*EXPERIMENTS IN OBSERVATIONAL SEISMOLOGY**

BY

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The following paper is an account of various experiments which I have made during the last three or four years in connection with the observation of earthquakes. Nearly all the experiments have been made with instruments of a very simple kind. Many of these are well known forms of seismoscopes and seismometers, and one of my chief objects has been to see how far these older forms of instruments were adapted to the registration of the earthquakes so often felt near Tokio. Besides speaking of these older forms of apparatus, something is said about the trials of instruments which are new and the results which have been obtained from them.

Amongst the various contrivances used for indicating or recording Earthquake movements, pendulums occupy a very important position. Mr. Mallet when referring to pendulums speaks of them as "the oldest probably of seismometers long set up in Italy and Southern Europe."

The pendulums here referred to are probably those which at the time of a shock were caused to oscillate. By recording this direction of oscillation, as for instance by causing a stile from the pendulum to cross a bed of sand, the direction of the shock was obtained.

Besides pendulums of this description which at the time of an earthquake act by moving, others have been designed to

* *This paper may be regarded as being to a great extent the history of a series of investigations which led to the use of the instruments which I now employ for recording and measuring Earthquakes, an account of which before long I hope to lay before the Seismological Society. J. M. Feb. 1882.*

act by remaining still. The idea of having at the time of the earthquake a steady body suspended above the moving earth on which we could write the motions of the moving ground, is by no means new.

In connection with pendulums of this description, we read of what are apparently actual motions of the ground having been registered.

As these are never more than half an inch, some are only $\frac{1}{16}$, whilst the shocks which gave these indications were sufficient to shatter buildings, it is probable that these measurements dependent on the inertia of pendulums are very close approximations to the truth.

These measurements were not made by a single instrument, but by a number of instruments distributed over the districts round Comrie in Scotland.

Among the Reports of the British Association for 1841, we find the report of a committee which had been appointed "for obtaining Instruments and registers to record shocks of Earthquakes in Scotland and Ireland."

In this report 2 seismometers are described, 1st a common pendulum seismometer, and 2nd the inverted pendulum seismometer. Two records are given in which it is clearly inferred that the ground moved relatively to the pendulum. To quote the exact words, the writer of the report says "The amount of displacement of the ground was, on the first occasion, half an inch; on the second less than half an inch." In another record we read, that "the seismometer in Comrie parish church had its points thrown half an inch to the west, which indicated, therefore a horizontal movement of the earth towards the east."

In the Report of the same committee in 1842, amongst references to a number of instruments which were being tried, we find the following; a horizontal bar loaded at one end is attached to the wall by a strong flat watch spring. "If the wall suddenly rises or sinks the loaded end of this horizontal rod remains, from its *vis inertia*, nearly at rest, and thus can move any light substance (as paper or a straw) brought against it, by the vertical movement of the ground." &c.

As another example of a pendulum which was designed to remain steady at the time of a shock, we may take the instrument of Prof. Cavallieri (Philosoph: Mag: IV Series Vol. 19 p. 102, 1860). Here a bed of sand is supposed to move beneath the stile of a pendulum which therefore gives a mark "equal to the back and forth motion of the earth." With pendulums of the descriptions which have been mentioned, spiral springs loaded at one extremity, which are generally intended to tell us something about the vertical motion of a shock, together with vessels containing liquids which have a pendulum like period of oscillation, might also be included.

For convenience these different pendulums may be divided under the following heads.

1st weights suspended by threads or wires. These are of two kinds. First those which at the time of a shock are supposed to remain at rest, and secondly those which are supposed to move.—The first I will call *Stationary Pendulums* and the second *Movable Pendulums.*

2nd weights supported on the end of springs like the watchmaker's noddy—These also are of two kinds. Namely, those which have been designed to remain at rest, and those which have been designed to move—These may be placed either horizontally or vertically. They may all be included under the head of *Inverted pendulums.*

3rd *Spiral spring pendulums.* These also may be designed either to remain stationary or to be set in motion at the time of a shock.

4th Vessels of liquid which by their oscillation tell something of the direction and the intensity of a shock. These may be called *Fluid pendulums.*

1. Stationary Pendulums.

In the construction of certain classes of seismometers, it has been assumed that at the time of a earthquake the bob of a properly suspended pendulum is practically a steady body.—If a pointer moved by the shaking earth is caused to mark upon this pendulum, or vice versa, if a steady pointer fixed to the pendulum marks upon the earth which is moving under-

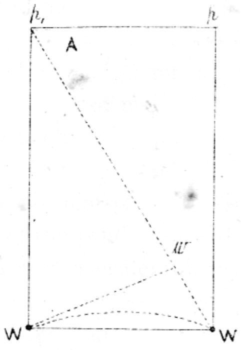


Fig 1

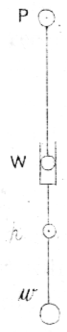
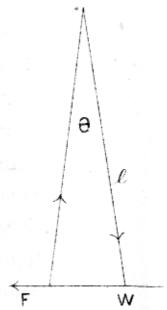


Fig 2



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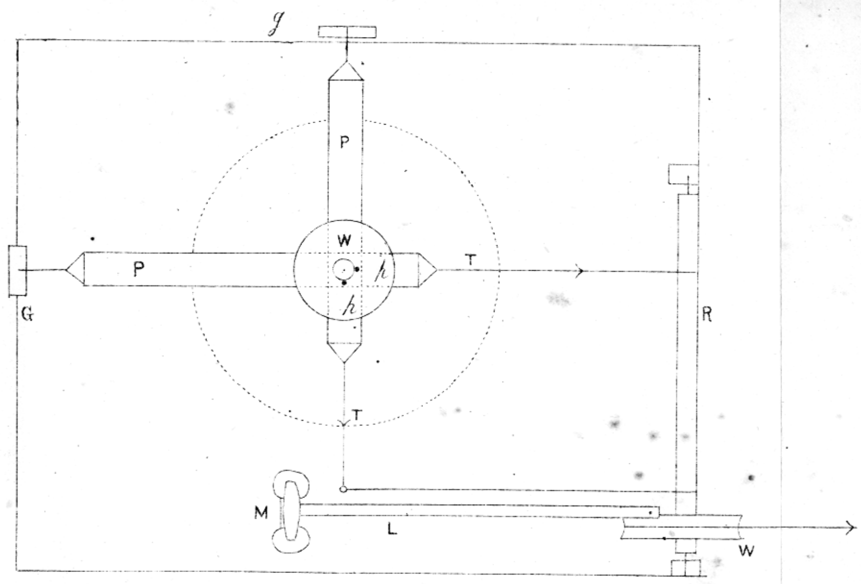

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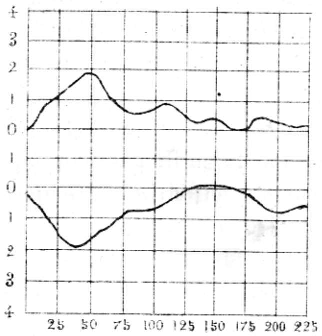

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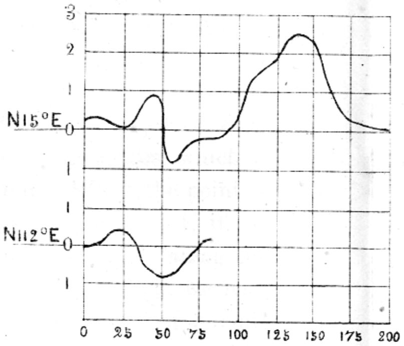

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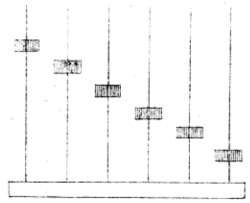

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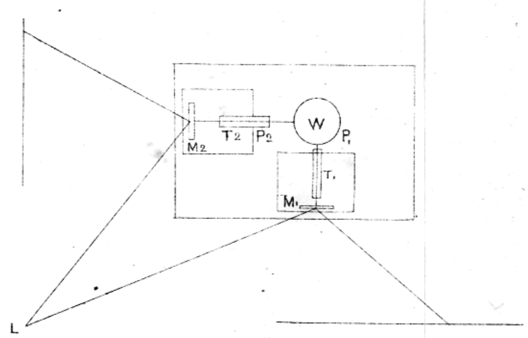

Fig 8

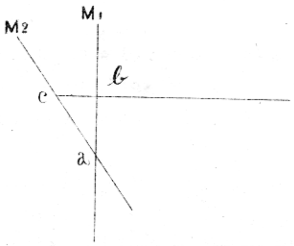

Fig 9

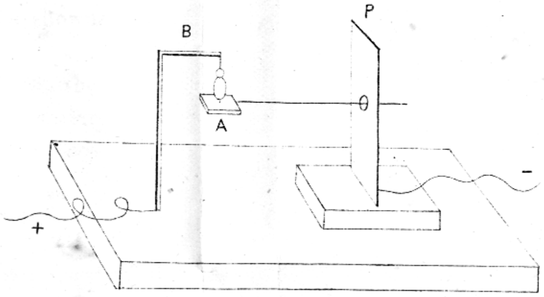

Fig 10

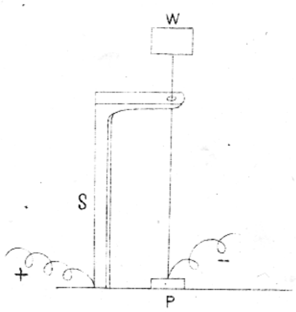

Fig 11

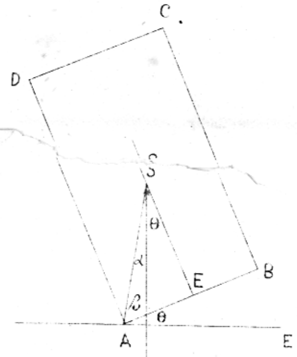

Fig 12

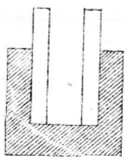

Fig 13

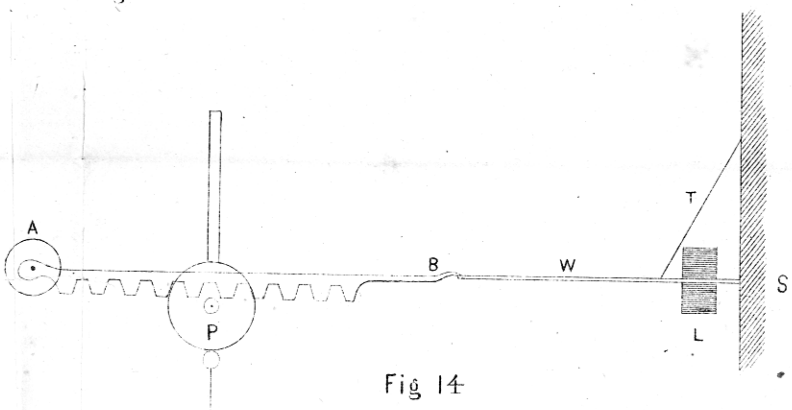

Fig 14

neath it, we shall in either case obtain a record of the earths horizontal motion. The correctness of this record will chiefly depend upon the steadiness of our pendulum. If the amplitude of an earth particle at the time of an earthquake is great, and the extent of motion in the registering pendulum is small, the results obtained may be practically very near the truth. If however the motion of an earth particle is small, say for instance not more than 2 or 3 millimeters, then a very small motion of the pendulum will introduce what will be comparatively a very considerable error in the records.

