

DISCUSSION

SOURCE MECHANISM OF THE ALASKAN EARTHQUAKE OF 1964 FROM AMPLITUDES OF FREE OSCILLATIONS AND SURFACE WAVES – COMMENTS

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Recently Ben-Menahem et al. (1972) (hereafter abbreviated to B.R.I.) proposed, on the basis of long-period surface-wave and free-oscillation data, a steeply dipping fault model for the Alaskan earthquake of 1964. This result differs from that obtained by Kanamori (1970) (hereafter abbreviated to K.70) who also used long-period Love and Rayleigh waves, but preferred a low-angle fault model. I would like to make several comments on this difference.

1. Difference in the data

B.R.I. used equalized spectral amplitude and phase at 250 sec while K.70 used equalized time-domain signals. This difference, however, does not seem essential. A more essential difference is in the selection of the stations. For Love waves, no essential difference exists between these two studies (see fig. 7 of B.R.I. and fig. 2 of K.70). For Rayleigh waves, however, a substantial difference is obvious. The radiation pattern of B.R.I. has a node in S25°E direction, while that of K.70 shows a large amplitude of Rayleigh waves of $T \approx 250$ sec in this direction. It is unfortunate that the node of

B.R.I. is determined solely by the data obtained at SPA (station Nos. 1, 2 and 13). It is known among some “experimental” seismologists that SPA is an extremely anomalous station: the signals on the long-period records at SPA are always several times smaller, irrespective of earthquakes, than would be expected from the instrument magnification (375). Inclusion of SPA always leads to a node in the N–S direction. Dr. Robert P. McCarthy (private communication, 1972) of the National Geophysical Data Center of N.O.A.A. suggests that this anomalous situation is not due to instruments but to the special station environs; the station rests on ice whose estimated thickness is 3000

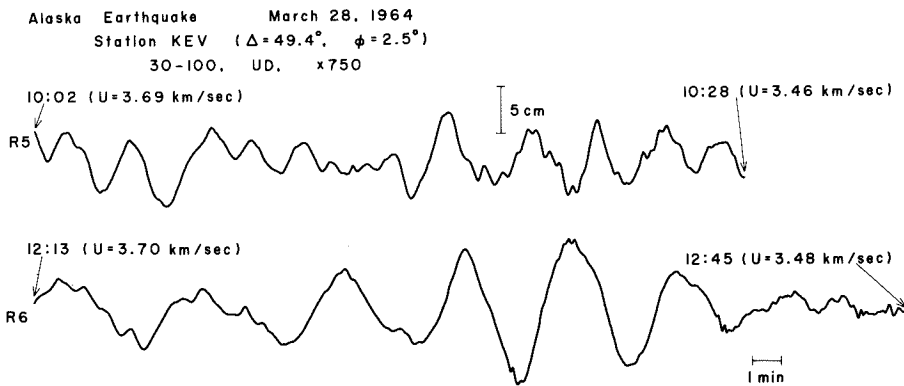


Fig. 1. Rayleigh waves R5 ($\Delta = 85497.8$ km, $\phi = 2.5^\circ$) and R6 ($\Delta = 114547.7$ km, $\phi = 182.5^\circ$) at KEV. Note the large amplitude of R6 and the large asymmetry.

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m. Actually several other stations indicate extremely large radiation of Rayleigh waves towards the south (e.g., R6 at KEV, NAI, SHI, and LAH, see Fig. 1). The azimuthal coverage of the Rayleigh-wave radiation pattern of B.R.I. is not very uniform. Except for SPA, the Rayleigh-wave data-set of B.R.I. has no stations over large azimuth ranges (from $S70^{\circ}E$ to $S10^{\circ}W$, and from $N70^{\circ}W$ to $N10^{\circ}E$). Thus the four-lobed pattern of Rayleigh waves at $T=250$ sec suggested by B.R.I. is not warranted.

2. Difference in the analysis methods

B.R.I. used the frequency-domain analysis and the numerical procedure developed by Ben-Menahem et al. (1968, 1970), while K.70 used time-domain analysis. The method of Ben-Menahem et al. (1968) which is based on the least-squares method aims at obtaining more objective results than by ordinary trial-and-error methods. This method in principle has a higher resolving power than the time-domain analysis. However, the choice of the data and the active parameters cannot be made completely objective. The time-domain analysis may be less objective, but is adequate for explaining the overall nature of the observations. In a broad sense, resolution is traded for the overall reliability, yet, as shown by fig. 4 of K.70, resolution good enough to discriminate a fairly large variety of fault models is retained. It seems that the choice of the method is to some extent a matter of philosophy and taste. Both methods should lead to physically equivalent results if they are applied appropriately to properly chosen observational data. I do not believe that the difference in the methods is essential to the difference in the results of B.R.I. and K.70.

