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THE STUDY OF EARTH MOVEMENTS IN CALIFORNIA[1]

IT has been the custom for a number of years for the retiring president to present in brief review a field of activity with which he has been associated rather than to undertake the detailed exploitation of any particular problem. I have therefore chosen to give some outline of an effort which has been in progress for four or five years only, but which is particularly appropriate to present in this time and place because there are so many in Washington who have been associated with it.

The present study of earth movements in California is planned to be of broader scope than a mere statistical study of earthquakes. It is intended to do somewhat more than record tremors with the seismograph, in order, by comparison with other similar records, to trace the path of the waves to a common center of disturbance, and so to obtain information about the manner of their transmission through the interior of the earth. This is a part of the project as heretofore, but we have wished to investigate not only the tremors which may be picked up here or there and their path traced to a distant point of origin, but their probable causes, the earth movements at the source, cumulative mass movements through which the enormous strains are set up and not alone the disturbances which indicate their release.

The particular project, out of which the present undertaking grew, started with Mr. Harry O. Wood, known to many of you as acting secretary of the Geophysical Union, following the war period; before that as the assistant in charge of the Seismological Station at the Kilauea Observatory, where he successfully recorded and analyzed local shocks of volcanic origin; before that as an assistant to Professor Lawson, at the University of California, and one of the collaborators in the preparation of the great monograph on the San Francisco earthquake of 1906. Guided by this experience, Wood prepared and published in the *Bulletin* of the Seismological Society a very elaborate project for the study of cumulative stresses and local earth movements in California on an extensive scale. Some of you may have read the scheme which he offered. It contemplated stations at intervals of 50 miles throughout the West Coast region and continuous observations over a period of years. Altogether, it was rather too extensive a project for any available agency and so after

[1] Address of the president of the Washington Academy of Sciences, January 13, 1925.

the matter had been considered by several interested institutions it was laid on the table, so to speak.

It is due to President Merriam, of the Carnegie Institution, that an actual beginning was made in this field of activity. He said, very practically, that while we might not, perhaps, undertake a two-million-dollar project for the study of earthquakes, it was certainly true that California offered a favorable field for such studies and America was not doing her share in the field of seismology. It was therefore desirable that a beginning be made. In consequence of this view President Merriam in 1921 appointed a committee, designated as the advisory committee in seismology, to formulate a plan whereby the Carnegie Institution might effectively enter the field of seismologic research, perhaps along the lines indicated by Wood, but in any event with a somewhat broader concept of the field than has been usual among existing agencies. This committee prepared such a project some four years ago and offered it to the president and trustees of the institution for their consideration. Then the thing happened which sometimes does happen, despite all expectations—the committee, having allowed its collective imagination to revel somewhat irresponsibly in the new field, was sharply recalled to realities and invited to put its plan into operation. That is how the advisory committee in seismology was called into existence and presently became an executive committee, entrusted with carrying out a rather comprehensive project for the study of earth movements in California.

The first definite step which was taken was an incidental result of Professor Lawson's examination of a long series of position observations which had been gathered over a period of years at the Ukiah latitude station. In 1921 Lawson published a paper embodying these data and drew from them the conclusion that there was evidence of a nearly continuous northward crustal creep in the San Francisco Bay region amounting to as much as a foot a year for some 20 years. Confirmation was immediately sought on all sides and first of all naturally at the Lick Observatory on Mt. Hamilton. The Lick Observatory is not a latitude station, nor indeed a station where position observations are regularly made, but it was in the same region and had many observations from which determinations of position might be computed. Consequently, at the request of the committee, the Lick Observatory examined its records over the period of time covered by the latitude measurements at Ukiah. In general these were reported to confirm the Ukiah observations.

