Request for Additional Information (RAI) on:

1. EPRI Report 1012044 and 1012045 ,"Assessment of a Performance-Based Approach for Determining Seismic Ground Motions for New Plant Sites, VI and V2" (G1.1)

2. EPRI Report 1012965, "Use of CAV in Determining Effects of Small Magnitude Earthquakes on Seismic Hazard Analyses" (G1.2)

3. EPRI Report 1013105, "Truncation of the Lognormal Distribution and Value of the Standard Deviation for Ground Motion Models in the Central and Eastern United States" (G1.3)

4. EPRI Report 1012966, "Effect of Seismic Wave Incoherence on Foundation and Building Response" (S2.1a)

5. EPRI Report 1012968, "Spatial Coherency Models for Soil-Structure Interaction" (S2.1b)

6. EPRI Report 1012967, "Effect of Negligible Inelastic Behavior on High Frequency Response" (S2.2)

7. NRC staff questions from the October 19-20, 2005 meeting with industry on Seismic Issues Resolution Program



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**1 Comments on EPRI Report 1012044 and 1012045 ,"Assessment of a Performance-Based Approach for Determining Seismic Ground Motions for New Plant Sites, VI and V2" (G1.1)**

1.1 Provide hazard curves for the twenty-eight CEUS reactor sites so that NRC staff can verify the results presented in Volumes 1 and 2 of G1.1.

1.2 Provide the correlation between the soil site amplification functions and the soil site hazard curves for the twenty-eight CEUS reactor sites.

1.3 Provide an alternative performance-based method to determine the safe shutdown earthquake (SSE) ground motion spectra using seismic core damage frequency (SCDF) as a metric. Use an appropriate SCDF metric to arrive at the design response spectra (DRS) close to those derived from the ASCE 43-05 method.

**2 Comments on EPRI Report 1012965, "Use of CAV in Determining Effects of Small Magnitude Earthquakes on Seismic Hazard Analyses" (G1.2)**

2.1 Provide CAV-filtered hazard curves for the twenty-eight CEUS reactor sites so that NRC staff can assess the impact of using CAV for determining the effects of small earthquakes on seismic hazard analysis. (Have the hazard curves labeled consistently so they can be compared with the curves provided in response to RAIs G1.1 and G1.2?)

2.2 Cumulative Absolute Velocity (CAV ) methodology and CAV threshold of damage (0.16g-sec) were standardized in the EPRI Technical Report (TR 100082) published in 1991. The author stated that "this value was determined by the Whittier earthquake (record no 281) which has the lowest CAV value associated with an earthquake of Intensity VII." The CAV associated with the record no. 281 is 0.28 g-sec, corresponding to a MMI VII site intensity, which is conservatively assumed to have damage potential to buildings of good design or construction. To demonstrate the conservativeness of the threshold, the report also compared industry buildings with different design standards and concluded that the threshold is "a factor of five lower than the lowest CAV value associated with documented damage to an industrial/power facility and it is about a factor of three lower than the lowest CAV value associated with documented damage to buildings of good design and construction."

Although the report did not clearly address why the CAV threshold was adjusted from 0.28 to 0.16 g-sec, a CAV of 0.177 g-sec (record no. 56) from the Hollister earthquake (3/9/1949, ML=5.3, site MMI= VII) was referenced in the report, implying that the 0.16 g-sec was proposed by referring 0.177 g-sec, which also corresponds to an site MMI of VII.

Following questions are related to the threshold of 0.16 g-sec.

2.2a Modified Mercalli Intensity Scale (MMI) is descriptive in nature. An MMI category assigned to a site varies based on different observers, and sometimes it was difficult to assign an MMI to a site because different structures behaved differently, such as high rise and lower rise buildings in Mexico City following the 1985 Michoacan earthquake.

provide the basis for selecting the 0.16 g-sec threshold as a quantitative measure of no damage to structures, in view of the qualitative nature of assigning an MMI to a site also please justify the limitation imposed by simplifying the complexity of ground motions into a single parameter of CAV.

2.2b In view of significant increase of strong motion earthquake recordings near industrial sites since 1980, please justify the usage of 0.16 g-sec as a conservative threshold for damage, by providing examples from recent strong motion recordings (from recordings near damaged and undamaged facilities) to show that 0.16 g-sec is still applicable as a conservative threshold for damage.

2.3 Ground motion recordings included in the WUS data are mostly recorded in the lower shear wave velocity soils (the largest cluster is corresponding to 200-300 m/s) and higher attenuation crust. How can this WUS CAV model be applied to the eastern US, which is different in term of surface soil shear wave velocities and attenuation characteristics?

2.4 Since previous earthquakes have demonstrated that structural damage is closely correlated to surface soil type, if CAV is not sensitive to a soil type please explain how can it be used to as a single parameter for structural damage.

2.5 The report indicates that the CAV model based on WUS data strong motion data set is applicable to the EUS because no significant bias in terms of CAV residuals distribution for the CEUS (figure 2-11 in the report). The residuals are the estimates of experimental error obtained by subtracting the observed responses from the predicted response. How are the CAV distributions with respect to uniform durations and PGAs for the CEUS?

2.6 Author states that "CAV is highly variable for recordings with PGA > 0.025 g threshold" for the CEUS data set. If so, why it is appropriate to apply the WUS CAV model to the CEUS? The CEUS data set has a total of 54 components from 9 events. CEUS data set was used with different PGA cutoff values, 0.03g for comparison with WUS CAV model, 0.04g when compared WUS CAV residuals, and 0.05g when compared duration residuals with WUS. What is the justification to compare CAV related model parameters with different cutoff PGA values? How many of those 54 components are still left after each change of the cutoff values?

2.7 Please explain why the numbers of strong motion components used are different, the attached table has 4252 components, but the report indicates a total number of 4422.

2.8 Please clarify which component of ground motion recordings is used during the CAV modeling. If both components are used, are their correlations being considered in the modeling?

2.9 Taiwan, Turkey and Japan are quite different in tectonic settings from the west US. Please justify why the data from those regions can be used in combination with west US data to model the CAV.

2.10 In Figures 2-5, 2-6 and 2-12, median CAV model for Vs=600 m/s for different magnitudes and PGAs, and uniform duration for PGAs were shown. How many strong motion components are available for Vs = 600 m/s? Why use only Vs=600 m/s subdata set?

2.11 In Table 2-2, an earthquake with magnitude of 4.3 was recorded both at the station 2627A and 2627B. Are these two stations at the same site? If they are, why the Vs30s are different?

2.12 Please explain the rational for the functional forms of Equation 2-1 used to predict CAV based on duration, magnitude, and shear wave velocity, and of Equation 2-2 used to predict uniform duration using PGA, Vs30 and magnitude?

2.13 Please explain the relative significance of different variables in the prediction equations for both CAV and uniform duration (Equations 2-1 and 2-2) and provide the statistics for each of the coefficients (variables).

2.14 Please explain why the distance factor is not explicitly expressed in the Equation 4-1.

2.15 Is CAV also dependent on the site location relative to the fault and fault types, like other ground motion parameters, e.g. PGA? If yes, what are the contributions from those factors?

## 3 Comments on EPRI Report 1013105, "Truncation of the Lognormal Distribution and Value of the Standard Deviation for Ground Motion Models in the Central and Eastern United States" (G1.3)

3.1 Provide hazard curves for the twenty-eight CEUS reactor sites with the revised sigma model so that NRC staff can assess the overall impact.

3.2 The report indicates that there are some epsilon >3 records in San Francisco, CA from the Loma Prieta earthquake in 1989. However, using the latest NGA attenuation relationships, Boore and Atkinson (2005), Abrahamson and Silva (2005) and Campbell and Bozorgnia (2006), no instances of epsilon >3 were found, see table attached. Please explain the difference.

| Station in San Francisco | Rrup km | Rjb km | Vs30 m/s | Obs 1-Hz SA (*g*) | Epsilon (A&S) |
|---|---|---|---|---|---|
| Diamond Heights | 71.33 | 71.23 | 582.9 | .09743 | **0.58824** |
| Cliff House | 78.68 | 78.58 | 712.8 | .2015 | **1.97060** |
| Presidio | 77.43 | 77.34 | 594.5 | .2448 | **2.02865** |
| Rincon Hill | 74.14 | 74.04 | 873.1 | .10072 | **1.13037** |
| 1295 Shafter | 68.16 | 68.05 | 338.5 | .05979 | **-0.75422** |
| Telegraph Hill | Etc. | | | | |

3.3 The report postulates that there are three causes for changes in NGA's standard deviation model: a) a larger data set, b) corrections of meta-data, and c) use of the geometric mean of the horizontal components after finding the optimal rotational angle. Two of the three possible causes, b) and c), are eliminated after further comparison with the earlier data used in the Abrahamson and Silva (1997) model, while added data from the new earthquakes is determined to be the cause. Did the other NGA modeling teams all reach the same result? Please provide further explanation regarding the other modeling teams and possible causes for changes in their standard deviations.

3.4 The report indicates that the meta-data change has no significant impact on the sigma. However, the 1997 data seem to differ significantly from the 2005 data in the inter-event part, when comparing Figures 5-4 and 5-5. Please explain this difference (and also at regional and teleseismic distance).

3.5 Please explain why variation in half-duration time derived from Harvard Moment Tensor catalog is a reliable indicator of source variation for different regions. Two strong aftershocks, which occurred on 9/22/1999 00:14 UT and 9/20/1999 21:46 UT, respectively, from Chi-Chi earthquake have the same moment magnitude (Mw= 6.4) and the same half-durations (4.0 seconds) in the Harvard Catalog, but two events produced substantially different PGA and SA (5 Hz) for stations within 70 km distance.

3.6 The author stated that the path effects should give the same variability in the CEUS and WUS because synthetic seismograms using various crustal models for the CEUS show the same variability as seismograms for the WUS. Please explain how much plane-layered models can capture the variability of path effects, where there is likely to be lateral variations of crustal structures.

3.7 Please explain what is the USNRC 2002 ground motion model referred on page 6-1 in Section 6.

3.8 The report indicates that the inclusion of Chi-Chi mainshock causes the increase in the intra-event standard deviation at large magnitudes and the inclusion of additional earthquakes in the magnitude range of 5-6 have a similar variability of the event terms resulting in a reduction of the inter-event standard deviation of magnitudes less than 6.0. Since the WUS ground motion functions mean to be used to predict the ground motions in California and other west US earthquakes, is it appropriate to introduce data from other regions which will obviously bias the ground motion prediction in this specific geographic area? Are the sigmas developed using additional data from other regions still valid for the area?

3.9 On page 6-18, the report states that the standard deviation of the stress drop from the paper submitted to BSSA by Atkinson and Boore is 0.6 natural log units (median value 140 bar). However, this quote of standard deviation was not found in Atkinson and Boore's 2005 submittal. Please explain.

3.10 Atkinson and Boore provided a direct estimate of total sigma (aleatory with inter and intra event terms) contained in their 2004 report to USGS. In that report the sigma was calculated from the residuals of the observations with respect to the predictions of their 2004 attenuation relationships. Their 2005 submittal to BSSA used the same dataset as their 2004 ground motion functions which appear to be the same as the 2005 ground motion function. The sigma reported in 2004 is 0.3 at 0.2 - 0.5 Hz and 0.4 at 5-10 Hz. It gives a direct estimate of aleatory sigma of 0.92 ln unit at 5 Hz. This sigma is significantly higher than 0.7 unit recommended by NGA. Please explain.

3.11 Please justify using the sigma derived from stress drop as the inter-event sigma when using attenuation relationships derived with a different median stress drop from the one found by Atkinson and Boore (2005).

3.12 The report recommends a inter-event sigma of 0.43, which is substantially less than the inter-event sigma value of 0.53 derived from the observed standard deviation of stress drop reported by Atkinson and Boore (2005). Please explain this discrepancy.

3.13 The 2004 EPRI ground motion report incorporates a distance dependence to the standard deviation to account for the use of the Joyner-Boore distance, $R_{JB}$. However, PEER NGA studies indicate that the increase in the standard deviation at short distances may not be appropriate. The report states that this may be due to the deeper earthquakes being more energetic than the shallower earthquakes and postulates that the deeper earthquakes have higher stress-drops on average than shallower earthquakes, "As a result, the expected distance-dependence of the standard deviation is counter-acted by the depth dependence of the stress-drop." However, one of the four EPRI (2004) models for aleatory variability assumes a larger stress-drop variability for the CEUS compared to the WUS. The report also mentions on page 5-7 that "other studies have not found stress-drop scaling with depth."

Considering 1) the lack of distance-dependence of the stress-drop, 2) the larger variability of stress-drops for the CEUS, 3) the sparseness of the CEUS data, and 4) the statement on page 5-5 of the report:

"If it is assumed that the ground motion will attenuate as 1/R where R is the point source distance and that there is no systematic differences in the sources as a function of depth (e.g. no stress-drop dependence with depth), then there should be an increase in the standard deviation at short distances due to the use of $R_{JB}$.",

Please justify the decision to eliminate the additional sigma to account for the use of $R_{JB}$ for events at distances less than 20 km.

## 4 Comments on EPRI Report 1012966, "Effect of Seismic Wave Incoherence on Foundation and Building Response" (S2.1a)

4.1 The report seems to be written well and different topics of discussion are well laid out.

4.2 An important gap in this study is the lack of any treatment of kinematic interaction of embedded foundations or any questioning of the validity of the use of the proposed Abrahamson coherency function for those foundations.

4.3 The standard practice of performing SSI analysis using coherent ground motion was based on observation and interpretation of data from down hole arrays that show a large percentage of the power of ground motion comes from vertically propagating waves. It appears that the recommended method of SSI analysis in this report is simply to reduce the amplitudes of ground motion at frequencies generally above 10 Hz, and then apply the reduced motion uniformly (coherently) across the entire foundation.

4.4 This report needs to clearly layout the approach and implementation scheme for using the SSE (design ground motion) derived from a performance-based approach in conducting engineering analyses. Detailed steps of the implementation in carrying out the SSI analysis using the incoherent motion approach, including guidance on soil parameter modeling are needed.

4.5 Complicated equations are described that use rectangular and square matrices, which are appropriately multiplied by column matrices to obtain resulting equations. These are described in text, however a step by step process of converting the matrices using conceptual layout in matrix form will enhance the reader's understanding.