3. Difference in the interpretations

There is an intrinsic difficulty in determining, from far-field observations, which of the two nodal planes of the double-couple source should be taken as the fault plane. For point source, the two nodal planes are equivalent. For finite source, the asymmetry in the radiation and/or the differential excitation of Love and Rayleigh waves at different depths can be used to select the fault plane. Kanamori (1970) found that the Love-wave amplitude is largest in SW direction while the Rayleigh-wave amplitude is largest in S–SE direction.

This result requires the rupture direction to be different from the fault strike. For such rupture propagation to be possible the low-angle nodal plane has to be taken as the fault plane. Because of the lack of Rayleigh-wave data in S–SE direction, B.R.I. did not find such difference in the direction of the asymmetry between Love and Rayleigh waves.

B.R.I. rejected the low-angle model because their low angle model requires extremely large dislocation, 80 m, which is incompatible with the field evidence. They obtained an average dislocation of 20 m if a steep fault extending to a depth of 125 km is adopted. Physically, these conclusions seem somewhat strange to me. Because the excitation of Rayleigh waves generally becomes less efficient as the depth increases, a larger average dislocation should be necessary for a steep fault extending to a large depth than for a low-angle fault of the same size but at a shallow depth. This is contrary to what B.R.I. found. Although I may have missed some subtle point in B.R.I., the above conflict seems to have resulted from the relation $dS = b dh$ used in the derivation of the source dislocation profile. It seems to me that the surface element dS should be put equal to the down-dip width $dh/\sin \delta$ multiplied by the fault length b , i.e., $dS = b dh/\sin \delta$. If so, the low-angle model would not require such a large dislocation as 80 m; then it would not be incompatible with the field evidence. It appears that the low-angle model is equally compatible with B.R.I.'s data.

B.R.I. presented several directivity data. Use of directivity function, first devised by Ben-Menahem (1961) and used by Benioff et al. (1961), is certainly powerful in determining the rupture parameters. However, for fault planes having a considerable width, one-dimensional directivity function may not always give consistent interference patterns. For the Alaskan earthquake, as mentioned by B.R.I., the zeros and poles of the directivities were not always consistent with the theory. This is probably due to the large width of the fault plane of the Alaskan earthquake. Kanamori (1970) did not use the interference pattern, but tried to explain the overall asymmetries of the amplitude. It is of some interest to examine to what extent the rupture parameters of K.70 ($b = 600$ km, $v = 3.5$ km/sec, rupture direction $S25^{\circ}W$) fit the directivity data of B.R.I. Fig. 2A, B, C show the results of such examination. The fit between the calcu-

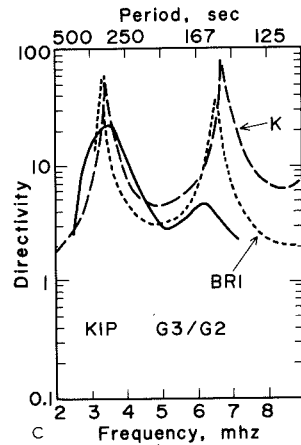
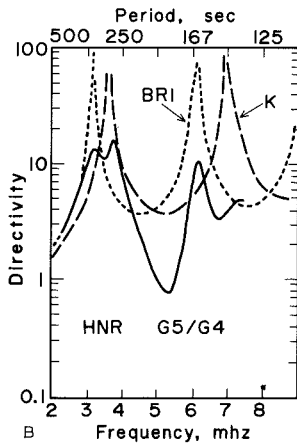
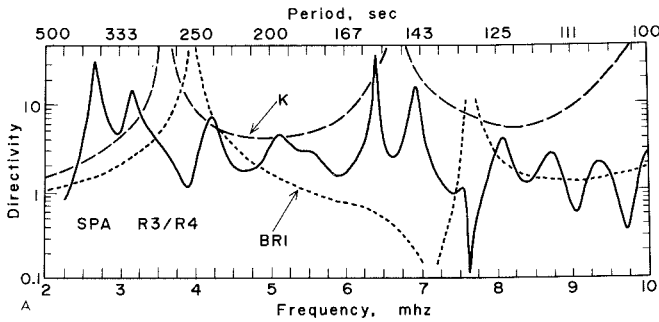


Fig. 2. Directivity at (A) SPA (Rayleigh wave), (B) HNR (Love wave), and (C) KIP (Love wave). The solid curve shows the observed directivity. The dotted and dashed curves show the calculated directivities for the rupture parameters of Ben-Menahem et al. (1972) ($b = 600$ km, $v = 3$ km/sec, $\phi = 245^\circ$ (for Rayleigh wave), $\phi = 225^\circ$ (for Love wave)) and Kanamori (1970) ($b = 600$ km, $v = 3.5$ km/sec, $\phi = 205^\circ$), respectively.

