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Contact

Niedersächsische Staats- und Universitätsbibliothek Göttingen
Georg-August-Universität Göttingen
Platz der Göttinger Sieben 1
37073 Göttingen
Germany
Email: gdz@sub.uni-goettingen.de

Remarks on the Paper of G. Schmerwitz on Central European Earthquakes

By **Harold Jeffreys,** Cambridge, England

Dr. Schmerwitz has carried out a detailed study*) of the Pg and Sg observations in eight recent earthquakes, using a satisfactory method of determining the parameters by least squares, and giving valid estimates of the standard errors. His main result is that the accuracies claimed hitherto for the estimates of focal depth have been much too great. I have been maintaining this for many years, but Dr. Schmerwitz's only reference to me consists in a criticism of my estimates of the uncertainties of the velocities as too low. My conclusion, for the six earthquakes retained in my final solution, was that the standard errors of the focal depths were all about 7 km; and this was after combining the whole of the information from six phases and assuming that the velocity of each phase was always the same **). I added (p. 212): "The uncertainties that remain, even after combining the data from several near earthquakes and for surface waves, indicate the futility of attempts to get accurate estimates of the epicentre, the velocities, the thicknesses, and the focal depth simultaneously from a single near earthquake". In an earlier paper ***) I discussed the evidence for focal depth for the earthquakes of 1911 Nov. 16 and 1913 July 20, and came to the conclusion, having regard to the residuals of the near stations, that the focal depth could not be found from Pg alone within 30 km. I gave the equations for determining the origin time, velocity and focal depth by maximum likelihood, and these equations, extended to determine the epicentre also, are given by Schmerwitz. I also pointed out the unsatisfactory feature of Inglada's method, that it makes the estimate of focal depth depend mainly on the more distant stations, where the effect of any possible change of the adopted depth would be utterly inappreciable. Schmerwitz also rejects this method as unsatisfactory.

Schmerwitz objects to my estimates of the velocities that I took the epicentres as known, and maintains that if I had introduced them as additional unknowns I should have got larger uncertainties. This is true qualitatively, but in one case, the Oppau explosion, the epicentre was known; in four, the two South German earthquakes, and the Tauern and Schwadorf ones, the distribution of stations in azimuth is good, and an error in the epicentre is unlikely to affect the estimate of the velocity much; and in the Jersey and Hereford ones the epicentres were found together with the velocities. I should therefore greatly doubt

*) Zeitschr. f. Geophys. **14**, 351—390 (1938).
) M. N. R. A. S. Geophys. Suppl. **4, 211 (1937).
***) M. N. R. A. S. Geophys. Suppl. **3**, 148—152 (1933).

whether the uncertainties of the velocities can be appreciably too low for this reason. The only earthquake considered both by Schmerwitz and by me was the Tirol one of 1930 Oct. 7. (Oct. 8 C. E. T.). For this my results were: Pg, 5.724 ± 0.026; Sg, 3.466 ± 0.021. His are, respectively, 5.68 ± 0.15 and 3.42 ± 0.04. The difference in the standard errors does not appear, however, to be due mainly to his allowance for three additional unknowns. I used 18 observations for Pg, with a standard error of 0.41 s., 15 for Sg, with a standard error of 0.66 s. He has used 22 for Pg and 16 for Sg, and the sums of the squares of the residuals, 20.4 sec^2 and 9.4 sec^2, lead to standard errors of one observation of 1.09 s. and 0.92 s. It appears therefore that most of the difference is due to my having rejected a number of outlying residuals. I do not wish to insist on this rejection having been right; but in most sets of seismological observations there are clear signs of departure from the normal law of error, and some means of allowing for the departure is desirable. I should now use a system of weighting instead, as introduced in my treatment of large earthquakes *). It is possible in this case that the estimated uncertainties are too low for this reason. But this has not affected my final results, because I found that the determinations for this earthquake were so discordant from the others, having regard to the apparent uncertainty, that I actually inferred that the uncertainty found was too low and rejected the determinations altogether. The treatment that I adopted was open to improvement, but the use of the χ^2 check has prevented it from giving a serious error in the summaries. In the earthquakes retained, the correct weights were used in estimating the constant terms (p. 200).

