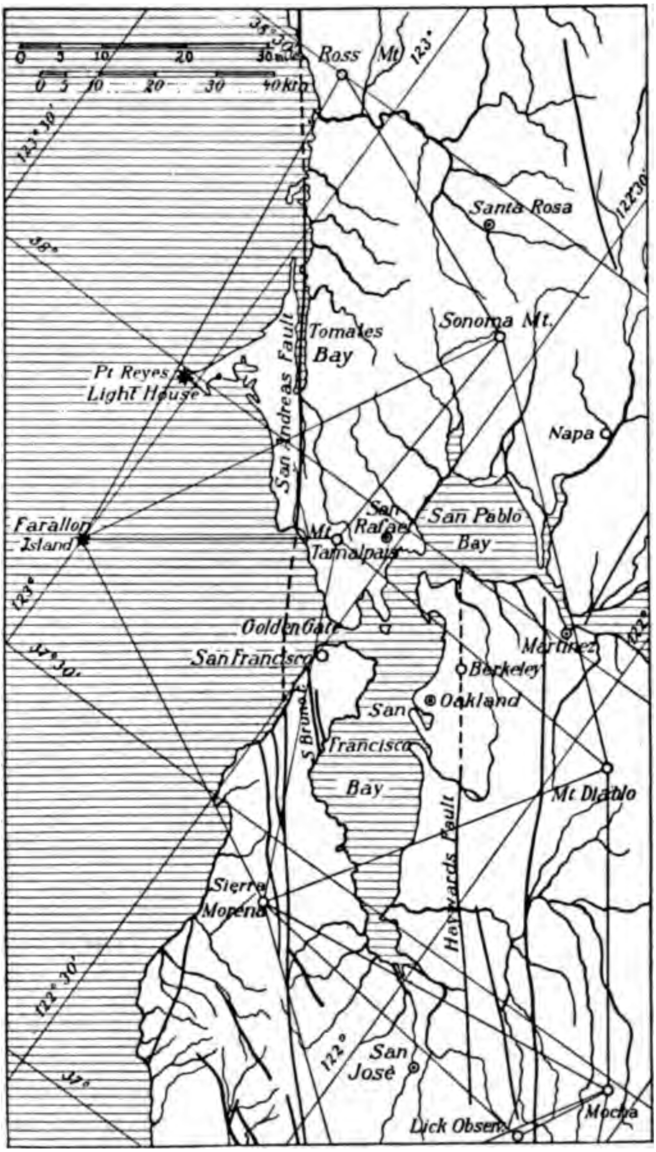
0 5 10 20 30 miles
0 5 10 20 30 40 km
38° 30'
123° 30'
Ross Mt
123°
122° 30'
Santa Rosa
38°
Sonoma Mt.
Tomales Bay
Pt Reyes Light House
San Andreas Fault
Napa
Farallon Island
Mt Tamalpais
San Rafael
San Pablo Bay
123°
37° 30'
Golden Gate
San Francisco
Martinez
122°
Berkeley
Oakland
S Bruno
San Francisco Bay
Mt Diablo
Haywards Fault
Sierra Morena
122° 30'
122°
San José
37°
Lick Observ.
Mocho

Fig. 1

II and

Number of points	Average distance from fault		Displac~~ement between~~ sur-veys I and II	
	East	West	South	North
1	4.0 miles		1.8 feet	
3	2.6 miles		2.8 feet	
10	0.9 miles		4.2 feet	
12		1.2 miles		9.7 feet
7		3.6 miles		7.8 feet
1		23.0 miles		4.8 feet

These displacements are shown graphically in figure 2.