The question as to how far a pendulum remains steady at the time of an earthquake shock, may be approached in three directions,—theoretically, by actual observations on different pendulums at the time of an enrthquake, or by experiment.

Theoretical Considerations. In considering this problem theoretically we have evidently to regard two distinct elements, 1st, how far motion may be expected in the bob of our pendulum in consequence of a movement of the point of support, and secondly, how much motion may be expected from the pushing or pulling of our registering apparatus, which is in connection with the moving ground. When the point of support of a pendulum p is shifted to p_1, see Fig. 1, if the motion is slow the weight will travel from W to W_1 along the line W W_1. If the motion of p to p_1 be rapid, then we may imagine W to be suddenly pulled along a curve of pursuit towards a position w from which it will fall in an arc to the position W_1. For any velocity of the point p intermediate to those of the two cases just considered, the weight in travelling from W to W_1, will describe some such arc as the one shown in dotted lines.

By a shifting of p to some point p_2 towards the right, the weight of the pendulum W would tend to assume smilar positions to those we have considered when the motion takes place towards the left.

In the case of the pendulums we are considering the shifting of p to p_1, will be much more rapid than the motion of W to W_1, and therefore before the whole of the path between W and W_1 has been completed the point of support will have reached p_1, and returned towards p.

Assuming what I have said to be true we may now take the particular case of a pendulum 1 metre in length which we will suppose has its point of support p suddenly moved back and forth through a distance of 10^{mm}.—The time taken to perform a single oscillation we will take as $\frac{1}{5}$ sec, a period which is probably not very far from the period of certain earthquakes.

The distance from p to p_1 will be 5^{mm}, and the time taken to move this distance will be $\frac{1}{10}{}^{sec}$. The distance moved by W whilst p travels from p to p_1 and back to p, which will occupy about $\frac{1}{5}$ sec, will be about the same as if the point of support had been stationary at some point A nearer to p_1 than to p. Assume that this distance $A.p = 3^{mm}$

The question then is, how far will a pendulum one metre long swing in $\frac{1}{5}$ second. In one second it will swing to W_2, when $W_1 \; W_2 = 6^{mm}$. Therefore in $\frac{1}{5}$ sec it will swing about 2^{mm}

As this motion has also to be considered in a direction to the right of p, this quantity must be doubled, and we thus get a motion of 4^{mm}.

To bring this motion to a minimum we must lengthen our pendulum, give a less period than $\frac{1}{5}$ sec for the swing of the earthquake. The only reason that I have in bringing forward this illustration is to shew that when the point of support of a pendulum such as those which have been, and are continually being used for earthquake measurements is moved, the bob does not necessarily remain, in consequence of its inertia, absolutely at rest—and its motion may possibly be considerable. For the more complete solution of this question when the motion of the point of support of a pendulum being given to determine the motion of the bob. I will refer to a paper on a new seismometer by Mr. Thomas Gray (see Transactions of the Seismological Society Vol. I. Part I. p 47.) How far the tendency which a bob of a pendulum has to follow its point of support may be prevented, is a matter for investigation.

In Dr. Wagener's Seismometer we have a portion of the registering apparatus working in contact with a heavy pendulum in a manner very similar to that in which the upper part of a small pendulum is caused to work against and check the motion of a pendulum, which is sometimes used for the purpose of measuring the rolling of ships.

This arrangement is somewhat as follows. In Fig 2, P is the point of support of a pendulum with a bob W. This bob or a point connected with it slides in a joint fitted to the upper end of a small pendulum with a bob W and a point of support at *p*.

In the rolling of a ship when the period of roll is slower than the period of the pendulums W and *w*, each tend to keep in a vertical line beneath their respective supports P and *p*. And as a consequence of this the upper part of the stiff lever like pendulum *w p* tends to check the motion of W.

In an earthquake however on account of the period of the earthquake being quicker than the period of the pendulums these conditions are changed.

First we shall observe that because at the time of an earthquake, W is on account of its inertia the steadiest point in the system, because *p* is moved in the same direction as P (which we will suppose is towards the right), *w* will be lifted to the right and thus form a couple tending to push W also to the right. That is to say the tendency of a contrivance like the above is during a single vibration to produce motion in W in the same direction as it tends to fall when following its point of support.

The general conditions govering a system like the one we are considering may be expressed as follows.

Let M.E = the moment of inertia of W round P and
m.e. = the moment of inertia of W round *p*.

1. Then if M.E = *m.e*, then
   P.W. *p* and *w* will move as a whole. If *p w* be the long arm of the lever intended to magnify the motion of the earth, in this case the magnification = 0.
2. If *m.e* < M.E then
   W will be lifted and the effect of this will be a tendency for W move in the same direction as *w*. In this case the magnification will be less than it should be.

3. If $m.e. > M.E.$ then
W moves more than w and the record is negative. And the magnification is as in case 2 beneath its true value.

So far it will be observed that only one portion of the causes tending to produce motion in the bob of a pendulum at the time of an earthquake have been considered, there yet remaining for investigation the forces acting directly on the pendulum by the registering apparatus in connection with the moving ground.—The simplest registering apparatus with which I am acquainted is a needle or fine steel pointer dropped though a small tube fixed on the pendulum,—the end of the pointer resting on a smoked glass plate which moves back and forth with the motion of the ground.

When the plate moves the friction of the pointer will tend to drag the weight of the pendulum along with it. Any other recording apparatus exerting a direct pull upon a pendulum, as for instance threads with pulleys and pointers, can not I think exert a less tendency in moving the pendulum bob than the friction caused by the weight of a needle sliding on its point across a surface of glass.

If in Fig 3, W be the bob of a pendulum and its length l, the force F required to deflect it a small distance as shewn in the figure will be $\frac{W}{l}$

Therefore for example if $W = 50000$ grammes (50 kilos)
and $l = 10{,}000^{mm}$ (10 meters)

$$f = \frac{50000}{10000} = 5 \text{ grammes.}$$

If $W = 5$ kilos. then $f = \frac{1}{2}$ gramme.

From such an example it is seen that the force required to deflect a long pendulum is exceedingly small.

Experimental Investigation. In oder to determine the extent to which it is possible by means of registering apparatus to move the bob of a heavy pendulum the following experiments were made.

The pendulum which was first employed was one belonging to a seismometer designed by Mr. Thos. Gray. This was a pendulum 60^{cm} in length with a short cylindrical bob about 18 lbs in weight. From the centre of the base of this bob 2 threads are fastened, the other ends of which pass round extremely small pulleys with very light pointers.—These threads are at right angles to each other. When these threads are simultaneously pulled tight by turning the pointers, it was expected that the weight would be pulled in the direction of their resultant, and on account of the friction of the pulleys and weight of the thread it would then be held in a position so that the wire which suspended it was no longer in a vertical line.

A pointer was then placed on the pendulum in a direction parallel to the resultant of the two threads and a point on this was carefully looked at through a microscope magnifying about 25 diameters. As a result it was found that the direction or extent of pull of the pendulum from its normal position when the threads were tightened was barely visible.—It may possibly have been $\frac{1}{20}$ of a millimetre.

A second experiment was to fix a small glass tube on this pendulum. Through this a sharpened small steel wire was dropped so that its point rested on a glass plate beneath.

By a slow motion of this plate beneath the pointer the pendulum was found to be pulled from $\frac{1}{10}$ to $\frac{1}{20}$ of a millimetre from its normal position.

By a very quick back and forth motion through about a centimetre trembling motions were produced in the bob the amplitudes of which were about $\frac{1}{20}$ ^{mm}.

By making the back and forth motion about the same as the period of the pendulum, in 10 or 15 seconds the pendulum got up a swing of 2 millimeters. In a third experiment the pendulum which was used was more than 30 ft. in length and carried at its extremity a cylindrical bob of about 80 lbs.

Fastened to the base of this there was a small glass tube and a sliding fine steel pointer—this latter resting by its weight on the surface of a piece of smoked glass. By gently sliding the smoked glass under the pendulum it was found that it might be permanently deflected to the extent of $\frac{1}{10}$ millimetre.

If the tube on which the smoked plate rested was gently tapped the weight came back to its normal position.

If the plate were moved quickly or continuously in one direction for a distance of about 1 or 2 centimetres a deflection of ½ a millimetre was produced—I may here also remark that by gently blowing at the pendulum a deflection of ½ a millimetre was obtained whilst by blowing heavily a deflection of 2^{mm} could be observed.

Some time ago I made several rough experiments upon a long 30 ft. pendulum which is hanging in my house, to see how far it was moved, first by a deflection of the point of support, and secondly by a movement of the registering apparatus which was a smoked glass plate on which a fine steel pointer sliding through a tube fixed on the pendulum was resting.

The point of support was moved by shaking the wire near its upper end back and forth for about 10 sec through a distance of about ¼ or ½ an inch—In consequence of this it did not appear that any motion was produced in the bob. The other experiment which also seemed to be without any effect upon the bob, was to slide the glass plate very quickly underneath the pointer which rested upon it.

Although in both these experiments I did not perceive any motion in the bob of my pendulum, had the experiment been conducted in the same manner as those just described it is possible that slight movements might have been detected.

From these experiments we see that with a short heavy pendulum even when we use registering apparatus of extreme delicacy, the frictional resistances they possess is sufficient to defect a pendulum $\frac{1}{20}^{mm}$, and therefore may possibly occasion an error of this amount in some of our measurements of earthquake motion. When the pendulums are long notwithstanding their bobs being large, the probabilities are that the error arising from similar causes will be much greater.—And finally in both long and short pendulums when the period of the earthquake is near to that of the pendulum or is any multiple of it the errors in actual measurement of earth motion may be very great.

In most seismometers the motion caused by the registering apparatus will be greater than in the examples which have been experimented on.

We thus see that we have two causes each tending to produce motion in a pendulum at the time of an earthquake, in the one case by communication of motion through the point of support, and in the second case by motion obtained through the registering apparatus.

If the period of the earthquake motion is synchronous with the natural vibrations of the pendulum, a swing may be readily produced; also a pendulum with a period which was of such a length that its period was nearly a multiple of the length which gave a period equal to that of the earthquake, would also be in danger of being set in motion.

And therefore it is quite possible that a somewhat short pendulum might be better than certain long ones. The chances are however against such an occurrence—and a long pendulum might often be better than a short one. A disadvantage of the long pendulum however is that is more easily set in motion by the registering apparatus than the short one.

Results of Experience. From the theoretical considerations and the results of experiment, both of which seem to indicate to us every possibility of an ordinarily suspended pendulum getting up an oscillation at the time of an earthquake, we may turn to the actual results of our experience at the time of an earthquake. For the last two years I have had a number of pendulums varying in length from 1 to 36 feet suspended in my house, for the purpose of experimenting on earthquake motion. These together with others which are suspended in the buildings of the Engineering College I will describe in detail farther on.

One pendulum is 36 feet long and is suspended from the top of a lightly constructed tower in the center of my house which is framed of wood.

On several occasions during the last two years, on account of this pendulum being in my house, I have had opportunities of closely inspecting it whilst it was being shaken by an earthquake, and several times within a few seconds of the cessation of the earths motions. On none of these occasions did I perceive the slightest horizontal motion. There was sometimes however a strongly marked vertical motion the suspending steel wire working up and down like a spiral spring.