Schmerwitz also objects to the use of the 1911 and 1913 earthquakes and of the Oppau explosion on the ground that the accuracy attainable then was insufficient. I agree that accuracy has improved; but so much use has been made of these shocks that a full treatment was necessary. For the first two a number of observations had to be rejected, but this was done without reference to the velocities indicated by other earthquakes, and if too many were rejected the result would be a spurious appearance of accuracy, which would be detected on comparison of different earthquakes by means of χ^2. A normal χ^2 was actually found, and there appears to be no ground for disregarding this information. The following table compares the standard errors of one observation of Pg in the earthquakes that I finally used and in those used by Schmerwitz.

S. German I	1.3	1935 Dec. 30 I	0.40
S. German II	1.34	1935 Dec. 30 II	0.40
Tauern	0.56	1932 Nov. 20	0.65
Oppau	0.81	1935 June 27	0.36
Jersey	0.93	1934 June 8	1.12
Hereford	1.14	1930 Oct. 8	1.09
Schwadorf	0.61	1935 Jan. 31	0.25

*) Bur. Centr. Séism. Int., Strasbourg, Trav. Sci. **14** (1936).

On the whole the differences are not striking. I am inclined to think that some of the later ones are too low, because two series of Californian earthquakes given by Gutenberg have given 0.77 s. and 0.9 s. for Pg, 0.55 s. for Pn. Since these readings were made on instruments with about four times the paper speed of some of the European ones, and with remarkably steady drum rates, the results for 1935 Jan. 31 and June 27 are probably accidentally close agreements, resting on 4 and 3 degrees of freedom respectively.

Schmerwitz does not combine his results for the velocities, and it is desirable to do so and see whether they agree with mine. For Pg, with the standard errors as given, they are as follows.

		w	ξ	$w\,\xi^2$
1935 Dec. 30 I	5.47 ± 0.21	23	$-.134$	0.71
1935 Dec. 30 II	5.57 ± 0.22	21	$-.084$	0.15
1932 Nov. 30	5.48 ± 0.11	80	$-.174$	2.44
1935 June 27	5.86 ± 0.16	39	$+.206$	1.66
1934 June 8	5.79 ± 0.11	80	$+.136$	1.49
1930 Oct. 8	5.68 ± 0.15	45	$+.026$	0.03
1935 Jan. 31	5.65 ± 0.02	2500		
			Total	6.48

The weights w are such that unit weight would mean a standard error of 1 km/sec. The weighted mean, with the exception of the last, is 5.654 ± 0.059. ξ gives the corresponding residuals, and

$$\chi^2 = \Sigma\, w\, \xi^2 = 6.48.$$

Since the expectation of χ^2 from 6 determinations, from which one unknown has been estimated, is 5 ± 3, this is quite satisfactory*). The weight of the last entry is incredible; if we divide it by 6 and combine with the remainder we get 5.652 ± 0.038. My value is 5.570 ± 0.020, so that the difference is 0.082 ± 0.042, which is not large enough in relation to the standard error to call for special comment.

For Sg the results are:

		ξ
1935 June 27	3.47 ± 0.03	$+.076$
1935 June 28	3.50 ± 0.04	$+.106$
1934 June 8	3.32 ± 0.03	$-.074$
1930 Oct. 8	3.42 ± 0.04	$+.026$
1935 Jan. 31	3.32 ± 0.03	$-.074$

The weighted mean is 3.394 ± 0.015. But four of the five residuals exceed twice the apparent standard errors, and it seems that the latter must be too low. The uncertainty can be found only from the scatter of the separate determinations. A simple mean is 3.406 ± 0.037, and it will be safest, in considering estimates

*) Since some of the estimates of the uncertainty rest on only a few degrees of freedom, this expectation is a little too low, but closer evaluation is not needed.

based on Sg, to multiply the standard errors by 2.5. My determination is 3.365 ± 0.008, and the agreement is near enough.

