*Note the comments for which the NRC has “significant concern” are highlighted in orange.

NRC Comments*

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1	Pages ix-x	“Based on the precision model evaluated, if an alternative assumption or parameter is used in a seismic hazard study, and it potentially changes the calculated hazard (annual frequency of exceedence) by less than 25 percent for ground motions with hazards in the range 10–4 to 10–6, that potential change is within the level of precision with which one can calculate seismic hazard.” It should be made clear that a certain level of precision does not relieve users from performing site-specific studies to identify potential capable seismic sources within the site region and vicinity as well as newer models and data. In addition this level of precision does not alleviate users from fixing errors that are discovered in the CEUS-SSC model as it is implemented for siting critical facilities. In addition, NRC has not defined a set value for requiring or not requiring siting applicants to revise or update PSHAs.	Text has been revised
2	Chapter 1	Subsection 1.3 ‘The study region” states that “The SSC model developed for this project is applicable to all sites within the project study region” This statement is not correct as the study region does not extend 200-miles beyond the US borders. Examples are FL and TX. The same paragraph also states “On the north and southwest, the study region extends a minimum of 322 km (200 mi.) from the U.S. borders with Canada and Mexico”. This is also not completely correct, as the models do not extend into Mexico. (see Figure 1.3-1)	Explanation added that the SSC model is applicable at all sites, but may need to be supplement for site-specific applications, depending on regulatory requirements.
3	Chapter 2	The document contradicts itself as well as some of the earlier project documents made available on the make up of the TI Team. While during the project workshops it was reported that the **TI team consisted of four members** and two resource experts. This current document states that **TI team includes 12 members**, shown in Figure 2.3-1 and also listed in Appendix G. Yet, another conflict exists in the document with Table 1.2. It states that “The CEUS SSC Project team consists of program and project management, a TI Lead, **TI team (consisting of about 15 seismologists, geologists, and hazard analysts)**, a participatory peer review panel (PPRP), specialty contractors, and sponsor representatives”. To address part of this discrepancy during the NRC presentations made by	The TI Team members are identified in Table 2.3-1 and their biographies are given in Appendix G. The expertise of each team member is discussed in Appendix G. No individual team member was given the responsibility of being a subject matter expert for a particular discipline because the team works as a team.

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		the members of the TI team, it was stated that TI team staff was later named as part of the TI Team and this was the explanation of the differences observed in the workshop slides and what is provided in this report. However, a further check on the names listed on the TI Team staff listed in the workshop reports show that TI Team now includes other names that were neither part of the TI Team nor was part of the TI staff as shown in the workshop presentations. Considering that the TI Team is the central core of the entire process from the beginning to the end of a SSHAC Level 3 study, why is there such an uncertainty in the make up of the TI team? Page 2-18 states "The members of the CEUS SSC TI Team span a wide range of expertise needed to conduct SSC, including the disciplines of geology, geophysics, tectonics, seismology, and hazard analysis". Please include in Figure 2.3-1 which member served as what subject matter expert in those disciplines quoted above.	
4	Chapter 2 / § 2.1.2.2	The schedule for the development of the CEUS-SSC has been very aggressive. While a SSHAC level 3 process appears to have been sufficient for the CEUS-SSC, there has been little or no chance for the proponents from Workshop #2 or CEUS proponents in general to provide comment/feedback/debate on the final logic tree weights and overall model development. Therefore, the assertion by the TI team that the SSHAC Level 3 process for the CEUS-SSC captures the CBR of the ITC is not as robust. Please comment on whether an additional workshop or perhaps having proponents at Workshop #3 would have strengthened the conclusion that the CBR of the ITC was accurately represented.	Section 2 has been extensively revised to clarify all of the process steps that were conducted, including four rounds of model building. Also discussed are the four PPRP briefings that were held with the specific purpose of reviewing the models and their technical bases, and the PPRP attended 8 of 11 working meetings in order to understand the evaluation and model-building process. It is explained that these activities provide the information that is needed for such a large regional study.
5	Page 2-38	"The goal in that discussion was to focus on the data that would be most useful in defining the SSC model at the annual frequencies of interest (e.g., 10–4 to 10–7/yr) for nuclear facilities." How do you do this? Please provide more detail as to how this goal was met.	Explanation added that this means close attention to uncertainties, because they can be important to mean hazard at low annual frequencies of exceedance.
6	Chapter 2 / § 2.4.7	Workshop #3 was intended as a feedback workshop; however, due to the aggressive schedule for the development of the CEUS-SSC important parts of the CEUS-SSC model were still not completed. This includes Mmax values for the source zones and weighting for some of the major branches of the logic tree. Also, the demonstration hazard calculations, which compared the CEUS-SSC model hazard curves to others such as USGS 2008 and modified	See comment #4 above. Proponent experts participate in the evaluation phase of a SSHAC project when alternative data, models, and methods are being evaluation. The project then moves into the integration phase and the TI Team conducts assessments and builds the SSC model. Feedback regarding the model-building process is provided by the PPRP. Feedback from proponent experts with regard to the integration process is not appropriate in a SSHAC