This I took to be due to a scissor like opening and shuting of the joints of the tower, thus raising and lowering the point of support. These earthquakes however were small ones, being under 10° as registered by Palmieri's tubes of mercury.

Had the pendulum been entirely free, from recent observations upon a second pendulum about 20 feet long which is also hung in my house, I am of opinion that a visible motion would have been produceed. Three other pendulums which were suspended in my dining room for over a year have also at the time of earthquakes never been observed to shew any motion. The dimensions of these pendulums are given on page 27.

One reason why these pendulums should at the time of an earthquake have remained practically steady whilst heavy lamps would often swing through a small arc, appears to be, that in the case of the lamp there is more friction by an iron hook at the point of suspension, than there is at the point of suspension of a properly constructed pendulum. Another reason why the motion in the pendulums if such motion existed should have been too small to be seen, will apparently find its explanation in the want of rigidity in the framework forming the support the point of suspension.

At the time of a large earthquake, as for instance that of Feb. 22d or even a shock like that of the 27th April, all pendulums whether long or short, whether their points of suspension were on rigid frames or frames the joints of which were loose, were set into visible oscillation.

At the time of the February shock the oscillations were violent. The direction of them I have given in an account of that Earthquake. I may remark that the oscillations of long heavy pendulums was on the 27th April observed by many of the officials connected with the Kobu Daigakkō.

Quite recently I have been informed by Prof. J. A. Ewing that the oscillations of his pendulum seismometer have recorded themselves upon his registering apparatus.

The conclusions which I come to upon this point are the following.

1. That at the time of an earthquake all pendulums have more or less motion. In certain cases dependent on the nature

of the pendulum, the frictional resistance of the registering apparatus, the rigidity of the framework carrying the point of the support and the character of the earthquake,—the motion of the bob of the pendulum may be so small ($\frac{1}{10}$ or $\frac{1}{20}$ of a millimetre) that the correctness of the record dependent on the steadiness of the pendulum is but slightly influenced. If a pendulum be provided with proper frictional resistance so as to render it *dead beat*, it seems possible to employ it very successfully in the recording of ordinary earthquakes.

2. In great or long earthquakes the errors may be so great as to seriously interfere with the correctness of the record.

3. Rigid frameworks for carrying the point of suspension do not protect the pendulum from acquiring a motion.

4. The effect of specially contrived loose framework to carry the point of support of a pendulum with certain slight frictional resistance below, may possibly be a better protection against motion in the bob than rigid frames. My chief reason in speaking of the motions of pendulums at the time of an earthquake, is to shew how far my experiments made with pendulum apparatus to determine the actual horizontal extent of earthquake motion may be in error. These experiments were as follows.

I.

These first experiments which I wish to describe were made with a pendulum suspended from the upper central portion of a tower like structure built in the central portion of my house.—The bob of this pendulum is a conically shaped mass of lead 1 foot long, weighing 31¾ lbs. The width of the base is 4 inches and the apex 1 inch. From the latter end it is suspended by a thin piano forte wire 36 feet in length. The top of this wire is made fast by being passed through a small hole drilled in a plate of iron, which latter is screwed to the timbers of the tower.

Driven into the centre of the base of this weight there is a bar of wood about 6 inches long on to the lower end of which a small glass tube about 4^{mm} in diameter is cemented. When the weight swings this tube comes within about $\frac{3}{16}$ of an inch of a board fixed on the top of a stake driven firmly in the ground. This distance allows a small smoked glass plate to be placed underneath the tube. The glass plate being in position, a pointed piece of wire of about the same diameter as the interior of the tube is dropped through the tube so that its pointed end rests upon the plate. This arrangement admits of vertical motion taking place in the weight without removing the pointer from the surface on which it is to mark.

When the pointer is thus placed in position I find that I am able to rapidly slide the glass plate about upon the board, the pointer meanwhile tracing a pattern upon it corresponding to my motions, without giving any apparent motion to the weight. This movement however can only be continued for a short period say 10 or 15 seconds. The results that I have obtained from this arrangement, which was first used in the latter part of 1878, although few, have in certain cases been very decided.—Some of the more important of these which have been recorded, results such I believe being free from errors due to the action of wind on the pendulum, carelessness of adjustment or other causes, have been given in a paper read before this society entitled "Seismic science in Japan" (see Transactions of the Seismological Society of Japan Vol. I). In the case of small earthquakes, that is shocks recorded by Palmieri's instrument as being below 1°, no measurements were obtained. An earthquake of a higher degree of intensity and in all cases where it was as much as or over 10°, left a very decided mark upon the smoked plate. These marks varied in length from 1 to 4^{mm} and I regard them as being *very close approximations to actual earth motions.* As a general result of these observations I may say that the method here adopted is unfit for general use, the records requiring magnification.

The only merit which the results may possibly claim appears to be the directions which they have given,—these being more definite than those which are usually given by seismometers. Assuming that the horizontal movements of the ground indicated nearly to represent the true movements of the earth, it would seem that the records of intensity as given on Palmieri's instrument are not proportional to the actual movements of the ground.

II

The following is a record of an attempt to determine the horizontal motion of an earth particle during an Earthquake at different intervals of time.

The pendulum used in these experiments was the one which has just been described.—The arrangements which were attached to the pendulum for these new experiments will be understood by reference to the Fig. 4. where W is the base of the pendulum bob to which there is fixed a spindle *w* carrying two small glass tubes with sliding pointer *p p*. These pointers rest upon the surface of two strips of glass which at the time of the earthquake are pulled by the threads T T and thus caused to move in directions at right angles to each other.—The pulling of these threads is caused by their being wound upon a roller R which forms the axle of the wheel W.—The wheel W is made to turn by a falling weight. Until an earthquake occurs, W is prevented from turning by a small pointer fixed at the end of the lever L which presses down upon its rim. At the time of an earthquake, by means of a mercury cup similar to that which is used in Palmieri's apparatus, an electric circuit is closed, and the electro-magnet M pulling upon the short end of the lever L raises the opposite extremity and frees W. To cause the glass plates which travel on a bed of glass, to move steadily, at the back of each of them there is a small thread and a hanging weight.

The roller, lever, magnet and sliding plates which are smoked, are all carried on a small table which is firmly fixed on the head of the stake S, driven deep into the ground.

On account of the frictional resistances, the time taken by the plates to travel a distance equal to their length, is about six seconds, and the speed at the commencement of the motion is not appreciably different to that when near the end. If the plates were allowed to run beneath the pointers when there was no earthquake, two straight lines were invariably produced.—If the post S were shaken whilst they were in motion, it was possible to obtain a series of ripples.

Experiments were made in order to determine, 1st, whether it was possible for the irregularities in the construction of the machine to cause ripples, and 2d, if vibrations of the post were registered at the time of the earthquake—they might be recognized by their appearance.

In consequence of a want of magnifying arrangements in my registering apparatus, the records I have obtained have in many instances been little more than straight lines.—In the case of large earthquakes however, the deviations from a straight line have in several cases been sufficiently great to allow me to magnify and plot the horizontal motions of an earth particle as recorded upon my smoked glass plates.

The followings figure are examples of such observations.

In the next two figures the actual record has been increased five times vertically, and diminished five times horizontally

Fig. 5 represents the shock of Dec. 3rd 1879 at 7. 8. 0. A.M. On Palmieri's instrument it was recorded as having the following intensities WSW 18°.30′ SSW 15°.20′ WNW 12°. 40′ SSE 9°.50′. The duration was 1 minute 50 seconds

Fig. 6 is a record of the shock of Feb. 22nd 1881. which is described in the Transactions of the Seismological Society Vol. I Part II.

In addition to the experiments just described, similar experiments were made with a wire pendulum hanging in the natural philosophy lecture room of the Imperial College of Engineering.—The length of this pendulum is about 40 ft. and its weight about 80 ℔s. At its lower end it carried a light sliding pointer resting on a smoked glass plate about 1 ft. 6 in. in length which at the time of the shock was drawn endways by the clockwork of a Morse's Recorder. This was set in action by the completion of a circuit by a method similar to that which is seen in Palmieri's instrument. During the time that this arrangement was at work only one *good* record was obtained. This was for the Earthquake of July 19th 1880.

The most important results which appear to be shewn by these experiments are, first, that the *earthquake motion is not* *regular,* that is to say that the motion is not harmonic, and secondly, that the rate of vibration of the shock is usually not a rapid one, *there not being more than two or three vibrations per second.* These results I am pleased to say have been confirmed by observations made by Prof. Ewing with an instrument of a totally different construction. From the results it was also possible to make approximate determinations of the direction of a shock, and also the maximum velocity of an earth particle.

2. Pendulums intended to Swing and thus indicate the Direction of a Shock.

Shortly after putting up the first of the long pendulums which I have spoken about, I suspended three small pendulums from the ceiling of my dining room. One of these was 3′. 11″ long and had attached to it a weight of ¾ ℔s. Its lower half was a spiral spring.—The second was a cord 2′. 3″ long carrying at its lower extremity a weight of 1 ℔.—The third was about 1′. 3″ long and carried a weight of 1½ ℔s. With the small earthquakes which occured during a period of over a year previous to Dec. 1879, no remarkable motion was ever observed in these pendulums. In fact, several times when they were observed during an earthquake they were practically steady points, and this at times when, as before stated, heavy lamps and pictures were observed to have swung through considerable arcs.

The object of this experiment, was to see whether a simple pendulum could be used to give the approximate direction of an earthquake shock.—In the case of tolerably heavy shocks, such as we have several times experienced during the last year, they certainly can be so used, but for small shocks their motion appears to be two small. Under all circumstances they must be observed at the time or within a few seconds of the time of shaking, otherwise, as has been pointed out by other observers, the plane of vibration of the pendulum will have so far changed that its direction of movement is no longer that of the movement with which it commenced. If the pendulum is long, heavy, and properly suspended, (see description of long pendulums pages 23 & 26.), the rate of change from the original direction is comparatively speaking very slow, and if this pendulum has a sliding pointer working on a smoked glass plate, the pendulum may be swung through a considerable arc and it will come to rest without having practically changed its direction as is indicated by the thick broad line which has been formed. The swinging of such a pendulum however, is only produced during a heavy earthquake.

Observing that the long pendulum in the Physical Laboratory of the Engineering College was swinging at 9 A.M. on 23rd of Feb. 1881 about 9 hours after the severe shock of the previous night had set it in motion, the problem suggested itself of measuring the arc through which the pendulum was swinging, and, knowing the time which had elapsed since it had been set in motion, to determine the arc through which it must have moved at the time of the shock.

The means in which this might be determined may be seen by consulting the following table

Giving the pendulum a swing of 40^{cm} it is found that

after a time of	0 min.	the amplitude is	40 c.m.
,, ,, ,, ,,	8 ,,	,, ,, ,,	35 ,,
,, ,, ,, ,,	18 ,,	,, ,, ,,	30 ,,
,, ,, ,, ,,	33 ,,	,, ,, ,,	25 ,,
,, ,, ,, ,,	53 ,,	,, ,, ,,	20 ,,
,, ,, ,, ,,	80 ,,	,, ,, ,,	15 ,,
,, ,, ,, ,,	120 ,,	,, ,, ,,	10 ,,
,, ,, ,, ,,	160 ,,	,, ,, ,,	7.5 ,,

This table was drawn up for me by students in the Physical Laboratory at the Engineering College.