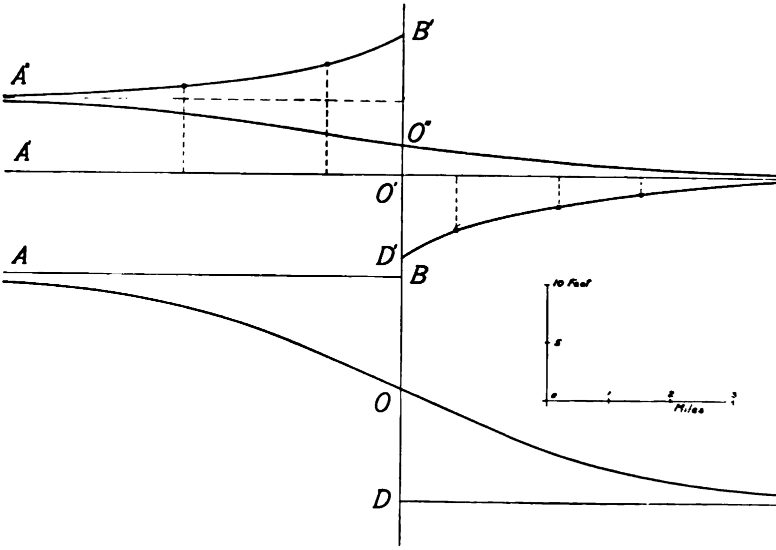

Fig. 2

The line $A'C''$ represents a line which was straight at th time of the second survey. At the time of the earthquake thi line was broken at the fault, and its two parts were separate about 21 feet, taking the positions $A''B'$ and $C'D'$. Three point and the distant Mt. Diablo determine the right-hand curve; tw points and the distant Farallon Light House, together with th fact that $D'B'$ must be 21 feet, determine that on the left. I the figure the distance scale is 1000 times the displacement scal because this difference is necessary to show the character of th displacements. The curvature of the lines is extremely smal the radius of curvature being nowhere less than 4000 miles, an the greatest change in direction of any part of the lines between the two surveys being about one minute of arc. What kind of forces could have caused this movement? Gravity could not have been the immediate cause of the sudden and nearly horizontal displacements, nor could volcanic explosions; the only forces capable of producing such movements are elastic forces. Since the material composing the earth's crust is elastic, and cannot rupture until it is strained beyond its strength, it is evident that an earlier relative displacement of regions on opposite sides of the fault had set up an elastic strain in the intermediate zone, which exceeded the strength of the rock, causing the rupture along the fault surface; and that the rock on opposite sides of the fault, under the action of its own elastic stresses, then suddenly sprang back to positions of equilibrium, the opposite sides moving in opposite directions, and relieving the elastic strain. This is the only satisfactory explanation of the observations and determined displacements. If a curved line, AOC (fig. 2), continuous at O, and with its two sides exactly like the two lines $A''B'$ and $D'C''$, but bent in opposite directions, had been drawn on the ground just before the earthquake, it would have broken and straightened out into two lines, AB and DC, at the time of the rupture. The line $A'O'C'$, straight at the time of survey II, must have been changed into the line $A''O''C'$ before the rupture, and, as we have seen, into $A''B'$ and $D'C''$ immediately afterwards.

All changes in the shape of a solid body may be reduced to two types,—changes in volume, either compressions or extensions, and slipping of one part past another, as the various cards of a pack may be made to slip over each other. When a beam is bent, the convex side is stretched and the concave side compressed, and the elastic forces thus brought into play resist the bending; and if the forces which bend the beam are removed, the elastic forces will cause it to straighten out again. If the cards in our illustration are held together by an elastic cement, we readily see that when they are made to slip slightly over each other there will be a resistance to the movement and on releasing the disturbing pressure they will return to their original position. This kind of a change in shape, when each card becomes indefinitely thin and the number of cards indefinitely large, is called a ***shear***.

When we notice the curvature of the broken line in figure 2, we are inclined to think that the rock was bent like a beam; but when we reflect that the breadth of the beam would correspond to the length of the fault, 270 miles, and the length of the beam to the distance from the fault to which the elastic strain extended, and which was probably not more than six, and certainly not more than ten miles, we see that the length of the beam would be so very short in comparison with its breadth, that the characteristics of a beam would be entirely lost. We must, therefore, look upon the elastic strain as a shearing strain alone, parallel with the fault.

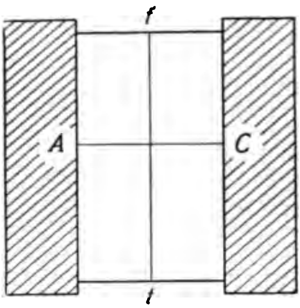

Fig. 3

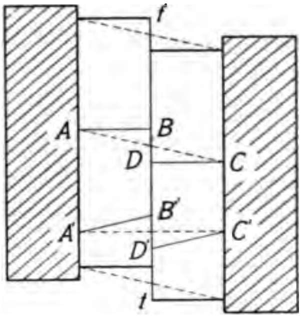

Fig. 4

We can imitate the movements experimentally. Two short pieces of wood were connected by a sheet of stiff jelly one-half inch thick, and two inches wide, and about three inches long, as shown in figure 3. The jelly was cut through along the line, tt', by a sharp knife, and a straight line, AC, was drawn in ink on its surface. The left piece of wood was then shifted about one-half inch in the direction of t', and a gentle pressure was applied to prevent the jelly from slipping on the cut surface. The jelly was sheared elastically and the line took the position AC' shown in figure 4. On relieving the pressure so that the friction was no longer sufficient to keep the jelly strained, the two sides slipped along the surface tt' and the line AC broke into two parts, AB and DC. (The broken lines represent positions immediately before the slip, and the full lines immediately after it.)

At the time of the slip A and C remained stationary, and the amount of the slip, DB, equalled the shift which A had originally experienced. A straight line, $A'C'$ (fig. 4), was drawn on the jelly after the left side had been shifted, but before the jelly slipped along tt'. At the time of the slip, the same movement took place in the neighborhood of this line as near AC, and $A'C'$ was broken into two parts, $A'B'$ and $D'C'$; the total slip, $D'B'$, being equal to DB.

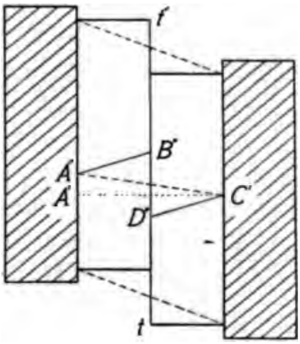

Fig. 5

A third experiment was tried; the left piece of wood was shifted one-half inch and a straight line, $A'C'$ (fig. 5), was drawn across it; it was then shifted one-fourth inch more and the straight line took the position $A''C'$. When the jelly slipped along the surface tt', the line broke into two parts, $A''B''$, and $D''C'$; the slip, $D''B''$, being equal to the total displacement of the left side. Two characteristics of the movement in the last experiment are to be noted; the total slip on the ruptured surface equalled the total relative displacement of the blocks of wood; and at the time of the slip the blocks remained stationary, and the whole movement was an elastic rebound of the jelly to a condition of no strain. These two characteristics could have been deduced from the elastic nature of the jelly without recourse to actual experiment. It is also to be noted that the displacements, measured from the line $A'C'$, were greatest at the fracture; that, on the right-hand side, they gradually diminshed to zero at C'; that the displacements on the left side were much greater than on the right; and that they gradually decreased with the distance from the fracture, but never became less than the displacement, $A'A''$, of the left block after the line $A'C'$ was drawn.