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		EPRI were not complete. In addition, as noted above in Comment No. 4, there were no proponents from Workshop #2 to provide feedback. As a result of these issues, please justify the conclusion that Workshop #3 provided adequate feedback on the CEUS-SSC model. Please discuss the TI team's plans to ensure that proponents will have a chance to provide feedback after the CEUS-SSC model is completed at the end of 2010.	Level 3 process.
7	Chapter 3	A Section needs to be added that actually describes the final CEUS-SSC Catalog in terms of number of events, magnitude ranges (i.e., basic statistics), comparison to other CEUS catalogs, such as USGS, etc. This is general information that would be useful to non-experts. Chapter 3 should also have a earthquake catalog table showing final declustered catalog as well as a table showing dependent events.	Comparisons of the number of events and the assumed completeness are presented in the revised report. Final catalog with flagged dependent events is included in Appendix B.
8	Page 3-2 / § 3.1.2	Consistency with the SSHAC process: "EPRI (1988) developed techniques to produce a catalog with a uniform size measure that is appropriate for unbiased estimation of earthquake occurrence rates for use in seismic hazard assessment. They were used in the EPRI (1988) study to develop a uniform catalog of mb magnitudes. A goal of the catalog development efforts in this study is to use these same techniques to produce a uniform catalog of moment magnitude values that have properly accounted for the uncertainty in size estimation as part of development of earthquake occurrence rates." Was EPRI's (1988) technique for developing a "catalog with a uniform size measure…appropriate for unbiased estimation of earthquake occurrence rates" evaluated by the TI team in accordance with the SSHAC process? Do other catalog developers use other techniques? Why was the decision made to use EPRI's (1988) technique?	The approach used by EPRI-SOG for unbiased estimation of recurrence rates was evaluated by the TI team by simulation testing and was modified for use in the final CEUS SSC model. These tests showed that the approach is the appropriate method to use. It is not a judgment question.
9	Chapter 3 / § 3.3	The description of the conversions to moment magnitude is insufficient. Why are the moment magnitude values used for the CEUS-SSC different from others? How large is the difference? More background is necessary. The sentence "A locally weighted least squares fit to the data shown in Figure 3.3.1-1 was used to correct the moment magnitudes reported in Atkinson (2004) to value of M used in this study" needs some context and further explanation. Each of the conversions subsections needs further context, detail, and justification.	Text on development of moment magnitudes greatly expanded.
10	Pages 3-6 & 3-7	Regarding conversion from intensity (Io) and felt area (FA) into M, it is not clear how those conversions are combined. Are they combined and averaged	The various size measure are combined using a variance weighting approach. If FA data is available, it typically gets a large weight because of the lower

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		or some measures have priorities over others? What if instrumental information is old and less reliable, but there is a good intensity or FA map?	variance of the estimated value.
11	Chapter 3 / § 3.3.4	Need to provide bases for priority scheme. Reference to EPRI (1988) is insufficient as this document is not widely available.	Additional justification provided.
12	Chapter 3 / § 3.4 & 3.5	The sections on catalog declustering and catalog completeness do not adequately describe the methods. Reference to EPRI (1988) is insufficient. Both of these sections need to be significantly expanded (perhaps with two new appendices). Bases for why these two methods meet the CBR of the ITC need to be provided. For example, have other SSHAC process PSHA studies used the EPRI (1988) declustering method? Would the use of another declustering method resulted in a significantly different set of dependent events? References are missing (Gardner and Knopoff (1974), Gruenthal (1985)). Similarly with regard to catalog completeness. Figure 3.5-1 shows quite variable cells. How were these cells developed? Is there really enough information to define differences in completeness at such a high resolution?	Sections expanded to provide basis for assessment. Comparison of declustering using alternative USGS approach provided. Not aware of other SSHAC process that have used EPRI declustering, but differences are not significant from USGS approach. Missing references added.
13	Chapter 3 / Figures	Need to provide more and higher resolution seismicity figures in order to see comparison with other CEUS catalogs as well as events within different source zones.	Figure added that shows the spatial distribution of earthquakes added to the USGS 2006 catalog.
14	Page 4-14	Section 4.3.3 states that Pa is used as a criterion in seismic source definitions. However, the document lacks a detailed discussion of this issue. Another section of the report states that “But experience has shown that only those tectonic features/hypotheses having a significant probability of being seismogenic (Pa greater than about 0.5) will have hazard significance.” How is Pa defined in this case? Is there a list of seismic sources not considered, because their Pa values were less than 0.5?	Probability of activity was defined in the EPRI-SOG Project as the probability that a particular tectonic feature was seismogenic and localized earthquakes of M≥5 over and above those produced by the background source. As discussed in the text, nearly all tectonic features in the CEUS were assessed to have a Pa less than 0.5 and, as a result, to not contribute significantly to hazard. The only features included in the CEUS SSC model are known active faults (e.g., Meers, Cheraw) with good evidence for recent displacement and a high probability of activity. That said, the CEUS SSC model is a regional model and any application of the model for site-specific purposes would need to consider possible local seismic sources, as required by the applicable regulatory guidance.
15	Chapter 4 / § 4.4.1	The strong preference (0.8 to 0.2) for seismotectonic source zones versus Mmax zones is not adequately explained. Currently, USGS (2008) uses the large source zone (similar to Mmax zones) approach while modified EPRI-SOG uses seismotectonic zones as opposed to large sources zones. Section 4.4.1 provides only one paragraph to justify the strong preference for seismotectonic source zones. In addition, the report does not describe the impact of this weighting on the final hazard curve results. Augment Chapter 4 to provide further justification for this weighting preference. If the weighting	Weights for the two models have changed and significant new justification for the weights has been added. Section 8 now shows sensitivity to seismotectonic vs. Mmax sources at all 7 test sites.