3. Attempts to determine the Vibrational Period of an Earthquake.

From what I have said respecting experiments to determine the total horizontal movement at Tokio which is chiefly situated on beds of alluvium, it was seen that an earthquake gives about *2 to 3 vibrations* of the earth per second, and that these vibrations are irregular.—This deduction is made from the observations upon a small number of shocks.—A series of observations may shew a certain variability.—As at other localities situated say for instance upon granite, the number of vibrations per second may be different. It may be of value for me to record for the benefit of those who desire to make such observations some of the simpler methods which I have followed whilst endeavoring to find out the period of the earth's motion. They were as follows.

1. At the time I suspended the three small pendulums in my dining room I anticipated that because they were of different lengths the one which swung would be the one which had a period indentical with or some multiple of that of the earthquake.

2. Following out this idea, about a year ago I had a number of small pendula constructed, the supporting string of which varied between 2^{in} and something less than a quarter of an inch. The weights for these pendula were discs of lead each about $1\frac{1}{2}^{in}$ in diameter and $\frac{1}{8}^{in}$ thick. Thin wire pointers of different lengths from the bottom of each of these discs made the *total length* of *each pendulum the same.*—These pointers worked over the surface of smoked watch glasses.

This contrivance was difficult of adjustment, and although their periods of swing were all short, the period of wobble of the discs were identical, and for these reasons the experiment failed.

3. At the suggestion of Mr. Gray the next experiment was to construct 6 inverted pendulums all of the same length but of different periods.—

These were made of piano-forte wire about 1.5^{mm} thick and 20^{cm} long. Fastened on these were small cylinders of lead about 1.5^{cm} in diameter and 1^{cm} long.—The first of these was fixed 2^{cm} from the top of the wire and the last 2^{cm} from the bottom. (see Fig. 7).

The first had a period of about one vibration per second.

In order to determine which of these springs vibrated, following a suggestion of Prof: Ewing, a small silk thread was fixed to the top of each of these wires and passed through a small hole in a metal plate just above the top of these springs. The threads were plastered down on the plate with a little castor oil.—The thread which had been farthest dragged through the hole after an earthquake indicated the spring which had been thrown into most violent motion by syncronizing with the period of the shaking.

Usually the extent of vibration was so small that the threads were not sufficiently moved in order to make definite observations. Numbers 2 and 3 seem several times to have been the ones in greatest motion. At the time of the earthquake of Dec. 23d number 2 was *seen* to be in violent motion. These experiment may be said to have given a rough confirmation of the results obtained from sliding plates, namely that the vibrational period of an earthquake is small, probably not being more than 4 swings per second.

Whilst these experiments were being carried on, a similar series of experiments were made by Mr. Gray who used a series of flat strips of bamboo loaded with pieces of lead of different sizes in order to give them different vibrational periods. The records were made by sliding pointers in glass tubes placed in an inclined position, so that the end of the pointer rested on a smoked glass plate. As these springs in consequence of being flat could only vibrate in one direction, two sets of them placed at right angles were employed:

4. Determination of the Existence of Earth Tremors.

The third group of experiments which I wish to describe were principally made with the object of determining the existence of and measuring the extent of earth tremors.

Here also the principle of the long pendulum has been employed, but instead of marking the effects of the shock directly upon a piece of smoked glass, the effects are registered by causing a small mirror to be turned and a ray of light reflected from the same to be deflected. The following general plan of the arrangement see Fig. 8, will make the system of measurement more clear. W is a lead weight of 20 lbs suspended by a pianoforte wire about 20 feet long. On two small frames which can be moved backwards and forwards upon a table immediately below the weight, there are two glass tubes T and T_1. Sliding through there are two pointers made of wire. The forward ends of these pointers rest in small holes made in the backs of two small mirrors M and M_1 suspended vertically by a silk thread. The stands carrying these tubes, pointers, and mirrors, are pushed in the direction of the weight until the other end of the pointer touch the weight. If the table on which these stands rest is moved in any direction, the pointers are pushed by the steady weight through the tube and the mirrors are revolved.

In front of each of these mirrors a scale divided into millimetres is placed, and at the point L there is a lamp. To set the instrument, the mirrors are pushed back until the pointers are in contact with the weight, and the position of the spots of light upon the scales are noted down. If motion takes place, then the positions of these spots of light are altered, and from the angular deflection which has taken place it is a simple matter to calculate the distance through which the pointers have been pushed forward. The resultant of these two motions gives the actual motion through which the board has moved.

The method of calculating an observation is as follows. (see Fig. 9).

Let M_1 be the position of the mirror before the shock, M_2 be the position of the mirror after the shock, and P be the pointer. The distance which the pointer has moved forward will be equal to $b\ c$. From the scale it is seen that the spot of light had moved through an angle of $\theta°$. Therefore the angle moved through by the mirror viz. the angle $b\ a\ c = \frac{\theta}{2}$

By measurement, $b\ a$ equals 3.55 millimeters

Therefore $b\ c = b\ a \tan \frac{\theta}{2}$. Call this x

Similarly the distance through which the pointer P_1 moved is calculated. Let this $= y$. As P and P_1 are at right angles, the shock equals in magnitude and direction one of the diagonals of a parallelogram with sides which are parallel to P and P_1 and respectively equal to x and y.

With this arrangement, which I have worked with for about 4 months, I may say that usually I found a movement to have taken place almost every time the instrument was looked at, which on average was 2 or 3 times per day. From these results it would at first sight appear that the ground in Tokio is almost constantly in a state of tremor. Before committing myself to this statement however, I shall hold over my present results until they have been repeated in duplicate with instruments designed with more care and susceptible of more careful adjustment. Three of such instruments have now been prepared. At the time of earthquakes, movements of considerable extent are obtained.

The actual multiplication of the instrument described is about 250, that is to say, if the pointer is pushed through a distance of 1 millimetre the spot of light on the scale travels a distance of about 250 millimetres.

The following are examples of records which I have obtained.

AN EXTRACT FROM THE RECORDS OF A TREMOR INDICATOR

Set	Date	Time	Reading	Values of θ
$\frac{3}{11}$	13	h m 4.0 p	$\frac{75}{115}$	$\frac{15}{2}$
$\frac{4}{9}$	14	6.0 p	$\frac{9.5}{9.5}$	$\frac{20}{2}$
$\frac{15}{8}$	15	8.0 a	$\frac{35}{85}$	$\frac{6}{1.30}$
$\frac{0}{85}$	15	12.0 a	$\frac{2}{8.5}$	$\frac{3}{0}$
$\frac{3}{4.5}$	16	3.0 p	$\frac{5.5}{7}$	$\frac{8}{6}$
$\frac{.5}{4}$	17	9.0 a	$\frac{6}{\cdot 55}$	$\frac{15}{3.30}$

Set	Date	Time	Reading	Values of θ
$\frac{1.5}{2.5}$	17	h m 8.0 p	$\frac{7}{4}$	$\frac{17}{3.30}$
$\frac{1.5}{5}$	17	8.30 p	$\frac{6}{5}$	$\frac{14}{0}$
$\frac{4}{4.5}$	17	10.0 p	$\frac{6.5}{6}$	$\frac{8}{2}$
$\frac{4}{6}$	18	4.0 p	$\frac{9.5}{6}$	$\frac{20}{0}$

The numerators and denominators in columns 1 and 4 are the readings indicating the position of the spots of light on the two scales. The dates and time refer to the time at which the readings in column 4 were made, and the instrument reset.

The first motion was caused by a small earthquake and the value of $\frac{x}{y}$ as calculated equals $\frac{.30}{.06}$ millimeters indicating a motion of .13^{mm} in a direction N 57° W or N 83° W. Palmieri's instrument gave as the direction of this earthquake WNW.

These observations were made in Feb. 1880

Note added in Feb. 1882.

The following are *examples* of observations made in Feb. 1881 with two instruments respectively marked I & II

These instruments were in the same building.

READINGS OF SPOTS OF LIGHT.

Feb. 10th 9 A.M.	I		II	
Set ——	55	360	50	430
11 A.M.	45	365	50	430
Set ——	40	380	50	430
3 P.M.	10	420	45	445
Set ——	20	450	35	455
6 P.M.	35	430	40	445
Set ——	20	425	34	435
Feb. 11th 8.30 A.M	50	370	50	425
Set ——	60	400	55	430

	12 Noon	30	410	50	440
	Set ——	25	405	35	435
	3 P.M.	30	430	40	435
	Set ——	30	430	40	435
	6 P.M.	30	460	40	435
	Set ——	30	460	40	435
Feb. 12	9 A.M.	60	190	85	360

From readings like these, it appeared that sometimes these was a motion to be observed every few hours. In these instrument the magnification was about 1000.

The nature of the instruments caused the resetting of the mirrors to be different. When one instrument indicated a motion, generally speaking the other instrument also moved.

Other methods which have been used for indicating earth tremors are as follows.

Any one who has had to work with mercury, as for instance in an artificial horizon, or at an astronomical observatory, knows how sensitive it is to slight motions,—a person moving even at a short distance being sufficient to cause ripples on its surface. If we had such a surface and were able to record its motions, I think we should be provided with a sufficiently delicate instrument for recording earth tremors.

Mr. Mallet in his experiments upon the rate of transit of a wave in various rocky formations took advantage of the movements produced in a vessel of mercury to indicate its arrival. The difficulty however which has to be overcome is to make the record. Photographic means might be found effectual. One plan which I tried for registering small motions in a reservoir of liquid, was to fill a large vessel with mercury and to connect the side of this with an inclined tube of small dimensions, thinking it probable that a slight motion in the large mass might give a great displacement in the small tube. The results were unsatisfactory. Another method which I tried was to bring a metallic point forming the end of a screw, in such close proximity with the surface of a dish of mercury that a slight ripple in the latter would be sufficient to cause a contact,—the mercury and the screw being connected with the poles of an electric circuit.

In this way I obtained sufficiently close contact that when the floor (which was a very firm one) was gently stamped upon in the neighborhood of the table which supported the dish of mercury, the connection was made complete, and some contrivance in the circuit then registered that motion had taken place.

Another arrangement which I tried, was to have a spiral spring hung over a vessel of mercury, the mercury and the spring forming the two poles of an electric circuit. In this way an earthquake movement may be observed both by the motion of the mercury and the spring. To make the apparatus more delicate than Palmieri's, to a portion of which this is similar, I increased the size of the vessel of mercury from a tube to a dish, and by a series of trials I also increased the sensitiveness of the spring, hanging on to the end of it a light weight.

These experiments I varied with springs of different length and in all of them as far as my tests went I must confess that my success was not equal to my wishes. The method in which I tested these different forms of instruments, however, was a very crude one. It consisted in placing the instrument on a strong table like a carpenter's bench and then tapping and gently endeavoring to shake this support in various directions, and then observing the motion which was produced in the instrument.

At the suggestion of my colleague Mr. Grey I experimented upon a variety of wires suspended in the same way as the little trap door on the top of Thomson's Galvanometer. For this purpose I fixed a thin platinum wire about 4 inches long, horizontally between two supports. On to this was soldered at right angles another thin wire of about the same length. This was so placed that on one side of the platinum wire about ¼ of an inch projected, whilst on the other side there was 3¾ inches. The consequence of this was that the platinum wire was to a slight extent in a state of tortion. On tapping the bench the cross wire shewed a slight motion, the greatest motion being when I had reduced its length to about 1^{in} and the platinum wire was in a moderate state of horizontal tension. Unfortunately this only responded to taps in particular directions and I therefore gave it up as unsuitable for the purpose for which it was required.