Now in the theory which had been worked out in beautiful detail by Reid, of Johns Hopkins, and published in Lawson's memoir on the 1906 earthquake, the conclusion was reached that the elastic rebound from cumulative strain carried to the point of rupture was the earthquake and that our most fundamental step in the study of seismologic activity, that is, of earthquakes, lay in finding the direction and magnitude of the forces indicated by the accumulating creep. Both Lawson and Reid had therefore been following that trail since 1906, perhaps even longer than that, and this publication of the records of the observations at Ukiah, presenting as it did the first step of tangible accomplishment in that direction, attracted wide attention. It was, however, not immediately acceptable in this form, that is to say, it could not command final acceptance on the basis of these figures alone, as Lambert, of the Coast Survey, was quick to point out. His analysis of the observations showed quite plainly that the residuals which indicated continuous movement northward were of the same or a smaller magnitude than the errors in the observations themselves, and so, while they might offer promising evidence of continuous creep in the direction suggested by Lawson, unless other support could be found they were not in themselves conclusive evidence of such a northward movement. Accustomed by tradition to the highest standards of precision and thoroughly familiar with the treatment of such data the scientists of the Coast Survey indicated very clearly that such a conclusion could be true only when certain assumptions were made regarding the distribution of errors in the corresponding records from stations in Italy and Japan, where like observations were taken, and these assumptions appeared entirely arbitrary. Where the precision of the astronomical observations is limited to something of the order of a meter distance on the ground and other disturbing causes beside crustal creep are within the range of possibility, a continuous crustal displacement of a foot a year becomes a somewhat precarious conclusion, and I think from the time of the publication of Lambert's paper it has been generally admitted that the conclusion requires further proof.

Quite naturally those who had been startled by the sudden apparition of a crustal creep of a foot a year along a thickly populated section of the California coast region immediately pressed their inquiry whether or not some other agency beside the astronomers might not return a more positive answer to the somewhat disturbing question which had been raised, and so we consulted the Coast and Geodetic Survey regarding the possibility of checking up the primary surveys through which all our landmarks and boundaries are controlled. Primary triangulations had been made in California at three different periods, one early, one in the seventies and one after the earthquake of 1906, and these, if the available bench marks happened to be fortunately placed, should contain conclusive evidence of a continuous crustal movement as great as a foot a year. It appeared upon examination that the last of the surveys (1907), and the only one falling actually within the period of the Ukiah observations, had to do mainly with the displacement which accompanied the San Francisco earthquake of 1906. There you will remember we had an apparent north and south movement, respectively, on the two sides of the San Andreas fault, with practically no vertical movement whatever. This displacement amounted to as much as 20 feet in various places and the crack along which the displacement occurred could be traced for 190 miles. The reoccupation by the Coast Survey in 1907 of the stations on either side of the fault yielded measures of relative movement along the fault; it did not yield a true record of the absolute movement nor a conclusive answer to our question for a reason which was not noticed at the time when the survey was made. The base line to which those observations (of 1907) were referred was itself within the disturbed region. The pair of hills forming the Mocho-Diablo base line lay exactly between Ukiah and Mt. Hamilton, and if these two observatories were moving northward at the rate of one foot a year obviously the base line of 1907 was moving also. Accordingly, it became necessary to make a further effort not only to ascertain just what movement occurred in 1906, if that should still be possible, but to establish suitable bases for the determination of future displacements in this region which should not be subject to these uncertain limitations. The chief of the geodetic division of the Survey, Dr. Bowie, became interested in the problem and after some consideration a definite plan was made which received the support of the California delegation in Congress and an appropriation of $15,000 was made for the specific purpose of establishing positions in this region. Whether for the study of drift or tremors it was plainly first of all necessary to lay a network of precise triangulation over the region through which movements could be established beyond all doubt. Three seasons of this work have now been completed. It was sought first to establish points in the San Francisco Bay region, and in particular this Mocho-Diablo line, by reference to a more stable region farther back from the coast. Upon the advice of the geologists it was deemed sufficient to go back to Mts. Lola and Round Top, near Reno, for that purpose, and so the survey began at those points and a double set of triangles was measured to San Francisco Bay and thence southward. It was found at once that when referred to these fixed points (Lola and Round Top) the Mocho-Diablo line had moved from four to five feet to the south since the earlier surveys, so that although the direction of apparent movement did not coincide with the movement indicated by the astronomical observations, nevertheless the base line was quite untrustworthy as a permanent reference base for such a study as the one in hand.

The survey was continued in the second season from Mt. Hamilton southward as far as Santa Barbara Channel, where the work was temporarily interrupted by fogs and the surveying party was transferred to the Mexican Boundary whence it worked northward, leaving a gap at the close of last season amounting to about 50 miles. This season has witnessed the completion of the survey along the coast and its extension eastward so as to tie into the Colorado River Basin, the most stable region known in this part of the country. The northern end is therefore tied fast with Mts. Lola and Round Top, the southern end with the Colorado River Valley. The observations will be continued during this winter northward to include Ukiah.