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		between Mmax and seismotectonic zones were more even, what would the resulting hazard curves look like in comparison?	
16	Page 4-22	What is the basis for using reduced cell sizes of ¼ degree for the a and b calculations? How is an earthquakes' location uncertainty accounted for in such small cell sizes? The catalog has significant amount of earthquakes which include more than 10km, and perhaps 20 km of location errors, which is almost the size of your rate calculation window of ¼ degrees. Rate estimates very much depend on the cell size selected and significant variations exits between adjacent cells (e.g., Figure 5.3.2-6). How are earthquake location uncertainties incorporated in rate calculations when such small cell sizes are used?	In principle, smaller cell dimensions are preferable because they allow finer spatial resolution. The absence of earthquakes in an individual cell does not create a problem because the penalty functions that promote smoothness in fact create a larger "effective cell size." Tests on the MIDC_A zone with objective smoothing indicate similar results for cell sizes of 0.25, 0.5, and 1 degree. The objective smoothing compensates for the cell size by arriving at solutions with smaller $\sigma_{\Delta\nu}$ (i.e., smaller differences between adjacent cells) for the smaller cell sizes. Uncertainty in epicentral location is ignored in this study and may be an issue for case A and in regions where there are many events and rates vary substantially between adjacent cells. In other cases or locations, the rate varies smoothly between adjacent cells anyway. According to the catalog, uncertainty in location has been less than 10 km since the mid 1970's. Because this issue affects active regions only, the effect of uncertain epicentral locations is lessened because a significant portion of earthquakes there are well located and there is a large number of them.
17	Page 4-39	Table 4-3. How were the indicators shown in this table defined? What were the criteria used. (Examples of concerns include assigning lowest weights to a) zones of weakness and tectonic/geodetic strain data/modeling, b) Cratons and historical instrumental seismicity, c) geologic evidence for potential zones of stress concentration/amplification and analysis of instrumental seismicity data, etc)	Additional explanation added that indicators come from the larger technical community. The indicators are only used as a means of identifying the types of data that may be applicable to the SSC model. Thus, it assists in the data evaluation process.
18	Chapters 4 and 5	The "degree of smoothing" level of the logic tree is simply labeled as "Objective" on each of the logic tree figures. The project report does not adequately describe the range of smoothing from high to low within each of the source zones. Augment Chapters 6 and 7 to discuss the smoothing within each of the source zones. Provide more discussion of high smoothing within some of the source zones which results in a "floor" level even for areas of low seismic activity.	As indicated in Section 5.3.2, the objective smoothing selects the appropriate range of smoothing parameters for each source zone and magnitude-weight case, based on the number of earthquakes and their spatial distribution. The resulting values are presented in Figures 5.3.2-4 through 5.3.2-9, and examples for ECC-AM and ECC-GC are provided. These issues, and the issue of the floor, are discussed at a generic level in Section 5.3.2. It is also worth noting that the range of smoothing options was expanded with the inclusion of case E.
19	Chapter 5 & Appendix H	The master logic tree shows that more than 33% is given to weighted magnitudes in recurrence calculations. Within the weighted distribution	The reasons for alternative magnitude weights, and their consequences in terms of the maps, are discussed in length in Section 5.3.2.2.1 of the revised

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		magnitudes in the range of 3.0 and 3.6 are given only 10% weight. A look at the earthquake catalog shows that more than 65% of the data are in this magnitude range. As such, the importance of the earthquake catalog has been diminished and will certainly result in lower rate estimates as evidenced in Figures 5.3.2-20 through 5.3.2-30. Provide more bases for the selected weights.	report. Also, the use of these weights dates back to EPRI-SOG, and the USGS follows a very similar approach by considering alternative low-magnitude cutoffs. It is not true that the introduction of magnitude weights leads to lower rates. The empirical proof of this statement is provided by Figures 6.4.2-1 through 6.4.2-3 of the revised report, which show approximately the same rate for cases A, B, and E.
20	Chapter 5 / § 5.2.1.1.4	This section provides an upper end truncation of M 8.25 for the CEUS. Please discuss why a similar lower end truncation value of say M 5.25 was not used. The ECC seismotectonic sources have Mmax distributions that start at M 5. Discuss the geologic implications of this result for the branch of the logic tree with Mmax=5. Also provide justification for M 5 Mmax values in terms of representing the CBR of the ITC.	A lower Mmax truncation of **M** 5.5 has been added and is justified in this section. The support for the upper truncation is also further justified.
21	Page 5-19	The only methodology used is significantly revised version of the EPRI 1989 study. It appears that the TI team has done significant updates to the EPRI methodology, and opted not to use alternative models, such as the kernel approach used by other members of the community. The new methodology has not been published nor has it received community approval in the literature. Considering these short comings, how do you justify solely relying on your models to calculate the rates and still stay within the SSHAC guidance of representing the CBR of the ITC?	Discussion added to justify the use of the penalized maximum likelihood approach to smoothing and to present the argument that all smoothing approaches are based on the same conceptual model of spatial stationarity. The penalized maximum likelihood approach developed for the CEUS SSC project is a refinement of the EPRI-SOG approach, which is part of an SSC model endorsed in Reg Guide 1.208 and has seen common use throughout the technical community in every Combined Operating License application filed to date. Section 5.3.2.4 provides the bases for selecting the approach over the other kernel approaches and reference to it is added to Section 5.3.1.
22	Page 5-19	Last paragraph states that “Also, *a*-values are neither “spiky,” reflecting too strong of a reliance on the exact locations and rate densities of observed events, nor too smooth, reflecting the belief that the observed record does not provide a spatial constraint on rate density variation”. However, the results shown in Figure 5.3.2-6 indicate a contrary view. a-values are very spiky and vary significantly between adjacent cells, indicating that in regions with moderate number of earthquakes, the penalty function is not effective and the rates vary quite significantly.	The sentence has been removed. The current model has three options for smoothing parameters (Cases A, B, E) that are based on different weighting of the magnitude bins used in the smoothing procedure. The results from this range of smoothing parameters is a range of maps from “spiky” to very smooth. Discussion is added to the text that this represents a reasonable range of parameters and range of resulting spatial variations.
23	Page 5-20	Regarding recurrence parameters, the BSSA paper of Lombardi (2003) is not mentioned. This was pointed out to TI team members during their visit to the NRC in August, 2010. Please comment as to whether this is the source of the misfit between the recurrence model and the data. It appears that the model overestimates the recurrence and this may be one of the reasons for the	We have examined the Lombardi paper and concluded that the issues identified in that paper do not affect the results of this study. A section was added (Section 5.3.2.6) that discusses the paper and its practical implications.