The last experiment illustrates, as well as a simple experiment can, what occurred at the time of the California earthquake; the sudden fling of the rock when the rock fractured along the San Andreas fault was due to the elastic forces set up in it by an earlier relative displacement of the regions on opposite sides of the fault, just as the fling of the jelly was due to the elastic forces set up by the relative shifting of the wooden blocks. As already mentioned, observations in the field showed that at the time of the earthquake there was a relative movement of the two sides, at the fault-surface, amounting to about 21 feet. The surveys show that the actual displacements which took place between surveys II and III diminished as the distance from the fault became greater; on the east side the displacement practically died out at a distance of six or ten miles from the fault, and on the west side the displacement apparently became equal to that of the Farallon Light House at about the same distance. All the phenomena are in close accord with the last experiment described above. The main difference consists in the fact that a straight line across the fault on the earth's surface did not break up into two straight lines, as in the experiment, but into two curved lines. We ascribe this curvature to the fact that the forces which produced the displacement of the ground were applied below the crust of the earth, whereas in the experiment they were applied to the outer boundary of the jelly.

The elastic rebound near the fault-surface, of course, took place suddenly at the time of the earthquake; between surveys I and II, and between II and III, there were relative shifts of very extensive regions, the fault-line being the line of separation between them for the second interval; but the surveys do not determine whether these shifts took place suddenly at the times of the great earthquakes of 1868 and 1906, or whether they were the effect of a slow, gradual movement continuing through the years. The experiments we have described might have been varied, and instead of a slow displacement of the block, gradually setting up an elastic shear, we might have set up the shear suddenly by a sudden displacement of the block; the movements at the time of the slip would have been exactly the same in the two cases.

We must turn to other considerations to determine which of the above cases represents the earth movements leading to the California earthquake.

A very important chapter in the history of the earth is concerned with the record of the various movements which have taken place in the crust. The vertical movements are thoroughly attested by the various heights to which strata, deposited under the sea, have been raised; and by the unconformities which exist in the geological column. The horizontal movements are shown by the compression of the strata into folds, and by great overthrust faults. That these movements have continued until recent time is shown by the existence of raised beaches, and other similar evidences; and that they have not been simply due to a general rising, or sinking, of the surface of the ocean follows from the fact that the elevations vary at different places. For instance, the Cretaceous strata of Maryland are now near the sea-level, whereas in the great plateaux of Utah and Arizona they have an elevation of about 8000 feet. Innumerable instances of the various elevations of strata once at the same level could be mentioned. Moreover, many of the raised beaches, which must have been horizontal when they were formed, are now tilted. We have excellent examples of this tilting in the account given by Baron de Geer of the raised beaches in Scandinavia; and in the tilted shore-lines of the old glacial lakes in the region of the Great Lakes. Mr. Gilbert's discussion of the tide-gauge readings at various points on the Great Lakes, and of the topographic changes taking place on their shores, shows, without reasonable doubt, that the tilting is going on at the present time; and that the difference of level between two points 100 miles apart and lying on a NNE and SSW line is changing by probably more than half a foot per century.

It is firmly established that since the beginning of geological history the crust of the earth has been in continual movement, rising in one place, sinking in another, here squeezed into folds, there somewhat stretched. It is generally assumed that these movements are slow and continuous, but the treatises and textbooks on geology are lacking in a discussion of the question.

In his principles of geology Lyell argues in a general way in favor of the slow movements of the land, but he is concerned more particularly with opposing the catastrophic ideas that were current at the time. He points out that past actions are probably the same as those going on at present, that the continued growth of corals on coral islands indicates, according to the ideas of Darwin and Dana, a gradual subsidence and that no sudden depression amounting to as much as 100 feet could have taken place without the destruction of the life of the coral-building polyps. He also argues that after earthquakes along the South American coast, the land was found to be raised a few feet, and considers that the great elevation of the coast shown by raised beaches, etc., is simply the sum of these small movements. He adduces the creep as observed in coal mines and points out that the plastic floors of some mines are gradually squeezed up to fill the space between the pillars left to support the roof, and that this movement in general is slow and continued.

Great weight is put on the slow changes of level in the Scandinavian peninsula, which were at that time much discussed, the movements of the temple of Serapis at Possuoli, and other slow changes of level. Since Lyell's time, many other similar movements have been discovered. It has also been pointed out that mountain ranges have been raised across the courses of certain rivers, but that the elevation has been so slow that the rivers have been able to cut down their beds as fast as the mountains rose, and have thus preserved their original courses. The course of the Potomac River, for instance, has not been changed by the elevation of the Appalachians.

It is quite evident that these arguments do not distinguish between continuous slow movements, and a succession of small sudden displacements, and Lyell was rather concerned with combatting the older catastrophic theory and advancing the uniformitarian ideas of Hutton. But, nevertheless, the universal acceptance of Hutton's views, and the lack of evidence that the undoubtedly slow changes of level were the result of many small but sudden movements, has led to the general belief that the earth's crust was subject to slow continuous movements. The existence of great folds in the rocks undoubtedly contributed to this belief; although the force of this argument does not seem to have been clearly stated. The idea was carried too far, and it was even supposed that the great displacements, which geologists have shown to exist at innumerable faults, were attained by a slow, steady, and long continued movement.

I am persuaded that besides the sudden fling when there is a slip on a fault, there exist in the crust of the earth slow and continuous movements not uniform, and not even always in the same direction, but still movements which are truly continuous, and have no trace whatever of sudden starts.

