REPORT

OF THE

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FOR THE ADVANCEMENT OF SCIENCE

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REPORTS ON THE STATE OF SCIENCE.

Seismological Investigations.—Twenty-second Report of the Committee, consisting of Professor H. H. TURNER (*Chairman*), Mr. J. J. SHAW (*Secretary*), Mr. C. VERNON BOYS, Dr. J. E. CROMBIE, Mr. HORACE DARWIN, Dr. C. DAVISON, Sir F. W. DYSON, Sir R. T. GLAZEBROOK, Professors C. G. KNOTT *and* H. LAMB, Sir J. LARMOR, Professors A. E. H. LOVE, H. M. MACDONALD, J. PERRY, *and* H. C. PLUMMER, Mr. W. E. PLUMMER, Professors R. A. SAMPSON *and* A. SCHUSTER, Sir NAPIER SHAW, Dr. G. T. WALKER, *and* Dr. G. W. WALKER.

I. *General.*

OWING to the cancelling of the Bournemouth meeting proposed for 1917, and to other reasons connected with the war, the present Report is made brief. It has been drawn up by the Chairman, except where specially mentioned.

The Committee asks to be reappointed with a grant of 100*l.* (including printing), in addition to 100*l.* from the Caird Fund already voted. The grant was formerly 60*l.*, with 70*l.* for printing—130*l.* in all; but during the war it has been reduced to 100*l.*, partly to meet the need for economy, partly because the printing has necessarily been less. The Government Grant Fund administered by the Royal Society has voted a subsidy of 200*l.* for 1917 as in recent years. With the above modification the budget remains practically the same as given in the 20th Report.

The Shide staff has remained unchanged, though it is probable that changes must shortly be made. The general question of organisation of seismology has not only been discussed at several meetings of the present Committee, but is also under consideration by the Geodetic Committee, appointed by Section A of the British Association in 1916. This Committee contemplates approaching the Government with a proposal for a Geodetic Institute. It has been suggested that before taking definite steps in this direction the similar needs of other branches of geophysics (seismology, magnetism, tides, &c.) should be reviewed; and the Geodetic Committee, suitably enlarged for the purpose, is proceeding to this review.

The collation of the records for 1913 was completed and printed as a separate pamphlet, with a preliminary discussion. Further discussion is given in a later section of this Report.

II. *Instrumental.*

The time signals at Shide have been received regularly, with some interruptions chiefly due to bad weather, and consequent derangement of the instrument.

The transit lent by the Royal Astronomical Society has been used in supplement; but it is found difficult to secure the instrument in a permanent azimuth. The method used has been to fix the feet to the pier with plaster-of-Paris; and this holds for a time, and then seems to give way for some unknown reason. On the first occasion it was naturally assumed that there had been some accidental blow to the instrument, but it is difficult to believe that this can have happened on all the occasions noted.

The following note has been received from the Astronomer Royal for Scotland :—

An Improved Method of Registration for Milne Seismographs.

An attempt was made to improve the trace of the Milne Seismograph at Edinburgh by using a very small source of light. Though some improvement was obtained, a limit was soon reached in the diffraction pattern resulting from the crossed slits. The following arrangement was then adopted. The boom ends in a plate of blackened aluminium foil, in the centre of which is a hole rather less than 1 mm. diameter. Over this hole is mounted a small lens, which forms an image on the bromide paper of a specially small source of light. The lens used is achromatic, of 9 mm. focal length. The fixed slit-plate was at first left undisturbed; but subsequently the slit was widened to about 1 mm. To obtain a sufficiently small source of light, a four-candle-power electric lamp with coiled-up filament is fixed 30 cm. above the end of the boom, and 10 cm. below this is placed a telescope ocular of 1 cm. focus.

The results show a striking improvement on the previous records. The trace is a line well under $\frac{1}{20}$ mm. thick, and can be magnified ten or twenty times with advantage. In this way the short-period oscillations of the boom, which formerly resulted in a blurring of the trace, are clearly resolved. To obtain the full benefit of the magnification it would be an improvement to run the paper at a much slower rate than at present (say, at 100 mm. per hour instead of 240 mm.).