4.6 The ASCE Journal of Geotechnical and Geoenvironmental Engineering issue of April 2003, Volume 129, Number 4 published an article, "Kinematic Soil-Structure Interaction from Strong Motion Recordings" by Seunghyun Kim and Jonathan Stewart. This article points out that the incoherence parameter is dependent on the site shear wave velocity. This paper also points out that the use of incoherent motion introduces torsional motion.

4.7 Page iv: As discussed here, seismic wave incoherence occurs because of the horizontal spatial variation of both horizontal and vertical ground motions. The variation in the horizontal input motions will result in torsional input at the foundation while the variation in the vertical motions will cause rocking of the base mat. Please discuss in detail the basis for not considering the torsion and rocking effects and state whether these effects will be considered in the individual plant ESP and/or COL applications.

4.8 Page iv: This section states that the seismic response is evaluated for rigid, massless foundations and for example structural models on foundation mats that behave rigidly. Please discuss how the results would be impacted by taking into account the flexibility and mass of the foundation and state whether these effects will be considered in the individual plant ESP and/or COL application. (See also page 1-2 for the same subject).

4.9 Page iv: This section states that the incoherency transfer functions depend on the foundation area and are independent of site soil conditions but that the resulting spectral reductions strongly depend on site soil conditions. This seems to be inconsistent. Please explain. (This statement also appears on page ix.)

4.10 Page v: This section describes some of the research activities and uncertainties that have been identified. These include: additional analyses for different and more complex foundation shapes; verification based on foundation responses in real earthquakes; sensitivity study; and validation through peer review. Please discuss the status of these tasks and provide assurance that these tasks will not impact the incoherency functions presented in this report.

4.11 Page viii: This page states that in this study, the assumption was made that mat foundations of typical nuclear power plant (NPP) structures behave rigidly. This assumption may not be valid in all cases. Please discuss the effect of mat flexibility on the results reported in this study and whether the mat flexibility will be considered in the individual plant ESP and/or COL application.

4.12 Page 1-1: This page states that this study considers both the "local wave scattering" and "wave passage effects" but that the final results are based on "local wave scattering" only. Please provide the basis of excluding "wave passage effects" and state whether the "wave passage effects" will be considered in the individual plant ESP and/or COL applications. See also Page 5-1.

4.13 Page 2-2: It is not clear which equation is plotted in Figures 2-1 and 2-2, and which equation is to be used for "no wave passage effect." Please explain.

4.14 Page 2-3: This section states that the ground motion data analyzed to develop the coherency functions have frequency content of 20 Hz and less but that the trends can be extrapolated to higher frequencies. It is not obvious why and how these trends can be extrapolated. Please explain.

4.15 Page 2-3: This section rightfully states that the mean input ground motion is the goal for the design of NPP structures, and as a result, the goal is to use mean coherency. However, this section further states that the coherency functions stated in the report are median coherency functions. Please provide justification for using the median instead of the mean coherency functions.

4.16 Page 2-3: Tables 2-2 and 2-3 do not seem to be consistent with Figure 2-4. Please explain.

4.17 Page 2-5: This section states that the shear wave velocity of the bedrock is 4300 fps but Table 2-3 indicates a value of 4150 fps. Please explain the discrepancy and its potential impact.

4.18 Page 2-5 and 2-6: This section quotes the EPRI 1993 Guidelines for Determining Design Basis Ground Motion. Please provide the full reference.

4.19 Page 2-6: This section states that the soil damping and shear modulus were determined based on an earthquake strain level of $10^{-2}$ %. This is the same strain value as was stated for rock. Please explain why the strain value for the soil is not higher than that of the rock.

4.20 The structural model to evaluate kinematic interaction is presented in Figure 2-8. The stick model has mass, stiffness and damping representing a fixed base condition. The use of this model for studying the kinematic interaction should be further explained. Presumably the inertial interaction part is to be evaluated in a separate step. In this context, the use of superstructure with masses hinders the demonstration of kinematic effects. Please explain.

4.21 It is stated in the general section that the goal is to obtain an engineering-modified input ground motion accounting for incoherency effects. Presumably, the modified ground motion will be applied as a completely coherent time function in the SSI analysis. It appears that the effect of proposed incoherency effect is only to reduce time histories along three orthogonal directions without any rotational input. This seems to render the very idea of incoherency incongruent. Please explain the value of this approach.

4.22 Based on Figures 4-1, the effect of incoherency transfer functions for the vertical and horizontal directions are about a factor of 2 apart. Can this be validated from actual recordings, or is this to be expected in the CEUS region? This effect also shows up later in the report.

4.23 Page 5-4: This section states that to study the effect of foundation shape, square versus rectangular foundations were considered, while different foundation sizes of square foundations were investigated to study the effect of foundation area. Please explain whether you have studied circular foundations, especially in light of the fact that a significant number of NPP foundations are circular. Please explain whether this effect will be considered in the individual plant ESP and/or COL application.

4.24 At the end of this chapter it is concluded that the incoherency transfer function (ITF) is independent of the input motion. This would be one of the most important points that would allow the use of the ITF without any dependence on the seismologically (performance-based) obtained ground motion spectrum. The validation of this point needs to be demonstrated by observed behavior.

4.25 Figures 5-17 and 18 show the reduction effect at PGA, but the reduced vertical PGA (0.15g) is less than the horizontal (0.2g). Can this be validated by observed data?

4.26 Page 6-10: This section discusses whether correction factors need to be applied to take into account rotational effects of torsion and rocking. Please elaborate on the statement "The exact solution includes rocking induced by consideration of incoherence but the incoherence transfer function (ITF) scaled solution only includes translational input motion".

4.27 This section also states that translational foundation response after SSI when subjected to rotations only were less than 0.01g, and the in-structure response was similarly low. Were these results for a soil or rock site? A soil site may be subject to more rocking. The staff would like to see the details of these results.

4.28 Furthermore, this section states that for the rock condition, no additional consideration of rotations due to ground motion incoherence appears to be warranted. Please explain if additional consideration of rotations due to ground motion incoherence would be warranted for a soil site.

4.29 Page 6-17: This section states that all the analyses in this report are conducted for surface foundations even though many NPP structures have embedded foundations. This section further states that it is anticipated that the effects of embedment and the effects of incoherence are independent of each other but that analyses to demonstrate this relationship have not been performed. Please provide the basis of this assumption and state whether embedment effects will be considered in the individual plant ESP and/or COL application.

4.30 Page 6-17: This section states that in-structure spectra for one horizontal direction and for a surface founded and embedded model are shown in Figures 6-26 and 6-27, respectively. It is not clear what these Figures illustrate. Please elaborate.

4.31 PGAs for horizontal and vertical direction are almost a factor of 2 apart, see series of figures marked 6 -1 through 6.

4.32 Figures 6-14 through 25 use the label SSI-CTF, but CTF does not seem to have a definition.

4.33 Page 7-2: This section states that it was judged to be slightly conservative to not include wave passage effects. Please explain if the wave passage effect might have a bigger impact on rocking and torsion.

4.34 The conclusions are well laid out; however the issue of embedded foundations is not discussed and majority of reactor designs use structures that are embedded to depths between 20 to 60 ft.

4.35 p. ix first bullet and p. 8-6 second bullet: It is stated that: "The basic effect of incoherence on seismic response of structures has been demonstrated and validated through recorded ground motions and analyses of their effects with alternative methods and programs." The report presents analyses utilizing a simulated time history based on the response spectra of Fig. 2-5 or using random vibration theory with power spectra derived from the response spectra of Figs. 2-5 and 2-6. Was there a separate analysis performed with recorded ground motions?

4.36 There are two typos in Table 2-1, p. 2-1: for the horizontal ground motion $f_c$ the first term in the expression should be –1.886+... instead of 1.886+..., and for the vertical ground motion $f_c$ the last term of the expression should be ...+1))$^2$) instead of ...+1))2).

4.37 The variation of the soil shear wave velocity with depth in Fig. 2-4 (p. 2-5) does not fully correspond to the values provided in Table 2-3 (p. 2-6). Additionally, it is stated on bottom of p. 2-5 that "...and then a half-space of bedrock at a shear wave velocity of 4300 fps". The entry for the half-space shear wave velocity in Table 2-3 is 4150 fps. Which is the soil profile used in the analysis?

4.38 It is stated on p. 2-8 last paragraph that: "For soil-structure interaction analyses and the evaluation of structure response including the effects of seismic wave incoherence, spectrum compatible time histories for the rock site were required. ... Three uncorrelated components were generated for two horizontal directions and the vertical direction." What was the time step for the generation of the time histories? What was the duration? What amplitude modulating function was utilized to transform the generated stationary time histories to non-stationary? It would be helpful if the time histories were presented.

4.39 It is stated on p. 2-9 that: "The SSI seismic analyses, by CLASSI and SASSI, were performed for the 150-ft square foundation footprint. For these analyses the foundation was assumed to be 15-ft thick. ..." Wasn't the foundation massless in the CLASSI and SASSI benchmark problem comparisons (Section 4)? Were there additional comparisons made? Why was a 15-ft foundation thickness selected?

4.40 Figure 2-8: Shouldn't the foundation footprint dimensions be < 150' or 225' > instead of < 100' or 225' > in the X-direction and < 150' or 100' > instead of < 100' > in the Y-direction?

4.41 There are some typos in Eq. 3-14 (p. 3-5): It should read $[H_T] = ([\alpha_s] + [\varphi_s] [D] [\Gamma_s]) [H_F]$ instead of $[H_T] = ([\alpha_s] / [\varphi_s]^T [D] [\Gamma_s]) [H_F]$.

4.42 It is stated in subsection "Procedure to Evaluate the Foundation and Structure Incoherent Response Spectra by CLASSI" that "The complete random vibration approach described above could have been employed herein" (p. 3-5), but that "Ground motion time histories are transformed into the frequency domain, SSI parameters (impedances and scattering matrices) are complex-valued, frequency-dependent, and the structure is modeled using fixed base eigensystems. SSI analyses are performed– output are time histories of interest from which in-structure response spectra are computed. The resulting in-structure response spectra at structure and foundation locations of interest include the effects of soil-structure interaction and seismic wave incoherence" (p. 3-6). This process is not random vibration analysis – this is a deterministic time history analysis utilizing the frequency domain.

4.43 It is stated on p. 3-6 that "For this application, a 6 by 6 complex incoherency transfer function matrix [ITF] is evaluated by taking the square root of $[S_{UoI}]$, the 6 by 6 complex cross PSD matrix of rigid massless foundation motion to unit PSD input. The scattering matrix for vertically propagating waves is replaced by the columns of the incoherency transfer function matrix at each frequency of interest that correspond to the directions of input excitation". Was the square root of the entire $[S_{UoI}]$ matrix considered in the approach by replacing all columns of the scattering matrix by the columns of the ITF matrix, as indicated on p. 3-6, or were only the diagonal elements of the ITF matrix considered as indicated throughout the rest of the report?

4.44 In Section 4 it is stated that the benchmark problem comparison utilized:

- Two different algorithms; CLASSI – stochastic method and SASSI eigen decomposition method
- Two different analytical approaches; random vibration theory (RVT) by CLASSI and time history dynamic analyses by SASSI

Regarding the second bullet: Both CLASSI and SASSI utilize a time history analysis, with the only difference being that the CLASSI approach described in the report transforms the time history in the frequency domain, conducts the evaluation in the frequency domain and transforms the results back into the time domain as noted in I-8. Hence, the results regarding this aspect should be expected to be identical, assuming that CLASSI and SASSI have been validated before regarding fully coherent incident motions.

Regarding the first bullet: The approach used in CLASSI is described in the report, whereas that of SASSI is not. However, Report TR-102631 (1997) describes an eigen decomposition approach for the incorporation of the spatial incoherence of seismic ground motions in SASSI through the module "INCOH", which also utilizes eigen decomposition. If the evaluations by SASSI are based on the approach described in Report TR-102631 (1997), the following is observed regarding the benchmark comparison: The CLASSI – stochastic method described in this report in Section 3 is identical to the stochastic approach described in the TR-102631 report (1997), also in their Section 3. The only difference is that this report incorporates the coherency matrix $[\gamma]$ fully in the analysis by using matrix analysis and taking the square root of the cross spectral density matrix of the rigid massless foundation motion $[S_{Uol}]$ (Eq. 3-2 on p. 3-2), whereas the "INCOH" module of the TR-102631 report performs an eigenvalue decomposition of $[\gamma]$ and retains its dominant modes. The module "INCOH" was validated in Section 5 of Report TR-102631 (1997) utilizing SASSI with previous studies conducted by Luco and Mita (1987) for a circular, rigid, massless foundation and Mita and Luco (1986) for the response of a flexible, cylindrical structure.

If the evaluations by SASSI are based on the "INCOH" module described in Report TR-102631 (1997), the benchmark comparison in this report simply suggests that the eigen decomposition of the coherency matrix $[\gamma]$ by SASSI contained sufficient number of modes to capture the full effect of $[\gamma]$ considered by CLASSI. Additionally, if this is the case, retaining higher modes in the decomposition would render the SASSI results in Figs. 4-1, 4-2 and 4-3 smoother, as are those evaluated by CLASSI.

4.45 What input motion was used in the benchmark comparison? It is stated on p. 4-1 that "Input earthquake excitation was the rock input motion for which the response spectra are shown in Figure 2-5". However, the maximum horizontal acceleration in Fig. 2-5 is ~ 1.48g whereas the maximum horizontal acceleration in Fig. 4-2 is ~ 1.0g, and the maximum vertical acceleration in Fig. 2-5 is ~ 1.38g whereas the maximum vertical acceleration in Fig. 4-3 is ~ 0.9g.

4.46 It is stated on p. 2-3 that a damping ratio of 0.02 is assumed, whereas on p. 4-1 for the benchmark problem the damping is considered as 1 percent. Also, the bedrock shear wave velocity for the bedrock is considered as 4300 fps on p. 2-5, 4150 fps in Table 2-3 and 6300 fps for the benchmark comparison. Which damping values and shear wave velocities were used? Or were there different response spectra and corresponding time histories developed for the benchmark problem? This may also be associated with I-11.