Now I think it safe to assume that this set of triangles as now established will serve to define accurately in direction and magnitude any surface displacement likely to occur in the region under discussion, that is, from Point Arena southward to the Mexican Boundary, including all the territory to the west of the Sierra Nevada Mountains. Already it has established the fact that the Mocho-Diablo base line of 1907 is within the zone of movement; indeed, it has done much more than this, it has shown that there is general movement southward on the east side of the San Andreas fault and movement northward on the west side; that the movement on the west side of the fault is much greater than on the east side, amounting in the extreme case to 24 feet (northward) and, more astonishing still, this maximum movement is not within the region of the slip on the fault plane in 1906 (the great earthquake) but to the south of any movement then recorded. Nevertheless, it coincides in direction (northward) with the earthquake displacement of the disturbed region on the same side of the fault lying to the north of it. This is a revelation of the greatest importance and interest which is not yet fully elucidated.

Perhaps before proceeding to the elaboration of further plans for the study of these local movements I ought to say that of course it is one thing to establish the surface relations, that is, the means whereby movements either of accumulating strain or sudden release will be located, it is equally important to know the sub-surface relations, that is, the geology, and there this committee has enjoyed the cordial and effective cooperation of the two universities of California and the U. S. Geological Survey. From the Survey Messrs. L. S. Noble and W. S. W. Kew have allotted a considerable part of their time to the detailed geology of the southern region along the San Andreas fault. That cooperation has been invaluable, though I can not give you very specific information in regard to it in advance of publication, except such as is contained in the Fault Map of California, lately published by the Seismological Society of America. Mr. Noble has reported the progress of his work each year and presently the Geological Survey will publish it. It has already served to locate the active faults in the southern part of the state, which are believed to be associated with the great San Andreas fault in the north.

It is an especial pleasure to speak of the interest which has been aroused by this work and the freedom with which most competent cooperation has been offered. Last summer while in California I was told by Professor Tolman, of Stanford University, that his entire class had been sent into the field with the San Andreas fault as their summer problem. The University of California has given like cooperation in the region north of San Francisco Bay. It is interesting also and of the greatest importance, while we are speaking of the cooperation which we have everywhere enjoyed, that the Navy Department, when it became known that these faults were being traced on the land and that the displacements expected were considerable, very kindly undertook a survey of the ocean floor to the west of the region which we were studying in order to trace the extension of these known zones of structural weakness beyond the coast line. It happened that the Navy Department had available a sonic sounding device developed during the war. Two of these were mounted upon the destroyers *Hull* and *Corry* and about 30,000 soundings made, extending out from the coast line to 2,000 fathoms depth. That was as deep as it was deemed worth while to go at that time. The Hydrographic Office has since compiled and published a chart from these soundings which is known to many of you. It has now been published for nearly a year, and is the first attempt to prepare a detailed contour map of any considerable area of the ocean floor.

While I am speaking of the Hayes sonic sounder and its use, it is important to add that last summer one of these sounders was placed on the Coast Survey vessel *Guide*, in charge of Commander Heck, for the purpose of ascertaining accurately the limitations of the instrument and the constants upon which its precision depends. It had been estimated by the Hydrographic Office that the soundings of the first survey were accurate to about 5 per cent. On the *Guide* it was found possible to reach an accuracy of 1 per cent. by making proper corrections for the temperature and pressure of the water in which the soundings were made. Indeed, it is now possible to make depth determinations with this sounding device with as much or perhaps even more accuracy than the positions at sea can be determined, for position determinations beyond sight of fixed land marks are rarely closer than a mile. It is necessary for us to consider these two measurements side by side and to recognize that the sounding is only valuable in proportion to the precision of the position measurement which we can associate with it. That also is a problem which is now being studied by the Coast Survey with the same care which characterizes all its activities.

This contour map of the ocean floor was first printed as a chart by the Navy Department, as I have already mentioned; afterward, with the permission of the Chief Hydrographer, Captain F. B. Bassett, it was associated with the land-fault map and reprinted by the Seismological Society of America in March of last year. This map is now available for distribution at the office of the Seismological Society of America, at Stanford University, and represents one of the very considerable forward steps which have resulted from the cooperative activity which I am privileged to describe to you to-night.