Milne-Shaw Seismographs.

It was submitted in the last Report that *the most important work of the Committee for the present lies in replacing the Milne machines* (either by Galitzin machines or) *by Milne-Shaw machines.* The difficulties of obtaining Galitzin machines have not decreased; but it is gratifying to report that a number of orders have been received for M.-S. machines, and that a generous subsidy of 200*l.* has been made from a private source. Hence Mr. Shaw has been working early and late to make a number of machines. It is perhaps better to reserve the list for the end of the war, but it may be stated that two have been safely delivered to America, and others are nearing completion. There have been considerable difficulties in obtaining some of the materials, but Mr. Shaw's patience and ingenuity have overcome them. In the course of construction and testing he has obtained a number of interesting results of which he has made the following notes. It may be added that, in spite of the pressure of this work, he has found time to make regular visits of superintendence to Shide, either in company with the Chairman or alternating with him. His instrumental skill and knowledge have been freely put at the disposal of the Committee throughout.

Notes on the Comparison of two Milne-Shaw Seismographs.
By J. J. S.

The testing of two Milne-Shaw seismographs, No. 8 and No. 9, at West Bromwich, during May of the present year, showed not only how standardised machines may be relied upon to give similar seismograms, but also afforded an opportunity of investigating the questions of daily tilt and microseisms, and the degree of sensitivity to which a machine can be set.

Statements have been published that, due to the mechanical imperfections of seismographs, there is a difficulty in obtaining long periods of oscillation ; anything of the order of 60 seconds being an impossibility. The facilities for making refined adjustment provided in the Milne-Shaw type prompted the writer to investigate the possibilities.

No difficulty was experienced in obtaining a period of 60 seconds, and, in order to test its constancy, the machine was left for five days, at the end of which time the period remained unchanged.

The period was then increased to 90 seconds ; the machine had now become highly sensitive to the slightest tilt. With a nominal magnification 150 times the horizontal ground movement, this period gave such a sensitivity to tilt that 1 sec. of arc produced 1·5 metre displacement of the light spot ; or conversely 1 mm. displacement corresponded to a tilt of 1 inch in about 5,000 miles.

A subsequent attempt produced a period of approximately 120 seconds, which represents a sensitivity of 1 mm. amplitude for a tilt of 1 inch in upwards of 8,000 miles.

With the apparatus in this condition the smallest movement of the observer affects the position of the light spot ; therefore the observer took up a seated position 6 feet from the column ; but even so, a swaying of the body in the chair produced an appreciable effect.

This machine, No. 8, was mounted upon a pier measuring about a cubic yard, built up from the cellar floor of the writer's house. The weight of one person (150 lb.) in a bedroom two floors above, and not immediately over the instrument, produced a tilt in the cellar floor of about ·04 second of arc, causing the light spot to be deflected more than 100 mm.

A test was made of the machine's sensitivity to temperature change at this 120 seconds period. Approaching the column for this purpose was out of the question, therefore the rays from a small Bijou incandescent gas mantle, with which the chamber was lighted, were, by means of a small hand mirror, so projected that they fell upon one side of the column and not upon the other. The heat from this small increase of illumination expanded the one side of the column sufficiently to drive the light spot off the scale.

When timing the period of oscillation in these higher sensitivities it was noted that the effect of amplitude upon the period became very marked—increasing the amplitude of swing rapidly increased the period.

With the pendulum set to oscillate in 10 or 12 seconds this difference amounts to only about $\frac{1}{10}$ of a second over a wide range of amplitude; but at 120 seconds the fluctuation becomes important. With a change in amplitude from 1,000 mm. to 100 mm. there was a drop in the period of 20 to 30 per cent.

Time did not permit of an investigation to determine the rate of change with differing periods. There was insufficient change in the damping ratio to account for the phenomenon, therefore it is probable that it is an extreme case of 'circular error.'