4.47 It is stated on p. 5-4 that the soil profile does not affect the ITFs, which are basically identical at all frequencies for soil and rock (Figs. 5-13 and 5-14). This should be expected if the matrix $[S_{Uol}]$ in Eq. 3-2 were controlled by $[\gamma]$ only, which is considered identical for rock and soil sites according to the coherency model. However, $[S_{Uol}] = [F] [S_{UGl}] [FC]^T$ (Eq. 3-2) also contains the scattering transfer function [F], and the ITFs are the square root of the diagonal terms of $[S_{Uol}]$. How dependent is [F] (Eq. 3-3) on the site properties or is it only function of location and frequency? If [F] depends on the site properties, this should be reflected in the ITFs, which, consequently, should differ for soil and rock sites.

4.48 In the subsection "Spectral Corrections" on p. 5-10, where random vibration analysis is utilized, what was the equivalent duration of the seismic motions used in the conversion between power spectra and response spectra?

4.49 It is stated in the subsection "Spectral Corrections" on p. 5-10, as well as earlier on p. 5-4, that the response spectra for the square 150 ft x 150 ft and the rectangular 100 ft x 225 ft are identical. Is there an explanation for this? It is also mentioned that the ITFs are identical for the two foundation shapes. Are all terms of the $[S_{Uol}]$ matrix in Eq. 3-2 identical (or close) for both foundation shapes?

4.50 On p. 5-11 it is stated that "It may be seen that spectral reductions are significantly greater than the ASCE 4 values for the rock site but are actually somewhat similar for the soil site." There seem, however, to be very significant differences between the ASCE 4 and the soil spectral corrections especially for the 150 ft square foundation in both horizontal and vertical directions, and the 300 ft square foundation in the vertical direction. Also, is there a reason behind the increase in the values of the horizontal spectral corrections at the 50.0 Hz frequency for all foundations supported on soil?

4.51 A previous analysis by Luco and Wong (1986) evaluated the response of a rectangular, rigid, massless foundation subjected to spatially random ground motions. Because their analysis and results are closely related to those presented in this report, their work is briefly described herein for clarity in this question.

4.52 Figure I-1 presents the layout and coordinate system of the Luco and Wong analyses. It is considered that the rectangular ($2a \times 2b$) massless, rigid foundation is bonded to a visco-elastic half-space with Poisson's ratio of 1/3 and material damping ratio of 0.01.

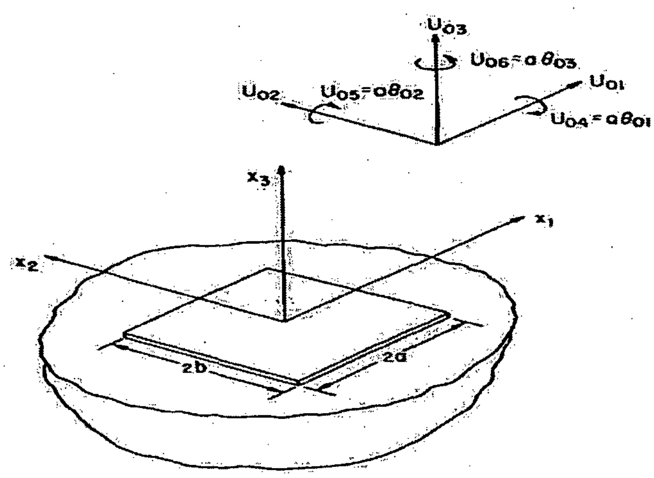



Figure I-1. Layout of foundation and coordinate system (from Luco and Wong, 1986).

The coherency expression of Luco and Wong (1986) is of the form:

$$\gamma_{LW}(f, \xi) = \exp[-(\nu 2\pi f \xi / \beta)^2] \qquad \text{(I-1)}$$

where $\nu$ is a coherency drop parameter associated with random inhomogeneities and variations in elastic properties along the path of body waves, $\beta$ is an estimate of the elastic wave velocity, $f$ is frequency in Hz and $\xi$ is separation distance in m. Figure I-2 presents a comparison of the Abrahamson coherency model used in this report for horizontal and vertical motions (Eqs. 2-1 and 2-2 in the report) with the Luco and Wong coherency (Eq. I-1 herein) for $\beta$ =1921.5 m/sec (=6300 fps), i.e., the one used in the benchmark problem, $\nu$ = 0, 0.1, 0.2, 0.3, 0.4, and 0.5 as used by Luco and Wong, and at separation distances of 10 m and 45.75 m (= 150 ft), the latter being the length of each side of the foundation in the benchmark problem. The approach described in Luco and Wong assumes that the coherency decay is the same in the two horizontal and the vertical directions. The value $\nu$ = 0 represents fully coherent motions. The model of Luco and Wong decays more slowly with frequency at the shorter separation distances than the Abrahamson coherency model. At the longer separation distance, the value of $\nu$ =

0.5 falls in-between the horizontal and vertical Abrahamson models. At longer separation distances, the Luco and Wong model falls off more sharply with frequency than the Abrahamson models.

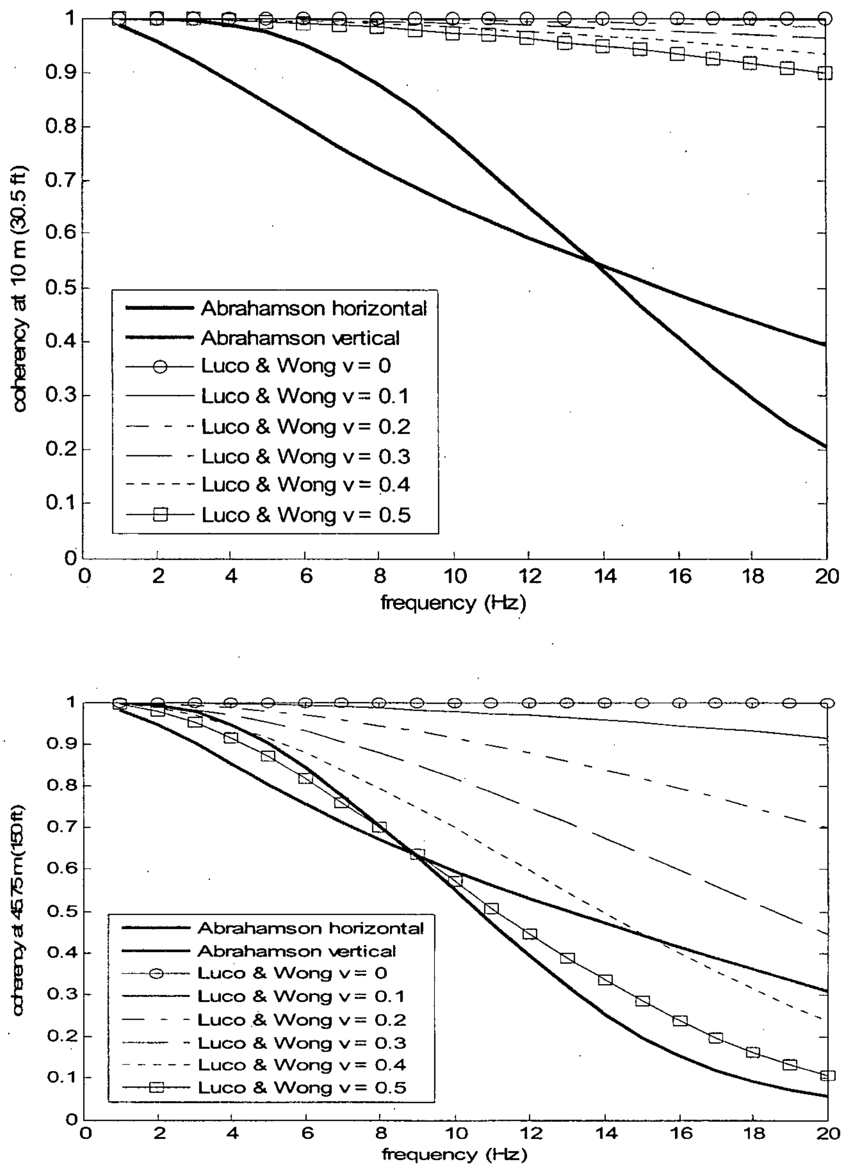
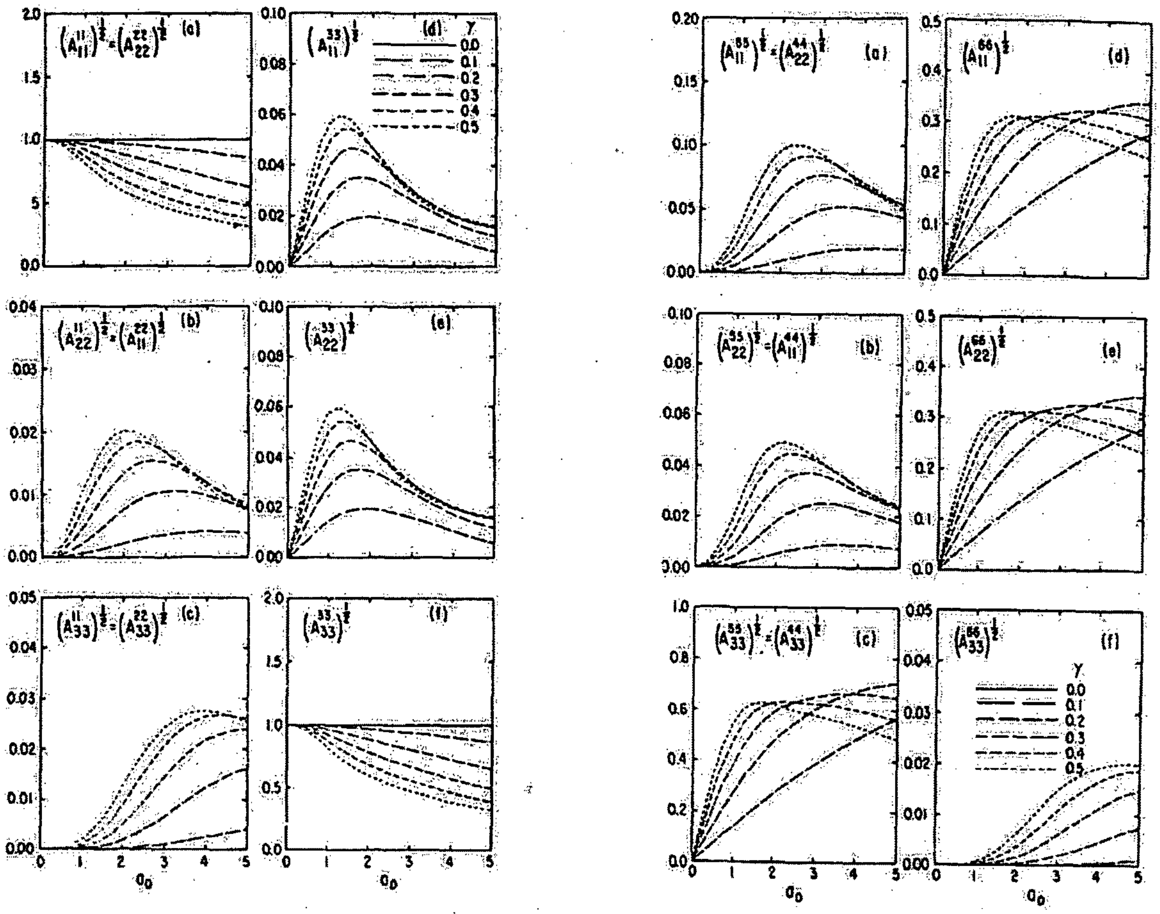



Figure I-2. Comparison of the Abrahamson and the Luco and Wong coherency models at separation distances of 10 m and 45.75 m (= 150 ft).

Luco and Wong's (1986) results for a square ($2a \times 2a$, Fig. I-1) foundation subjected to motions experiencing loss of coherency are presented in Fig. I-3a for the translational response components, and Fig. I-3b for the rotational response components. The results are presented as functions of the dimensional parameter $a_o = \omega a / \beta$ and for variable values of *v*. An increase of "$\Delta a_0 = 1$" in the dimensional parameter $a_o$ in the figures, considering that $2a$ = 150 ft, is equivalent to $\Delta f = 13.37$ Hz for $\beta$ = 6300 fps used in the benchmark comparison, and $\Delta f = 9.125$ Hz for $\beta$ = 4300 fps suggested on p. 2-5, yielding maximum values for the frequency at $a_o$ = 5 of 66.85 Hz and 45.62 Hz, respectively. According to Luco and Wong (1986), $\sqrt{A_{ii}^{jj}}$, $i$ =1, 2, 3 and $j$ = 1, 2, ..., 6, can be interpreted as the amplitude of a transfer function between the $i$-th component of the excitation and the $j$-th component of the response (Fig. I-1). In this sense, $\sqrt{A_{11}^{11}} = \sqrt{A_{22}^{22}}$ in Fig. I-3a, subplot (a), corresponds to the ITFs provided in the report in any of the two horizontal directions, and $\sqrt{A_{33}^{33}}$ in Fig. I-3a, subplot (f), to the ITF in the vertical direction. As can be seen from Fig. I-3a, loss of coherency in a specific direction results in significant reduction of translation in the corresponding direction ($\sqrt{A_{11}^{11}}$, $\sqrt{A_{22}^{22}}$, $\sqrt{A_{33}^{33}}$), but affects only minimally the translational response in the other directions. The decay of the transfer functions $\sqrt{A_{11}^{11}}$, $\sqrt{A_{22}^{22}}$, $\sqrt{A_{33}^{33}}$ in Fig. I-3a is much slower than the ITFs presented in the EPRI report, possibly because the Luco and Wong model produces significantly higher coherency values than the EPRI model (Fig. I-2). Figure I-3b presents the results for the rotational components of the foundation. According to Luco and Wong (1986), $\sqrt{A_{ii}^{55}}$ can be interpreted as the amplitude of the transfer function between the $i$-th component of the excitation and the foundation response $\tilde{U}_{05} = a\tilde{\theta}_{05}$ (Fig. I-1), i.e., rocking about the $x_2$ axis. Hence, $\sqrt{A_{33}^{44}}$ and $\sqrt{A_{33}^{55}}$ represent the rocking transfer functions due to the vertical excitation, and $\sqrt{A_{11}^{66}}$ and $\sqrt{A_{22}^{66}}$ the torsional response caused by the horizontal motions. Figure I-3b then suggests that the rocking response about the $x_1$ and $x_2$ axis are mostly associated with the vertical component of the free-field ground motion, and the torsional response about the $x_3$ axis is associated with the $x_1$ and $x_2$ components of the ground motion.