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		higher hazard results shown in the Chapter 8 for some of the demonstration sites.	
24	Page 5-31	Considering the number of earthquakes are low in CEUS, and Figures 5.3.2-14 and 5.3.2-15 show that differences in eight alternative models (at the 10-5 exceedance levels) can be as much as 250% (0.4 vs 1.0). Does this methodology adequately capture the uncertainty or introduce additional errors due to low number of events?	Although the hazard from the lowest realization may be quite low, tests indicate that the use of eight realizations captures the uncertainty in hazard. In particular, the 24 realizations from cases A, B, and E capture the mean, median, and 85% hazard curves.
25	Chapter 6 & Appendix H	Why do NMESE_W and NMESE_N have so different Mmax assignments? Is this a justifiable difference?	The difference is due to the maximum observed in this source. The NMESE_N contains the Southern Illinois paleo earthquakes that define the maximum observed. The NMESE_W does not contain these earthquakes and the max obs is significantly smaller.
26	Chapter 7	While the study region zone has an Mmax distribution from 6.4 to 7.9, the alternative Mmax zones (MESE & non_MESE) have Mmax distribution ranging from 6.1 to 8.1. Considering the study region encompasses both MESE and non_MESE zones, why is there a mismatch in lower and upper bound Mmax values? Similar issue exists with the MESE_N Mmax zone and the corresponding seismotectonic zones (nap, ghex, ecc_gc, ecc_am, ghex, rr, slr, gmh). While the lower and upper bound Mmax values of the MESE_N are 6.1 and 7.8, seismotectonic zones covering the same area have lower and upper bound Mmax values of 5.0 and 8.1, respectively. Should the higher level seismic sources (e.g., MESE_N) have the similar characteristics of its subcomponents?	All of the distributions span essentially the same range. There are differences in the percentiles at each magnitude such that the 5-point representation produces some differences in the distributions used for hazard. The revised model distributions do not have this problem.
27	Chapter 8	Chapter 8 provides demonstration hazard calculations for 7 test sites. Comparisons are made in between the current model (CEUS-SSC) and USGS (2008) and COLA (modified EPRI-SOG) PSHAs. CEUS-SSC hazard curves are typically higher than USGS or COLA hazard curves. Please discuss these differences and postulate on the reason for the higher hazard using the CEUS-SSC model.	Seismic hazards have been recalculated at all 7 test sites, and current results generally show the CEUS SSC model hazard to lie close to or between the EPRI-SOG and USGS model hazards. Where this is not the case, explanations are given.
28	Appendix B	The declustering process used to eliminate dependent events may be eliminating too many events. It appears that several of the earthquakes classified as dependent events may in fact be independent earthquakes in the region. One of the two identical magnitude earthquakes, for example, in a region is listed as a dependent event. Similarly, low magnitude events such as ~ M* =4.0 identified as earthquakes having aftershocks even four years after what is considered to be the main event. A thorough analysis is needed to assess the validity of the results obtained by the declustering process.	A comparison of the declustering method results obtained using the USGS implementation of the Gardner and Knopoff method was made and showed no significant difference. This comparison is documented in the report
29	Appendix E /	It is stated that the SAR method is useful for sediments less than 50 ka old	Appendix E subsection 2.1.3.3 revised for clarity.