This variation of period with amplitude, even when small, suggests that some standard amplitude should be used when determining the period. 10 mm. is the prescribed standard with the Milne-Shaw machine.

The Milne-Shaw boom is short, and the magnification includes mechanical leverage. Though the friction of same is extremely small, it was expected that it would be sufficient to operate against obtaining excessively long periods, and would compare unfavourably with a simple elongated pendulum of the Milne type. The result was quite the reverse, thus establishing the fact that the air resistance on a long boom forms the major part of the total friction, and suggests that, though the design is simple, it is not necessarily the best for obtaining free oscillations.

The second machine, No. 9, was mounted upon a pier in an out-building 60 feet from No. 8. It was oriented in the same azimuth, and connected in series with the same time circuit. The constants of both machines were made equal, viz.:—

Period	12 seconds.
Sensitivity to tilt . .	26 mm. = 1 sec. of arc.
Magnification . . .	150 : 1.
Damping ratio . . .	20 : 1.

The early part of the month was favoured by calm nights, and it was at once observed that the microseisms were identical on both machines, in epoch, phase, period, and amplitude, thus demonstrating that microseisms are pure ground movements as distinguished from convection currents in the observatory or instrument cases, and that the term 'air-tremors,' as used by so many observers, is a misnomer; for it is not conceivable that air disturbances should so exactly coincide in separate buildings 60 feet apart. It was found that when the microseisms were intermittent they could easily be identified by their amplitude and number of waves in a group, also by the interval between successive groups (see A and B, Plate). In the past, microseisms have been investigated by observing their period and intensity, and the results compared with similar data from other stations. It is here suggested that a more fruitful method may be by gradually separating two or more machines, commencing with a few hundred yards or a mile or two, and, if the trains of waves could be still identified, increasing the distance to form a base line of sufficient length to determine their speed of propagation and the direction in which they were travelling. If this much could be achieved it is possible their cause and origin might be discovered.

A horizontal pendulum has two types of sensitivity, one to tilt, and the other to horizontal displacement. The former is regulated by the inclination of its supports; the latter is a result of its design, and is proportional to the ratio of the leverage about the mass acting as a steady-point, and magnifies and records the horizontal ground movements; this sensitivity is termed its 'magnification.' The constants of one of the instruments were then altered so that, while the magnification remained the same, the tilt sensitivity was raised very considerably. But alterations in sensitivity to tilt had no effect upon the recorded amplitude of the microseisms, suggesting that microseisms are purely compressional waves.

The constants used were :—

No. 8. Mag. 150	Tilt 1 sec. =	110 mm.
No. 9. ,, 150	,, ,, =	26 mm.

On May 4 a small earthquake shock was recorded at a moment when the constants of the machines were alike, and it was gratifying to find that two damped machines when properly standardised may be relied upon to give similar results. The Plate shows comparable sections of this record from each machine; also part of the record taken at Shide (126 miles distant), which shows the same characteristics as the other two—note the isolated movement C. (Up the film at West Bromwich compares with down the film at Shide, and the paper speed is slower.) The letters A, B, C, &c., identify corresponding movements. The discrepancy at G was due to a fault in the driving motor, which has since been remedied, otherwise the West Bromwich records are identical.

On May 15 the maximum of another small shock was recorded when the periods were 40 seconds and 12 seconds respectively, and the damping 7 : 1 *v.* 14 : 1. The longer period gave from three to seven times the amplitude according to the impressed earth period. As the dampings were unequal, the result was not strictly comparable; but it was noted that the longer period, notwithstanding the advantage of less damping, was much slower to take up the earth wave. The long period pendulum showed a lag of from 5 to 9 seconds behind the other, according to the impressed wave period being short or long respectively.

This points to the desirability of machines conforming to some standard period if the times from different records are to be strictly comparable.