(a) translational response

(b) rotational response

**Figure I-3**. Foundation response to seismic ground motions exhibiting loss of coherence (from Luco and Wong, 1986). The coherency drop parameter "*γ*" in the figure is referred to in the text as "*v*" because "*γ*" in the EPRI reports refers to the coherency function.

Figure I-3b indicates that rocking caused by the vertical motions (subfigure (c)), and torsion caused by the horizontal motions (subfigures (d) and (e)) can be significant, and increase as *v* increases (and coherency decreases, Fig. I-2). It is also noted from Fig. I-3b, subfigures (c), (d) and (e), that as *v* increases, the peaks of $\sqrt{A_{33}^{44}}$, $\sqrt{A_{33}^{55}}$, $\sqrt{A_{11}^{66}}$ and $\sqrt{A_{22}^{66}}$ shift towards the lower frequencies. On the other hand, the negligible effect of rocking discussed on p. 6-10 of the EPRI report appears to be counter-intuitive, in that the large reductions in the translational response due to incoherency presented in the report do not result in any rotational effects. Since the Abrahamson coherency model drops more rapidly with frequency and separation distance than the Luco and Wong model (Fig. I-2), this should lead to even higher values for the rotational transfer functions, but it is stated in the report that their effect is negligible. Does the rotational effect become negligible in the report's study, because, due to the sharp decay of the Abrahamson coherency model, the peak of the rotational transfer functions shifts to such low frequencies where the ground motions do not contain much energy? How do all elements of the [$S_{Uol}$] matrix of Eq. 3-2 (not only the translational ITFs) behave at different frequencies?

4.53 There is insufficient information provided in the report to evaluate the effect of embedment and incoherence on p. 6-17. It is stated that "It is anticipated that the effects of embedment and the effects of seismic wave incoherence are independent of each other". This depends on whether coherency is a function of depth or not. It is also stated on p. 6-17 that Figures 6-26 and 6-27 suggest "independency of embedment and incoherency". If the same coherency model was used for the surface and the embedded structure, then the comparison between Figs. 6-26 and 6-27 indicates only the effect of embedment, not coherency. Also, in Fig. 6-26, what is the meaning of the response at an El. -21' for a surface structure?

## 5 Comments on EPRI Report 1012968: "Program on Technology Innovation: Spatial Coherency Models for Soil-Structure Interaction"

5.1 This report relies on previous work and references to academic work. It is not complete in itself, such that members of the public would be able to review the report and understand the theoretical bases for the approach proposed. Please complete or expand references.

5.2 It is very important that the proposed coherency model for calculating soil-structure interaction effects be validated against observed behavior of large light-weight foundations. Is validation available; please describe. If not available, please indicate an alternative.

5.3 This formulation is completely based on instrumental recordings at surface on small pads that are more indicative of particle motion rather than scattered wave motion that could be experienced by nuclear plant foundations located at depths of 50 to 60 ft. Seismic energy distribution at particle level and wave level can be significantly different. It would be necessary to demonstrate that the proposed coherence function can be used for embedded foundations.

5.4 The dense array data are from surface recordings. Are there any recordings at depth? If so, how is the energy distribution of the motion at depth and at surface? How is the coherency between adjacent records at depth developed?

5.5 The underlying theory and assumptions involved in the use of the proposed approach need to be clearly stated. Terms such as tapered time series are used without explaining what the tapering function is.

5.6 The report starts off with assertions that SMART-1 and LSST array data provide well calibrated empirical models without providing the basis for the statement. What is the basis for this assertion.

5.7 It is recognized in the report that topography influences amplification of ground motion at higher elevations. The extent to which magnitude, depth, local geology and directivity of ground motion propagation influence coherency of vibratory ground motions recorded within distances comparable to the foundation dimensions of a nuclear plant structures is not clear. Please document the cases.

5.8 Assumptions related to the physical nature of the coherency of propagated motion should be clearly stated at first, the parameters that strongly influence the observed coherency should be identified, then the results of sensitivity studies undertaken to mitigate the effects of sparsity of data, and uncertainty of the nature of future ground motion should be presented.

5.9 Terms such as data taper, lagged coherency, and number of time samples should be defined and the sensitivity of predicted coherency of the ground motion to these parameters should be presented.

5.10 Soil-structure interaction (SSI) effect is modeled upon a basic assumption of vertically propagating shear waves. Please discuss the influences of the type of seismic waves incident upon the site on predicted coherency model.

5.11 Based on the Figure 3-1, it appears that coherency falls off sharply above about 15 Hertz. Please present the correlation coefficients between adjacent recordings from the data base used to derive the curves in Figure 3-1. It is not clear that the behavior of a rigid foundation (most nuclear plant structures with their layout of intersecting shear walls make the entire foundation very rigid compared to the compliant subsurface material) would not modify differently than those coherency coefficients recorded by a dense array of instruments on pads of very small footprints.

5.12 The strain dependent soil properties used in the SSI calculations are derived from an assumption of vertically propagating motion that is coherent from point to point on the foundation attachment locations. Please develop and provide guidance on modeling of soil properties when calculating SSI effects.

5.13 All data processing techniques and model derivations for the results presented in this report are described in previous EPRI and Caltrans reports, and, specifically: N.A. Abrahamson, J.F. Schneider and J.C. Stepp, "Spatial coherence for strong ground motion for application to soil-structure interaction", Electric Power Research Institute RP2978-1, 1990, and N.A. Abrahamson, "Empirical plane-wave coherency models for horizontal and vertical components", Appendix F in *Seismic ground motions report for San Francisco-Oakland Bay Bridge East Span Seismic Safety Project*, Report to Caltrans, 1998, which are not publicly available. These reports should be made available.

5.14 p. 2-2, third paragraph: "As a result, the plane-wave coherency is smaller than the unlagged coherency." "unlagged" should be changed by "lagged".

5.15 It is unclear which data sets were used in the evaluation of the plane-wave coherency model of Eqs. 3. Table II-1 herein summarizes the information provided in Tables 3-1 to 3-3 of the EPRI Report. Table 3-1 of the EPRI Report summarizes the characteristics of the arrays used, Table 3-2 the characteristics of the events recorded and Table 3-3 the duration of the time windows per event and per (dense) array used for the evaluation of the coherency model.

It is stated on pp. 3-1 and 3-9 that, in developing the model, the data from the USGS Parkfield and UCSC ZAYA arrays were not included because the low coherencies observed at these arrays could be due to topographic effects. On the other hand, these two arrays appear in Table 3-3 with the utilized time window length for the evaluation of the coherency. It is also stated on p. 3-9 that "The two arrays with topographic effects were excluded from this analysis (USGS Parkfield and UCSC ZAYA). Two of the remaining arrays, Pinyon Flat and Stanford (temp) did not have reliable relative timing (this does not affect the lagged coherencies comparisons given in Abrahamson *et al.* (1992). The plane-wave coherency residuals excluding the two arrays are shown in Figure 3-3." Were Pinyon Flat and Stanford also excluded from the evaluation of the coherency model due to timing problems or were they only partially considered through their lagged coherency but not their plane-wave effect?

**Table II-1**. Summary of array and event information

| **Array** | **Site Class** | **Topography** | **No. of Stations** | **Station Separation** (m) | **Magnitude** | **No. of Events** (Table 3-2) | **No. of Events** (Table 3-3) |
|---|---|---|---|---|---|---|---|
| EPRI LSST | Soil | Flat | 15 | 3 – 85 | 3.0 – 7.8 | 15 | 13 |
| EPRI Parkfield | Soft Rock | Flat | 13 | 10 – 191 | 3.0 – 3.9 | 2 | 2 |
| Chiba | Soil | Flat | 15 | 5 – 319 | 4.8 – 6.7 | 9 | 9 |
| USGS Parkfield | Soft Rock | Ridge Tops | 14 | 25 – 952 | 2.2 – 3.5 | 9 | 9 |
| Imperial Valley Differential | Soil | Flat | 5 | 18 – 213 | 5.1 – 6.5 | 2 | 2 |
| Hollister Differential | Soil | Flat | 4 | 61 – 256 | 5.3 | 1 | 1 |
| Stanford (Temp) | Soil | Flat | 4 | 32 – 185 | 3.0 – 4.0 | 4 | 4 |
| Coalinga (Temp) | Soft Rock | Flat | 7 | 48 – 313 | 5.2 | 1 | 7 |
| USGS ZAYA (Temp) | Soft Rock | Mountains | 6 | 25 – 300 | 2.3 – 3.0 | 3 | 3 |
| Pinyon Flat (Temp) | Hard Rock | Flat | 58 | 7 – 340 | 2.0 – 3.6 | 6 | 6 |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| SMART-1 | Soil | Flat | 39 | 100 – 4000 | 4.0 – 7.8 | 20 | |
| SMART-2 | Soil | Flat | 8 | 200 – 750 | 4.0 – 5.5 | 2 | |
| Total No. of Events | | | | | | 74 | 56 |

The use of 10 or 8 arrays affects the percentage of "rock" vs. "soil" samples in the evaluation:

If 10 arrays were utilized (Table II-1 without the arrays with topographic effects – blue font color), and considering the number of events from Table 3-3 (last column in Table II-1 with 20 events for SMART-1 and 2 for SMART-2), then there are 3/10=30% rock sites and 15/66=22.7% rock events.

If 8 arrays were utilized (Table II-1 without the arrays with topographic effects – blue font color, and without the arrays with timing problems – violet font color), and considering the number of events from Table 3-3 (last column in Table II-1 with 20 events for SMART-1 and 2 for SMART-2), then there are 2/8=25% rock sites and 9/56=16.1% rock events.

Also, was each event or was the ensemble of all events at an array considered as an individual sample in the evaluation of the coherency model?

The red numbers in the two last columns of Table II-1 indicate differences in Tables 3-2 and 3-3 of the report regarding the number of events per array used in the approach. For example, how many events were used for Coalinga (temp): 1 as per Table 3-2 or 7 as per Table 3-3? Were 15 or 13 events from the EPRI LSST array utilized?

5.16 It has been observed that there is a difference in the pattern of the coherency decay at shorter and longer separation distances (i.e., coherence decay is dependent on separation distance). For example, Figure 1, taken from Abrahamson *et al.* (1991), depicts the differences in the coherency decay recorded during the same earthquake at the LSST array (event 12) and the SMART-1 array (event 43); the event's characteristics were $m_b$=5.6, depth 2 km, and its distance from the array 4 km. Abrahamson *et al.* (1991) observed that, "In general, the coherencies predicted at short station spacing using the SMART-1 coherency functions are larger than measured from the LSST array. This is especially true at frequencies greater than 5 Hz". Riepl *et al.* (1997) made a similar observation from their analyses of an extensive set of weak motion data recorded at the EUROSEISTEST site in northern Greece: the loss of coherence with distance for their data was marked by a "cross over" distance, that distinguished coherency for shorter (8-100 m) and longer (100-5500 m) separation distances. Can a single expression for the coherency reasonably represent the different exponential decay observed at long and short separation distances?

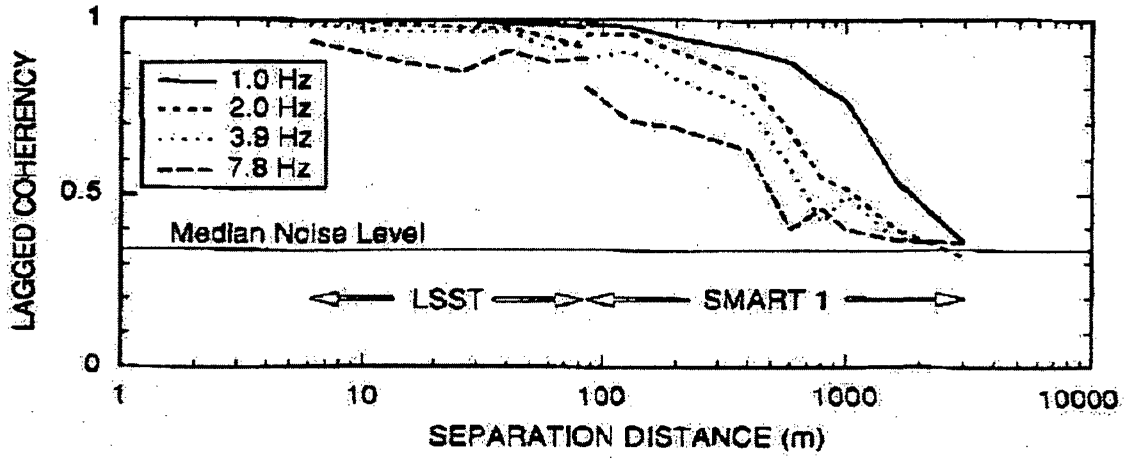



**Figure II-1**. Comparison of the coherency decay recorded at the LSST array (event 12) and the SMART-1 array (event 43) (from Abrahamson *et al.*, 1991).

5.17 The issue of whether "rock" and "soil" sites can be grouped together is a subjective one. To illustrate this point, coherency estimates at rock and soil sites are needed. The report provides the final coherency estimate from all array data in Eqs. 3-1 through 3-3, but does not contain coherency estimates from each individual array. Information and results for coherency estimates in the horizontal direction from a data set similar to the one used in the report are available from previous published work in journals and conference proceedings. However, for the vertical motions, such information is not available. Hence, in the following, only the horizontal direction of the motions is addressed.