Subsequently I made a number of experiments upon straight springs of various lengths and thickness fixed horizontally and vertically but without obtaining any result worth recording. In all the experiments my object was to obtain an instrument which was sensitive to very slight tremors. With Palmieri's spring, the screw above the surface of mercury, and the cross wire like the galvanometer trap door, I may say that to a certain extent I succeeded, and with instruments constructed with sufficient delicacy on any of these principles I think it would be possible to obtain a register of earthquakes which now pass by unnoticed.

A more simple instrument than any of the above, and one which may be constructed for the cost of a few pence, is the one shewn in Fig. 10. The drawing is full size.

A is a small piece of sheet lead on to which there is fastened a loop and a pointer, each made from very thin copper wire. The loop is hung on a hook or another loop fixed to the bent wire B. The pointer passes through a hole in a vertically placed metal plate P.

It will be found that if a small flat weight be suspended in this way and stood upon a table it is susceptible to the slightest motion. If it be properly balanced an extremely slight tap of the finger upon the table will be sufficient to give it a rapid tremulous motion. This motion is magnified by the pointer which will continually come in contact with the sides of the hole. To register these contacts, B and P are made the poles of an electric circuit. In this circuit the wire is at one point wound 8 or 9 times round a small piece of iron wire about 3 inches long. When contact is made this wire becomes a magnet. Near one end of this iron wire I have placed a small compass needle. This is at such a distance that when there is no current passing, the needle is not sufficiently powerful to be drawn in contact with the iron wire, but when this latter becomes magnetized by contact having been made, it is attracted, and once having come in contact, they remain together even should the current be again broken. To make the action more certain, I have placed two such contrivances with their pointers at right angles in one circuit. If copper wire is used throughout, I find that it needs cleaning every few days.

If platinum wire be used in the place of copper wire, I find that because the wire is thin and platinum is not so good a conductor as copper, movements and mechanical contacts may take place without electrical contact.

One objection to an instrument of this sort is, that its sentiveness makes it difficult to manipulate and it requires a special appartment to avoid interference. During the time which I employed it, I obtained several contacts which were not recorded as earthquakes by Palmieri's instrument. The necessity of cleaning the copper wires is another objectionable feature. It would of course be possible by attaching an arrangement like that in Palmieri's instruments, to register the time of contact.

The whole instrument can be made sufficiently small to stand upon a surface not larger than that of a penny piece.

The sensitiveness of this contrivance apparently depends upon its small mass. Springs like those of Palmieri's are also extremely sensitive insomuch as when they are once set in motion they continue to move for a long period,—but if we place one of these springs upon a table and then tap or shake it, it will be found that as compared with the above contrivance that it is extremely difficult to produce any visible motion.

Another extremely simple and at the same time practical seismometer may be made from a small compass needle. If a light, small sensitive compass needle be placed on a table, it will be found that a small piece of iron like a nail may be pushed so near to it that the needle assumes a position of extremely unstable equilibrium. If the table now receives the slightest tap or shake this condition is overcome and the needle flies to the iron and there remains. By making the support of the needle and the iron the poles of an electric circuit it would be possible to register the time at which motion took place with considerable accuracy.

The following table gives the contacts made by a needles (securely fixed upon a block of stone) during a portion of the time that I was experimenting with them.

The frequency of the contacts as compared with the records of Palmieri's instrument may be judged of by the fact that in June whilst the compass gave ten indications of motion, Palmieri's instrument only recorded four.

1879 May	8th	8	A.M.	Oct.	15th	4	P.M.
		12	„		16	8	A.M.
	11th	8	„		16	1.3	P.M.
	19	8	„		17	2	A.M.
	27	12	„		22	8	„
	29	8	„		22	12	„
June	8	8	„		23	12	„
	15	8	„		25	1	„
	16	8	„		29	12	„
	24	8	„	Nov.	1st	12	„
	25	8	„		2	8	„
	26	12	„		3	8	„
	27	8	„		3	4	P.M.
	28	8	„		5	4	„
	29	12	„				
	30	8	„				

Experiments with Microphones.

For a few months experiments were made with microphones whilst endeavoring to make a record of earth tremors, but I can not say that as yet these experiments have been attended with any great success.

The microphones which I have chiefly used have been small doubly pointed pencils of carbon about 3^{cm} long, saturated with mercury and supported vertically in pivot holes bored in other pieces of carbon which were the terminals of an electric circuit. These microphones were screwed down on the top of stakes driven deeply in the ground. They were covered with a glass shade thickly greased at its base. The stakes were in the ground at the bottom of a small pit—about 2 feet square and 2 feet deep which was lined with a box. The box was covered with a lid and earth to the depth of 9^{in} or 1 foot. One of these pits was in the middle of a lawn in the front of my house and the other was at the foot of a hill at the back of the house. The wires from the microphone passed through the side of the box into a bamboo tube and thence up to my dining room and bed room. In one of the circuits there were 3 Daniell's cells, a telephone and a small galvanometer. I used the galvanometer because I found that when there was sufficient motion in the microphone to produce a sound in the telephone, a motion in the needle of a galvanometer was produced. In case motion should take place in the magnetic needle during my absence, it was held deflected by a small piece of iron against which it had come in contact with the movement.

The sensitiveness of the arrangement may be judged of from the fact that if a person walked on the grass within 6 feet of the microphone, each step caused a creak in the telephone and the needle of the galvanometer was caused to swing and come in contact with the iron. Dogs running on the grass had no effect. A small stone 1 or 2^{in} in diameter thrown from the house so that it fell near to the microphone pit caused a sharp creak in the telephone and a movement in the needle.

The nature of the records I received from this contrivance may be judged off from the following extract from my records.

1879. Nov. 12th	$7^h\ 0^m$	P.M.	contact of needle
	$7^h\ 2^m$	P.M.	difficult to set the needle
	$7^h\ 3^m$	,,	needle swings & telephone creaks
	$7^h\ 4^m$	,,	,, ,, ,, ,, ,,
	$7^h\ 5^m$	,,	,, ,, ,, ,, ,,
	$7^h\ 6^m$	,,	,, ,, ,, ,, ,,
	$7^h\ 10^m$	,,	3 more swings
	$7^h\ 11^m$	,,	again ,,

"Here I went out took away the covering and examined the microphone. Nothing wrong was to be observed. All that I saw was one small ant which I killed. I do not think that this could have caused the disturbance because it could not get near the instrument."

On the succeeding nights I experienced similar disturbances and it seemed as if they might possibly have been the prelude to several small shocks which occurred about this time (15th, 16th and 17th Nov.). On the 17th Nov. at 8 A.M. the needle was found in contact and again at 5 P.M. and at 6 P.M. the shock of a small earthquake was felt *which caused a rattling sound in the Telephone for about 1 minute after the motion had appeared to cease.* The needle swung considerably but did not come in contact.

The following are extracted from another portion of my records.

1880 Feb. 15th contact during the night
— 16 „ „ „ afternoon between 4 & 7 P.M.
— 17 „ „ „ night
— — „ „ „ afternoon „ 4 & 6 P.M.
— — contact at 8 P.M.

Here observations ceased for several days.

Feb. 23 contact during the night
25 „ „ „ afternoon between 12 & 4 P.M.
26 „ „ „ night
29 „ „ „ „
&c &c &c.

The great objection to these observations is that it is possible that the movements and sounds which I have recorded might with the exception of one case when the shaking was actually felt, possibly have been produced by causes other than that of the movement of the ground. To determine this I subsequently put up two distinct sets of apparatus to determine whether the motions of each were synchronous. So far as I went this appeared only to be sometimes the case:—but this is a question difficult to determine unless a recorder of time be added to the apparatus.

The greatest objection to observations of this sort is that the sensitiveness of the instrument is not constant. After a current has been running for several days it is no longer sensible to slight shocks, it appears as if its resistance had been increased. To overcome this, it is necessary to resharpen the carbon points and bore out the pivot holes every 3 or 4 days. Farther the battery varies. This might to some extent be overcome by using a battery with large plates. These two causes tend to reduce the sensitiveness of the galvanometer like recorder,—the deflection of the needle gradually becoming less and less and therefore day by day needing a greater swing to bring it into contact with the iron. For reasons such as these this instrument to be used successfully appears to require considerable attention, and as my duties which keep me from home prevent my giving this attention, the records I have obtained are not so satisfactory as I should hope to obtain under more favourable circumstances. To avoid the trouble of resharpening the carbons I have recently employed a contrivance similar to that shewn in Fig. 11. An aluminium wire W loaded at one end with a piece of lead passes easily through a hole in an aluminium wire standard S, and rests with its pointed end on a small plate P. S & P form the terminal poles of an electric circuit.

This contrivance appears to possess all the sensitiveness of an ordinary microphone whilst if it receives a sudden impulse there is a break in the current for a definite interval whilst the wire W passes across from one side of the hole in S to the other side.

5. Experiments to determine a simple Seismometer for direction and intensity.

From theoretical considerations and also from actual observation we know that the effect of an earthquake and also its direction is affected by the nature and contour of the ground through which it passes. This was forcibly illustrated to us in the shock of Feb. 22d 1880. In Tokio instruments and contrivances gave very different measurements and directions for this shock. These differences however were I have reason to believe partly due to differences in the instruments. As to how far a valley or hill or a change in the nature of the rocks modifies the direction and intensity of a shock we have no actual knowledge.

With the hope of doing something towards the solution of this problem, for some time past I have been seeking for a simple cheap seismometer to give the direction of a shock and also something of its intensity, it being my intention to place a number of these on the hills and in the valleys near my house. For the solution of this problem it will be seen that the machines when under the same conditions ought to give the same results, and therefore simplicity is one of their first essentials. From the *few* experiments that I have thus far made, it appears to be an exceedingly difficult matter to obtain the same record from two machines which appear practically similar, even when they are placed within a few feet of each other. It would appear as if slight differences exist in the machines or else in their connections with the ground which cause considerable difference in their respective records.

Experiments with vessels filled with liquids. I was led to make experiments with vessels filled with liquid, partly because they are mentioned by Mr. Mallet in the Admiralty Manual of Scientific enquiry as a means of recording earthquakes, and partly because they appeared to afford such a simple means of making observations on the direction and relative intensity of a shock. The results of my experiments which were as follows, were exceedingly unsatisfactory.

1. Bowls filled with oil were found to be somewhat difficult to clean and then reset. Also, unless the vessel was a large one, ordinary earthquakes gave too small a motion of the liquid from which to make accurate measurements. This latter objection has been an objection common to most of these experiments, and to use large vessels, especially when a number of them have to be employed is exceedingly inconvenient.

2. Water (or even acid and water) washing up the sides of a wooden bowl the sides of which had been covered with chalk did not leave a sufficiently distinct mark to be definite. Most coloured liquids leave a mark which is indelible.

3. A rim of hard or soft note paper placed round the inside upper part of a glass filled with ink and water, gives a good record when the glass is shaken. If however the paper is *very close* to the solution, in less than two days capillarity

causes the liquid to ascend the paper to a distance of nearly ¼ inch, which is above any line which would be produced by an ordinary earthquake. If the paper is not near the liquid, the motion of the earthquake may not be sufficient to cause the liquid to reach the paper. To adjust paper on a number of vessels even if the upper part of the vessel has a moveable internal telescopic tin lining is a difficult matter so that all the vessels shall be sufficiently similar to shew a similar result when in the same position.