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	§ 2.1.3.3	and provides decadal scale resolution for dating sediments, particularly of eolian origin, that are less than 100 yr old. What are the equivalent ages and resolutions for the MAAD technique? Also with respect to bleaching (OSL), how can you ever determine if the sediment has been completely reset?	
30	Appendix E / § 2.2.1	This section does not explain what all the different features look like and what their relationship is to one another, and what is their genesis. Figures are not well annotated. A figure to illustrate various features and categories of features along with pertinent, important references or study areas would provide a summary of the content of this section and also show the reader where the section is headed.	Section 2.1.1 revised to include additional description of liquefaction features, including a new figure (E-2) that illustrates many of the liquefaction features and their relations to one another that are used in most paleoliquefaction studies. This figure also is cited in Section 2.2.1. Also added a new table (2.1.1-1) that summarizes the type and prevalence of liquefaction features in each of the regional datasets and selected references.
31	Appendix E / § 2.2.4	In estimating the magnitude of a paleo earthquake based on paleoliquefaction data it is stated that the uncertainty is on the order of 1 magnitude unit. How is that determined or derived?	Section 2.2.4 revised to include additional explanation. Also, new table (2.2) added to .summarize uncertainties related to interpretation of paleoearthquake parameters.
32	Appendix E / § 2.2.5.1	Provide more explanation about estimating recurrence intervals. This paragraph is very vague. Why you include if there are not more specific details? What simulations are referred to and what data from the paleo earthquakes are used?	Section 2.2.5.1 revised to include additional detail regarding age estimates of paleoliquefaction features and paleoearthquakes.
33	Appendix E / § 2.2.5.2	The methodology of determining the length and completeness of the record is described in a vague qualitative manner. The brevity of the explanation will not provide to the technical person much of a foundation for how to proceed to answer this issue. Only the New Madrid area is used as a single example of a limiting condition for length of the paleoseismic record (liquefiable deposits no older than 5 Ka yr old). Are there specific studies that attempt to quantify the length and completeness of the record at New Madrid or Charleston or any other historic/modern seismic source zone? How have sea level changes over geologic time affected liquefaction fields? What is being looked for?	Section 2.2.5.2 revised to include further clarification that sediments must be saturated to liquefy during earthquakes, thus the significance of sea level changes and possible effects on the paleoearthquake record. Added example of fluctuating sea level changes at Charleston as possible limitation on completeness of paleoliquefaction record for mid to early Holocene.
34	Appendix H	The HID plays a key role in understanding the CEUS-SSC model and should be more descriptive. In particular, Sections 2 ,3, 4, and 5 need to provide more complete descriptions of the earthquake catalog and source zones (RLME, Mmax, Seismotectonic). Furthermore, the HID should be cross-referenced to the Chapters within the main report.	The HID is intended to be used in conjunction with the main report. It is intended for use in hazard calculation, not understanding of the model. However, more cross-referencing with the main report has been included.
35	Page H-9 / Table 3	Since this is a source geometry and characterization effort, and no attenuation is discussed, Table 3 is confusing. It implies that future earthquake ruptures in Mmax zones will either be strike-slip or reverse. I assume this is because the models were tied to some ground motion prediction equations that deal with these types of ruptures. But, if these are	The seismic source characteristics are not dependent on any particular GMPE. However, the decision regarding which future earthquake characteristics need to be defined is related to the anticipated use of the SSC model in the future. The future earthquake characteristics defined in Section 5.4 are those that are anticipated to be of use in the future when the NGA-East GMPEs are

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		just seismic sources, would not it be better to keep the sources independent of the GMPEs? It implies the sources are fixed to the GMREs. What if we have new GMPEs in the future that deal with normal fault movements, will this mean we need to revise the models?	developed.
36	Page H-10 / Table 4	Mmax table shows NonMES_W with relatively low Mmax values. Provide justification for 65% of the weight to Mmax of 5.8 or smaller within this source.	Mmax distribution has been revised along with all of the others.
37	Page H-20	Seismotectonic source Mmax values contradict with the observed largest earthquakes listed in the catalog in several of the sources. (e.g., ECC lower bound Mmax is 5.0, there is one event with M* = 5.71 in this source, another one is MidC_(A throu D). Lower bound Mmax is 5.3. There are at least seven earthquakes with larger magnitudes in this zone. Similarly, PEZ, RR, SLR, NAP, MES_N, nonMES_w, AHEX and ECC_AM are the other sources with lower bound Mmax being smaller than the largest observed earthquakes.	The maximum magnitude distributions incorporate the uncertainty in estimating the magnitude of the past earthquakes (e.g. Figures 6.3.1-1). Thus, it is possible that the lower tail may fall below the nominal magnitude value assigned to an earthquake. In addition, M* is not an estimate of the size of an earthquake, It is used for unbiased recurrence parameter estimation.

Central and Eastern United States Seismic Source Characterization for Nuclear Facilities
1016756, Draft Report, July 31, 2010

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38	General	There are several references missing in the Reference list. A complete check must be conducted.	
39	Page 2-34	"The NGA-East study is being conducted using a Study Level 3 process. All of the principal participants have been identified and early work has been initiated to develop the project databases." This is a confusing statement, because developers of attenuation models have not been identified yet, RFP was postponed.	
40	Page 3-1 / Para 3.1 / Lines 9-10	"The following were the specific goals for earthquake catalog development:" The specific goals are not listed. What are they? If the above statement is meant to imply that the goals are §3.1.1 *Completeness*, §3.1.2 *Uniformity of Catalog Processing*, and § *Catalog Review*, than it should be explicitly laid out and not implied.	Text modified to clarify
41	Page 3-1 / § 3.1.1 / Para 2 / Line 1	Multiple errors exist with Geological Survey of Canada (GSC) abbreviation. Example: "It is recognized that the USGS and ~~CGS~~ GSC catalogs represent a synthesis of catalog information…"	Incorrect references to GSC corrected
42	Page 3-3 / § 3.1.3 / Line 4	Correction: "…catalog development process was review by seismologists with extensive knowledge and …"	corrected
43	Page 3-4 / § 3.2.4 / Lines 1-3	Correction: "Non-tectonic and erroneous earthquake entries were identified using lists compiled by ANSS, ISC, and the NEIC Mining Catalog, and by information given in a number of the studies listed in Section ~~3.2.4~~ 3.2.3."	corrected
44	Pages 3-7 & 3-8	Are MN and mbLg are the same magnitude measures? mbLg defined by Nuttli (1974), also referred as MN.	Clarification added
45	Page 4-3	How do you determine "credible" and how do you define alternatives that had "minor" differences and did not require inclusion?	