Dr. Hiller has kindly called my attention to the fact that several of the South German stations have recently been equipped with instruments with a paper speed of about 60 mm per minute, and the same applies to Strasbourg and the Swiss stations. Since smoked paper records give a clearer line than photographic ones, the fact that the standard errors are smaller than for the Californian earthquakes is not surprising. The error of reading is not, however, the only one; in particular there are the comparisons of the seismograph with the observatory clock and of the clock with the wireless signal. The very small standard errors for 1935 Jan. 31 and June 27, however, may well represent accidental close agreements.

Such agreements, however, have been known to arise in some cases from what is not usually regarded as a fault: extreme care in observing. In some series of observations given by Karl Pearson the means of 25 consecutive observations were no steadier than those of 2 to 15 should have been on the hypothesis of the independence of the errors; and if such observations are summarized by the usual method of least squares the apparent accuracy will be much too low. Now in seismological studies, where all the records of an earthquake are read by one observer, there is in any case a risk of a personal error, which can be tested only by comparison with other observers; but also the danger of correlation between the errors is serious. If the same record is read several times, possibly after working out residuals, it will be very difficult for the observer to prevent his readings from being affected by those at neighbouring distances, and the uncertainty found on the hypothesis of the independence of the errors will be much too low. In the three studies that I made, each record was read only once, and they were taken in the order of receipt, which was not the order of epicentral distance. My uncertainties were larger than in some other studies, but this method would avoid the risk of a spurious appearance of accuracy. In any case it seems to be important that in special studies it should be stated what precautions, if any, have been taken to avoid the danger of a correlation between the errors.

Dr. Hiller tells me that in his studies similar precautions were taken, and also that the accuracy was confirmed by the agreement of his readings with those of Dr. Wanner.

We now come to the estimates of focal depth. n is the number of degrees of freedom, that is, the number of observations reduced by 5, to allow for the number of parameters estimated.

		n	$P(t)$
1935 Dec. 30 I	Pg 46 ± 11	4	0.015
1935 Dec. 30 II	Pg 37 ± 11	4	0.03
1932 Nov. 20	Pg 35 ± 18	5	0.11
1935 June 27	Pg 21 ± 10	3	0.13
	Sg 16 ± 4	2	0.07
1935 June 28	Sg 9 ± 7	2	0.33

		n	$P(t)$
1934 June 8	Pg negative	10	
	Sg negative	10	
1930 Oct. 8	Pg 31 ± 38	17	0.42
	Sg negative	11	
1935 Jan. 31	Pg negative	4	
	Sg 11 ± 13	6	0.42

The outstanding feature is that in spite of the number of estimates near 30 or 35 km. that have been given, usually with uncertainties of about 2 km., Schmerwitz's correct method has given four negative determinations (really h^2 negative) out of 12, and two others are less than the standard errors. The validity of the rest can be judged from the values of $P(t)$. t here is the ratio of the estimate to its estimated standard error, and $P(t)$ is the probability of getting such a value of t, or a larger one , if the true value was 0. $P(t)$ is taken from Fisher's table*). Values of P less than 0.05 are usually taken as ground for suspecting that the true value is not 0, of 0.01 as ground for considerable confidence that it is not. The only values less than 0.05 are for the two earthquakes of 1935 Dec. 30, and as these are merely the extreme cases selected out of 12 it appears that they may well be accidental. Thus there is no evidence in these determinations that the focal depth was ever large enough to be asserted from the observations of Pg and Sg to be different from zero. In that case Schmerwitz's conclusion that the velocity decreases with increasing focal depth will fail. The result would be found if the apparent variation of depth is random. For if the depths are all small, and the velocity the same, $dt/d\Delta$ at intermediate distances would be the same. But if a random error leads to a positive estimate of depth this would decrease $dt/d\Delta$, and the actual value would be recovered only by reducing the estimated velocity.