Abrahamson *et al.* (1991) evaluated a coherency model from data recorded at the EPRI LSST array. The model that they provided, hereafter referred to as the EPRI LSST coherency model, has the form (Abrahamson *et al.*, 1991):

$$\tanh^{-1}|\gamma(f,\xi)| = (2.54 - 0.012\xi)[\exp\{(-0.115 - 0.00084\xi)f\} + \frac{1}{3}f^{-0.878}] + 0.35 \quad \text{(II-1)}$$

$$h(f,\xi) = \frac{1}{1+\left(\dfrac{f}{18.7}\right)^4} \quad \text{(II-2)}$$

$$\gamma_{pw}(f,\xi) = |\gamma(f,\xi)|\, h(f,\xi) \quad \text{(II-3)}$$

where $|\gamma(f,\xi)|$ is the lagged coherency, $h(f,\xi)$ is the correction factor for single plane wave propagation and $\gamma_{pw}(f,\xi)$ is the plane-wave coherency. Equations II-1 through II-3 are not valid for separation distances greater than approximately 100 – 150 m. In a later work, Abrahamson *et al.* (1992) compared their EPRI LSST model with coherency estimates from a number of arrays, the majority of which were also used for the development of the model in the report (Eq. 3-1), which, hereafter will be denoted as the new coherency model. Figures II-2 through II-5 present the results reported in Abrahamson *et al.* (1992). It is noted that the values of the coherency estimates were "read" from the Abrahamson *et al.* (1992) paper and are, therefore, approximate; however, they indicate the trend of the data.

Figure II-2 presents the arctanh lagged coherency for the soil sites at separation distances of 15-30 m and 50-80 m. The numbers in parenthesis indicate the number of events at each array used in the coherency estimates by Abrahamson *et al.* (1992). The solid lines in the subplots of Fig. II-2 are for the minimum (15 m) and maximum (80 m) separation distances of the EPRI LSST coherency (Eq. II-1) and the dashed ones for plus and minus one standard deviation. Figure II-2 suggests that the coherency estimates at the soil sites fall, to a significant degree, within the plus and minus one standard deviation of the EPRI LSST coherencies. It would be interesting to also compare the range of plus and minus one standard deviation from each of the arrays where data of more than one events were recorded. Figure II-3 presents the lagged coherency of the soil data and the EPRI LSST coherency for the minimum and maximum separation distances. Figure II-3 further confirms that, even though the coherency decay for each soil array follows its own pattern, the trend of the decay of the data with frequency at the various arrays is similar.

Figure II-4 presents the arctanh lagged coherency for the rock sites at separation distances of 15-30 m and 50-80 m, with the numbers in parentheses indicating again the number of events used in the evaluation of the coherency at each array. The solid and dashed lines in the figure are the same EPRI LSST results of Fig. II-2. The circles and squares in the figures indicate the results for the USGS Parkfield and ZAYA arrays, respectively, which were not considered in the evaluation of the new coherency model of the EPRI report due to topographic effects. Indeed, their trend (Fig. II-4) is very different from the EPRI LSST coherency. It may be postulated from Fig. II-4 that the results for the remaining rock arrays fall, more or less, within the plus and minus one standard deviation of the EPRI LSST coherencies, but it appears that their trend is different. Figure II-5 plots the lagged coherency of the rock data and the EPRI LSST coherency for the minimum and maximum separation distances. This figure illustrates more clearly than Fig. II-4 that the rock coherencies have a different trend – "flatter, and more slowly decaying – than the EPRI LSST coherency. As indicated for the soil sites, it would also be interesting to compare the range of plus and minus one standard deviation from each of the arrays where data of more than one events were recorded.

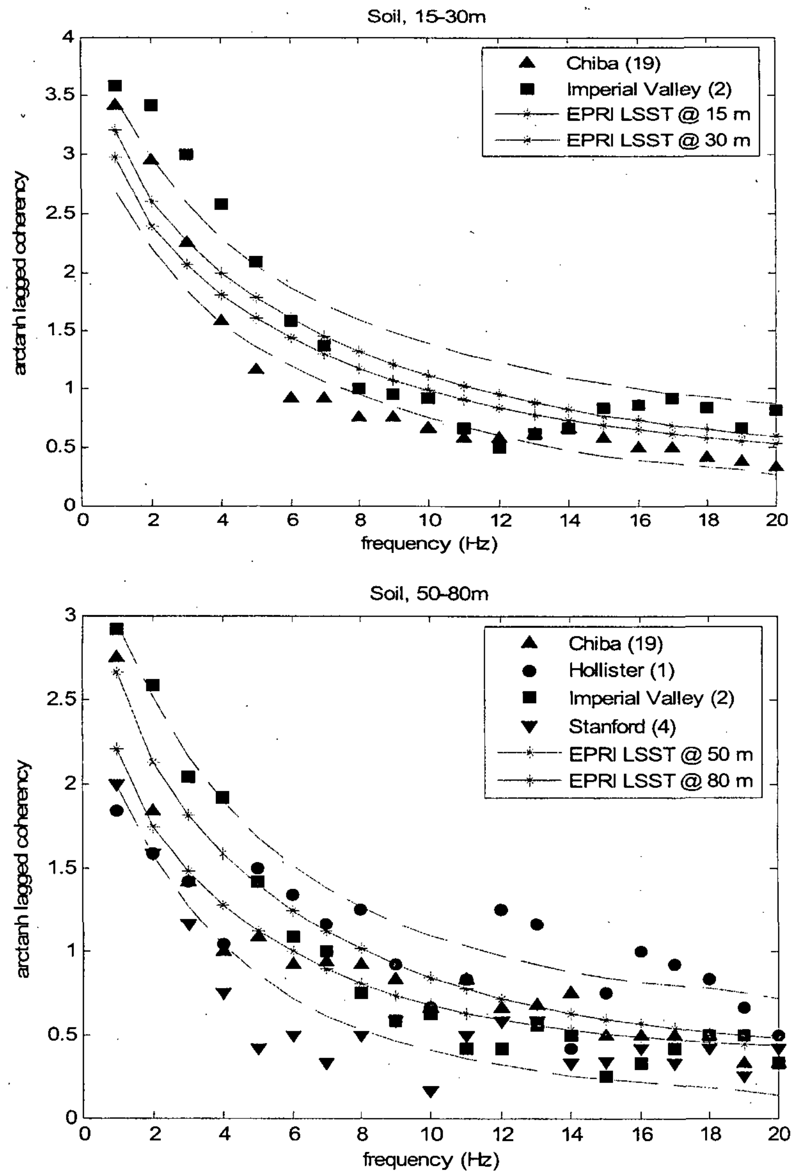



**Figure II-2**. Arctanh lagged coherency from Abrahamson *et al.* (1992) for soil sites. Solid lines are for the minimum and maximum separation distances in each subplot and dashed lines for plus and minus one standard deviation.

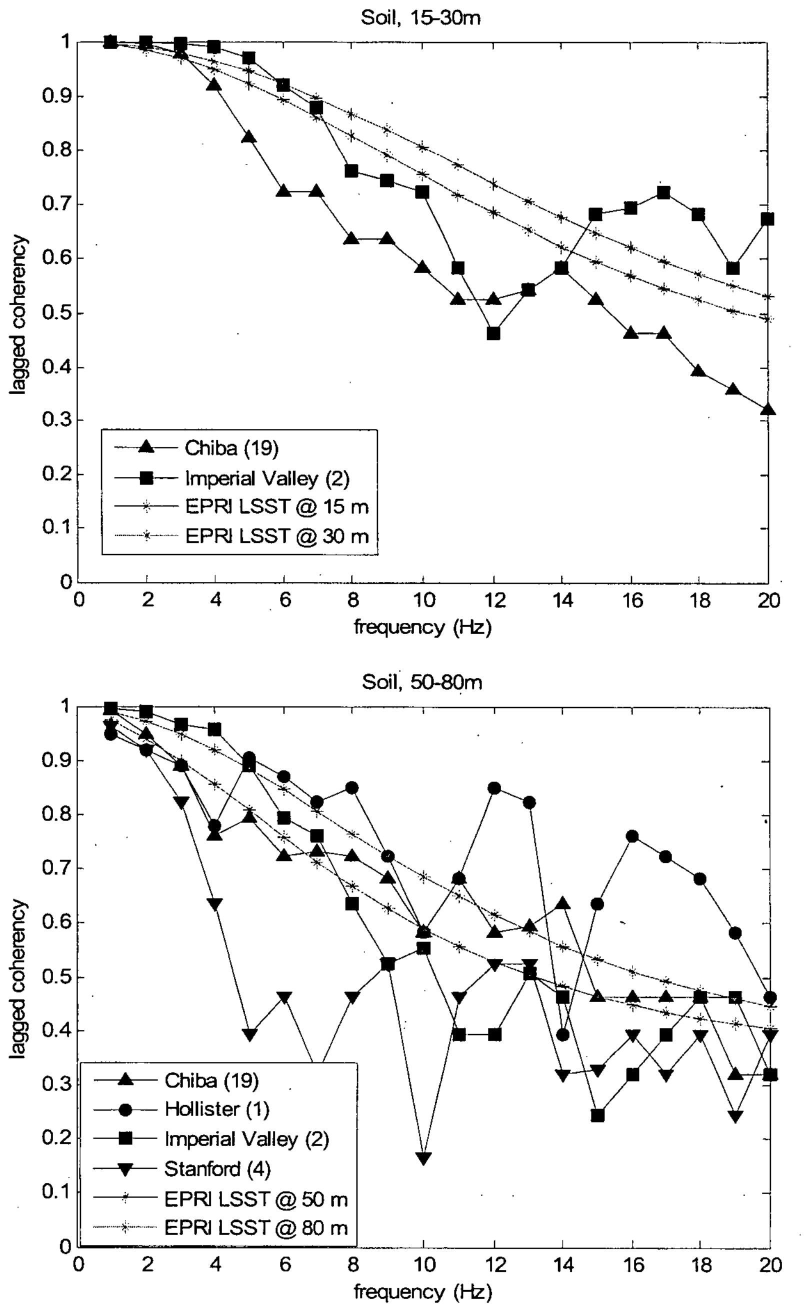



**Figure II-3**. Lagged coherency from Abrahamson *et al.* (1992) for soil sites.

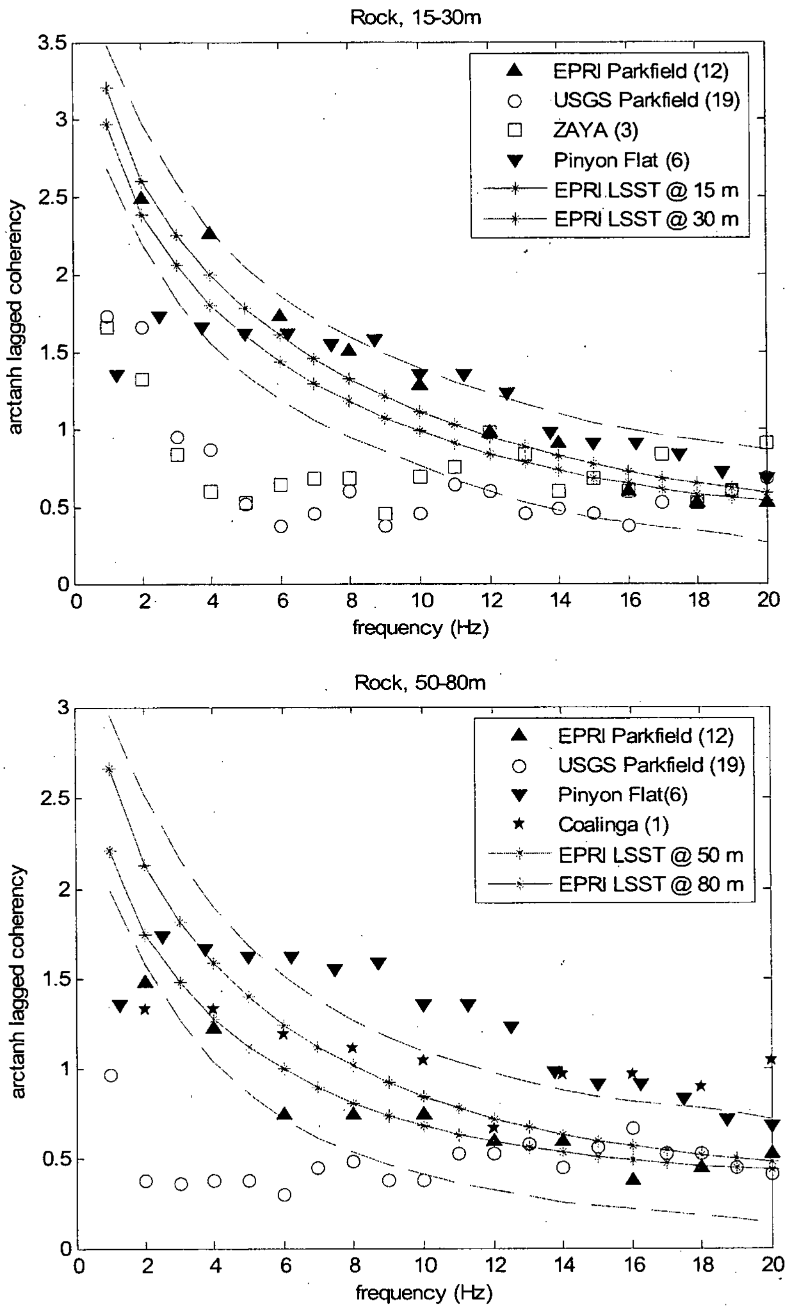



**Figure II-4**. Arctanh lagged coherency from Abrahamson *et al.* (1992) for rock sites. Solid lines are for the minimum and maximum separation distances in each subplot and dashed lines for plus and minus one standard deviation.