These steps, as I have outlined them, will have given you a reasonably tangible account of the efforts which have been made, first to discover and map the zones of structural weakness, and second to prepare the landmarks through which to establish the direction and magnitude of future movements whenever they may occur there. It is particularly our purpose to learn something of the accumulating stresses, the creeps, if you like, from which eventually the earthquakes come. I think few of us who have studied this question at all can have any other idea in our minds than that an earthquake is a release of accumulated stresses which have strained parts of the structure beyond the elastic limit. If we approach the structure as engineers, we neither violate nor lose sight of any principle of geologic analysis. We may examine it as we would a steel structure, inquire into its age, its distribution of temperature, its loading, we may examine its joints, and its vibrations; indeed, there is an established routine for such examinations.

If we examine any considerable section of the west coast region as a structure we shall perhaps first recognize the fact that it is very badly loaded. Not far from the coast line and parallel to it lies the highest mountain range within our territory, the Sierra Nevada, with peaks rising more than 14,000 feet above sea level; a few miles out to sea the continent breaks off at the "continental shelf" to a depth of 12,000 feet or more below sea level—also approximately parallel to the coast line. Between these two there is a structure subject to this difference in load amounting to more than five miles of rock. It is surely not surprising that great faults are found between these two features and parallel them.

Also we have in California regions of very great temperature disturbances. There are hot springs in great number and many areas where the temperature of boiling water is reached but a few feet below the surface. These are regions of movement and of local instability. There are also anomalies in gravity here greater than are found elsewhere in this country, indicating perhaps that recent geological erosion has also contributed to the inequality in the loading. All these things point to the factors which will enter into any analysis of this problem considered as a study of structures subject to strain. Now if the structure is to be studied thoroughly and competently, it is not enough merely to have laid a network of absolute measurement over it to detect displacements, neither is it enough to have mapped exposures indicating the geologic sequence of strata. We must also do what we can to ascertain the effect upon this composite system, of variations in the dynamic relations involved. If it be true that the earthquake represents a major or minor rupture under local overload, then we must study the tremors in this structure as we would in any other, to locate the point of weakness. This, in our case, is not at all a question of securing a record of all those major tremors which affect the earth as a whole, that is to say, those which are as readily detected in Washington or Berlin, Italy or Japan, but rather the minor local tremors of short-wave length and 50 or 75 mile range, which carry the evidence of incipient break-down or perhaps of creep. Such tremors are common in California but have been little studied. Indeed, we have had no instrument which was properly sensitive to these short waves heretofore. And so one of the foremost tasks which this committee set for itself was to secure the design of a seismograph which would record these short-period local shocks—the same class of shocks, by the way, to the study of which Wood's original project was mainly directed. Wood, J. A. Anderson, of Mt. Wilson Observatory; Frank Wenner, of the Bureau of Standards, and Arnold Romberg, then connected with the Kilauea Seismologic Station, were brought together in Pasadena for conference. Out of this conference came a design for a vertical component instrument of Galitzin type with a period of two seconds which promises well but is not yet completed.

Independently of this development Dr. Anderson, with the cooperation of Mr. Wood, contrived a wholly different type of instrument, based upon the torsion principle, which I have already had the privilege of describing within the hearing of many of those here present. With this instrument, of which one component (NS) was set up at Mount Wilson Laboratory and one component (EW) at the California Institute of Technology in 1923, more than one hundred local earthquakes were recorded in the first six months.

Anderson brought with him the latest model of this instrument for the annual exhibit of the Carnegie Institution of Washington in December and it has been placed here for your inspection. Without going into descriptive details which have already been printed in the *Journal* of the Optical Society of America, the instrument has this advantage over any seismograph yet developed in that it may be readily tuned to be sensitive to any period likely to be encountered. The two instruments set up in Pasadena were arranged to operate with a free period of 0.8 of a second and a static magnification of about 1,200. The instrument before you has a like period and a magnification of something over 4,000. Without other changes than those of adjustment and the substitution of a somewhat differently distributed mass, the same instrument has also been used with a period of 15 seconds and low magnification corresponding with the teleseismic instruments in common use. In view of the interest of the Coast Survey in a teleseismic instrument, one of these seismographs, with a free period of 15 seconds and a magnification of about 300, was placed in continuous service in Pasadena a year ago. Its records were most satisfactory.

In theory and in practice, in adjustment and in adaptability to all purposes, this instrument is extremely simple, is as portable as a galvanometer and for the purposes of this investigation promises to be most effective.