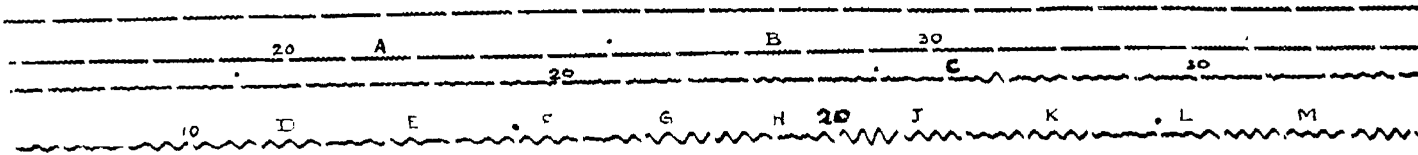
A further important observation was the fact—previously referred to in these Reports—that two sites comparatively near together may be quite different as regards daily wandering of the zero.

In the Plate the difference in the spacing of the lines on the two machines shows this clearly. The one with the wider spaces was taken in the out-building, and the displacement corresponded to an elongation of the sunny side during the day and a contraction at night, and was greatest on hot days. Time did not permit of sufficient investigation to discover whether the pier, cast-iron column, or whole house were affected. If only one or both of the former, then, since the heat rays are from the infra red end of the spectrum, protection may be afforded by interposing some athermanous substance, such as glass or water.

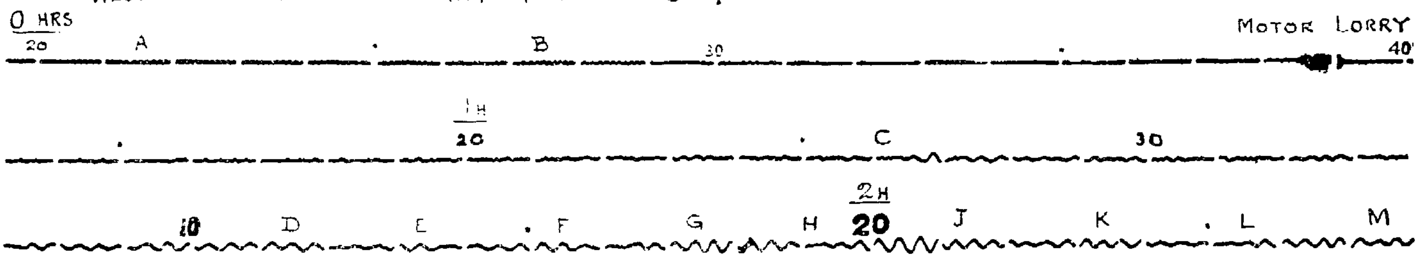
In the 1915 Report attention was drawn to two machines in the same azimuth, at Shide, behaving quite differently as regards wandering. The Milne-Burgess was not only less affected, but also showed a time lag behind the Milne-Shaw. The Milne-Burgess machine was fitted with a large glass cover. It seems probable that the athermancy of glass to dark heat may have been the cause of the observed effect.

If the iron column only is affected, then a great advance might be secured by making the column of substances with very low co-efficients of expansion, such as silica or invar steel.

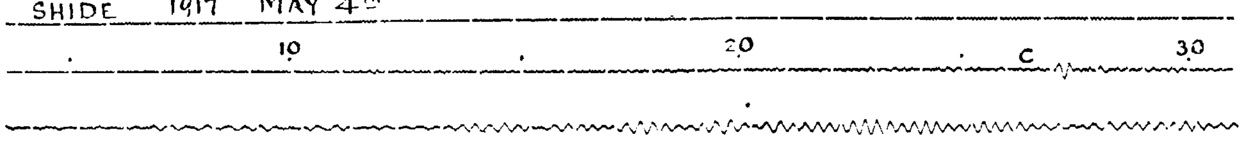
WEST BROMWICH 1917 MAY 4TH No 8



WEST BROMWICH 1917 MAY 4TH No 9



SHIDE 1917 MAY 4TH



III. *Tables for P and S.*

'The Large Earthquakes of 1913' were collated and printed in a special pamphlet of xii+74 pages. In the xii pages of introduction a provisional analysis of the residuals for P and S is given; but, the deductions there made not proving workable, a new analysis of all the 1913 and 1914 material has been undertaken, in which the residuals for different types of machine were kept separate. The help of Dr. G. W. Walker, F.R.S., in pointing out some errors and unworkable deductions is gratefully acknowledged.

