ON THE OBSERVATION OF AN EARTHQUAKE AT THREE OR MORE STATIONS TO DETERMINE SPEED AND DIRECTION OF TRANSIT.

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Recent seismographic observations have conclusively established certain characteristics of earthquake motion which have an important bearing on the problem which gives the title to this paper. We now know that many, perhaps all, earthquakes consist of a long series of movements. Often these begin gradually, and many seconds pass before the maximum motion is reached. In some cases no one of the series of impulses stands prominently out from the rest, as greatly bigger than those which precede or follow it, but there is a gradual rising and falling off in the motion, succeeded, it may be, by other equally gradual risings and fallings before the disturbance wholly ceases. Some earthquakes do indeed show a suddenness in their beginnings, but this character is far from universal, and even in these there are usually subsequent movements which rival the first in amplitude. The movement of the ground continues for one, two, three, perhaps more, minutes. In a word, the term "shock" however well adapted it may be to name the phenomenon at its origin, or to express the subjective effect of an earthquake on persons who perceive it, is singularly inappropriate as a description of the disturbance which we observe at a distance from the source. That is no sudden, isolated, sharply defined event:—it lasts long, and it begins and ends so gradually that we cannot trace the beginning or the end.

We need not then attempt to specify the time of the occurrence of an earthquake in any smaller unit than the minute; and even our observations of when the earthquake was *first* felt will be liable to errors of no small magnitude, depending on the delicacy of the means we use for detecting its arrival. If for instance we use a clock which is to be mechanically stopped, the time it shows will depend on the sensibility of the stopping mechanism, and two clocks, however nearly alike in their construction, will very probably be stopped by different impulses out of the many which make up the so-called shock. Hence if we use clocks of this kind for finding the difference in the times of arrival of the earthquake at different stations, we are liable to errors, due to what may be called the personal equation of the stopping mechanism, of magnitude greater perhaps than the very differences we wish to measure. It is true that by increasing greatly the distance between the stations we make any given error relatively less important, but on the other hand, the character of the complex system of waves will be more liable to be different at the different stations, and hence the error itself may be much increased.

The same objection applies equally when the difference in time is given by electric signals sent automatically on the arrival of the "shock" at the several stations and recorded by a chronograph.

In an accurate observation of the speed of transit of an earthquake we must be able to identify the same wave in the disturbance as it appears at the different stations: not only this, but we must be able to identify *the same phase of the same wave.*

I propose to do this by putting up three, or if practicable four, of my recording astatic horizontal lever seismographs (recently described to the society) at a corresponding number of stations not very distant from each other. The directions of the recording levers at all the stations are to be the same. Each seismograph will have its own independent clock, and it is of no consequence whether the speed of the clocks is the same, so long as each runs with a fair degree of regularity. So far, the arrangement will give three or four independent and approximately similar records of an earthquake. To complete the scheme, it is only necessary to mark time on all the revolving plates during the disturbance. This I purpose to do by having a pendulum at one station which will be released during the earthquake and will, at each tick, send an electric time-signal which will be recorded on each plate. The pendulum will be provided with an escapement and driving drum arranged so that after making any desired number of ticks it shall come to rest. This is in order that the time marks on the plates may never extend over so much as one revolution. It will therefore be easy to distinguish the first of these marks, which will show the relative positions of the plates at the same instant of time, while the succeeding marks will give a means of finding the velocity of each plate. The fact that the first signal is not contemporaneous with the beginning of the shock is of no consequence: it will be given some time during the disturbance and that is enough. The data will enable us to see how far each plate was ahead of or behind the others when *the same phase of the same wave* was recorded upon each.

The distance between the stations should be large enough to give a time interval capable of fairly exact measurement by the method just described; but on the other hand it should be so short as to make it unlikely that the form of the waves will differ much within the area of the experiment. To this consideration must be added that of the greater labour which distant stations would involve in their supervision. I am of opinion that distances of 1000 or 1500 feet, which may be expected to give time intervals of about one second, will amply suffice. Professor W. S. Chaplin, to whom I am indebted for many valuable suggestions in this matter, has urged the use of four rather than three stations. The distance just named would of course be far too small to give, with four stations, two directions converging to the seismic centre, but it is quite likely that the check on accuracy furnished by a fourth station would be worth the additional expense and trouble.

The same method of experiment may be advantageously applied to determine the velocity of transit of an artificially produced disturbance. In that case the direction of transit is known (being, in a homogeneous medium, the straight line joining the observing station with the source) and the receiving instrument may be set so that one of its levers will be at right angles to the direction of transit, and record the normal wave. The other lever, perpendicular to the first, will record the transverse wave, if any appreciable transverse wave exists. If so, the velocities of both transverse and normal waves can be determined. I do not expect however that any appreciable transverse wave will be found to exist in the soil on which Tokio stands. We know that the upper stratum of that soil is saturated with water to within a few feet of the surface; and it appears scarcely possible that it can have much transverse elasticity. In the earthquake records which I have hitherto obtained I have not succeeded in detecting any trace of transverse waves appearing later than the normal waves. It has been suggested, and it is of course conceivable, that what we observe are the transverse waves, and that the normal waves are not sensible. This hypothesis is highly improbable, but in any case the experiment described in this paper if carried out will answer the question, since it will show whether the direction of oscillation of a surface particle is in line with or at right angles to the direction of transit.