lated and the observed directivities is shown for SPA (the station anomaly does not affect the directivity) for Rayleigh waves, and for HNR and KIP for Love waves. The fit is reasonably good. The dotted curve in Fig. 2A which is calculated with B.R.I.'s rupture parameters ($b = 600$ km, $v = 3$ km/sec, rupture direction $S65^\circ W$) is significantly different from the curve shown in fig. 5 of B.R.I. I do not see the reason for this difference. For stations in the nodal direction such as ISA, the directivity calculated for the rupture parameters of K.70 does not explain the data shown in B.R.I. The radiation in the nodal direction may have been greatly affected by the irregularity of the source and the structure.

4. Discussions and conclusions

B.R.I. used the free-oscillation data. However, the free-oscillation data cannot discriminate very well the steep-fault model from the low-angle model. If a steep-fault model explains the data, its conjugate low-angle fault should be able to explain the data as well. In fact, Abe (1970) showed that the free-oscillation data can be consistently explained in terms of the low-angle fault model of Kanamori (1970).

It is interesting to note that, despite the major difference in the data and in the interpretation methods, the average potency of 1600 unit faults obtained by B.R.I. is consistent with the seismic moment of $7.5 \cdot 10^{29}$ dyn \cdot cm obtained by K.70: if we use $6 \cdot 10^{11}$ dyn/cm² for the average rigidity, the potency of 1600 unit faults is equivalent to a moment of $(1600 \cdot 10^{15}) \times (6 \cdot 10^{11}) \approx 10^{30}$ dyn \cdot cm. This agreement suggests that the determination of the seismic moment by long-period waves is reliable.

Although the analysis method developed by Ben-Menahem et al. (1968) is certainly very useful and will have wide applicability, I do not agree with B.R.I.'s conclusion that the steep fault is preferable to the low-angle fault.

The major observational fact that led me to pick up the low-angle model is the large asymmetry of the Rayleigh-wave radiation in N-S direction. The low-angle model in which a rupture direction is oblique to the fault strike is consistent with the overall radiation pattern of Love and Rayleigh waves (see fig. 1, 2, 7, 8 of K.70). Obviously the planar fault plane with uniform dislocation and one-dimensional rupture propagation cannot explain every detail of the surface waves excited by the actual faulting of the Alaskan earthquake which is presumably more complex in spatial and temporal characteristics. Unless the difference in such details affects in a major way the overall radiation pattern of surface waves, the low-angle fault is preferred to the steep fault. Even though the asymmetry could be explained by some other mechanism, the low-angle model would not be worse, if not better, than the steep model.

SOURCE MECHANISM OF THE ALASKAN EARTHQUAKE OF 1964 FROM AMPLITUDES OF FREE OSCILLATIONS AND SURFACE WAVES — REPLY

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The points that we wish to make in our reply are:

- (1) Our results are consistent with that part of the W.W.N.S.S. data which according to our criteria was usable and dependable.
- (2) The objections raised in K.70 are unfounded.
- (3) The low-angle model is not compatible with our data.
- (4) K.70 surface-wave data do not warrant a preferred low-angle solution.
- (5) There are errors in K.70 which render his results doubtful.

We shall now dwell on these points in some detail.

Acknowledgment

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1. Choice of data

Because of the entangled traces on many seismograms we found it absolutely necessary to take special measures to avoid misidentifications and contaminations by unwanted signals. I do not know by which criteria Kanamori selected his 19 stations out of the 91 available W.W.N.S.S. However, each of our 34 stations was admitted upon the fulfillment of the following conditions:

- (1) A continuous trace could be recovered and separated over the entire preassigned group-velocity window.

(2) A coherent pair of vertical and horizontal Rayleigh-wave components of the same station, could be traced.

(3) A coherent pair of Love signals on both horizontal seismograms of the same station, with no corresponding member on the vertical seismogram, could be traced.

(4) The existence of a reasonable fit of measured group-velocities of each signal with standard dispersion curves.

In addition, no Rayleigh signals with order higher than 5 were used, since the few that we could separate were found to be contaminated by other phases in the common spectral window.

2. Objections in K.70

These will be discussed in the order in which they appear in his comments.