Nevertheless his result is of much importance as giving additional evidence of the unsatisfactory character of attempts to determine focal depth from Pg and Sg alone. It may be remarked that the estimates that have usually been obtained by these methods are contradicted by all other evidence. Stoneley has studied the dispersion of Love waves; denoting the thicknesses of the upper and intermediate layers by T and T', he finds**) that the group velocities agree with any of the following: $T = 19$ km., $T' = 0$; $T = T' = 15.7$ km.; $T = 13$ km, $T' = 26$ km. At the outside the thickness of the upper layer cannot be more than 19 km.; and a focus cannot be 30 km. deep in a layer whose thickness is only 19 km. at the most. The method will not separate T and T', but leads to an equation connecting them that may be written

$$4T + T' = 78 \pm 8 \text{ km.}$$

Using Rayleigh waves, but neglecting the intermediate layer, I got values of 17 to 29 km. from waves of different periods***); these will be reduced somewhat

*) Statistical Methods for Research workers, 1936, Table IV.
) M. N. R. A. S. Geophys. Suppl. **1, 530—531 (1928).
***) M. N. R. A. S. Geophys. Suppl. **3**, 261 (1935).

if the intermediate layer is taken into account. In my last study of near earthquakes I found complications from a number of systematic errors, and finally found that the only datum left about the thicknesses was

$$T' = 9 \pm 3 \text{ km.},$$

with a possibility that this standard error might be a little too low. Study of the $pP - P$ and $sS - S$ intervals in Japanese deep focus earthquakes *) has led to the equation

$$(T - 17) + 0.85\,(T' - 9) = + 5.7 \pm 1.3.$$

Combining this with the surface wave equation gives

$$T = 15 \pm 3 \text{ km.};\quad T' = 18 \pm 4 \text{ km.}$$

This is the solution that I have used in constructing the P and S tables. If the near earthquake value for T' is also used, a least squares solution gives

$$T = 18.8 \pm 2.0 \text{ km.};\quad T' = 12.4 \pm 2.4 \text{ km.},$$

with the following residuals:

T' (near earthquakes)	$-$ 3.4
Deep foci	+ 1.0
Love Waves.	$-$ 9.6

These are fairly satisfactory in relation to the standard errors, giving a χ^2 of 3.2 on 1 degree of freedom; but I am inclined to think that the apparent accuracy of T' from near earthquakes is too high and that more weight might legitimately be attached to the surface waves. In any case a thickness of the upper layer appreciably over 20 km., and therefore focal depths over 20 km. in earthquakes showing Pg and Sg, are out of the question.

It has been suggested that from considerations of isostasy the thickness of the upper layer in an elevated region might be greater than in average Eurasian conditions, such as the surface waves refer to; but the average elevation of the regions surrounding these epicentres above the average of Eurasia is not more than 1 or 2 km., and though an increase of 5 km., might be possible on this ground, the data have to be stretched at every point to permit one of 10 km.

The difficulty of reconciling a T of more than about 25 km. with the measured rate of outflow of heat from the earth would be appalling; this also suggests values in the neighbourhood of 10 to 15 km.

To sum up, I agree that my estimates of the uncertainties of the velocities are a little too low; I should be quite prepared to admit that they should be multiplied by about 1.2, but I should be very much surprised if they need to be multiplied by 1.5. I welcome Schmerwitz's careful discussion of the uncertainties of the estimated focal depths, which adds force to what I have already said on the same subject; but when these uncertainties are taken into consideration there appears to be no evidence that focal depths in the upper layers can be estimated from Pg and Sg in these earthquakes alone. Such evidence might be found if the

*) M. N. R. A. S. Geophys. Suppl. 4, 451 (1939).

constant terms in the formulae for the times of arrival of Pg and Sg were compared with those for Pn and Sn.

