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Review of the Reports Dealing with the Simulation of Earthquake Ground Motions for San Onofre Nuclear Generators Station Unit I for the Nuclear Regulatory Commission.

by

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At the outset, I would like to reafirm my overall support for the general approach used by TERA-DELTA in attempting to estimate the strong ground motion characteristics at the San Onofre site. I believe that the approach is useful and sheds light on a number of important questions. A number of negative comments follow. These arise because TERRA-DELTA is attempting to solve a very difficult problem - a problem for which most (if not all) researchers agree that we do not yet even understand the basic physics of the earthquake rupture process much less know how to solve the problem. TERA-DELTA has attempted to overcome our lack of knowledge by use of a simplified fault rupture process and the introduction of randomness. It is interesting to note each of the "improved" models developed by TERA-DELTA that are of the major improvements was the introduction of more randomness. This seems counter-productive to the basic goal of a calculational effort. In my view of the usefulness of a calculational effort lies in our ability to say that we understand the basic physics of the problem, that our model incorporates in a reasonably correct way the basic important parameters and physics. Then by bounding the various parameters we can calculate reasonable worst case results appropriate for - in our case - the earthquake in the posulate offshores zone of deformation at the San Onofre site.

I accept that there is considerable randomness in the earthquake faulting process. However, the continued introduction of more and more randomness in place of understanding the physics of the problem is very troublesome - thus I cannot agree that the model has been properly calibrated.

In fact one key problem that I have with TERA-DELTA's work is over the criteria they used to argue that they have adequately calibrated their model. They argue that approximate equality of peak acceleration and "some match" to smoothed response spectra is more than adequate to show that their model conservatively models the possible earthquakes at the San Onofre site. I, on the other hand, feel that some match of the more important phases of the time series are necessary before we can argue that the model used is reasonable. If, for example as I strongly feel is the case, the initial slip velocity behind the rupture front is highly variable rather than uniform as modeled by TERA-DELTA, then it is important to know where the energy which gives rise to the peak ground motion originates before one can use such a match to judge what values of slip velocity are reasonable. This can only be done by matching the important features of time series.

The next report by Day, (Ref. 1.), shows that the introduction of variations in tectonic stress along the fault has a very significant effect on

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both rupture velocity and slip velocity. This report by Day also shows that (at least for one reasonable model) that the slip velocity is a function of the stress drop and introduces rapid changes in rupture and slip velocity. These rapid changes in rupture and slip velocities could have significant effect on the time series of the ground acceleration and on the high frequency end of the response spectra.

In my past reports (References 2 and 3), I also indicated that in my view the major deficiency of the TERA-DELTA's reports is that they have not established conservative bounds for the key parameters of their model appropriate for the postulated SSE for the San Onofre site. TERA-DELTA's studies show that the really important parameters of model that control the higher frequency ground motion (>/H_z) are:

1. V_O = initial slip velocity (dynamic stress drop)

2. Rupture velocity

3. Both the micro-incoherence and the marco-randomness introduced

4. Q values used

5. Geologic structure

Not included in the TERA-DELTA study, but in my view of considerable importance, is the nonlinear behavior of the soils at the various sites used for calibration purposes.

In my past reports I discussed why I felt that TERA-DELTA's calibration of V_O was inadequate. My views are still much the same as I do not feel that Supplements II and III address my prime concerns, which were with the modeling of V_O, Q and the randomness introduced into the model. The study by Day (Ref 1) serves to underline these concerns and introduces a new concern which deals with low rupture velocity was modeled. My understanding of the TERA-DELTA report is that the rupture velocity is always less than the shear wave velocity. The time of rupture was randomly chosen, but is always slower than 0.9 β (β= shear wave velocity). The study by day and the work of DAS and Aki suggest that the rupture velocity can be larger than the shear wave velocity - in fact in Day's study it generally was larger. In addition, we can expect random variation in rupture time, but at least Day's study suggests that as the rupture grows the rupture velocity grows. This potential correlation does not seem to be included in the TERA-DELTA model.

Of potential importance is the fact that the slip function studied is always a member of the same family of functions and very smooth except at the start and stopping of the rupture. The slip velocities calculated by Day show additional character which could have significant influence on the high frequency content of the spectrum. The potential importance of such variations in the slip function seems to need study. All of the randomness introduced by TERA-DELTA may cover the ranges of variation in the slip function that might be postulated including different functional form of the slip function, but this is not at all evident rom the sensitivity studies presented.

In Supplements II and III TERA-DELTA attempted to provide added verification for their choice of V_O = 800 cm/se. In Supplement II the Long Beach earthquake of 1933 and the San Fernando earthquake were modeled, and in Supplement III the recent Imperial Valley earthquake was modeled.

The first problem that I have with the Long Beach earthquake modeling is that the resultant time series is shorter than the recorded time series. I commented on this point with regard to TERA-DELTA's modeling of the 1940 Imperial Valley record at El Centro. This shortness could come from the Q's used and or lack of complexity in the rupture model such as variations of V_O, starting and stopping of the rupture etc. In addition, the modeling seems to give too high a peak acceleration. It is hard, to make judgements about the modeling because of the late start of the instruments and lack of other studies to pin down some of the important parameters of the rupture process.

The San Fernando earthquake has received considerable study.