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46	Page 4-13	Discusses 3 different definitions of seismic source. Please explain your definition of a seismic source.	
47	Page 4-22 / last Para	The text does not follow the logic tree shown in Figure 4.4.1.3-1. The 4th paragraph in Seismotectonic Zone Branch states "The next node of the logic tree addresses the approach used for assessing seismicity rates and their spatial distribution…." , but the next branch of the logic tree is the crustal thickness variations.	
48	Page 5-6	Suggestion: Change the header of section 5.2.1 to "Approaches to Mmax Estimation in CEUS"	Change made
49	Page 5-12 / Para 3 / Line 5	Change "expressed in natural log units" to "multiplied by *ln*(10)"	Both are included
50	Page 5-12	Define "z" in Equation 5.2.1-4.	Clarification added
51	Page 5-12 / last line	Put a "space" before "After".	fixed
52	Page 5-15	Suggest the following lines to be added before Equation 5.2.1-6: $$E(M_{\text{max-obs}}) = \int_{m_0}^{m^u} m \frac{dF_{\text{max-obs}}(m)}{dm} dm$$ Integration by parts $$E(M_{\text{max-obs}}) = mF_{\text{max-obs}}(m) \Big\|_{m_0}^{m^u} - \int_{m_0}^{m^u} F_{\text{max-obs}}(m)dm$$	added
53	Page 5-16	The "z" in Equation 5.2.1-9 does not appear to be the same "z" as in Equation 5.2.1-4. If that is the case, use a different symbol.	Eq 5.2.1-4 revised
54	Page 5-20 / Para 3 / Line 10	Insert '(' before '[10]'	
55	Page 5-21 / Para 3 / Line 1	Correction: "The likelihood function for the model parameters given the data ~~given~~ above takes the form…"	

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Comment No.	Page / Para. / Line	Comment / Proposed new text	Responsible Author
56	Page 5-21	How was Equation 5.3.2-2 constructed? Write down the model distribution using the mean rate in equation 5.3.2-1 first.	
57	Page 5-22	It might be helpful to continue Equation 5.3.2-5 with an additional line: "$= \left(\frac{1}{N}\sum_i^N m_i\right) - m_0$".	
58	Page 5-22	In Equation 5.3.2-6 it is not clear why the weight $w(m_i)$ should be in the exponent of the first term.	
59	Page 5-22 / Para 4 / Line 11	Correction: "…to obtain the maximum-likelihood estimates of ~~estimates of~~ v and β in closed form."	
60	Page 5-24	It seems that a factor of '2' is missing in Equation 5.3.2-10.	
61	Page 5-24 / last line	Delete extra spaces in $f(x, y)$.	
62	Page 5-25	Insert a sentence such as "assuming a normal distribution for variation of β in each cell" before Equation 5.3.2-11.	
63	Page 5-25	In Equation 5.3.2-11 index *k* should be changed to *j.* Also, either change $\sigma_{\Delta\beta}$ to $\sigma_{\Delta b}$ or rather all $\sigma_{\Delta b}$'s to $\sigma_{\Delta\beta}$.	
64	Page 5-25 / Para 1 / Line 7	Delete "the product extends over all cells," after Equation 5.3.2-11. It is already mentioned in the line preceding Equation 5.3.2-11.	
65	Page 5-30	Last paragraph states "these results indicate some moderate effect on…" However, it appears that the differences are more than moderate. Figure 5.3.2-11 shows one hazard curve is 2 times higher than the alternate.	
66	Page 5-30 / Para 4 / Line 6	Rephrase: "Note that the vertical scales for activity rate are different in the two ~~graphs~~ figures."	
67	Page 5-31 / Para 4 / Line 2	Insert space after '7.5'	
68	Page 5-45	Fix page numbers for subsequent pages in Chapter 5. Print page numbers on both odd and even sides.	

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Comment No.	Page / Para. / Line	Comment / Proposed new text	Responsible Author
69	Figure 5.2.1-6	In the text the vertical axis in this figure seems to be assigned to $(m^u < z) = 1 - F_{max-obs}$, but in the figure the vertical axis represents $F_{max-obs}$.	Clarified P(mu <=z)
70	Figure 5.2.1-5	In the legend, make the “Kijko (2004) K-S” line longer.	Fixed
71	Figure 5.2.1-6	In the text of Chapter 5 it describes the plot of $P(m^u < z)$ vs m^u, whereas in the figure $F(m^u)$ is plotted vs. m^u.	Clarified P(mu <=z)
72	Figures 5.2.1-7 and 5.2.1-8	As mentioned by one of the authors of the report, in the legend, “Final” and “Distribution” should be combined into one.	corrected
73	Figures 5.3.2-2 through 5.3.2-5	It is not clear where on the plot “HIGH” and “LOW” (on the right-hand-side of the figure) represent. If “HIGH” and “LOW” are definite numbers, there should be tick marks at appropriate locations along the right-hand vertical axis.	
74	Chapter 5 Equations	Although authors have provided references for equations, it would help if the steps leading to equations such as 5.2.1-6, 5.2.1-8, 5.3.2-2, … are put in an appendix.	??? These are taken from published papers for the most part
75	Figures 5.3.2-20 through 5.3.2-25	The lines do not really fit the data.	
76	Chapter 5 Figure Captions	In general, the figure captions are not descriptive enough. It would help the reviewers if they are more explanatory.	Captions reviewed
77	Chapter 6	Pages in Chapter 6 are numbered incorrectly: PDF-page 301 is 6-1 and PDF-page 371 is also 6-1?	
78	Page 6-74 / Figures 6.4-7 through 6.4-16	The lines don’t fit data. Needs explanation.	
79	Chapter 8	The text identifies the highest hazard contributing source for each of the test sites. However, the hazard curves of individual seismic sources contributing to the total hazard at each test site appear to be weighted. For example, for the test site at Topeka the document identifies the MIDC_A as the most contributing source, and shows the hazard curves of all contributing sources in Figure 8.2.7A.	

