DISCUSSION

"THE IMPORTANCE OF SURFACE WAVES IN STRONG GROUND MOTION". W. D. Smith.

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Smith advances a hypothesis that surface waves make an important contribution to observed seismic intensity, and that differing excitation of surface waves accounts for otherwise anomalous observations of intensity for a given magnitude and distance.

According to Smith's hypothesis, an earthquake of given magnitude radiates the same body waves independent of depth, but radiates a varying amount of rapidly attenuating surface waves, the amount generated being a function of depth. The magnitude is assigned on a basis of body waves measured at a distance. If the hypothesis is correct, earthquakes in regions A and C, both being "deep" by having low strain within 5km of the surface, should show equal intensity for equal magnitude and distance. Also, at the epicentre, type C should show greater intensity than type A due to the geometrical effect (type C having epicentres closer to the surface). Neither effect is observed.

The suggestion in Smith's section 4 that the presence of surface waves explains the lack of correlation between peak acceleration and observed intensity is questionable. By considering different effects at different frequencies, Smith is merely implying that the intensity is a function of spectrum as well as peak acceleration. This is doubtless true, but we do not require surface waves and their attenuation to generate different spectra. This can be a function of source and local geology just as easily.

Magnitudes are derived from displacement records of long period recorders distant from the epicentre. It is not surprising that they are difficult to relate to intensity, acceleration, velocity or response spectrum. Close to the epicentre it would be more logical to define for example a peak acceleration magnitude on the basis of acceleration records of low amplitude, and to derive an attenuation relation relating magnitude derived from peak acceleration, distance, and peak acceleration. This could be done for peak velocity or for response acceleration just the same as for peak acceleration.

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The problem of relating parameters of strong motion records to observed intensity is related to structural response to ground motion independent of the origin of that ground motion.

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Stephenson's comments emphasize that the problem of estimating the likely ground motion at any given site is by no means solved, and with this I fully concur. My paper was intended only to suggest that surface waves play an important role, perhaps more important than had been realised.

His criticism of my explanation for low intensities in region C, however, overlooks the point that the small source size typical of Fiordland earthquakes will give rise to a smaller proportion of low frequency energy than is usual elsewhere in the country. I suggested that this has significant bearing on the low intensities observed in Fiordland and also during the Opunake earthquake of 1974. But I claim no complete solution to the problem, only a qualitative suggestion.

Stephenson is of course correct in his statement that source effects and local geology can drastically alter the spectrum of ground motion. My comment on surface waves was merely intended to point out that their dispersive and attenuative properties will contribute to the lack of correlation between intensity and acceleration. The problem is too complicated to be explained by one factor alone.

Magnitude was defined by C. F. Richter as the logarithm of the trace amplitude, in mm, on a Wood-Anderson torsion seismograph at 100 km from the epicentre. Richter also developed an empirical function of distance for the purpose of determining magnitude at other distances. This procedure has since been extended to allow for focal depth, other instruments, and determination at teleseismic distances. But magnitude is an empirical scale, not designed to measure acceleration or earthquake energy or to do anything else but provide a comparison between earthquakes. It was never expected that magnitude would provide a complete description of the earthquake source. So I disagree that it would be "more logical" to define a magnitude scale based on acceleration. Such a scale would be more helpful for predicting peak accelerations, perhaps, but it would simply be another empirical scale. And is peak acceleration the best parameter to characterize ground motion? The present scale does seem to give a good estimate of duration, another important parameter. As seismology advances further it should become possible to

evaluate much more precisely the relationships between ground motion and the various source parameters of the earthquake, and even to include microzoning effects.

Stephenson's final comment on structural response is true, but with one qualification. Many of the descriptive statements in the Modified Mercalli scale refer to the effects on geological features, particularly at high intensities. This might lead one to expect, therefore, that high values of intensity and acceleration would correlate better than low ones, but there is little evidence for this.

My paper was not intended to do more than provide a qualitative explanation for empirical results presented earlier, and to suggest that surface waves may be more important than has hitherto been realised. A complete explanation for the variation of ground motion from site to site from earthquake to earthquake is still well beyond our grasp.