The weakest argument is based on the fact that many known movements of the land are extremely slow and are spread over long intervals of time, and that there are no indications at all that they are simply the sum of a number of small sudden displacements. Professor John Milne, in describing the reports giving information regarding the changes of level on the Japanese coast, states that some of his informants consider that these changes were due to a great earthquake, "although in no case has it been stated that the changes accompanied such disturbances." One might suppose that the continuous nature of the movement could be determined by a series of tidal observations. But many years must elapse before the movement can be detected at all, and therefore it is quite impossible to prove by this means that it has been actually continuous.

The strongest argument is based on the plastic deformation of the rock. The great overthrust faults, the folded and contorted strata, the existence of slaty cleavage, the thinning of the strata and the flattening of fossils, prove beyond question that the rock has been subjected to enormous pressures. Whatever may be the ultimate cause of this pressure, it is certain that its transmission from one part of the rock-mass to another is by means of elastic forces in the rock; there is always a certain amount of elastic compression or distortion under external force, which indeed determines the amount of force transmitted and is itself determined by the amount of the external force. If a book is placed on the table, there is a slight elastic compression of the table itself which exercises an upward pressure on the book and supports it against the pull of gravity; and there is a general compression of the book itself, for each part must exert a sufficient elastic force on the part above to support its weight. When power is transmitted along a shaft, the force is applied at one end turning the shaft; the elastic shearing force thus brought into play is transmitted along the shaft, each part being slightly twisted on the part beyond it, and thus exerting on it a turning force. Elastic forces are essential to our well-being; we make use of them at every moment of our lives.

We must look upon the rock under pressure as suffering a certain amount of elastic compression, or distortion. Rock, in common with other substances, is not perfectly elastic, but has some plasticity, and under the long continued action of an external force it will gradually change its shape without fracture. We have evidence of this not only in nature but in the laboratory. When Messrs. Nagaoka and Kusakabe made experimental determinations of the elastic constants of various rocks they found a certain amount of plastic yielding even during the few minutes involved in their experiments. And Professor Adams has succeeded in deforming small specimens of marble without fracture by supporting them in a strong steel tube while subjecting them to enormous pressures. If, however, the rock had not been supported on its sides, the great pressures, as in certain methods of testing the strength of materials, would have cracked it into many pieces. Marble slabs over old graves, supported at their ends, have sunk slightly in the middle, not by elastic bending but by a plastic distortion.

We have then the following facts: the rock in the earth's crust has been subjected to strong forces; it is plastic; it is supported beneath and on the sides by the surrounding rock and above by the weight of the rock resting on it; therefore, as in Professor Adams' experiments, it must be slowly deformed; and by this yielding the elastic forces are reduced or at least prevented from increasing as rapidly as they otherwise would. The existence of faults proves that when the forces become too great fractures occur and, moreover, that sudden plastic deformations do not occur; for if they did the elastic forces would be kept down below the breaking point, and fractures would be averted.

The objection might be made that rock at great depths and, therefore, under heavy pressure could not fracture, but might be suddenly deformed; but if it could suddenly change shape plastically, it could also change shape by a sudden slip along a fault. Moreover, the sudden plastic yielding implies either the sudden reduction of the forces resisting plastic deformation, which is contrary to our knowledge of the properties of matter, or the sudden increase in the deforming forces. We can readily picture to ourselves fairly steady forces like gravity, or the forces brought about by the disturbance of isostatic equilibrium; but the only sudden forces, except those caused by blows or by the release of elastic strain, are explosive forces, and no one would imagine that the great foldings of the strata were due to successive pressures brought about by a great number of explosions. And as the observed foldings and contortions of the rock are what we should expect from a slow plastic flow, it seems superfluous, to say the least, to ascribe it to sudden movements. And if there is a slow plastic yielding there are slow movements of the rock; for the movement of a given part of the rock must be equal to the movement of the rock in front of, or behind, it.

We can show that the elastic strains which caused the California earthquake were not suddenly developed immediately before the shock, but that the strain existed to some extent twenty-five and fifty years earlier. It should be noticed that in the first experiment with the jelly the elastic rebound of points on the right side of the fracture brought them to exactly the same position, relative to the right-hand block, that they held before the strain was set up. In the second experiment, where the relative positions were determined after the strain was set up, the jelly on the right was displaced downward after the slip. This is exactly what occurred in the movements on the eastern side of the San Andreas fault. The points on that side were displaced southward between the times of the second and the third surveys, showing conclusively that at the time of the second survey the ground was already in a state of elastic strain. This is brought out also in another way. The third experiment shows that the total slip at the fault-plane, at the time of the rupture, is exactly equal to the total relative displacement of the blocks of wood; therefore, we must infer, since the total slip on the San Andreas fault amounted to about 21 feet, that the shift of the distant regions must have been as great; but it was found that between surveys II and III the shift was only 5.8 feet and between I and II 4.6 feet; that is, in all, only about 10.4 feet since the earliest surveys, some fifty years before the shock. We can therefore say definitely that the shift which set up the elastic strains which finally resulted in the earthquake had already accumulated to about half its final amount fifty years earlier; that between surveys I and II it increased to about three-quarters of its full amount, and that the last quarter was added between surveys II and III. In the experiments the elastic rebounds of the two sides were equal and in opposite directions. The surface rocks on opposite sides of the fault are not identical as is the jelly, but it is probable that at no great distance below the surface they are similar, which would lead us to expect *approximately* equal rebounds on the two sides. We have no determinations of the displacements just before the earthquake, and therefore no measures of the actual rebounds, but Professor Lawson has pointed out that if we assume the displacements to have been continuous and uniform during the interval between surveys I and II, and to have continued at the same rate up to the time of the earthquake, then the region about the fault would have been so far north at that time that the rebounds on the two sides would have been practically equal, just as with the jelly. It is hardly possible in view of the above history, of the relations just mentioned and of the difficulty of imagining forces capable of suddenly moving and stopping large areas, not to be convinced that the shift accumulated gradually.