The correction of greatest importance refers to the identification of S at distances exceeding 90°. The provisional analysis of the 1913 results shows that there are several phenomena which have been confused as S, but are really separate. They were denoted First Set, Second Set, &c. The First Set lay near the adopted tables, but there were few of them; the Second Set, arriving about a minute *earlier* than the First at $\Delta=100°$, is favoured by the great majority of records from $\Delta=90°$ to 105° (115 records against 29), and for this reason was assumed to be the true S. This assumption led to the inference that the times of transmission for S and P became nearly constant beyond $\Delta=100°$, and an explanation was suggested why they became faint or even disappeared (p. viii). But Dr. G. W. Walker pointed out some grave objections to this identification, which must clearly be given up, with its consequent inferences. The true S is the 'First Set,' not far from the existing tables; the 'Second Set' is probably the Y phenomenon to which attention was called in the 20th Report, and it follows that *most* observatories have recorded Y in mistake for S; only at Pulkovo and in cases where special care is taken has the true S been identified. The examples given in the 20th Report will serve to show how readily the mistake may be made. Y comes before S; and if the *first* big movement is taken, it is natural to record Y.

The discussion in the introduction to the 1913 earthquakes was made in terms of S—P, under the assumption that the ratio of S to P was nearly constant. But this assumption was one of the faulty consequences of the wrong identification of S, and falls with it. Hence a new discussion of S and P separately was undertaken, using the whole of the 1913 and 1914 material together. The data were written on cards which could be arranged in various ways, and it was determined to separate the different types of machine. It was found that there was enough material to find the errors of the tables from Galitzin machines alone, and even more for Wiechert machines alone. Accordingly these two determinations were made and are given side by side in Table I.

The first column shows the mean Δ for the group. On the cards Δ was entered to 1°, and a long list of residuals was made for every 1°; but examination showed that little was gained by grouping in less than 5° sets. Under G are shown the mean residuals for Galitzin machines, followed by the number of records, and under W the corresponding numbers for Wiechert machines. In forming these means obvious mistakes were excluded; there is no practical difficulty in doing this except near $\Delta=90°$ for S, to which we must devote special attention.

It will be seen that there is a systematic difference W–G of +3·3s. for P and +5·5s. for S. If these mean values be subtracted from the columns for W–G, the numerical mean of the residual differences is ±2·3s. for P and ±3·5s. for S.

Table I.

Observed Corrections to P and S from Galitzin and Wiechert Machines in 1913–1914.

	P					S				
Δ	G	No.	W	No.	W−G	G	No.	W	No.	W−G
°	s.		s.		s.	s.		s.		s.
8	0·0	7	+4·4	43	+4·4	− 1·7	3	+ 8·8	25	+10·5
13	+ 0·6	8	+3·2	22	+2·6	−12·9	7	− 1·1	14	−11·8
18	− 0·3	18	+0·3	34	+0·6	− 0·6	16	+ 4·1	28	+ 4·7
23	− 0·5	20	−0·3	41	+0·2	− 6·7	21	− 1·4	29	+ 5·3
28	− 6·2	26	−4·2	39	+2·0	−10·9	23	− 7·7	34	+ 3·2
33	− 5·3	20	−1·0	26	+4·3	−24·0	10	−10·7	14	+13·3
38	− 6·6	22	−3·3	33	+3·3	−18·8	20	−13·5	26	+ 5·3
43	− 2·0	13	−0·2	26	+1·8	−12·5	12	−14·4	18	− 1·9
48	− 3·1	27	+2·8	25	+5·9	−10·2	24	− 3·8	21	+ 6·4
53	− 1·9	24	+3·6	14	+5·5	− 2·1	17	− 3·0	10	− 0·9
58	+ 1·8	43	+8·8	17	+7·0	+ 2·8	37	+ 2·2	9	− 0·6
63	1·5	34	+5·4	14	+3·9	− 4·0	34	+ 6·8	11	10·8
68	+ 2·2	47	+2·2	16	−0·0	− 3·9	46	+ 1·3	11	5·2
73	− 1·0	54	+4·7	38	5·7	5·5	51	+ 3·2	36	8·7
78	− 1·7	51	−1·1	56	+0·6	− 8·3	51	− 1·1	53	+ 7·2
83	− 1·6	37	−1·4	101	0·2	−11·8	36	− 7·1	99	+ 4·7
88	+ 2·1	36	+1·0	52	−1·1	−16·7	36	−14·1	55	+ 2·6
93	− 5·7	19	−1·8	34	+3·9	—	—	—	—	—
98	− 7·3	11	−0·7	35	+6·6	—	—	—	—	—
103	−12·7	24	−3·2	14	+9·5	—	—	—	—	—
108	−11·4	21	—	—	—	—	—	—	—	—
113	−15·6	9	—	—	—	—	—	—	—	—
118	− 6·4	7	—	—	—	—	—	—	—	—
123	− 1·2	6	—	—	—	—	—	—	—	—