4. Lamp black powdered over the surface of water in a glass vessel is kept away from the sides of the vessel by a capillary surface. When motion takes place, the water flows beneath the floating powder which remains stationary and it is a difficult matter to make the lamp black come in contact with the glass—when it does come in contact it does not stick. Lycopodium acts similarly.

5. If a glass which has been smeared over with a tincture of galls be dried and partly filled with a weak solution of iron perchloide, this latter does not give a mark by washing up the side of the glass. Also the precipitate which is formed will not stick to the glass.

6. If a glass which has been covered with a solution of Potassium ferrocyainde be dried, and partly filled with a weak solution of iron perchlodide when this latter washes up against the side of the vessel, a sharp blue line is formed.

Many experiments of this kind were made, the Potassium ferrocyainde being usually mixed with a little gum arabic.

Whilst fresh, the action of the perchloride was satisfactory but after a few days it apparently looses its power of coloration.

The vessels used were like beakers, and the liquids had depths of from 1 to 6 inches. In no case was the "wash" of the liquid produced by an earthquake sufficiently great to be of any practical importance.

For the time of oscillation of a fluid pendulum consisting of mercury in the two arms of a bent tube these arms being vertical and parallel, Mr. Mallet gives us the formula.

$$T = \pi \sqrt{\frac{.5 \times l}{g}}$$

l being the length of the column of liquid. If we have a vessel in which the depth is great as compared with its other dimensions the same formula may be taken as giving us an approximation to its period of oscillation.

Therefore if we assume that in an earthquake there are two swings or one complete period per second, which assumption is not very far from what I have already shewn to be determined by experiment, if we wish to construct a pendulum which shall synchronize with such a movement and therefore be likely to be set in motion, its length l should be

$$l = \frac{g\,T^2}{\pi^2 \times .5} \text{ when } T = \frac{1}{2}$$

$$\therefore l = 1.45 \text{ feet.}$$

From this it would seem that the fluid pendulums which I have used have been somewhat short. Longer ones were not used because they were not convenient.

Much might be said about the nature of the liquid which might be used. The motion of a liquid will depend partly on its own friction. Alcohol when set in motion ought to move a long time because it is so frictionless whilst mercury will chiefly continue to move because of its weight.

The liquid we choose amongst other things depends upon our mode of registration.

In the case of a large earthquake no doubt the "wash" would have been greater and therefore perhaps more definite. The reason for my failure in these experiments is probably in great part due to my having used vessels of too limited dimensions. Large vessels however such as tubs, especially when a number of them are required, are difficult to manipulate and I do not know of any simple method by which an indication of the "wash" upon their sides is to be recorded.

Observations made with vessels of liquid are theoretically extremely simple, practically however they seem to be surrounded with so many difficulties that attention ought to be given to their study. I here speak only with reference to the effects produced by *small shocks such as we experience in Japan.*

Floating Bodies.

As the records given by the "wash" of an oscillating liquid were usually so very small my next endeavour was to magnify these. A description of one of my experiments in this direction will explain the remainder. A wooden tub about 2 feet in diameter was filled with water and on the surface of this a circular disc of wood 1^{in} thick and about $1'$. $6''$ in diameter was floated. From the center of this rose a mast about $1'.6''$ in height.

It was found that a very slight motion of the water caused the top of the mast to oscillate through a considerable angle. Similar experiments were made but on a *very small scale* with pieces of metal floating in mercury. To register the motion of the top of the mast a light pointer was caused to move on the surface of a smoked glass plate. It was found however that unless the motion of the water was violent, the frictional resistance of this pointer was sufficient to keep the mast steady. The indicator with the least frictional resistance, which I employed was a bundle of silk threads tied to the top of the mast, drawn out in various directions and allowed to hang loosely over a large wire ring, up through the center of which the mast passed. On shaking the tub the motion of the mast drew up the threads lying in the direction of motion to the greatest extent.

Frictionless as this contrivance was, although it was tested by several actual earthquakes, (either the motion in the water was so small or else the resistance of the threads so great) *no indication of the shocks whatever was recorded.*

Had the disc been on a vessel of mercury (the resistance to sinking being increased) it is very ~~probable that~~ the mast would have had sufficient power to give a record. An apparatus of this kind however would be expensive.

The reason for not using a set of bent tubes filled with mercury as employed by Palmieri and recommended by Mallet, was 1st that they only give approximate directions and secondly that when a number of such instruments are required they are expensive.

Experiments with columns.

Mr. Mallet in an article on Earthquake Phenomena in the Admiralty Manual of Scientific Enquiry recommends as a simple form of rough seismometer a number of small cylinders or prisms stood on end. During the last 3 or 4 years I have employed a number of columns similar to those recommended by Mr. Mallet. The first columns which I employed and still continue to use have dimensions as follows.

They are cylindrical and made of iron.

	Length	Diameter	Weight
1	150^{mm}	4.5^{mm}	22.2 grains
2	150^{mm}	5.5^{mm}	34.5 „
3	150^{mm}	9^{mm}	83 „
4	150^{mm}	15^{mm}	142.3 „
5	150^{mm}	18^{mm}	221 „

Numbers 4 and 5 remained standing for about a year but not having fallen during that time they were removed.

Although it has often happened that these columns have been shaken by earthquakes (3 or 4 times per month) it is very seldom that any of them have fallen. When we experience an earthquake which by Palmieri's instruments is recorded as having an intensity of something over 5° or 6° the two smaller columns are sometimes overturned. If we have a shock of over 20° the remainder may fall. This however is of rare occurrence, and when it does occur as in Feb. 22d 1880, so many chimneys, gravestones and other objects are disturbed, that to determine the direction of the shock special registers are hardly requisite.

These columns stand on the top of a stone pedestal originally set up as a stand from which to make astronomical observations. Unless I had practical experience with these columns it would have seemed to me incredible that at the time of an earthquake the smaller of them could possibly have remained standing. I say this because the smallest of these columns is so slender that to stand it on the small brass base on which it rests, I have to employ a servant who often occupies many minutes before he is successful, my own hand not being sufficiently steady. A column with dimensions like this I regard as being unpractical, first because it is difficult to set up, and secondly it is hardly probable, owing to slight irregularities in its form, that by its fall it indicates the direction of the shock.

I would here suggest that columns such as I have just described are not of the proper form to record earthquakes of medium intensity. What is required in a column which is to be overturned by an earthquake is first that it shall have a base sufficiently large that there is no difficulty in making it a level surface and secondly it must have a base which enables it to be stood up without difficulty, and thirdly the column must have a shape which will allow it to be easily overturned. To attain these conditions it would seem better to use a body shaped more like an inverted beer bottle than to use a common cylinder as suggested by Mr. Mallet. The reason of this appears to lie in the fact that the inverted bottle is a more quickly vibrating body than a cylinder of the same height.

Mr. Mallet in his calculations respecting the overturning of columns, building &c by an earthquake assumes that the *mass receives a momentum due to the quickest velocity which* the earthquake gives and he then regards the mass as being left alone until overturning or fracture has occurred.

From watching a column when shaken on a table it is seen that if the motion commences gently, (which is certainly a condition of the greater number of the earthquakes we feel in Tokio) then the column before it falls is caused to rock. During this period of rocking it is receiving momentum in positive and negative directions. If the column rocked with a harmonic motion then the question of its falling would greatly depend upon the relation between its periodic motion and that of the earthquake. The column however has a period dependent upon the angle to which it is tipped. The earthquake has so far as we know at present a period of about 2 or 3 complete vibrations per second and these may be irregular. The motion of the column will be understood from Fig. 12.

Let A B C D be column its center of gravity being at S tipped to an angle θ°. The force pulling the column down =

W $\times$ A b. but A b = A d cos θ = (A E — d E) cos θ = {S E tan $(\alpha + \theta)$ — S E tan θ} cos θ $\therefore$ W $\times$ A b = S E cos θ {tan $(\alpha + \theta)$ — tan θ}*

When θ approaches zero W $\times$ A b approaches S E tan α

When θ „ 90°, the above expression approaches zero.

It must be remarked that when θ is small, the elasticity of the prism and the base on which it rocks, must have a considerable effect in altering the amplitude of the motion.

However we see generally from the above that the couple causing the column to return towards the vertical is greater the smaller the angle of tip, and from this it would follow that the rapidity with which it returns will be quicker. It would seem that the increasing rapidity of vibration with which a body like a prism standing on a plain comes back to a position of stable equilibrium after having been slightly deflected, is not altogether unlike the, increasing rapidity in the succession of "skips" made by a ball which has been projected horizontally along a plain—or the rebounding of an elastic ball when thrown down vertically and bounding freely off the plain.

When the angle through which the prism has been deflected is very small then the problem of its rocking practically coincides with that of the rebounding ball. When the angle is large, the time of falling is greater for the constrained body than it is for a free body like the ball. Therefore the rocking body will have periods of rocking which for any two amplitudes will be in a somewhat greater ratio than the roots of the amplitudes (Because $s = \frac{gt^2}{2}$ & $t \propto \sqrt{s}$). Thus if the center of gravity of the rocking body be raised distances a and a_1, and p and p_1 be the corresponding periods (a_1 and p_1 being respectively greater than a and p) $\sqrt{\frac{a}{a_1}} > \frac{p}{p_1}$

My colleague Mr. Thomas Gray has very kindly given me the following note upon this problem.

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* *The points b and d are at the interstictions of the vertical from S with A E and A B.*

Let S be the center of inertia of the prism A B C D and let the angle S A B equal β. Suppose now that the prism is tipped through an angle θ and then allowed to fall towards the vertical; at any time t let the angle B A E equal φ and we have from the conservation of energy

$$\left(\frac{d\varphi}{dt}\right)^2 = \frac{2ga}{k^2}\{\cos(\theta+\beta) - \cos(\varphi+\beta)\}$$

where $k =$ the radius of gyration round A.

Now as we are only concerned with the cases where θ and consequently φ are small this equation may be written

$$\frac{d\varphi}{\sqrt{(\theta+\varphi)\sin\beta}} = \frac{\sqrt{2ga}}{k}\,dt$$

Integrating this equation between the limits $\varphi = \theta$ and $\varphi = 0$ we obtain for the time of falling from angle θ to angle zero

$$T = \sqrt{\frac{2\theta k^2}{ga\sin\beta}}$$

If A D the height of the prism be d, then $d = 2a\sin\beta$

$$\therefore T = \sqrt{\frac{4\theta k^2}{gd}} = \sqrt{\frac{2h}{g}} \text{ where } h = \frac{2\theta k^2}{d}$$

Hence the time becomes the same as that for a falling body when $\theta.\frac{2k^2}{d}$ is substituted for the height.

The bearing of these remarks on the overturning of columns by earthquakes, appear to be as follows.

1st If the body at the 1st shock receive sufficient energy of rotation to carry its C. G. over the edge on which it tipped, the body will fall.

2d Supposing the energy received is not sufficient to carry it over such an edge and also that the Earthquake motion is periodic.

From what has previously been said it would seem that there is a particular angular motion which a rocking body might assume, the period of which might synchronize with that of the earthquake. If the angle of tip is smaller than this particular angle the period of rocking will be faster than that of the earthquake. As a consequence of this the body will have reached the plain before the back shock is completed and the angular velocity of the body will be thereby augmented and also its amplitude.