To summarize, we may say, that for many years, perhaps for a century, a slow relative movement to the north of the region under the Pacific Ocean, just west of California and comprising a part of the coast, was taking place and setting up a shearing strain in the coast region, which finally became too great for the rock to endure; that a fracture occurred along an old fault-line and that the two sides sprang back towards positions of no strain under the action of their own elastic stresses, the amount of the sudden rebound diminishing as the distance from the fault-line increased, and being no longer measurable at a distance of less than six miles from it.[1]

The summary above describes all the mass movements that occurred at the time of the earthquake in a zone about fifty-six miles wide, and excludes from that zone the movements of blocks of the crust as a whole. The gradual diminution in intensity of the disturbance to the eastward of the zone, with the exception of alluvial tracts, where the terrane caused an increase, indicates that no fractures occurred to the eastward; and the intensity at the Farallon Light House shows that no fracture occurred for some distance west of it. As far, therefore, as negative evidence goes, no block movements occurred; and, indeed, at the time of the earthquake all the known movements can be accounted for without assuming them. Moreover, the lack of dislocations on other faults not far from the San Andreas fault is a very strong argument, from an observational standpoint, against block movements at the time of the California earthquake.

A great amount of energy was set free at the time of the earthquake; the law of the conservation of energy points out that it was not created at that time, but must have existed earlier in a potential form. The sudden earth movements were practically horizontal and, therefore, it could not have been in the form of gravitational energy, the energy which would have been liberated if a block of the earth's crust had suddenly sunk. But we have seen that the earth movements were merely elastic rebounds, and therefore the energy must have been in the form of potential energy of elastic strain; a form nearly related to the energy stored in the spring when a clock is wound up, or the energy in a bow when the arrow is drawn back, ready to fly. It is an easy matter to calculate the amount of energy contained in the rock in the form of elastic deformation by the work done as the two sides of the fault flung back to positions of equilibrium. This equals the total elastic force multiplied

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[1] This conception of the causes of the California earthquake of 1906 was first stated by Professor Lawson in the *Report of the California Earthquake Commission*, vol. 1, pp. 147–151. It was developed further in the second volume of the report.

by half the relative slip. If we take the length of the fault at 270 miles, its depth at twelve and a half miles, and the average slip at thirteen feet, we find that the work done was 130,000,000,000,000,000 foot-pounds. It is not surprising that the liberation within a few seconds of this enormous amount of energy should be followed by such great destruction in the megaseismic district, and that seismographs in all parts of the world should record the disturbance.

Energy is never created; it merely changes its form. Whence, then, came the energy of elastic strain which existed in the great rock-spring before the earthquake? We have ascribed it to the slow movements of the crust and of the underlying material; but the question still persists, Whence came the energy of these slow movements? It may have been gravitational, as we shall see a little later when we consider general suggestions that have been advanced to account for earthquakes, but we cannot answer with confidence. If we could follow this energy, step by step, back into the infinite past, we could solve the riddle of the physical universe.

Let us now turn our attention to the manner in which rock breaks along the fault-line.

Since the fracture results from elastic strains due to slow displacements of neighboring regions, it is practically certain, on account of irregularities in the displacement and in the character of the rock, that the stresses will not reach the limiting strength of the rock over the whole area of the fault, or even over a large fraction of it at once, but will begin in a very limited area, and extend from there at a rate not greater than the velocity of compressional elastic waves in the rock. When the rupture occurs at a point, or a small area, the elastic stresses are no longer supported there, and are therefore transferred to neighboring points, which in turn give way, and thus the rupture spreads along the fault-surface until the elastic strains are so reduced that they can carry it no further.

But the transfer of stress is not instantaneous; time is required for the successive parts of the rock to be sufficiently displaced to bring the strain in their neighborhood to the actual breaking limit, and the amount of time necessary to accomplish this depends on the elasticity of the rock and on its density, on exactly the same quantities, which, we shall see later, determine the velocity of propagation of elastic waves; so that it is impossible for the fracture to extend more rapidly than the rate of propagation of the fastest elastic waves, which are of the compressional type, like the waves of sound; and in general it would extend less rapidly. It is clear that, if the strains were everywhere very close to the breaking limit, a very small movement and therefore a very short time would be necessary to bring the strain to that limit; whereas, if the strain were further from the limit, a greater movement and a longer time would be needed. This is probably one reason why some earthquakes of small intensity last for a number of seconds.

The progressive extension of the fracture is entirely in accord with our general experience. When a bridge or structure collapses, the break always starts at some particular point and extends from there; and the time taken for the complete downfall is far greater than would be necessary if the break occurred everywhere simultaneously, and workmen often have time to spring from the falling structure and save themselves. An avalanche or landslide begins in a small way and gathers material and momentum as it descends. When a chair breaks it does not often happen that there is a sudden collapse, but one part breaks after another, and the occupant usually has time to spring to his feet. When an ice-jam in a river gives way, one part always yields first. When a sheet of paper is torn the tear begins at one side and passes across the sheet. Even when gunpowder is fired, a measurable time is necessary for the explosion to spread from its starting point to other points of the mass.