In the last Report corrections to the adopted tables for P were suggested of about +17s. near Δ=19°, and about −23s. near Δ=30°, with correspondingly larger corrections for S. These large corrections are not confirmed by the present investigation, which shows quite small corrections in this neighbourhood. The attempted explanation of the phenomenon PX on these lines must therefore be given up. There is no doubt about the reality of the phenomenon, or of that designated Y; but the explanations of both must be sought on other lines.

Turning to the bottom of the table we see that records of P fail rapidly after Δ=110° even for G machines, and entirely for W machines. After Δ=105° there are two records only by W machines at all near the tables: −7s. at Δ=106°, and +10s. at Δ=115°. Other records are (in seconds of time):—

Δ	
° °	
106–110	−91 −61 +62 +148 +156 +242 +268
111–115	−110 +36 +147 +154 +205 +214 +271
116–120	+204 +269 +328 +359 +611
121–125	+180 +192 +194 +196 +197 +197 +198 +208 +209 +209 +211 +218 +274 +277 +283 +331 +340 +621

At distances Δ=121°–125° it is clear that the records of P relate to what has been called PX, following P by about 200 seconds, but it is not altogether clear whether the transition is gradual or abrupt. Several investigators have treated PX as the true P; have remarked on the discontinuity'; and have inferred surfaces of discontinuity within the earth to account for it. The view taken in the last Report is that PX is not P; that the true P may be recorded alongside PX, but that as P tends to become faint after $\Delta=90°$, PX is read as P by mistake. By $\Delta=121°–125°$ P has become so faint as only to be read very rarely even, on Galitzin machines (and only twice on W as above). Galitzin machines mistake PX for P at times. We have

Other Records of Galitzin Machines.

Δ		Δ	
° °	s. s.	° °	s. s. s. s.
96–100	+75 +220	111–115	+68 +114 +195 +257
101–105	+169	116–120	+61 +200 +274 +297
106–110	+77 +256	121–125	+51 +60 +199

The discussion of the tables for both P and S beyond 90° thus involves special difficulties and is deferred to the next Report. Meantime the following table gives the best values for P and S that can be got from the above discussion, as applicable to G machines. In deriving them the W machines have of course also been used, with the above constant corrections. Further a little 'smoothing' has been used, and the values of S and P compared and slightly modified so that the ratio of S to P may vary continuously; the change appears indeed to be sensibly linear. The tenths of seconds are inserted merely to make the tables smooth; in deriving the corrections, the means of five consecutive figures in the adopted tables have been taken.

TABLE II.

Suggested Corrections to Adopted Tables for G Machines only.