With regard to the uncertainty of the velocities, my omission to allow for that of the epicentres should be considered in relation to the object of the investigation, which was to find, as far as possible, the thicknesses of the layers and the focal depths by comparing all the phases. It was necessary for this purpose only that estimates of the velocities should be made by combining all the earthquakes, so that the errors introduced into the estimates of the constant terms by those of the velocities should not be large compared with the uncertainties of the mean residuals. So long as this is satisfied it does not matter if the uncertainties of the velocities are a little too low; and in any case the contribution is allowed for when the final solution is compared with the constant terms and the validity of the uncertainty is checked by χ^2. If χ^2 is found too large at this stage it can be used to correct the uncertainty. It had been widely claimed (1) that the velocities differed in different earthquakes (2) that these earthquakes led to estimates of the focal depth from Pg alone. I found that (1) the estimated velocities were perfectly consistent, even with my slightly too low estimates of uncertainty (2) that even taking the velocities and the epicentres as accurate, the uncertainty of the focal depths found in this way was so large that the results were quite useless. An increase of the uncertainty would therefore strengthen both of my conclusions.

I think that Schmerwitz's method is unnecessarily laborious. The transformation to rectangular coordinates is unnecessary. The distances can be found from Turner's formula

$$2\,(1 - \cos \Delta) = 4 \sin^2 {}^1/_2\, \Delta = (a - A)^2 + (b - B)^2 + (c - C)^2$$

where A, B, C are direction cosines of the radius to the epicentre, a, b, c of that to the station, with respect to fixed axes at the centre of the earth. Geocentric direction cosines of the stations, and materials for finding those of the epicentres, are available to four figures *). If x and y are the displacements needed by the epicentre to the south and east, Y the increase of origin time, h' the increase of focal depth, an equation of condition will be

$$y + x \frac{\partial t}{\partial x} + y \frac{\partial t}{\partial y} + h' \frac{\partial t}{\partial h} = t\,(O - C)$$

where t is the calculated time of transmission; the right side is the residual at the station against the trial solution. Also if x and y are measured as angles, as seen from the centre of the earth, and

$$\frac{\partial t}{\partial \Delta} = p,$$

$$\frac{\partial t}{\partial x} = p \cos \alpha; \qquad \frac{\partial t}{\partial y} = - p \sin \alpha$$

*) The Geocentric Direction Cosines of Seismological Observatories, British Association, Gray Milne Trust, 1938.

where α is the azimuth of the station from the epicentre, measured from north through east. Thus all the equations of condition can be written down easily. If the velocity is to be found, it will require an additional term proportional to t (calc.) and the solution can then be completed. The east longitude of the epicentre must be increased by y sec φ, where φ is the latitude. If the trial epicentre is right within $0^0.2$ or so it is not necessary to have accurate values of α, since they can be read off a map, for near earthquakes, or from a globe, for distant ones, with as much accuracy as is needed. It is most convenient to carry the whole work out in degrees, converting velocities to km./sec, if required, only at the end of the work.

There appears to be evidence that continental near earthquakes have foci distributed through most of the depth of the upper layer, and in consideration of the evidence about its thickness it would be legitimate to take the depth of the focus, where Pg and Sg are read, as 10 ± 6 km.; this could be used as an additional equation of condition, and with it I should expect a considerable reduction to be found in the uncertainties of the velocities. The recent observational material appears to be excellent, but its full meaning will not be known until summary values of the velocities have been found from all the earthquakes together.

It would be too much to expect that the upper layer is homogeneous, and any irregularities of velocity will produce scattering. The effect of this has been studied qualitatively*). If the original disturbance is a simple displacement, the most rapid increase of displacement on a station will appear to travel with the mean velocity. But the actual observer looking for the beginning of the movement will read it earlier. The result may be either an apparently too high velocity, if his criterion is a given fraction of the final displacement at that distance; or a correct velocity with systematically early readings, if the criterion is a given absolute displacement. There are signs that these effects actually occur. If they do, a closer analysis of the recent observations might show systematically higher velocities than mine. This would not say that either set of observations was invalid; on the contrary, comparison of the two would give useful information about the scattering.

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*) M. N. R. A. S. Geophys. Suppl. 4, 220.

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