The original instruments have now been in use about two years. The only additions so far made have been: (1) Provision for adjusting the magnet up or down with reference to the steady mass, in order to provide overdamping, critical damping or any desired fraction of the latter; (2) a foot-screw several inches in front of the suspension in order to provide either accurate vertical adjustment of the suspension or a measured departure from the vertical as may be desired; (3) a mirror was substituted for the lens in the path of the light beam in order to double the magnification by means of a second reflection from the moving mirror. It is now possible to obtain a static magnification of four or five thousand. One limitation has developed in the seismograph. In case of a considerable earthquake, with an instrument of high magnification recording photographically, a very wide and rapid excursion of the ray of light is to be expected and its trace, in consequence, will be somewhat faint. An extremely intense source of light was therefore employed, and reduced to about one fifth of its normal intensity by a rotating sector. The record then consists of strong dots which, for normal recording, form a continuous line, for a strong earthquake a discontinuous but well-defined curve. This arrangement has served the present purpose satisfactorily, but a simpler device may be found to accomplish the same purpose. I hope those of you who are interested will take the trouble to examine this instrument. It is proposed to equip a number of stations with them during the present year.

This plan of primary triangulation, then, has given us a control upon surface displacements which I think has never been surpassed before in any region of equal extent. The geological study of the region and the submarine soundings have given us much information regarding its structural weaknesses (faults). Now by the use of this instrument we cherish the possibility of establishing the sources of tremors and the direction of their path along and across the zones of weakness. Already some opportunity has been found to test this mode of analysis. There have been two heavy discharges of explosive in California, one of 182 tons of dynamite, within about forty miles of Pasadena, and in both cases a very precise record was obtained. Nothing was known in advance about the first case, but in the second it was possible to obtain very accurate timing and an excellent measurement of the rate of transmission of the wave between the site of the blast and the recording instrument. It is therefore believed to be quite possible, by the use of these instruments, properly distributed, to locate the probable sources of most of the tremors in California, whether they originate near the surface, or, as we suspect in the case of the 1906 earthquake, the displacement had its origin at considerable depth.

It is in our minds also to determine tilt. Those of you who have followed the work of Professor Jaggar know that for a number of years he has been making careful observations of tilt on the flanks of the great volcano Mauna Loa and on the crater-rim of Kilauea, on the island of Hawaii, to see whether, from the variation in tilt, he can predict volcanic outbreaks. So far I think he has not altogether succeeded in that; or at least he has made use of other evidence in addition to tilt in the predictions he has made hitherto. Be that as it may, he has shown that tilt is definitely measurable. Now if the bulging of the mountain can be measured then obviously certain other kinds of "creep" can be measured, and it is definitely a part of our purpose to develop apparatus in California through which these slow, cumulative displacements may be determined both in direction and in magnitude in the same fashion as it has been possible to obtain a measure of the bulging of Mauna Loa in Hawaii. If we succeed in attaining that and have at our disposal competent laboratory determinations of the elastic limitations of the participating rock types, we shall presently be in a position to establish the degree of probability of release of such cumulative stresses by earthquake. I am speaking now of prediction in place rather than of prediction in time. It is not unlikely that prediction of the probable place of release of such strain can be undertaken with reasonable certainty in future. Prediction of the probable time of rupture involves factors which are not yet within our reach.

It is also a part of our purpose to measure internal pressures. It is not difficult to arrange an apparatus which will give a continuous record of deep-seated pressures which will also be of value in the elucidation of these structural relations.

If I have been successful at all I have outlined to you what is probably the most comprehensive plan that has yet been developed in the study of earth movements. It is characterized, more than anything else, by the effective cooperation between research organizations with widely different points of view. I have named the agencies, all well known to you, which are participating with us in this particular enterprise. The Coast Survey and the Hydrographic Office, approaching the problem from their respective viewpoints, have given us a trustworthy system of coordinates upon which displacements are to be plotted and relations shown; the Geological Survey and the universities of California have added the geology and subsurface relations; and finally the California Institute of Technology and two departments of the Carnegie Institution of Washington, Mt. Wilson Laboratory and the Geophysical Laboratory have aided in the development of appropriate apparatus, the establishment of stations, etc., in preparation for the systematic study of tremors. The trustees of the Carnegie Institution of Washington have set aside certain funds for the project, and the advisory committee in seismology has undertaken the administrative responsibility. Such a number of vigorous agencies, thus brought together, can not fail to accomplish a great deal more than any one could do alone; such a representative group of agencies, I believe, can not fail of success.

ARTHUR L. DAY

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THE SECOND GENERAL ASSEMBLY OF THE INTERNATIONAL GEOPHYSICAL UNION

THE second general assembly of the Union took place in Madrid, beginning October 1, in the build-