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Comment No.	Page / Para. / Line	Comment / Proposed new text	Responsible Author
		Considering that the background sources are almost identical in these alternative models (MIDC_B, MIDC_C and MIDC_D) why are the hazard curves of these background sources vary by almost an order of magnitude?	
80	Page 8-5 / Para 2 / Line 1	"Figure A: Contribution to 10 Hz hazard by background source (e.g., see Figures 8.2-1A, 8.2-2A etc.)." Add, for clarity	
81	Page 8-5 / Para 3 / Line 1	"Figures B-D: Total hazard and contribution by background and RLME source for PGA, 10 Hz, and 1 Hz (e.g. see Figs 8.2-1B, 8.2-2B etc; 8.2-1C, 8.2-2C, etc., etc). For clarity add samples for Figures A, B, and C so reader better understands authors meaning	
82	Page 8-10 / Figure 8.2-1A	Add a list of the acronyms used in the legend. The acronym are apparently described in Chapter 7, the list needs to be close to the figure so that the reader can visualize/understand/be convinced that the authors and their code is capturing the effects of the various contributors to the seismic risk – a footnote to the figure might work. For clarity – the acronyms used in the figure's legend are not listed anywhere near the figure and it is very difficult/impossible to use/understand the content of the figure without a convenient legend. Where the acronyms explained earlier in the text, i.e., chapter 1 or 2?	
83	Page 8-12 / Figure 8.2-1D	the line colors in the legend are too close together, in their frequency for clarity of reading – symbol type should be used – dots, dashes, & combinations – for identification. For clarity – colors too similar.	
84	Chapter 9	Figure numbering is inconsistent with text.	
85	Page 9-51 / Figure 9.4-41	Why is Houston COV so different from others?	
86	Appendix E	It would be helpful to have some small simple tables in the different subsections to help summarize the extent and content of some of this material.	

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87	Appendix E	For some of the regional datasets a summarizing/concluding statement is clearly made within the data description section. It would be useful to have this sort of statement is consistently made for each and every regional area covered. Section 1.2.6.2 has an summary/concluding statement.	Added summary Table 1.2-1 and related text to report.
88	Appendix E / Figures	The figures associated with this appendix need work: • The NRC never received all the figures, E-26 through E-34, plus E-1 are missing. • There are no descriptive titles on the figures; therefore it is a nuisance to page back and forth between text, figure, and figure list. • The figures need more annotation on them so the reader can find what is needed to see about the figure. • The text and the figure referenced don't always match.	Summary statements added as needed.
89	Appendix E / § 1.2	It would be helpful to have a table of the 8 regions with the summary information so that the reader could see in a glance on one page how the database is set up from a conceptual perspective and gives an idea of types of data, quality of data and final interpretation about the region.	Figures and figure call-outs in text edited for completeness and accuracy. Captions and figure numbers added to figures.
90	Appendix E / § 1.2.2.1	2nd Paragraph has a sentence citing GPR data that images feeder dikes beneath sand blows. The figure cited is GIS maps and not GPR images. Cite the GIS map elsewhere and substitute and actual image of feeder dikes beneath sand blows.	Created summary Table 1.2-1 and added related text to report.
91	Appendix E / § 1.2.2.1	In the description of the Marianna area a NW lineament is sort of dropped into the discussion without any further information. Please explain/define the nature and current state of knowledge about the NW lineament. Is an explicit correlation with the pale liquefaction features intended?	Added figure of trench log and GPR image and referenced in 2nd paragraph;

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			also referenced GIS maps elsewhere in text.
92	Appendix E / § 1.2.6.2	Recalibration of published dates may require a more formal and expanded treatment in this appendix. Did Talwani and Schaeffer have any input into this recalibration? It would be informative to provide a simple table of comparison dates.	Referred to Daytona Beach lineament in first paragraph. Text revised to add information about the lineament, its relation to sand blows in the area, and the interpretation that it is the surface expression of an active fault.
93	Appendix E / § 1.2.8	It is not clear if the Saguenay earthquake liquefaction features are included in the Charlevoix SZ or are they treated as separate regions?	Subsection 6.1.2 of main report provides additional discussion of recalibration, including figures. Recalibrated dates provided in database.

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			Recalibration was performed using data in Talwani and Schaeffer (2001) without formal input from Drs. Talwani and Schaeffer.
94	Appendix E / § 2.1.2	It is difficult to keep track and understand the various techniques and case histories in this section. Perhaps simple table to list techniques, uncertainties, other qualifiers and significant pubs that illustrate case history would help the reader understand the scope of this section.	Yes, Saguenay earthquake liquefaction features are included in the Charlevoix SZ. Clarification added to Section 1.2.8.2. and to Table 1.2.
95	Appendix E / § 2.1.3.6	Provide an explanation in text for how the terms soil structure, consistence and reaction are used.	Created a table summarizing applicable time period and precision of each dating technique as well as CEUS regions applied and key reference

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			selected for each region.
96	Appendix E / § 2.2.1	The 2 figures cited in this section are not described in the text adequately nor are the figures annotated. Describe and annotate the figures.	Deleted term 'reaction' from text and glossary, added 'soil structure' and 'consistence' to glossary.
97	Appendix E / § 2.2.4.1	Provide more explanation of Figure E-62. Also, explain that comparative studies of paleoliquefaction features with New Madrid and Charleston earthquakes are used to estimate earthquake magnitude for the paleo earthquake, yet it is stated that the uncertainty is related to the estimate of the Paleo earthquake magnitude is no better than for the historic earthquake (usually .25 and .5 to 1 magnitude unit respectively). Clarify this statement and also explain why in the previous paragraph the uncertainty of 1 mag unit on the paleo earthquake is provided.	Additional annotation added to figures and explanation added to captions and report text. Reader is also referred to Section 6.1.5.4 of the main report.
98	Appendix H	While logic trees state "Mesozoic Extended" and "non-extended" boundary, the text in a few places says "Mesozoic-Extended" and "non-Mesozoic Extended". It is confusing to know if there were actually models that dealt with extensional regions outside the Mesozoic extension. In one place, there is a Paleozoic extension source, but overall it seemed as if non-extended and non-Mesozoic-extended are used synonymously. This may again be as a problem of not having a good intro to model geometries in the introduction section.	Text revised for consistency with main report
99	Appendix H	M* is used for magnitude definitions. Is it different from M? How?	M* no longer used, reference