(1). The data of SPA did not agree with the other data that Kanamori used. He was lucky to find somebody at N.O.A.A. who told him to put the blame on some 2700 m of ice. Due to the low magnification (375) at SPA, Love and Rayleigh waves from that station are immaculate and belong to the best documented data from the Alaskan shock. How some 3 km of ice can harm a giant wave-length of 1200 km is beyond my intelligence. Furthermore, the Love-wave data from SPA are consistent with our other Love-wave data (fig. 7 of B.R.I.) from nearby azimuths and also with Kanamori's own Love-wave data! (fig. 5 and 8 in K.70.) We have used SPA Rayleigh-wave data again in connection with the Rat Island shock of Feb. 4, 1965 (Ben-Menahem and Rosenman, 1972, table 7 and fig. 16, 17) and obtained very good agreement with readings at other stations with the same azimuth. Does this mean that the ice-cap at SPA melted away in the course of one year? Clearly, no one claims that the magnification at any W.W.N.S.S. station is known to high accuracy at periods which correspond to the steep portion of the instrumental magnification curve (see discussion in B.R.I.) but in this respect SPA is not worse than any other station and there is no justification of throwing the data away just on account of the ice-cap. Incidentally, I have always had some difficulties with the north-Indian station of LAH which tends to show excessive Rayleigh-wave ampli-

tudes. The reason, I believe, is the anomalous Himalayan structure which contributes a low value to the second derivative of the group-velocity with respect to the period. I therefore think that the LAH Rayleigh-wave data of K.70 have not been properly compensated for this effect.

(2). The second objection in K.70 is based on the premise that ". . . the excitation of Rayleigh waves generally becomes less efficient as the depth increases". This is a wrong statement for the period range of interest: 150–300 sec. On the contrary, amplitudes for dip-slip sources in this range increase with source depths of 0–200 km, as can be readily seen from the figures and tables of my Q_R function (Ben-Menahem et al., 1970, fig. 4, 5). In fact, Q_R increases by one order of magnitude at $T = 150$ sec as the source's depth increases from 20 to 120 km. Kanamori makes the same error in his paper (p.5035, top of right column). The alleged conflict that Kanamori claimed to have found is therefore nonexistent and a low-angle model should definitely require dislocations as large as 80 m. It seems to me that K.70 should look for possible errors in his Rayleigh-wave radiation-pattern program.

(3) K.70 calculated the theoretical directivity of SPA with a rupture direction $S65^\circ W$ and claimed to have found a significant difference from the curve shown in B.R.I. In the conclusion to our paper we stated that our strike azimuth is in the range $S(45^\circ-65^\circ) W$. Nowhere in the paper did we say that we have computed the directivity of SPA for $S65^\circ W$. Our directivity search-program did find a best fit at $S50^\circ W$. The statement in K.70 ". . . for stations in the nodal direction such as ISA, the directivity calculated for the rupture parameters in K.70 does not explain the data shown in B.R.I.", seems to me rather strange. Is it not sufficient that the data should fit our rupture parameters?

3. Errors and inconsistencies in Kanamori's paper

(1) Our analysis showed (fig. 8, B.R.I.) that the amplitudes of Rayleigh waves of BAG, ANP, SEO (Azimuths ranges $279^\circ-287^\circ$) are comparable to those at COP $R_4(192^\circ)$. While Kanamori admitted COP, he rejected BAG, ANP and SEO. As a result, his radiation pattern (fig. 1 in K.70) at the above

azimuth range is incomplete. Had he admitted these stations he would have come closer to our four-lobe pattern. Thus, the synthetic Rayleigh pattern shown in his fig. 7 does not fit the data, and his argument in favor of a low-angle model seems somewhat weak.

(2) We have shown earlier that Kanamori was wrong about the dependence of the excitation of Rayleigh waves on depth. He thus cannot make his own observations of the ratio of Love to Rayleigh waves (p. 5035 in K.70) fit the theoretical values. This, in turn, upsets his depth determination and hence also the fixation of his dip angle of 20° . Had he used the correct theoretical value for Love/Rayleigh ratio he would have obtained a dip angle of 70° – 80° .

(3) Kanamori claims that he had observed a large asymmetry in the radiation patterns of Rayleigh waves in a direction normal to the strike. We have shown in (1) that this asymmetry is fictitious and arises solely because of his rejection of key-data. There is therefore no need whatsoever to complicate the mechanism and find rescue through the introduction of an additional source parameter.

(4) From equations (1–66) and (1–75) in my 1961 paper it is possible to calculate the directivity for the general case in which the rupture direction is

inclined at an arbitrary angle α to the strike-direction. The results for Kanamori's model ($\delta = 20^\circ$, $\alpha = 41^\circ$) are such that no sharp peaks of the directivity are expected at any station. This fails to explain our observations (B.R.I. fig. 5) as well as those of Toksöz et al. (1965) reported in B.R.I. (fig. 6).

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