Two instances have been found where the progression of a crack in the ground, or rock, has apparently been seen. The following is taken from the "Sun" newspaper, of Attleboro, Mass., of January 23, 1903:

"The experience of the town of Whitman was repeated by Attleboro yesterday, when an earthquake or frost crack or something of the kind made its appearance. There was a hollow rumbling, a shaking of buildings, a small-sized panic among east-side residents, and a fissure opened in the ground of great depth and unknown length. . . . . The disturbance . . . was immediately followed by the appearance of the crack, which did not open simultaneously its whole length, but gradually, from its northern to its southern terminus."

Professor Niles, describing the expansion of the rock in the quarry at Monson, Mass., and the accompanying cracks, writes: "These cracks, or rents, are more commonly formed slowly, but sometimes suddenly."

It is interesting to note that Mallet believed the great Neapolitan earthquake of 1857 to be due to a fracture in the underlying rock, which began in a limited area and extended to greater distances.

Indeed, the progressive method of breaking is general, as it depends upon the elastic properties of solids. Absolute rigidity would be practically necessary to ensure simultaneous rupture over a very large area. It may be objected that it is trivial to emphasize the difference of a few seconds in the time of rupture at different parts of the fault; but the difference is not so very small. If the rupture of the San Andreas fault began near its middle point, it must have taken at least half a minute, and it may have taken more than a minute, to reach the ends of the fault; and moreover, deductions based on the supposed simultaneity of fracture have led to conclusions regarding mass movements, the place of origin of the vibrations, and the interpretation of instrumental records, which are quite out of harmony with the conceptions advocated here.

As the different parts of the same fault do not fracture simultaneously, so there is no probability of neighboring faults fracturing at the same time. If two faults are only a few miles apart, it may happen that the relief of strain at one will increase the strain at the other sufficiently to start a rupture there, if it is already strained nearly to the limit. The vibrations from one fracture, under the same conditions, might start the rupture of a second. In all these cases the rupture begins in a very limited area of a single fault, and extends along the same and perhaps to other faults, but never at a greater rate than the velocity of compressional elastic waves; as this velocity may, in some instances, be as great as four miles per second, it is quite clear that only the most accurate time observations could serve to determine the starting point of the rupture; and that the majority of observations would not be accurate enough to show that disturbances did not start simultaneously throughout the megaseismic district.

At the time of the rupture the rigidity of the rock would not permit very large movements of the two sides of the fault until the fractured surface had greatly increased in size; but when the large movements came they would cause the severest part of the shock. The friction at the fault would make these movements irregular, so that the vibrations sent out would not be a steady, strong series, but would vary so much in intensity that they would produce the effect of strong shocks separated by weaker intervals. At the time of the California earthquake, the severest part of the disturbance did not come until thirty seconds after the beginning of the fairly strong shocks; and it was felt from thirty to sixty seconds. The time necessary for the sides of the fault to reach their positions of equilibrium under the elastic forces, free of friction, would have been only a little more than two seconds. The duration of the severe shocks at any place was partly due to friction on the fault-surface, partly to the time necessary for the extension of the fracture, and partly to the arrival of shocks from more distant parts of the fault.

The friction of the two sides of the fault when the dislocation is taking place, and their sudden starting and stopping (the latter due largely to the friction), are the causes of the vibrations which are propagated elastically to a distance; and they all have their origin in the rupture surface. It has been suggested that the origin of the vibrations may lie in a volume and not on a surface; and that the sudden folding of the rock or the movement of a block as a whole would cause elastic vibrations to emanate from the whole volume moved. This idea seems erroneous. If the rock were sufficiently plastic to fold very rapidly under the compressive forces it would not be sufficiently elastic to send out vibrations; and if the rock yielded elastically to a suddenly applied force, the vibrations would start from the boundary where the force must be applied. We shall see that blocks do not move as a whole; but even if they did, vibrations would not originate in their volume any more than vibrations would originate in the volume of any other body falling under gravity; for vibrations are started, not by simple velocity, or acceleration, but by the differential velocity in contiguous elements. Friction starts vibrations by causing rapid changes of velocity at the surface of the slipping mass. If a block were suddenly started, or stopped by elastic forces, the vibrations must start from the boundary, where alone the forces could be applied.

We may now sum up our general results. The observations of the California earthquake and the deductions drawn from them based as they are upon the elastic properties of rock and upon the well-known relative movements of different parts of the earth's crust, have led to certain general conceptions of the mass movements which take place before and at the time of tectonic earthquakes, which may be expressed as follows:

1. The fracture of the rock, which causes a tectonic earthquake, is the result of elastic strains, greater than the strength of the rock can withstand, produced by the relative displacements of neighboring portions of the earth's crust.

2. These relative displacements are not produced suddenly at the time of the fracture, but attain their maximum amounts gradually during a more or less long period of time.

3. The only mass movements that occur at the time of the earthquake are the sudden elastic rebounds of the sides of the fracture towards positions of no elastic strain; and these movements extend to distances of only a few miles from the fracture.

4. The earthquake vibrations originate in the surface of fracture; the surface from which they start has at first a very small area, which may quickly become very large, but at a rate not greater than the velocity of compressional elastic waves in the rock.

5. The energy liberated at the time of an earthquake was, immediately before the rupture, in the form of energy of elastic strain of the rock.

These statements, which may be called the *elastic rebound theory of tectonic earthquakes*, do not broach the original cause of earthquakes, which lies in the source of the slow movements accumulating the elastic energy, but merely give the *modus operandi* of the accumulation and liberation of this energy.

They are opposed to Lieut. Colonel Harboe's idea of focal lines, which assumes that the fracture extends practically as far as the earthquake is felt; and to the block movements of several writers, who suppose that the earth's crust breaks up into individual blocks, each of which moves as a whole to a new position of equilibrium.