	Corrections			New P	New S	Ratio
Δ	P	S	S–P			
°	s.	s.	s.	s.	s.	
8	+ 2·4	− 1·1	− 3·5	123·4	215·3	1·745
13	+ 0·3	− 7·7	− 8·0	192·5	335·7	1·749
18	+ 2·2	− 5·6	− 7·8	259·0	454·0	1·753
23	+ 0·4	− 7·9	− 8·3	316·6	556·3	1·757
28	− 1·8	−13·3	−11·5	366·2	645·3	1·762
33	− 5·3	−18·7	−13·4	410·5	724·9	1·766
38	− 6·5	−18·8	−12·3	451·7	799·4	1·770
43	− 6·5	−15·8	− 9·3	491·7	872·4	1·774
48	− 3·2	−10·4	− 7·2	530·4	943·2	1·778
53	+ 1·2	− 4·7	− 5·9	567·4	1011·5	1·782
58	+ 4·1	− 1·3	− 5·4	602·9	1077·3	1·786
63	+ 4·4	− 0·9	− 5·3	636·2	1139·7	1·792
68	+ 3·9	− 2·3	− 6·2	668·1	1199·5	1·796
73	+ 2·2	− 5·3	− 7·5	698·0	1256·7	1·800
78	− 0·2	− 8·9	− 8·7	726·8	1311·3	1·804
83	− 2·1	−13·0	−10·9	754·1	1364·0	1·808
88	− 4·3	−17·4	−13·1	780·3	1414·6	1·813
93	− 6·6	−21·1	−14·5	805·6	1464·1	1·818
98	− 9·5	−23·6	−14·1	830·3	1512·4	1·822
103	−10·3	−23·5	−13·2	854·5	1560·3	1·826
108	−10·2	−22·8	−12·6	878·0	1607·2	1·831
113	− 9·6	−19·3	− 9·7	901·6	1654·5	1·835
118	− 8·9	−12·9	− 4·0	924·7	1700·9	1·840

As regards seismographs other than G, the corrections to G appear to be sensibly constant at any rate to a good first approximation. They are assigned by the 1913 and 1914 results as follows, omitting some of which observations are scanty :—

TABLE III.

Corrections to G from other Seismographs.

Type	Correction to P	No. Observatns.	Correction to S	No. Observatns.	Correction to S–P
	s.		s.		s.
Alfani	+ 3·8	17	− 3·2	17	−7·0
Bosch	+ 1·8	27	− 2·9	22	−4·7
Bifilar	+ 1·3	19	− 9·4	16	−8·1
Bosch-Omori . .	+ 1·9	18	+ 4·0	39	+2·1
Cartuja	+ 3·3	36	+ 6·8	31	+3·5
Heidelberg . . .	+ 2·3	27	+ 4·3	34	+2·0
Hamburg . . .	+ 4·1	39	+ 1·2	32	−2·9
La Paz	+13·8	17	+17·0	10	+3·2
Mainka	− 0·7	124	− 0·2	112	+0·5
Omori	+ 6·5	95	+ 9·8	57	+3·3
Omori-Alfani . .	− 0·7	21	+ 3·5	10	+4·2
Omori-Ewing . .	− 2·6	29	−11·5	17	−8·9
Riverview . . .	+ 1·6	25	+ 7·2	29	+5·6
Stiattesi . . .	− 0·6	53	− 1·6	65	−1·0
Vincentini . . .	+ 0·6	48	+ 2·6	33	+2·0
Wiechert . . .	+ 3·3	720	+ 5·7	493	+2·4

It will be seen that the corrections to the tables for G instruments, and the differences between one instrument and another, are such as to involve sensible corrections to the determinations of epicentre; and the next step is clearly a revision of the epicentres before forming definitive corrections to tables. This revision, and the analysis of the errors beyond $\Delta=90°$, must be reserved for another Report.

Distribution of Epicentres.

The epicentres for 1913 and 1914 were entered on a map and were found to lie near two great circles cutting at right angles. The first of these has its pole just east of Malta, in

longitude 17° E., latitude 35° N.,

which is the centre of a very good 'land-hemisphere,' the boundary of which skirts the E. coast of Asia; it crosses both the Americas, and leaves Australia and several of the large islands in the 'water-hemisphere.'