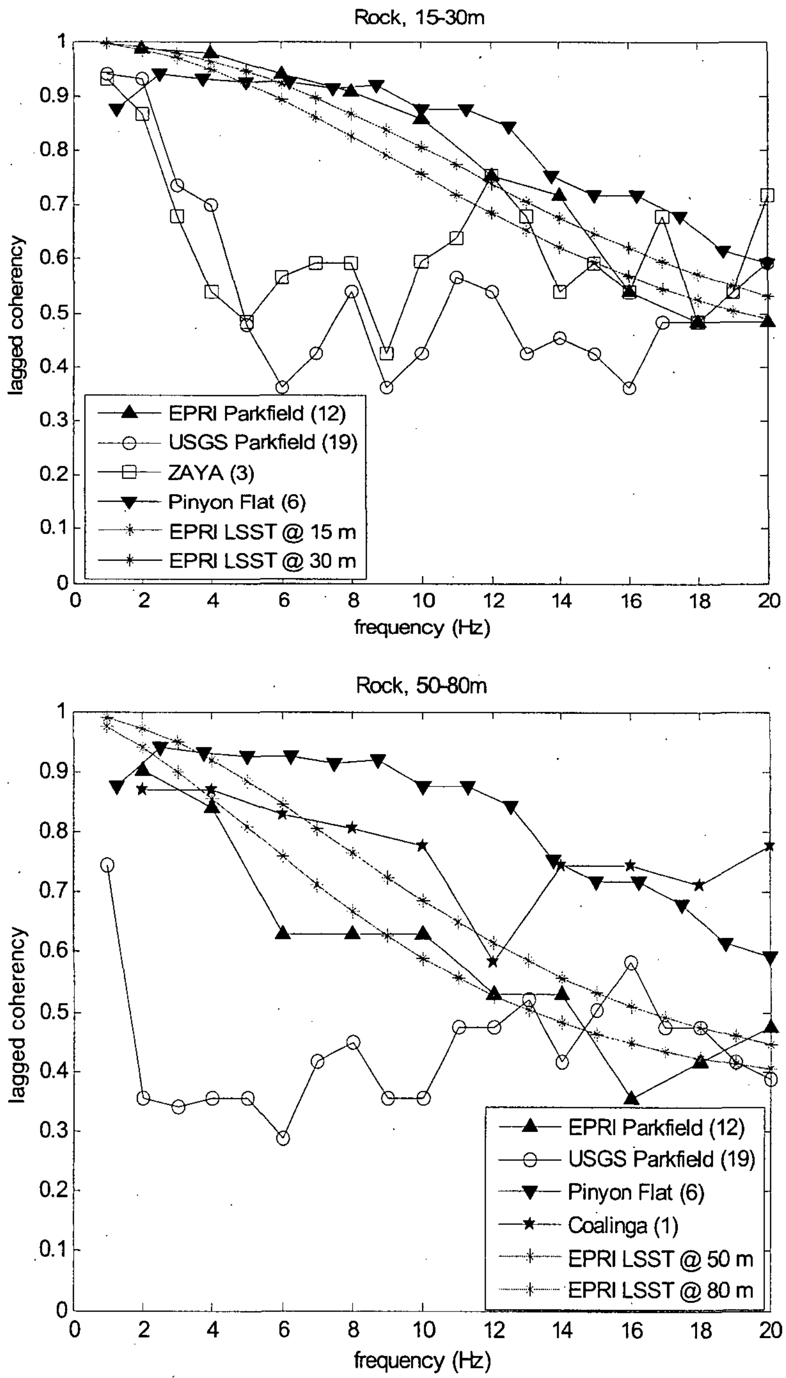



**Figure II-5**. Lagged coherency from Abrahamson *et al.* (1992) for rock sites.

The results and comparisons in Figs. II-2 through II-5 were provided by Abrahamson *et al.* (1992) in terms of lagged coherency, but not in terms of plane-wave coherency. It is noted that, for the EPRI LSST model, the lagged coherency and the correction factor for the single plane wave propagation are separable (Eqs. II-1 through II-3). On the other hand, it appears that, for the new coherency model, the two effects are grouped together. The new coherency model is presented in Eq. 3-1 of the report and repeated here for convenience:

$$\gamma_{pw}(f,\xi)=\left[1+\left(\frac{f\tanh(a_3\xi)}{a_1 f_c(\xi)}\right)^{n1}\right]^{-1/2}\left[1+\left(\frac{f\tanh(a_3\xi)}{a_2 f_c(\xi)}\right)^{n2(\xi)}\right]^{-1/2} \qquad \text{(II-4)}$$

with parameters defined in Tables 3-4 and 3-5 of the report.

To associate the results and comparisons of Figs. II-2 through II-5 with the new coherency model, the new plane-wave coherency model (Eq. II-4) is compared with the EPRI LSST lagged and plane-wave coherency models (Eqs. II-1 and II-3) in Fig. II-6 for separation distances of 15, 30, 50 and 80 m. Figure II-6 suggests that the plane-wave EPRI LSST coherency model is similar to the new plane-wave coherency model for the range of separation distances considered, and, hence, the lagged coherency of the EPRI LSST model can be used as a "proxy" for the lagged coherency of the new model. It is noted that the new coherency model correctly accounts for unit coherency at zero frequencies and separation distances.

The following questions then arise, given the "flatter" and "slower" decay of the rock sites in Figs. II-4 and II-5: If the rock sites were considered by themselves, would the resulting "rock" coherency model compare well with the EPRI LSST/new coherency models? Furthermore, since the number of soil arrays/events is significantly higher than that of the rock arrays/events (as indicated in II-3), can it be that the trend of the data at the rock sites is "buried" within that of the soil sites? Also, what was the frequency content of the motions for each event? What was the frequency range for which a predominant slowness was identified from the data? Why were some recorded events not considered in the evaluation of the new coherency model? For example, from the 15 events recorded at EPRI LSST (Abrahamson *et al.*, 1991) 13 were used for the new model, and from the 12 events recorded at EPRI Parkfield and the 19 events at Chiba (Abrahamson *et al.*, 1992) only 2 and 9 events, respectively, were used for the new model (Table II-1).

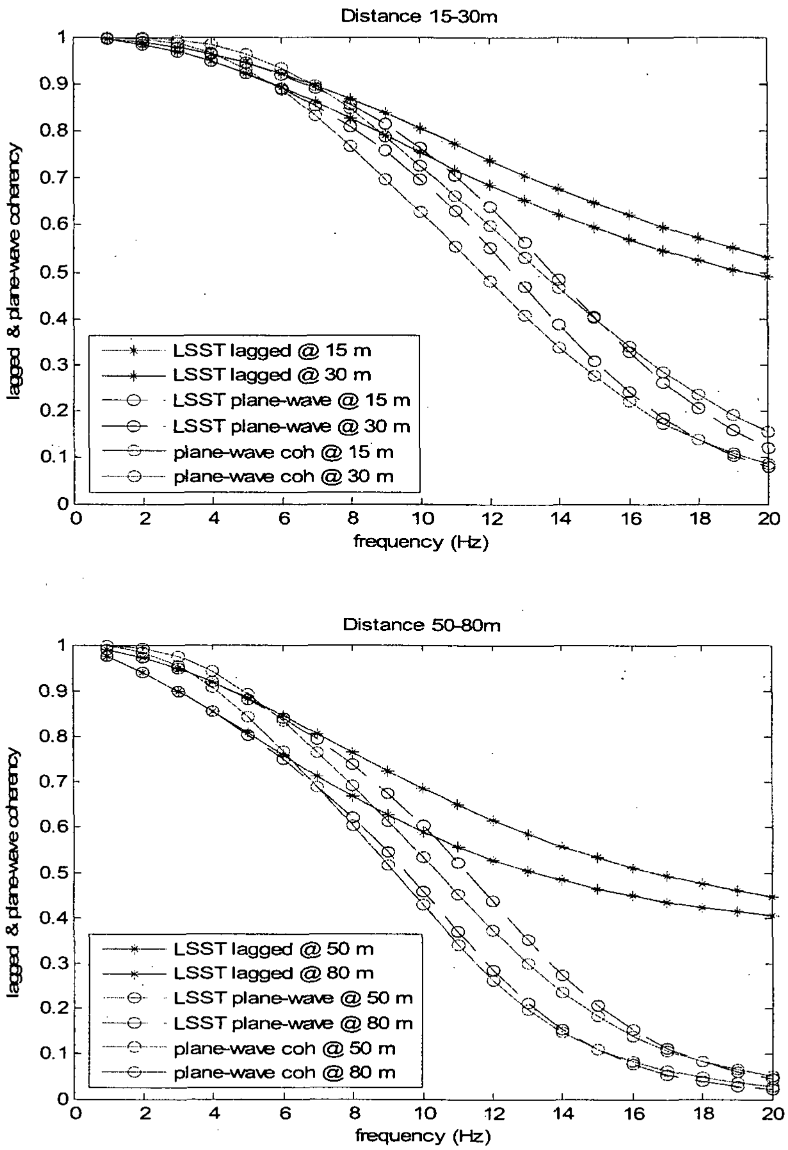
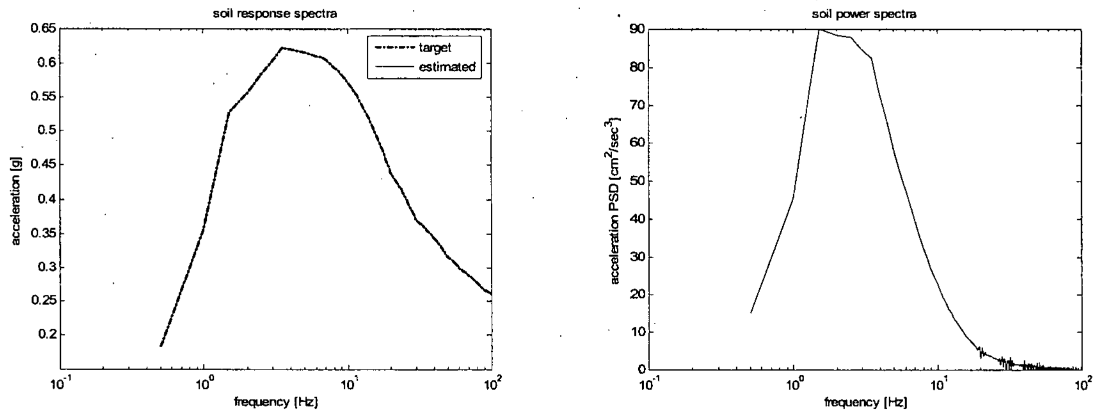


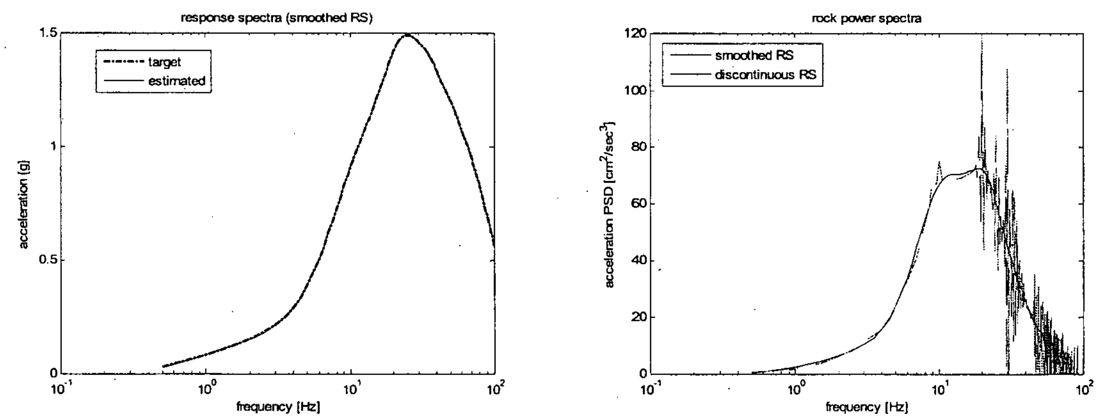

**Figure II-4**. Comparison of EPRI LSST lagged and plane-wave coherency with new plane-wave coherency model.

5.18 To evaluate the frequency content of the seismic motions and compare them with the exponential decay of the coherency models with frequency, the soil and rock response spectra were converted to equivalent power spectral densities (PSDs). Figures II-7 and II-8 present the response spectra and the corresponding PSDs for the soil and rock sites, respectively. The PSDs are approximate: (1) the values of the response spectra were "read" from Figs. 2-5 and 2-6 for the rock and soil sites, respectively, provided in the EPRI Report 1012966; (ii) the conversion was made using a modified Park approach (Park, 1995) that utilizes Davenport's approximation for the upcrossings and a duration of 16.8 sec; (iii) the rock response spectrum, due to its discontinuities at higher frequencies yielded a highly "spiked" PSD (Fig. II-8b). To eliminate these spikes, the discontinuous response spectrum was fitted with a 20[th] order polynomial. The PSD of the smoothed response spectrum eliminates the spikes resulting from the actual response spectrum discontinuities (Fig. II-8b). The soil PSD (Fig. II-7b) is sharply peaked, and all PSDs oscillate rapidly in the higher frequency range. The response spectra evaluated back from the PSDs are in good agreement with the target response spectra (Figs. II-7a and II-8a). Therefore, even though approximate, the PSDs are indicative of the frequency content of the motions.




(a) response spectrum (b) power spectral density

**Figure II-7**. Soil response spectrum and equivalent power spectral density




(a) response spectrum (b) power spectral density

**Figure II-8**. Rock response spectrum and equivalent power spectral densities.

Figure II-9 presents the normalized PSDs for soil and rock and the EPRI LSST lagged and plane-wave coherencies at separation distances of 10, 30, 60 and 100 m. The plane-wave coherency (Eq. II-3) can be used to model the wave field by a single inclined plane wave (Abrahamson *et al.*, 1991). The plane-wave coherency in Fig. II-9 decays sharply passed the dominant frequency range of the soil PSD. However, it decays sharply from the beginning of the dominant frequency range of the rock PSD. Does this imply that a very significant part of the dominant frequency range of the rock sites cannot be considered as a single inclined wave but energy coming from different paths?

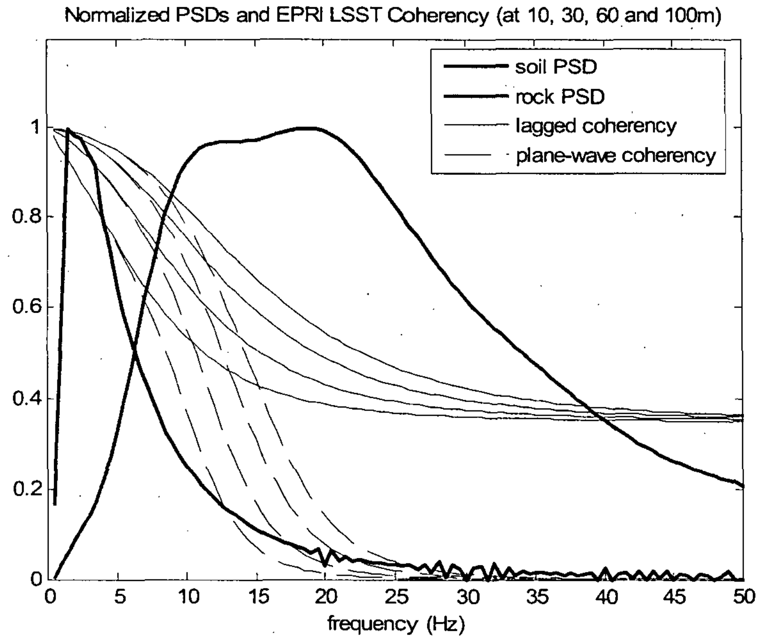



**Figure II-9**. Normalized power spectral densities at soil and rock sites and EPRI LSST lagged and plane-wave coherency at separation distances of 10, 30, 60 and 100 m.