This increase continues until the period of rocking equals that of the Earthquake. After this the amplitude can not become greater, for if at any time it should be greater, similar reasoning would shew that the earthquake oscillations would begin to diminish it.

Hence we see that the maximum period of vibration of the rocking body must be less than that of the Earthquake before it can be overturned.

In addition to the straight columns, I have used others of various shapes. These have been partly of iron and partly of wood. Their dimensions are as follows.

Iron columns.

7. Truncated iron cone. Height 150^{mm} Diameter of ends 37^{mm} and 11^{mm}

8. Truncated cones of equal lengths placed base to base Height 150^{mm} Diameter of ends 11.5^{mm} and 12^{mm} Diameter at the junction of bases 37^{mm}

9. Truncated cones placed base to base Height 150^{mm} Diameter of ends 10^{mm} and 12^{mm} Height of upper cone 30^{mm} Diameter at the junction 37^{mm}

Wooden columns.

Truncated cones made of Boxwood.

	Height	Diam : of Base	Diam. of top
10	75^{mm}	5^{mm}	36^{mm}
11	50—	5—	19—
12	50—	3—	19—
13	25—	3—	12—
14	25—	4.5—	13—

The value of columns as indicators of direction may be judged of from the following examples, which appear also to shew that columns do not accurately indicate direction.

For comparison the records as given by Palmieri's instrument are placed in brackets. Examples.

Nov. 22d 1878. 11^h. 8′. 5″ P.M. (8° WSW and ENE) column No. 1 fell 5° W of N

Nov. 26th 1878. (6° WNW and ESE) no column fell.

Nov. 10th 1880. 1.30 A.M. (12°.10′ SSW and NNE) column No. 1 fell to 30° N of W.

No. 10 fell to West. No. 12 to East. No. 13 to 30° S of E.

Dec. 19th 1880. 12 P.M. (2° SSW and NNE) column No. 1 fell to N 60° E. No. 10 to SW. No. 14 to SE.

Dec. 20th 1880. 12 P.M. (7° WNW and ESE) one fell to E. another fell to NE.

Dec. 23d 1880. 10 P.M. (22°.50′. WNW and ESE)

Nos. 1 and 2 fell to E. No. 3 to SW Nos. 8 and 9 fell to N. No. 10 to NE. (No. 8 fell on the top of No. 3) Nos. 11 and 12 fell to N 50° W. Nos. 13 and 14 fell but rolled. Those not mentioned had never been set up.

Jan. 24th 1881. 6 A.M.—(4°. 20′ WNW and ESE) No. 1 fell to 60° E of N.

Looking at the results of the earthquake of Dec. 23d it is seen that the smaller columns fell first, in one direction, and that the larger columns fell afterwards in another direction. This would indicate either that the shock (due to normal and transverse waves) had been in two directions, (the feebler and first portion of it almost E and W, whilst the stronger second portion of it had been approximately NE and SW) or that the little columns had fallen during the first part of the shock whilst the larger ones had only been caused to rock, as the shock continued the rocking became greater, *and the plain of rocking changed before the column fell.* The latter I believe to be the true explanation, and the change in direction of the plain of rocking may be illustrated experimentally. If a column with a circular base rocks before it falls and during this rocking turns upon its edges, the value of columns as indicators of direction can not be so great as might be first supposed. It would seem that the smaller columns which are thrown down quickly ought to be the best indicators of direction insomuch as they have only rocked and turned upon their bases for but a short time before falling. Such columns however have their bases so small that it is a difficulty to obtain them accurately at right angles to their length.

If all the columns about which I have spoken and the base upon which these columns rested, had been mathematically perfect, the errors I have spoken of would probably have been much smaller. An approach sufficiently near to mathematical accuracy is in practice however difficult to obtain.

6. To determine the direction from which a shock originates.

To determine the direction from which a shock originates by a single instrument appears to be a problem surrounded with considerably difficulty. If an earthquake were the result of a single blow and normal vibrations were well defined then the direction towards which a column fell would be the direction from which the shock originated. From the examples of the falling of columns which I have given, it will be seen that they do not fall as theory would lead us to expect. The reason of this discrepancy lies in the fact that the theory is incomplete and that the earthquake instead of being a sudden blow, appears usually to begin as a series of minute oscillations gradually increasing in amplitude. This is what our sensations tell us and it is what has been confirmed by instruments. If the first movement of the earthquake were ever sufficiently strong to overthrow a body like a column, the result we seek for, might now and then possibly be obtained. A body which is easy to obtain and which is thrown over more readily than a column, is a strip of Glass like that used for a microscopic slide. For some time I employed strips of glass of different sizes arranged as radii to a quadrant of a circle. They often fell but the results were never very satisfactory.

The propping up of bodies.

As the most delicate of our columns or self supporting bodies but seldom fall with the small shocks we so often feel in Tokio, I tried the system of propping up in a position as close to the vertical as possible small columnar bodies like pins. In this way indicators of earth movements having considerable sensibility could be produced in a very simple manner. This same principle was applied in a microphone like instrument already described.

It was also applied when endeavoring to determine the direction from which the shock originated as is shewn in Fig. 13. This represents the cross section (almost full size) of a small rectangular trough about 1 inch long. Leaning against the insides of this trough are two *thin* strips of glass. These it will be observed can fall inwards but not outwards. It was expected that if the first blow of the shock approached from the right the strip on the left side would fall forwards against the strip on the right and thus prop it up. The position in which the strips were found after a shock when we had a series of these troughs, would shew the side from which the shock came. So far as I experimented the results were not good, but I still think it possible that in a few special cases a definite result might now and then be obtained.

Dr. Wagner has suggested that the forward swing or blow of a shock is more intense than the back swing and that a registration and comparison of these two blows might help us in the solution of the problem I have been discussing.

7. Determination of vertical Motion.

For the determination of vertical motion at the time of an earthquake I first employed a number of vertical springs. Two of these springs had the following dimensions.

1. Diameter 3½ inches, 2 feet long and carried a weight of 20 ℔s. Thickness of wire about ⅜ inch. This spring makes about 4 vibrations per second.

2. Diameter 2½ inches, 16 inches long. Thickness of wire about ⅛ inch. It was loaded to make about 1 vibration per second.

Two other springs which I have employed were lighter than these.

A record of the motion of these springs was made by means of a small horizontally placed steel pointer resting on a vertical glass plate against which it was kept in contact by means of a light spring.

These arrangements at the time of a strong earthquake gave indications about 1^{mm} in length. At the time of the February shock, spring number, gave 3^{mm}, and number 2 gave from 5 to 15^{mm}.

Records of this description which vary with the spring which is employed appear to be unintelligible, and their only value appears to be that when they are employed so that swinging is avoided they may be made to indicate the existence of vertical movement. Theoretically it would seem that if the pointers from such springs were caused to write upon moving plates, we ought to have the earthquake vibrations and those of the spring combined like ripples on a wave, so that the resultant diagram might be analysed and the motion of the spring separated from that which had been produced by the earthquake. As the motion of the earthquake appears to be so small, it would need considerable magnification, before being visible, and the accomplishment of this would require considerable mechanical ingenuity.

2. A second attempt which I made to determine vertical motion was a modification of a system first used by Dr. Wagener which was proposed to me by my colleague Mr. Gray. Dr. Wagener employs a buoy partially emersed in a vessel of water. At the time of a shock this by its inertia is supposed to remain practically at rest, whilst the vessel and the water in which it floats moves up and down. Mr. Gray proposed a buoy which with the exception of a long thin stile was *completely* sunk. I have experimented with both forms of apparatus. One which was used was a cylindrical tin canister with conical ends about 2 feet long, and carrying about 14 ℔s of sand. With this weight, it was completely sunk with the exception of a long thin wire. The depth to which the wire sank, was determined by a small quantity of shot placed in a small cup on the stem. From the top of this stem a short fine silk thread passed round a light pulley 10^{mm} in diameter moving a light bamboo pointer. This instrument notwithstanding the lightness of the registering apparatus which was used to magnify the motion, did not appear to be practical. The addition or subtraction of a single shot from

the cup caused it to sink or rise which as indicated by the pointer was a considerable distance. The evaporation of the water also caused movements in the pointer. It is not sufficiently powerful to be self registering even by means of a fine steel pointer moving on the surface of a smoked glass plate. After several earthquakes no motion was indicated. The severe shock at 11 P.M. on the 23 Dec. 1880 gave 1.1^{mm} of vertical motion. I may here remark that all instruments in which water is employed give trouble during the winter months by freezing.

3. Not being able to find a contrivance by which it was possible to satisfactorily measure the actual extent of vertical motion, I sought for a contrivance which would synchronise in its motions with the vertical motions of the earth, and although not giving their actual amplitudes might by its synchronisms give their number and their amplitudes relatively. Here again I was indebted to a suggestion of my colleague Mr. T. Gray who proposed the use of a vessel with a corrugated metallic bottom which was to be filled with water. Not having as yet been able to obtain a corrugated bottom for a vessel, their being difficulties in its manufacture, I have employed a square Kerosene tin, the bottom of which is partially corrugated. Fixed to the bottom of this there are bearings for an axle at the end of the short arm of a light lever. The fulcrum of this lever which is carefully pivoted is attached to the base of a wooden frame on which the can is suspended. By experimenting on a table I find that when this vessel is partially filled with water and its bottom in consequence to some extent bulged out, the end of the pointer gives a definite movement for every upward or downward motion of the table. A number of blows struck upon the table no matter how quickly are each distinctly reproduced by a movement of the pointer. This instrument is sufficiently powerful to write its movements on a glass plate, and if the plate were movable it would I think give a record of the successive upward motions of the earth when such motions exist. It has already recorded by lines about 2^{mm} in length on a fixed plate the vertical motion of several of our recent

shocks. A contrivance on a similar principle but where the bottom of the vessel is a sheet of india rubber partially supported by flat steel springs has just been completed.

8. Localization of the origin of several Recent Shocks.

At the meeting of the British Association in 1880, a sum of £25 was generously placed in the hands of Andrew Ramsay, Esq., Director of the Geological Survey of Great Britain, and myself, as an assistance towards the investigation of the earthquake phenomena of Japan. A portion of this sum has been expended upon a problem which is chiefly of local interest, namely the determination of the origin of the shocks we so often feel.

In order to determine the origin of shocks, two independent methods have been employed; and the results obtained from these methods up to the present time are, I am pleased to say, confirmatory of each other. One depended on the observation of the time at which shocks were felt in Tokio as compared with Yokohama. The general result of these time observations, so far as they have gone, appears to be that several *of the recent shocks have been felt at Yokohama from* 15 *to* 30 *seconds before being felt at Tokio.* The mode of arriving at this result has been to use two clocks, with apparatus attached which caused them to stop at the time of the earthquake. The apparatus, I may remark, is so sensitive, that it has been found impossible to have the clocks hung upon the walls of an ordinary house; the vibrations caused by persons walking, by the shutting of doors etc., often stopping them. In consequence of this, they have been fixed upon stakes driven in the ground, and detached from any portion of the building. One of these clocks is in my house in Tameike, Tokio; and the other is in the house of Mr. W. H. Talbot on the Bluff in Yokohama; two places which are distant from each other about fifteen miles in a straight line. In order to compare these two clocks together, facilities have been very kindly given to us for using a telegraphic signal, which every day is sent from the Central Telegraph Office in Tokio throughout Japan. We have thus been enabled to obtain results, which, even with the help of chronometers, it had previously been found impossible to arrive at; and now, the differences of time, with telegraph time as the standard, at which our clocks are stopped, can I think, be always known within a few seconds. The only error which enters into the estimates is the determination of the portion of any given shock at which our respective clocks stopped. If one stopped at the commencement and another near the end of a shock, it will be readily understood that the error may be great until the clocks are caused to work in conjunction with some apparatus upon which the whole of the shock is being recorded, and at the time when the clock is stopped the stopping apparatus is caused to make a mark upon the record which is being drawn.* Until this is done, I do not see how accurate results can otherwise be obtained. This method, however, is an expensive one; and as the contrivances for stopping the clocks which are now being used, are similar, the errors I speak about ought not to be very great.