Many of the studies of this event suggest that the rupture process was highly variable with the most energetic part of the rupture occurring on the laser part of the fault. The only way to check to see if in fact the uniform modeling used by TERA-DELTA provides a calibration for V_O is to be able to determine if higher values of V_O should have been used on the lower fault and lower values on the upper fault under the recording site. We also need to assess what impact this might have on the TERA-DELTA model. However, as we do not have the computed time series to compare with the recorded data it is very difficult to assess how reasonable the model is. In addition, it is very hard to assess the correctness of the topographic amplification factor used. The problem is once again related to a question of needing a comparison of where the energy is coming from for both the model and the recorded data.

The Imperial Valley earthquake of 1979 provides a number of stations as well as stations with obsolete time which could be used to calibrate TERA-DELTA's model. They have made little use of such data and once again use a uniform stress drop model with random rupture velocity and other random parameters. As I discussed in Ref. 4 the location of the center of energy release could be a number of kilometers from the El Centro array. Also as I pointed out in Ref. 4 there is some evidence that a major barrier could have existed several kilometers south of El Centro array. The TERA-DELTA model would put the effective center of every release much closer to the El Centro array. It seems to me that this can be resolved because we know when in time the energy arrived. The initial wave shapes should also provide additional insight into the correctness of any model.

I find it noteworthy to contrast the difference in duration between the records recorded on the El Centro array and by the Bonds Corner Station. Also, the Mexican is much longer duration (like Bonds Corner) than that recorded by the El Centro array. This indicates to me the rupture process was very complex and nonuniform. Once again this could only be resolved by comparison of wave shapes and arrival time (which for the first time are available).

One of the important questions that the TERA-DELTA reports attempts to address is the focusing of the seismic energy towards the site. Considerable randomness is introduced in the model to reduce the focusing effects. Day raises one interesting objection to the TERA-DELTA model on Page 30 of this report. Also, the nonlinear behavior of the soil is of possible importance when comparing calculated linear results to recorded results. Without some assessment of the nonlinear effect and comparison of wave shape, type and arrival time I find it impossible to judge how real and necessary both the micro and marco randomness used are. In addition, the computed spectra for Imperial Valley seems to lack energy in the period range of 1 to 4 seconds.

These larger period waves seem to show upon the recorded accelerograms at a number of stations suggesting that there may be large coherent zones of rupture. The potential implication of this is that there might be too much randomness in the TERA-DELTA model - or the scale (1 km) is too small for some zones of the fault. Too bad TERA-DELTA didn't compute the M from recorded motion data to compare to the M_L's of their simulated earthquake. The M_L's of the simulated earthquake appear to be low suggestions that more 1 sec wave energy is needed.

The geologic structure and Q's are important parameters. For example, Fig. 4-13 of May, 1978 shows a considerable variation (factor of 2 or more) in the computed spectra as a function of geologic structure for several different sites. The importance of Q is hard to determine as to some extent the value of V_0 was chosen relative to the Q model used. But what isn't clear is how much the Q model might effect the spectrum for longer and larger fault rupture sequences. If indeed Q is independent of frequency, then changes in Q would most likely be a second order effect relative to some of the other concerns discussed above. On the other hand, Q is not independent of frequency but increases with frequency, then how Q is modeled could be important. The sensitivity studies do not really address the role and importance of variations in Q.

In order to provide added comparisons TERA-DELTA used the data and a regression analysis to determine how the ground motion varies with distance.

In my view, the regression model given in Chapter 2 of Supplement III also appears to use a question of metrics, of distance from fault trace and yet was an attenuation of $1/CR+20)^{1.75}$ for horizontal accelerations and $1/CR+10)^{1.75}$ for vertical. Such a model might suggest that the main source of energy is at some depth. Very near the fault changes in distances of a few kilometers are very important. To simply use the closest distance to the fault trace can introduce considerable confusion. This is discussed in some detail by Shakal (Ref. 5).

In closing, I still feel that TERA-DELTA has not yet properly calibrated their model. If the strong ground motion recorded at the various sites used for comparison and calibration was due to:

(1) high stress drop earthquakes

(2) the region of large stress drop was very near the recording site as modeled

(3) nonlinear behavior of the soil is of second order of importance

(4) the scale of randomness used by TERA-DELTA and the amount of randomness introduced to reduce focusing is appropriate

then the results can play the role TERA-DELTA wasn't there to play. On the other hand I feel that several of the earthquakes used were not high stress drop earthquakes or that the zone of high stress drop was at some distance from the recording site. I also suspect that the nonlinear behavior of the soil is important. In addition, I think that the manner in which randomness is introduced and its scale needs more careful calibration. Overall I think that to acceptably calibrate their model, TERA-DELTA must look at wave shapes and arrival times in order to address the above points.

If my analysis given in Ref. 4 (Table VII, which updates Table I of Ref. 2), is correct, then the May 1940 Imperial Valley event for the event nearest El Centro, Parkfield, the 1979 Imperial Valley and Coyote Lake earthquakes are low stress drop earthquakes. I applied the same analysis to the 1933 earthquake and also found it to be a low stress drop event of the same order as the other earthquakes. This only leaves San Fernando as a high stress drop event used to calibrate the model. However, as discussed above there is considerable question about the way TERA-DELTA modeled San Fernando. For these reasons I feel that we must consider the results of TERRA-DELTA's modeling to be mean values. I would think that we could well expect a factor of 2 uncertainty at all frequencies including peak acceleration. This factor of 2 would correspond to the one sigma level. This is assuming that high dynamic stress drop earthquake is possible in the offshore zone of dezonation. If only low dynamic stress drop event like Parkfield or Imperial Valley can occur then the Housner spectra at 0.679 seems reasonable in light of TERA-DELTA's modeling.

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