5.19 A previous study (Schneider *et al.*, 1990) analyzed a small (M 3) earthquake recorded at the EPRI Parkfield array and compared it with the Lotung coherency. The epicentral distance of the earthquake was 9 km from the array and its depth was 9 km. The EPRI Parkfield array site is characterized as soft rock (Table II-1). Figure II-10, taken from Schneider *et al.* (1990), presents the comparison of the EPRI Parkfield coherency for the small event with the median and 16$^{th}$ and 84$^{th}$ percentile LSST coherency for separation distances of 10 m and 60 m. Even though the EPRI Parkfield coherency falls within the 16$^{th}$ and 84$^{th}$ percentile LSST coherency (except at low frequencies for the 10 m separation distance), there is an observable increase in the single-event EPRI Parkfield coherency past 20 Hz. The type of behavior at higher frequencies indicated in Fig. II-10 cannot be extrapolated from the exponential decay at the lower frequencies. Are there additional data available at such high frequencies? Is a similar trend observed for other sites/events?

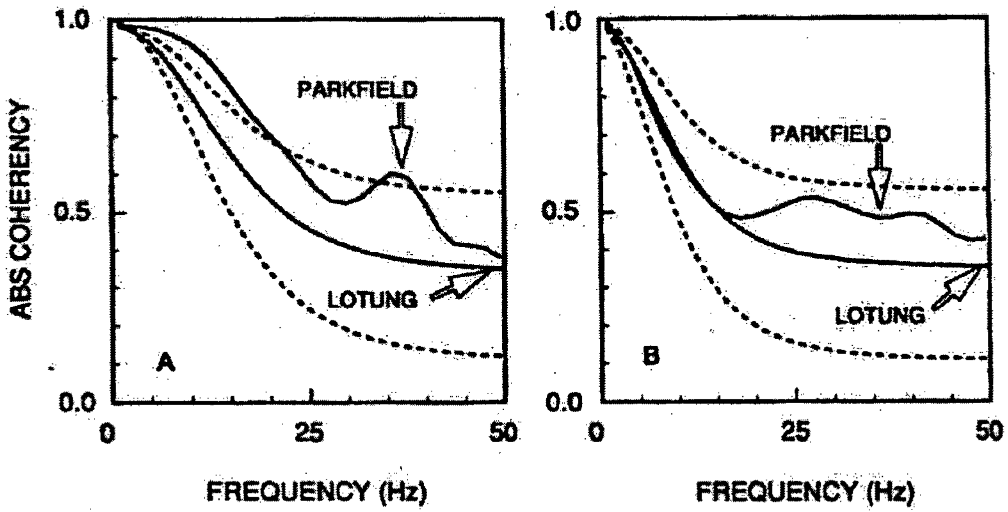



**Figure II-10**. Comparison of the EPRI Parkfield coherency of a small event with the LSST coherency for (A) 10 m and (B) 60 m station separation distance (from Schneider *et al.*, 1990).

5.20 In the comparison of residuals of the arctanh plane-wave coherency of Fig. 3-3 of the EPRI report, reproduced here in Fig. II-11: There appears to be an array (or arrays), for which the analyzed time window permitted frequency domain transform with a frequency step of $\Delta f \approx 0.5$ Hz. The coherency residuals for these events are consistently negative at frequencies 0.5, 1.5, ..., 11.5 Hz, which suggests a negative trend for these array(s) at lower frequencies. If the data from the different arrays were distinctly presented in the figure instead of being grouped together, would there be additional observable trends?

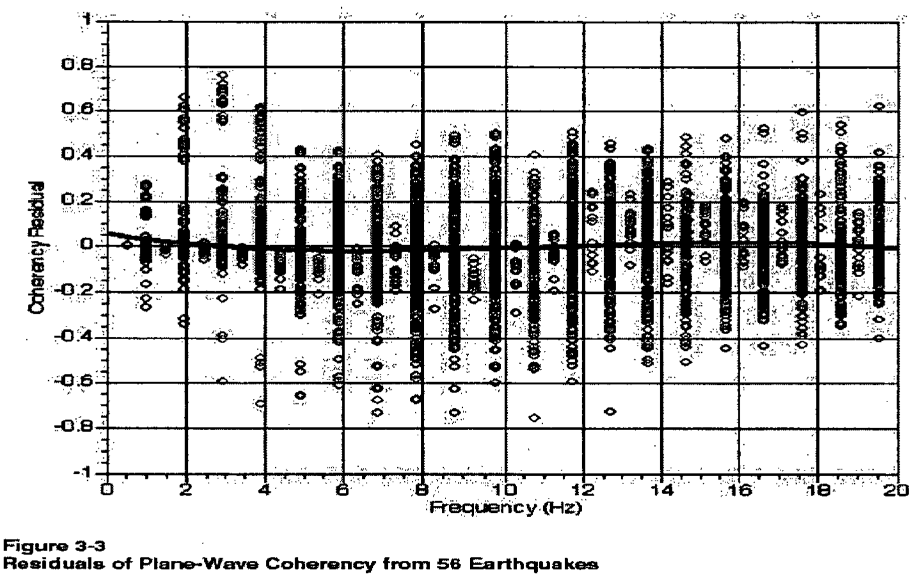



**Figure II-11**. Figure 3-3 from EPRI report

5.21 Figures 3-4 and 3-5 of the EPRI report, which present the coherency residuals as functions of magnitude at 5 Hz and 10 Hz, respectively, are combined in Figure II-12. Similarly, Figs. 3-6 and 3-7 of the EPRI report, that present the coherency residuals as functions of distance at 5 Hz and 10 Hz, respectively, are combined in Figure II-13. The 5 Hz figures are less populated than the 10 Hz ones: For example, Fig. 3-4 (in Fig. II-12) does not present any data at 5 Hz for magnitudes lower than 4.3, whereas data are presented in the 10 Hz plot (Fig. 3-5), and the number of data points at higher magnitudes appears to be less for the 5 Hz plot (Fig. 3-4) than the 10 Hz plot (Fig. 3-5). Figure 3-6 (in Fig. II-13) does not present any data at 5 Hz for distances of 6 and 9 km, whereas data are presented for the 10 Hz plot (Fig. 3-7). There are also 4 data sets at distances between 10 and 20 km for the 10 Hz plot (Fig. 3-7), but only two for the 5 Hz plot (Fig. 3-6). Again, the number of data points appears to be less for the 5 Hz plot (Fig. 3-6) than the 10 Hz plot (Fig. 3-7). Can Figs. 3-4 and 3-5 be provided with all data? If the difference in the number of data points at 5 and 10 Hz is caused by the different □f's in the Fourier transform of the motions at the various arrays, can data be provided over a range of frequencies centered at 5 and 10 Hz, respectively, so that the trend of the data at the lower and higher frequency can be recognized?

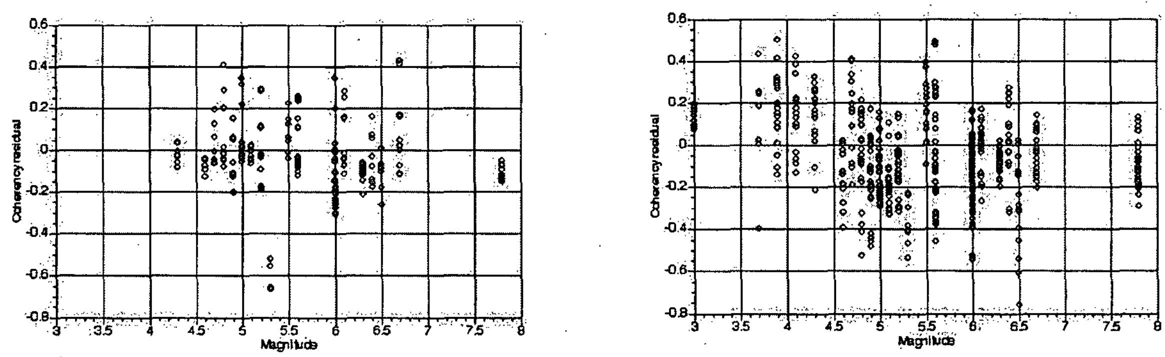



Figure 3-4
Plane-Wave Coherency Residuals at 5 Hz

Figure 3-5
Plane-Wave Coherency Residuals at 10 Hz

**Figure II-12**. Figures 3-4 and 3-5 from EPRI report

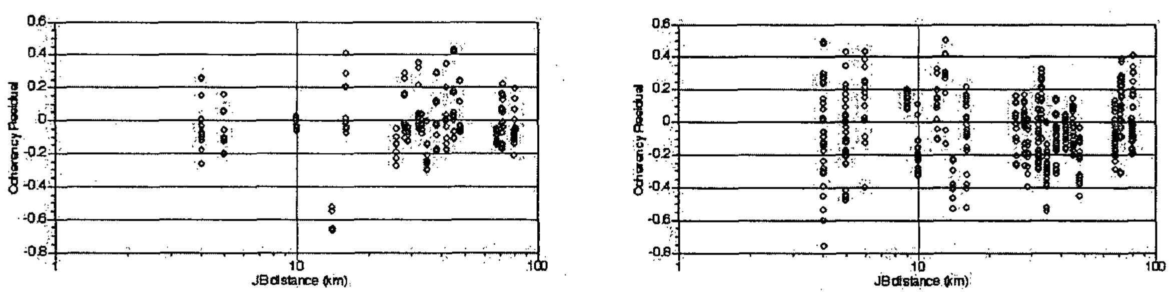



Figure 3-6
Plane-Wave Coherency Residuals at 5 Hz

Figure 3-7
Plane-Wave Coherency Residuals at 10 Hz

**Figure II-13**. Figures 3-6 and 3-7 from EPRI report

5.22 The issue of the variation of coherency with depth is raised in both the EPRI Report 1012966 and the earlier Report TR-102631 (1997). Some of the arrays, e.g., EPRI LSST, EPRI Parkfield, Chiba, have downhole recordings. Was coherency with depth or coherency at depth investigated?

## 6. Comments on EPRI Report 1012967, "Effect of Negligible Inelastic Behavior on High Frequency Response"

6.1 The NRC staff and its consultants had previously reviewed the EPRI draft report TR-102470 entitled "Analysis of High-Frequency Seismic Effects" and provided its comments in a letter dated March 22, 1995. Please provide your response to the comments enclosed with the NRC letter of March 22, 1995.

6.2 NRC staff stated in its letter of March 22, 1995 that the staff considers the laboratory test program contemplated in EPRI's letter of June 1, 1993, an essential part of validating the methodology in the report, and that the final evaluation of the methodology will depend on the outcome of this part of the effort. Please discuss your plans and schedule for completing the testing phase of this effort.

6.3 Please clarify if this methodology will only be used to define the amplified response spectra (ARS) for the design and analysis of equipment anchored to the floor by fillet welds. It should not be used for design of structures and piping. The methodology should not be used for equipment anchored by other means, e.g., bolts, partial penetration, or full penetration welds. The methodology should also not be used for equipment sensitive to high frequency motions such as relays, contactors, etc.

6.4 The report identifies several recommendations as necessary to support the findings of Task S2.2. Please discuss the implications of these additional studies and what is NEI's position with justification pending the completion of the studies. These recommendations are further discussed under specific comments below.

6.5 As shown in Figure 2-1, the modified response spectra are reduced significantly for frequencies > 10 Hz. There may be equipment in the plant, e.g., RPV internals, certain regions of piping, stiff equipment that might have resonance for these high frequencies. Figure 2-1 also indicates a reduction in ZPA below 0.3 g. Please justify the reduction in the ZPA and the filtering out of the spectra at these higher frequencies for high frequency equipment.

6.6 This study is incomplete in many ways. There are many recommendations for additional studies. An important aspect is a lack of any treatment of vertical ground motion related responses and whether or not there are any reductions in the high frequency range.

6.7 Under highly idealized modeling of in-structure response of cabinets anchored with fillet welds there is reduction in the response spectra at high vibratory frequencies. Behavior of real structures is more complex. It has been argued that in civil structures small 0.01 inch cracks can reduce response at frequencies above 25 Hz. However, low level vibratory ground motion recorded on top of basemat, in the free field and on top the dome of a containment structure show that the motion at the top of dome is amplified in the high frequency range all the way up to 100 Hz. Observations from recorded motion should be carefully examined before the staff can consider any reduction in response spectra for equipment qualification.

6.8 Page 1-1: This section states that for the surrogate case of the fillet weld, the actual deformation due to the seismic demand is an order of magnitude less than the maximum limiting deformation. However, it is not appropriate to quantify this margin in general terms, since it will depend on the seismic demand, which is a function, among other things, of the site seismicity, characteristics of the site and the structure, and the location of the equipment in the structure.

6.9 Page 2-1: This section states that for a 25 Hz system, with SA = 0.5g, SD < 0.01 inch, which can be considered as negligible (actually SD = 0.008 inch for this case). How can this be used in a generic sense when this value of SD ~ 0.01 inch is only valid for SA = 0.5 g and for a system with f = 25 Hz.

6.10 Page 3-4: This section states that a representative indication of the ratio of the transverse weld strength to longitudinal weld strength for cyclic loading can be obtained as the average of the ratio for the tension induced shear specimen and the compression induced shear specimen. This seems to be an arbitrary assumption that has not been validated experimentally or analytically. Please provide justification or develop an alternate methodology of determining this ratio.

6.11 Page 3-4: This section states that the deformation ratio at maximum load is 0.04917. Please clarify what is meant by "maximum load", and whether it is the same as the ultimate load.