The other has its pole in

longitude 236° E. = 124° W.; lat. 48° N.

A third circle cutting both these at right angles does not seem to be active; its pole is at 122° E.; 20° N.

In view of the proposed revision of epicentres (which, however, is not likely to modify these figures seriously), the reproduction of the maps is held over for the present.

[The following note was received after the rest of the Report had been sent to press.]

Focal Depth and the Time Curve. *By* Dr. G. W. Walker, *F.R.S.*

Assuming that P, the first impulse on a seismogram, corresponds to a longitudinal wave from the focus of an earthquake, the slope of the time curve for P as a function of the epicentral distance Δ is connected with the apparent angle of emergence $\bar{e}$ by the well-known relation

$$\frac{dT}{d\Delta}=\frac{1}{V_2}\left\{\frac{1-\sin\bar{e}}{2}\right\}^{\frac{1}{2}}$$

V_2 being the speed of transversal waves at the surface.

$\bar{e}$ has been directly measured by Galitzin at Pulkovo for Δ from 2,500 kms. to 13,000 kms., and the results differ markedly from the values of $\bar{e}$ calculated from Zöppritz's time curve for P. Galitzin finds a clear minimum of 42° for $\bar{e}$ at $\Delta=4{,}000$ kms., whereas no minimum is indicated in the calculated values of $\bar{e}$ (*cf.* 'Modern Seismology,' p. 54).

Further observations are required before we can regard Galitzin's results as characteristic of the whole earth, but I think it will be difficult to explain these results as peculiar to Pulkovo.

It is important to see how far we can reconcile these conflicting results.

By graphical integration of the observed values of $\bar{e}$, we get the time curve, and using Zöppritz's value of V_2 I find that the two curves can be fitted from 6,000 kms. to 12,000 kms. with a time discrepancy of ±11 seconds. The discrepancy would, however, reach 100 s. at 3,000 kms.

Using a larger value of V_2 we can fit the curves from 3,500 kms. to 8,000 kms., with a discrepancy of only ±5 sec., but the discrepancy mounts up beyond those limits of distance. It is not yet possible to decide what compromise is most reasonable. We may note, however, that considerable discrepancy may be allowed for distances < 3,000 kms., as soon as we admit finite depth of focus.

Kovesligethy has shown the connection that exists between a minimum angle of emergence and focal depth, and the obvious inference from Galitzin's results is that the focal depth is about 1,300 kms., or even a little more.

This a very startling result, being 10 times the greatest estimate of depth hitherto given. Yet there appears no escape from the conclusion if we accept Galitzin's results, and it is remarkable that this depth is about the same as the depth of Wiechert's layer of discontinuity.

If such a depth of focus is correct, the whole question of reflexions has to be re-examined. As a qualitative guide to this, I have considered a uniform earth with focal depth 0·2 of the earth's radius taking $V_1/V_2=\sqrt{3}$. Some remarkable results follow, which I can indicate but briefly.

(1) Surface reflexion of waves either entirely longitudinal or entirely transversal over their whole path cannot occur till $\Delta=103°$, and beyond this there are two paths for a once-reflected wave. There are no paths for a twice- or multiply-reflected wave. I suggest the possible association of this with the ambiguous character of S at 90°, noted by Professor Turner.

(2) PS and SP waves are no longer coincident in point of time. PS does not occur until $\Delta=149°$, and beyond this there are two possible

paths, but there is no PS_2 or higher term. On the other hand, SP with an infinite series SP_n may begin at $\Delta = 11°$, and there is only one path for each member until $\Delta = 99°$, beyond which there are two possible paths for each member.

In the range $\Delta = 11°$ to 99°, the manufacture of Rayleigh waves goes on.

Actual figures for the earth will, no doubt, modify these numbers in the sense that they will be smaller than those for a uniform earth, and careful analysis must be made to see if the phenomena of the seismogram are consistent with a focus as deep as 1,300 kms.

Meanwhile, it appears desirable to draw attention to this direct inference from Galitzin's measurements.