It must not be supposed that earthquakes are caused only by horizontal movements on a vertical fracture, as in the case of the California earthquake. Any kind of a fracture is sufficient, and the movements may be horizontal, vertical or oblique. When rocks have been folded in the earth's crust it is not uncommon to find scratches on the limbs of the folds resulting from the slipping of the strata upon each other. Professor Smoluchowski has suggested that this slipping might be a cause of earthquakes. It seems quite certain that, as the rocks were being folded by horizontal pressure, the friction would at first prevent any such slipping of the strata; but as the elastic forces become stronger, slipping would occur suddenly with an elastic rebound of the adjacent strata, which would constitute an earthquake. It is probable that the elastic strains set up in this way and the consequent rebounds would never be very great and, therefore, that severe earthquakes are not originated in this way.

Let us glance for a moment at the accounts of some other great earthquakes and see if the movements of the ground which accompanied them were similar to those found at the time of the California earthquake, and if the ideas of elastic rebound which we have developed can be applied to these.

A very severe earthquake occurred in the Province of Cutch, near the mouth of the river Indus, in 1819. An extensive, flat plain, known as the Rann of Cutch, only a few feet above the level of the sea, and which was indeed formerly a sea bottom, occupies a large area in this region. It is traversed by a small distributary of the Indus, called the Pooraun or Koree, but for some years before 1819 no water had flowed through this channel on account of dams built across it further up. The great shock occurred a little before seven o'clock in the evening of June 16 and was so severe that all the villages in the neighborhood were destroyed, and a mosque at Ahmedabad, about 250 miles to the east, which was erected nearly four hundred years earlier, fell to the ground; the vibrations of the earthquake were felt in northwest India, to a distance of eight hundred miles.

In the midst of the Rann and near the old bed of the Pooraun, stood the Sindree fort, where customs were levied on commerce. At the time of the earthquake the land in the neighborhood of this fort sank a distance of about ten feet. Water apparently burst up from the ground and rolled in from the sea by the channel of the Koree; an immense lake was formed, of unknown extent east and west but about six miles from north to south, which was a few feet deep and covered all but the highest parts of the region. Two or three miles to the north of Sindree appeared a scarp, ten or twenty feet high, running in an easterly and westerly direction for an unknown distance, but apparently about fifty miles, which was called by the natives "The Allah-Bund," or "Mound of God." Mr. A. B. Wynne, in the *Memoirs* of the Geological Survey of India, has described the geology of the region, and collected the available information regarding the earthquake. He concluded that the land south of the Bund had sunk, but that the Bund itself did not represent an elevation, as was generally supposed at the time of the earthquake, but was merely the scarp left by the depression of the land to the south. This depression did not extend indefinitely, but from the depth of the water which accumulated there it is evident that the greatest depression occurred near the Bund and diminished toward the south. Indeed, there are some reports of a slight elevation about eighteen miles south of the Bund, though they are probably not very reliable. No account is given of any change on the seacoast, forty or fifty miles to the southwest, except the apparent deepening of the channel of the Koree, which may be due to scour. A tidal wave would undoubtedly have followed a sudden depression of the coast, but none was mentioned. The water which appeared over the plain was supposed, by some, to have come from the sea through the Koree; but this does not require a tidal wave, for the level of the new lake was so low that in August, 1827, at the time of the monsoons, the sea water was driven up the Koree and made the lake brackish. Earlier in the summer it was fresh on account of the floods mentioned below.

In 1844, Captain Baker, of the Bengal Engineers, made a map and section of this region. "On the 11th of July he found the 'mound' where cut through by the Pooraun (or Koree), nearly four miles in width, but in other places it was said to vary from two to eight miles. Its greatest height was on the border of the lake, above the level of which it rose 20½ feet. From this elevation *it gradually slopes to the northward till it becomes undistinguishable from the plain.*"

In 1826 heavy floods caused the Indus to break through the dams and to pour down its former channel across the Bund. Mr. Wynne thinks that if the Bund had actually been elevated, the stream would not have crossed it, but would have flowed to the side. Professor E. Suess accepts Mr. Wynne's explanation and considers that there was no elevation of the Bund, but that "it is simply a case of the eruption of the subterranean water and the consequent subsidence of a sharply defined portion of the muddy ground."

Dr. R. D. Oldham, having found a tracing of Captain Baker's map and section, which were apparently unknown to Professor Suess, as he does not mention them, concludes, after a review of Mr. Wynne's memoir, that the Bund was actually elevated ten feet at the scarp line with a gradual slope down towards the north and that there was an approximately equal depression immediately south of the scarp. He writes: "On the other hand, and opposed to the arguments which can be urged against an elevation, we have the map and section, and the very definite statement, evidently based on careful leveling, that there was an actual upward slope of the ground immediately behind the southern scarp of the Allah-Bund. There seems, consequently, good grounds for maintaining the older view that the Allah-Bund was an elevated tract, but there can be no doubt that the estimates of its height do not correctly represent the amount of elevation, but of the sum of this and the depression which certainly took place to the south. The former cannot have exceeded ten feet, the latter amounted to as much or more, and the two together represent the estimates of the height of the barrier as seen from the south, estimates which range up to 20½ feet."