The following is an example of the results already obtained.

December 7th 1881. Telegraph time.

						H. M. S.
1.	At Yokohama	Talbot's	clock	stopped	at...	6.24.02
2.	„ Tokio	Milne's	„	„	„ ...	6.24.16
3.	„ „	Palmieri's	„	„	„ ...	6.24.33

Observation number 3 was made at the Yamato Yashiki observatory, and was very kindly furnished to me along with others, by the director, Mr. Arai Ikunosuke. From observations number 1 and 2 we see that the shock reached Yokohama 14 seconds before it reached Tokio; whilst from observations 2 and 3, 31 seconds. This discrepancy may be an illustration of what I have just spoken about, namely, that different contrivances for stopping clocks are of different sensibilities.

Although if we had more elaborate instruments our deductions might be more useful, still the results which have

* *This is a method which was first definitely enunciated by Prof. Ewing. It had however been previously employed practically by Mr. J. Gray and myself. J.M. Feb. 1882.*

been obtained seem to establish one important fact—namely that the origins of several of the more recent shocks are *much nearer to Yokohama than they are to Tokio.*

The first attempt to determine the interval in time between the arrival of a shock in Tokio and Yokohama was by means of ordinary watches.

At the time the shock was felt I immediately glanced at the second's hand of my watch then at the minutes, and finally at the hours. Several of my friends in Tokio and in Yokohama who very kindly assisted me in this investigation did the same. Immediately after the shock the Tokio watches were collected put into a box filled with cotton wool and started off by train to Yokohama about 18 miles distant. Here within from 2 to 3 hours after the shock they were compared with the Yokohama watches which had also been compared amongst themselves. Occasionally but only occasionally, were the results satisfactory.

Subsequently Mr. Yamaou Yôzô very kindly put at my desposal two chronometers one of which was kept in Tokio and the other in Yokohama. These were used as standards and after a shock all times taken in Tokio were compared with the Tokio chronometer, whilst those in Yokohama were compared with the Yokohama chronometer. As these chronometers had unfortunately a slightly irregular rate after a week or practically after every shock it became necessary that they also should be compared together. The transference of one chronometer to another by train no matter how carefully they were carried seemed to be unsatisfactory. For persons engaged in business and in fact for any who are not in the possession of good instruments, abundance of time, and considerable experience, the determination of time within so small a period as is required for earthquake observation, is altogether impracticable. For these reasons the comparisons were made by the transfer from Tokio, after comparison with the Tokio chronometer, of three lever watches. This method was found preferable to using watches alone, as it was possible to see how much the watches had lost or gained (independent of their ordinary rate) by the vibration of the train &c. This loss or gain was sometimes found to be from 8 to 18 seconds. In computations it was assumed that half this quantity had been lost or gained by the journey in going and half in returning.

It was only occasionally that the results obtained in this way were satisfactory. I mention these methods of making time observation as I think they shew that the difficulties which have to be overcome in an operation which at first sight appears to be simple, are practically very great, and that from such methods it is only occasionally that good results can be obtained. The first method described, where the telegraph signal is employed is undoubtedly the best.

I will now turn to the second and more important method which has been employed to localize our earthquakes. This has consisted in establishing seismometers at several points round Yedo Bay, the chief aim of which was to determine at these points the direction from which the shock came. At Yokosuka an instrument has been placed in the care of Mr. John Parr; at Yokohama instruments are in the hands of Mr. Thomas Rose and Mr. W. H. Talbot: in Tokio, at Senji, an instrument has been given to Mr. G. Adolf; and at Kisaradzu two instruments were placed under the charge of a Japanese assistant Mr. Meshima. To all these gentlemen who have taken charge of instruments I tender my best thanks. The nature of the results which have been obtained from the united efforts of these observers may be judged of from the following examples.

I.—The earthquake of January 7th, 1881.

1. The maximum motion at Kisaradzu was N. 84° W.
2. „ „ „ „ Tokio „ N. 22½° E.
3. „ „ „ „ Yokohama „ N. 24° W.

By drawing lines upon a map parallel to the above directions through the places at which they were respectively observed, we obtain intersections each of which is *within* 2 *miles of Yokohama.*

II.—The earthquake of January 22nd, 1881.

1. The maximum motion at Kisaradzu was N. 87° E.
2. „ „ „ „ Yokohama „ N. 6° W.

These two directions intersect one another at a point indicating an origin *about* 4 *miles in a S. S. E. direction from*

Yokohama at the entrance to Mississippi Bay.

III.—The earthquake of January 24th.

1. The maximum motion at Kisaradzu was N. 80° E.
2. „ „ „ „ Yokohama „ N. 17° E.
3. „ „ „ „ Yokosuka „ N. 49° W.

These three directions intersect in three points, within 1½ miles of each other, *at the head of the Kanasawa inlet about 7 miles S.S.W. from Yokohama.*

In looking at results like these it must be remembered that the origin of an earthquake is perhaps not a point, but more probably a fissure which may be of considerable length; and therefore, each of the points of intersection as indicated upon a map by drawing in the directions which are here given, are only approximate indications of the position of an origin. Farther, I must remark that the directions, as indicated by any instrument with which I am acquainted, are liable to serious errors. Palmieri's instrument only aims at giving a direction within 22½ degrees, while others which theoretically appear perfect as indicators of direction are practically found subject to very serious divergences. By dint of experiments the instruments which are being employed are gradually improving, and it is hoped they may before long rank among the most accurate. They are similar to the one described at the second meeting of the Seismological Society by Mr. Thomas Gray,—pendulum instruments *with friction pointers,* and recording the motion on three components. If these instruments are affected by two sets of movements, this is shown in the impossibility of working out the records. It is probably owing to confusion of this description which has prevented the localization of many of our shocks.

As a still farther indication that the origin of the numerous shocks which we have so lately experienced are near to Yokohama, it is observed that the Yokohama instruments appear to shew a much greater amount of vertical motion than any of the others; and moreover, in Yokohama many slight earthquakes are recorded which do not appear to have sufficient energy to make themselves felt in Tokio.

Clock Stopping Apparatus.

The following is a brief description of a simple contrivance for stopping a clock which I have found to be exceedingly sensitive. See Fig. 14.

P is the pendulum of the clock with a small piece of wire standing out at right angles to its face. This is shewn as a black dot. This wire as the pendulum swings, passes beneath a series of teeth cut in a strip of wood lighty hinged at A and terminating at the other end B with a piece of stiff wire. So far, this contrivance is identical with one proposed by Mr. Mallet in the Admiralty Manual of Scientific Enquiry. If such a contrivance is allowed to fall, the teeth catch in the projecting pin of the pendulum and it may arrest it at any portion of its swing.

It was not until after many experiments that I succeeded in contriving a simple arrangement which at the time of an earthquake would allow the toothed lever to fall. This arrangement consist of a piece of stiff wire W on which, near to one end is a small cylinder of lead L. The short end of this wire is pointed, and rests in a pivot hole made in the head of a drawing pin pressed into the side of the clock case S. To prevent this wire from falling it is held up by a small silk thread T fastened to a second drawing pin. As suspended it is very unstable and instead of remaining at right angles to the clock case, it swings round against it. When however the wire B rest on the end of W, it retains its position as shewn in the figure.

It is now difficult even to shut the clock case without causing a disturbance, resulting in the stoppage of the clock. A clock with this arrangement has been found by Mr. Talbot and myself to be so sensitive to footsteps the closing of doors &c. that it can not be suspended on any ordinary wall. I here speak of the walls of strongly framed timber houses such as are so common in Japan. In order to prevent a clock with this arrangement being stopped by causes other than earthquakes it has been found necessary to attach the clock to a stake driven in the ground, so that it is altogether disconnected with the floors or walls of a house.

The reason for the remarkable sensitiveness of this contrivance appears to be, that if the clock case receives a small displacement at right angles to W, the weight remains steady by its inertia, whilst the long arm of W in contact with B multiplies the initial motion in proportion to the relative lengths of the short arm of W to the long arm of W. In this way for a small motion of the head of the drawing pin on which W turns, there is a large motion of the end of W on which B is resting.

Theoretically a shake or a blow parallel to the length of this wire would not give the multiplication of the initial motion here spoken of.

For this reason theoretically two such horizontal levers ought to be employed. Practically however it seems impossible to give a motion in that direction to which the apparatus does not seem to be just as sensible as to a motion in any other direction. The only motion which does not result in stopping the clocks appears to be a *very slow* easy swing.

Conclusion.

The records which I have now given are the results of many experiments made during the last four years. Many of them are the record of failures and these have been referred to so that others wishing to record earthquakes may avoid similar errors. The unsatisfactoriness of the results obtained from many of the old simple Seismometers has been pointed out. The reason that they have been so repeatedly recommended would seem amongst other things to have been the want of sufficient opportunities to experiment with such instruments and also perhaps from the non-recognition of the true nature of an earth vibration as produced at the time of an earthquake. Exceptionally we may perhaps experience sharp and sudden blows and also horizontal motions of many inches in extent. As my experience has not been with earthquakes of this description my remarks must not be considered as having reference to them. Many may think that since seismology has of late years made such great and rapid strides investigations such as many of mine have been, are behind the times. Those who hold ideas like these, need I think only be reminded of the vast amount of valuable information which might be gathered in, if we had scattered around us persons provided with *simple* instruments, in order that they should change their opinions. For the working out of certain problems it is quite certain that many instruments which from the number of them which are required must of necessity be simple, and if I have done a little towards shewing the value of instruments of this description I shall be satisfied. Through the kind assistance granted to me by the British Association, during the last year I have been able to commence a series of experiments of a more elaborate description. About the results of these I hope to speak in a future communication.

DISCUSSION.

Mr. Ewing said that Mr. Milne's paper contained so much interesting and suggestive matter, that the lateness of the hour, stopping discussion as it did, was much to be regretted. There were two points he should like to notice very shortly. He understood the author to say that if he found two of his delicate pendulum and mirror instruments for showing earth tremors agree in their indications, he should accept these as correct. The conclusion seemed scarcely valid, since various disturbing causes, such as changes in temperature and in the dryness of the air would probably affect both similarly. Again, Mr. Milne's device for stopping the pendulum of a clock during any part of its swing would no doubt show, to the fraction of a second, when it stopped, but the question remained, at what part of the earthquake disturbance did that happen? An earthquake was not a single isolated event but a protracted series of movements, often beginning very gradually, and extending over one or more minutes of time; and it would be hard to tell which impulse stopped the clock. For this reason he held that Mr. Milne's and other like arrangements for finding the difference in time of arrival of a shock at different places were liable to errors sometimes greatly exceeding in amount the differences which were sought to be measured.

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