6.12 Page 3-4: This section also states that the deformation ratio at rupture is 0.05594. Please clarify how this ratio is obtained.

6.13 Page 3-6: The discussion in this section for cyclic loading such as seismic is focused on the surrogate anchorage of a fillet weld loaded in transverse shear. Please discuss how the conclusion would change for equipment attached by full penetration welds, partial penetration welds, or bolted anchorage.

6.14 Page 3-7: This section states that displacement ratios up to +-8 were obtained. Please explain what is meant by displacement ratios.

6.15 Pages 3-5 to 3-7: This section addresses the issue of cyclic response effects assuming equivalent number of cycles =10. Please provide the basis for this assumption.

6.16 Page 4-1: This section states that the simplified models do not allow friction sliding or free rocking between gaps. Please discuss the implications of not allowing friction sliding or free rocking.

6.17 Page 4-2: For the shear resistance model discussed in this section, even though the equipment has low-aspect ratio, the weld will be subjected to bending in addition to shear. Please discuss the implications of ignoring bending stresses in welds.

6.18 Page 4-4: Please note the typo in the equation in the middle of the page, just before equation 4-1.

6.19 Page 4-8: It is not clear how the equation at the top of the page is obtained. Also, what is $SA_c$ and why is it equal to $SA_y$?

6.20 Page 4-12: This section states that rotational inertia of the equipment is ignored in the overturning model. Please discuss the impact of ignoring rotational inertia of the equipment.

6.21 Page 4-12: Please note the typo in the equation at the bottom of the page.

6.22 Page 4-13: In the line below Equation 4-7, eq.(4-6) should be replaced with eq. (4-1).

6.23 Page 4-15: This section states that values of $F_\mu$ were computed using different values of parameters, *a* and *b*, until an optimum set of values was obtained. Please explain how the optimum values were selected.

6.24 Page 4-15: This section states that the models developed in the prior study and in this study are valid for horizontal input motion only. Does this imply that the high frequency reduction will not be applied to the vertical motion or that the industry will be conducting further work for the vertical motion.

6.25 Page 5-1: This section states that this approach requires that the structure amplification, as measured by the ratio of an in-structure spectrum to input spectrum, be included in the application of the models developed in Chapter 4. However, these factors cannot be determined generically. They are dependent on the input motion characteristics, site conditions, and the structural model and its dynamic characteristics.

6.26 Page 5-2: This section discusses the criterion for the spectral acceleration of the oscillator whose frequency is greater than $f_{ZPA}$. Please describe how this project intends to define ZPA cutoff frequency.

6.27 Page 5-3: This section states that given that the "modes are well separated", the amplification factor will have a maximum when the in-structure oscillator is tuned to a structure mode frequency. Please discuss the implications if the modes are not well separated and are "closely spaced."

6.28 Page 7-2: This section states that the selection of a transverse loaded fillet weld is meant to represent a low bounding approximation of any of several non-linear mechanisms present. Please discuss how it can be shown and the data that supports that the transverse loaded fillet weld represents the lowest ultimate limit deformation.

6.29 Figure 7-1: Two of the three colors in this figure are almost the same which makes the figure confusing. Please redraw.

6.30 Page 8-1: This section states that more recent test programs have shown that the actual weld deformation at fracture is at least a factor of two greater than the values predicted by Lesik and Kennedy. However, this conclusion is still based on limited data and further tests, especially for cyclic loading, may give different results.

6.31 Page 8-1: This section states that the conclusion that strength degradation effects due to reverse cycle loading do not need to be incorporated is based on "very limited low-cycle fatigue tests." Please discuss if and what further tests are planned to support this conclusion.

6.32 Page 8-1: This section further states that this conclusion should be reviewed by researchers active in the field of seismic testing of welded steel connections. It further states that EPRI plans to initiate this review subject to adequate funding availability. Please discuss the plan, approach, schedule, and details of this review and further testing.

6.33 Page 8-1: This section states that the models developed in this project are for horizontal input motion only. It further states that EPRI plans to initiate additional model development for vertical input motion subject to adequate funding availability. Please discuss the plan, approach, schedule, and details of this work.

6.34 Page 8-4: This section states that "if the ground input has a PGA at frequency less than 100 Hz, the amplification ratio is simply the ratio of the in-structure ZPA to the ground PGA and is not an oscillator amplification." Please clarify this statement as it would seem that the amplification should be a function of the structure and the oscillator frequencies.

6.35 Page 8-4: This section specifies values for maximum spectral amplification as 2.5, the strength margin ratio as 1.6, the range of *"e"* as 1-4, and the range of scale factors as 0.4-4. However, these values are for the example cases and may be different in actual cases.

6.36 Page 8-4: This section states that the use of a constant amplification factor is identified as an assumption requiring verification, and that EPRI plans to initiate a verification study subject to adequate funding availability. Please discuss the plan, approach, schedule, and details of this study.

6.37 The approach in this report, as in its predecessor, is based on the notion that force reductions can be permitted as a result of nonlinear structural actions. Since the results of the 2005 study are intended to be used to address high-frequency response issues in earthquake-resistant design, this philosophical stance for designing SSC in NPP, and any limitations in its use, should be articulated clearly.

6.38 The assessment is based on the analysis of a conservative "worst case" situation: an electrical cabinet connected to the floor with a transverse 3/16-inch fillet weld, leading to a connection that is stiff but relatively weak (elastic limit at approximately 0.001 inch; deformation capacity 0.010 inch). The analyses presented of this component/anchorage system in support of the conclusions are exhaustive. I understand the logic of identifying such a case to demonstrate the basic concept; as was the goal of the 1993 report. However, in developing a design tool, it would have been preferable to have an analysis of a second ("median") case and perhaps a third case as well. This would yield a sense of the level of the conservatism if the approach were to be applied to design of other (perhaps generic) equipment items.

6.39 The constitutive model describing the force-deformation relation in the fillet weld (Lesik/Kennedy) appears to be quite conservative in light of more recent data (Bowman; Grondin). Would the use of the more recent data – in particular, the much higher deformation capacity of the fillet weld - have any notable impact on the results?

6.40 The authors dismiss the possibility of fatigue damage, asserting that the equivalent number of full stress cycles during an earthquake is less than 10. In SSC where the fundamental frequency is on the order of 10 Hz or greater, this assumption requires more careful justification. Moreover, many fillet welds have a built-in crack due to lack of penetration. In such a case, an S-N approach to fatigue resistance (cycles to failure) is less persuasive than a fracture mechanics approach.

6.41 The authors dismiss the possibility of fatigue damage, asserting that the equivalent number of full stress cycles during an earthquake is less than 10. In SSC where the fundamental frequency is on the order of 10 Hz or greater, this assumption requires more careful justification. Moreover, many fillet welds have a built-in crack due to lack of penetration. In such a case, an S-N approach to fatigue resistance (cycles to failure) is less persuasive than a fracture mechanics approach.

6.42 The sliding and rocking analyses from the 1993 report were re-derived directly in the 2005 report as single degree of freedom “shear resistance” and “overturning resistance” models. The "equivalent linear" formulation lacks transparency. It requires nonlinear analysis to obtain (at least initially) the empirical constants (*a* = 1.6 and *b* = 0.3) that define the effective frequency ratio and damping in the equivalent linear system, and necessitates the iterative calculation of the modified spectrum summarized in Table 6-1. With modern computation support, such equivalent linear methods have lost some of their appeal. If inelastic actions are to be permitted (see comment 1), the seismic demand could just as easily be based on a yield spectrum that is derived directly from the UHS/EUS ground-level response spectrum, with appropriately defined force reduction or ductility coefficients. Such coefficients are no more difficult to define (in fact, the analyses must be performed to calculate empirical constants *a* and *b* in the equivalent linear formulation), and their audit trail is clearer.

Table 8.1 of *ASCE Standard 43-05*, dealing with equipment and distribution systems, stipulates “inelastic energy absorption factors, $F_\mu$,” under the assumption that the seismic demands will be determined through an elastic spectrum, de-amplified by $F_\mu$. For electrical cabinetry, for example, $F_\mu$ ranges from 2.0 (large-deformation limit state A) to 1.0 (essentially elastic limit state D). Since the inelastic behavior is already incorporated in the “equivalent linear” formulation in TR 1012967, the formulation in TR 1012967 appears to be incompatible with *ASCE Standard 43-05*.

6.43 The parameter study of amplification of ground motion at different elevations of equipment mounting points within the structure is comprehensive. To preserve the fidelity of response at frequencies up to 100 Hz, theoretical continuous (Bernoulli-Euler, shear or Timoshenko) models of the structural system rather than discrete models were used. Such theoretical models are better able to capture the higher-frequency contributions to dynamic response. The mass density in the continuous models selected for these parameter studies was assumed to be uniformly distributed. Unless this uniformity is typical of SSC in NPPs, it is not obvious why the inferences drawn from these studies are necessarily more valid than those drawn from a refined discrete mass model, such as the 33-element model identified as Case 2 in Figure 5-3. Furthermore, it is asserted (p. 5-15) that the Timoshenko model is the best representation of typical NPP structures. Accepting this statement, the exhaustive presentation of results for *all* continuous models obscures the fundamental result: the de-amplification of the ground motion at elevation due to inelastic action of the equipment anchorage. In my view, it would have been preferable to invest this computational effort in analysis of one or two alternate, more typical, components.

What is the relevance of the European hard rock spectrum to the current study? Its characteristics appear somewhat different from those for ground motions in the CEUS.

6.44 The concept of uncertainty does not appear in this assessment. Only a "worst-case" situation was considered. A "worst case" analysis is inconsistent with a probabilistic approach to safety analysis and risk mitigation, which strives, at least, for a best-estimate and a measure of uncertainty. Accordingly, I don't see how the results of this study can be used in a PSA or in risk-informed decision-making.

For design purposes, a "mean plus one standard deviation" rather than an envelope on the amplification factors (Figures 5-15 through 5-19) might be equally appropriate, especially in view of the "worst-case" nature of the component considered. Was any consideration given to this?

6.45 The effect of spatial incoherence in ground motion over large foundation areas in reducing the required spectral ordinate for design should be kept distinct from the reduction due to inelastic action in SSCs (e.g., equipment anchorage).

6.46 The recommendations in Chapter 7 to address functional failures or qualification of active equipment are quite general: qualify for higher input, utilize high-frequency stress testing, or use "other data" (perhaps from military programs) to qualify the function. If testing is to be used in lieu of analysis, there ought to be some way to ensure that the qualification level is approximately the same either way and would be consistent with the regulatory performance objective. How would that outcome be guaranteed? Perhaps specific test protocols to ensure that the performance levels measured by test and by analysis are comparable were not in the scope of the EPRI task. It might be well to undertake such a task in the future.

6.47 The authors have recommended a future study to compare design forces obtained from the proposed procedure with design forces obtained in-structure response spectra determined for unmodified ground motions. This study should be given a high priority. Furthermore, it would be well to consider several equipment items, in addition to the worst cases above, and to estimate the uncertainties associated with the seismic demands. The results of such a study would support the design guidelines in Section 8 of *ASCE Standard 43-05*, provided that the recommendations were presented in a form that is consistent with that *Standard*.

6.48 Page 8-5: This section identifies three recommendations as necessary to support the findings of Task S2.2. These recommendations are related to:

- An expert review of the effects of low-cycle fatigue on fillet weld performance
- Reduction of vertical in-structure response spectra.
- Indirectly reduced in-structure response spectra versus directly reduced in-structure response spectra.

Please discuss the plan, approach, schedule, and details of these studies.

6.49 The PSHA is supposed to integrate all potential earthquakes in the site region that are potentially damaging to NPP systems into the definition of the seismic hazard. The lower-bound value of the CAV is intended to be the measure of ground motion that can be damaging to the facility in the structural sense; that is, cause motions of the facility that can develop strains within elements that exceed the yield strain (or some such equivalent structural concept). The objective of the study is therefore to eliminate the contribution of small magnitude events from the PSHA, as these will have little, if any, chance of inducing element strains exceeding yield for "well-designed and constructed facilities". The CAV model is then intended to show that high frequency components of the seismic hazard provide little contribution to inelastic strains and therefore need not be a concern from a structural point of view.

The objective of the CAV study is then to provide a basis to limit the contributions of small magnitude events to the defined seismic hazard for the site. These contributions are generally to the high frequency end of the defined seismic hazard. Thus, the outcome of the CAV evaluation is to reduce the high frequency contribution to the seismic hazard.

The objective of the study entitled "Effect of Negligible Inelastic Behavior on High Frequency Response" is much the same; that is, indicate the inability of high frequency motion to cause small inelastic strains in structural elements which are a measure of element damage. The two studies are apparently discussing the same phenomenon. If both elements are to be used in an evaluation of the site seismic hazard, the CAV study can be used to reduce the high frequency component of the design spectrum, while the results of the second study can then be used to further reduce the high frequency component of the input spectrum. Since these two studies are both associated with development of small inelastic strains in structural elements, this appears to be double-counting this effect? Please explain.

## 7 NRC staff questions from the October 19-20, 2005 meeting with industry on Seismic Issues Resolution Program.

7.1 Provide an alternative performance-based method to determine the safe shutdown earthquake (SSE) ground motion spectra using seismic core damage frequency (SCDF) as a metric. Use an appropriate SCDF metric to arrive at the design response spectra (DRS) close to those derived from the ASCE 43-05 method.

7.2 The DRS obtained from the SCDF metric approach should have spectral acceleration values at 1 and 2.5 Hz that do not fall below some minimum values.

7.3 SCDF metric should be used for 1, 2.5, 5 and 10 Hz, rather than the average of 5 and 10 Hz currently indicated in RG 1.165.

7.4 Use a set of CAV-filtered hazard curves for the above items.

7.5 Undertake peer review of CAV filtering and input data.

7.6 Undertake peer review of incoherency model.

7.7 NRC requests validation of incoherency effects through some recorded event.

7.8 NRC requests Dr. Kennedy to make a presentation of the alternative performance-based method using SCDF metric.

7.9 Conduct peer review of S2.2 results (high frequency knock-down) and underlying assumptions by equipment vendors.

7.10 List of all NEI scheduled products for inclusion in meeting summary. Facilitate NRC review of the NEI's performance-based seismic design approach with the goal of reaching a common understanding of the approach in an integrated manner.