When we consider the general character of the movement which took place at the time of this earthquake, Professor Suess' explanation seems entirely inadequate. Is it possible that the subsidence of a portion of the land due to the squeezing out of the contained water could present the characteristics noticed at the Rann of Cutch? We have a well-defined scarp some fifty miles in length and about twenty feet in elevation sharply dividing the land which was depressed from the region not so affected; we find the subsidence was greatest at the scarp and diminished towards the south, with no other scarp limiting its area; and we find that the lake so formed was not merely a temporary lake due to the sudden supply of water and quickly drained, but that it remained as a permanent lake for some time and that it is still occasionally flooded either by fresh water from the rains and surrounding streams, or by salt water driven in from the sea by the southwest monsoons, conditions which did not exist before the earthquake. And we have Captain Baker's positive statement that the Bund slopes downwards towards the north, and his section shows the slope, which was, however, so gentle that it could not have been detected by the eye alone.

In view of our present knowledge, I think we may represent very simply the movements which took place at the time of this earthquake as follows:

The Rann of Cutch, formerly below the sea level, was gradually raised by vertical forces which were stronger toward the north. An elastic shearing strain was thus set up which finally resulted in a rupture of the rock along an east and west line, with an upward fling of the northern side to form the Bund, and a corresponding downward fling of the southern side, to form the lake, the total relative movement being about twenty feet, practically the same as the relative horizontal displacement at the time of the California earthquake. It seems rather strange that Professor Suess, who pointed out so clearly the relations of earthquakes to fault-lines, should not have seen that in this case the scarp was merely the surface indication of the general movement on an underlying fault.

On January 23, 1855, a severe earthquake shook the region about Wellington, New Zealand. A very interesting account of the changes produced at the time of this earthquake is given by Lyell, from which the following is taken.

Wellington lies near the southwestern corner of the peninsula which puts out from the northern island and is bounded by Cooks Strait. In the middle of this peninsula lies the broad, flat Wairarapa Valley, trending northeast and southwest. It is bounded on the northwest by the Remutaka Mountains and is separated from them by a great fault. These mountains extend further south than the plain and form the western side of Palliser Bay. After the earthquake, it was found that the mountains along the line of the fault for a distance of about ninety miles had suddenly risen nine feet and large fissures appeared between the rock and the soft material of the plain. An engineer, who was engaged at the time in making a road along the side of Palliser Bay, found clear evidence, by the height of a line of shells clinging to the rock, of the elevation of the mountains, but apparently no evidence was found of a counter movement in the plains. This, however, is not surprising, as the slight variation in slope, which alone could have shown a depression of the plains, would easily have eluded detection; and, moreover, the softer material of the plains may have been dragged up by the rock. The northwest coast of the peninsula, about twenty-three miles from the fault-line, experienced no elevation, but Port Nicholson, about half-way between the west coast and the fault, was raised about four feet on its western and five feet on its eastern shore. Lyell looks upon this variation in elevation of the land at the time of the earthquake as showing how the strata may be tilted by varying amounts of elevation, but I think we are justified, with our present knowledge, in looking upon this as an example of elastic rebound, and not necessarily a step in the general tilting of the rocks.

New Zealand is a land of many faults. They have been more thoroughly studied in the South Island, where they have very materially influenced the topography of the region. Indeed, the topographic features there are very similar to those of the great San Andreas rift, described in the report of the California Earthquake Commission. Parts of the South Island are subject to frequent and violent earthquakes, which have resulted from movements along these faults; and, it is most interesting to note that surface dislocations at the time of earthquakes had revealed many of these faults to the inhabitants, by whom they were called "earthquake rents," before they were known to geologists. Some of these faults continue across Cooks Strait and apparently connect with known faults on the North Island. One of them is the fault on the side of the Remutaka Mountains, on which the movement of nine feet occurred at the time of the earthquake of 1855. Three of the faults converge in the neighborhood of Wellington, and it is quite possible that some displacement occurred there at the time of that earthquake, but we have no account of it, and it seems probable that if there had been any distinct vertical movement on these faults it would not have been overlooked.

Displacements on faults reaching to the surface have taken place at the times of many earthquakes. For instance, the great Owens Valley earthquake of 1872, when there was an increased elevation of the Sierra Nevada along its eastern face; the earthquake of September 1, 1888, when fences were broken and offset from five to eight feet at the Clarence fault in the South Island of New Zealand; the Mino-Owari earthquake of 1891, when a great fault appeared across the main island of Japan, with both vertical and horizontal displacements; the Sonora earthquake of 1877, when two faults appeared on opposite sides of the mountains in the Sonora Province, Mexico; and the Nippon earthquake of 1896 in Japan, where movements also occurred on two distinct faults ten or twelve miles apart; and many others might be mentioned. At the time of all these shocks there were very evident displacements along the faults.

The Cutch and Wellington earthquakes offer positive evidence in favor of the gradual dying out of the displacement as the distance from the fault increases; the Clarence, the Owens Valley and the Mino-Owari earthquakes support the idea by negative evidence, inasmuch as no other faults were found to limit the movement and to suggest the displacement of a block between two faults; the Sonora and Nippon earthquakes distinctly suggest movements of a block, but we shall see, when we consider the elevation or depression of mountain ranges, that these earthquakes may be explained satisfactorily without assuming such a movement.

In the case of the California earthquake it would have been impossible to prove that the elastic rebound gradually died out with increased distance from the fault, if it had not been for the successive exact surveys which were made in this region. The change in the amount of displacement diminished very slowly with the distance from the fault; the difference in a distance of a thousand feet in the neighborhood of the fault, where it was greatest, was only about six inches. It is not surprising, therefore, that the data regarding other earthquakes, where no such surveys were made, are, with the exception of the Cutch and Wellington earthquakes, insufficient to show a similar distribution of the earth movements. But the data in no way oppose the idea; and the positive evidence and the general reasoning seem quite strong enough to establish it.