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			made to section 3 and

USGS Comments and Responses

Comment	Summary of Revisions to Report
General comment: Many of the references cited in the text are not listed in the References of chapter 10.	Additional references have been added.
Executive Summary The executive summary provides a basic description of the master logic tree, RLME and distributed seismicity sources applied in the analysis, and a description of the results. One point that should be discussed is why this model differs significantly from previous models including the USGS and COLA models. This is important in gaining acceptance of the document. Pages viii-ix of the Executive Summary contain lists of source zones that do not match the lists in the Table of Contents and text. Page ix last sentence on page states that if an alternative assumption or parameter is used in a seismic hazard study and it potentially changes the hazard by less than 25% from ground motions with hazards in the range 10-4 to 10-6 , it is within the level of precision with which one can calculate seismic hazard. In the Chapter 9 comments we discuss how the typical differences in ground motions between the CUES SSC and the COLA and USGS models are much larger than 25% so we are not sure if this result is reasonable. It may be the case if the same users developed the model using the same parameters and methods a second time that they would have this level of precision.	Additional explanation added to explain the differences in the projects. List of sources has been revised. Chapter 9 makes it clear that the level of precision relates to the hypothetical case where the hazard analysis is conducted at the same point in time by two different groups.
Acronyms SHmax is defined as “maximum horizontal shortening”. In contrast, the Zobacks and others use SHmax to mean maximum horizontal stress, compression, or principal stress, with no implication of shortening or other deformation. Until horizontal pressure solution, seismic or aseismic faulting, or other deformation occurs, the crust merely supports the SHmax. Has SHmax been used anywhere to mean deformation instead of stress?	Definition revised
Chapter 1 – Introduction The Introduction provides a clear description of the purpose of the document, the approach used in developing a SSHAC level 3 analysis, and a description of how the TI team determined that they were accomplishing the goals of obtaining the center, body, and range of the informed technical community. 1.1.5 p 1-4, paragraph 1: Extra comma after “facilities”? 1.1.5 p 1-4, paragraph 1: You state that the national seismic-hazard maps (NSHM) focus on annual frequencies of exceedance (AFE) of 10^{-2} to 10^{-3}. The documentation of the 2008 NSHM points out that the NSHM include maps with AFE as low as $4x10^{-4}$, and 35 of the 53 map sheets show this AFE (http://earthquake.usgs.gov/hazards/products/conterminous/2008/maps/). Thus, AFEs of the NSHM approach the upper limit of the SSC AFEs that are intended for design purposes. 1.2.2 p 1-5: The Section heading doesn’t look right? 1.4.1 p 1-8, paragraph 3: “E” should be italicized in “RLME”? 1.4.4.5 p 1-11, paragraph 4: Missing word or words in first sentence?	Revisions made to correct items noted.
Chapter 2 – SSHAC Level 3 Process And Implementation Chapter 2 provides a necessary discussion of the meetings and goals of the project. 2.1.1.2 p 2-11, paragraph 2: Unclear phrasing in the second half of the crucial second sentence. The difference between PEGASOS results and the older results were shown to be caused by “an appropriate treatment of the ground motion aleatory variability and an error in the calculations in the previous hazard study ….” Was the treatment appropriate in the older study or in PEGASOS? 2.1.2.1 p 2-17, paragraph 1: Extra “like” in last sentence?	Revisions have been made to respond to comments, or the sections no longer exist.

Comment	Summary of Revisions to Report
Table 2-2 Names duplicated, misformatted, etc.: Al-Shukri, Ravat, Baldwin, Mueller?	
Chapter 3 – Earthquake Catalog The earthquake catalog is one of the most important components of the hazard analysis for the CEUS. The CEUS SSC catalog is a major improvement over previous catalogs in that it incorporates more regional catalogs and has developed moment magnitude estimates. This catalog is one of the most important contributions of the CEUS SSC project to the hazard community. The USGS reviewed the catalog and made several suggested changes. However, it is difficult to assess whether or not these changes were implemented in the final catalog. 3.1.2 p 3-2, paragraph 3: EPRI (1988) missing in References (Chapter 10)? Is this reference accessible to most readers? 3.1.3 p 3-3, paragraph 1: Should be “an important”, not “and important”? Should be “seismologists”, not “seismologist”? 3.2.1 p 3-3, paragraph 3: Should be “Mueller”, not “Muller” 3.2.1 p 3-3, paragraph 4: The list of USGS source catalogs is not correct. For CEUS the USGS uses: NCEER91; Sanford’s New Mexico catalog; Stover & Coffman; Stover, Reagor & Algermissen (state-by state catalogs); PDE; and DNAG. If the first sentence of the paragraph is rewritten accordingly, then should sources like SUSN, ISC, and ANSS be added to the list of regional catalogs in Section 3.2.2? If you are still using ISC as a source catalog, has it been filtered to avoid the problems discussed in the previous USGS review? The next sentence is confusing: Do you mean the primary USGS purpose, or the primary purpose of including these as separate sources in CEUS-SSC even though they had already been included by USGS? (I’m guessing the latter, but it’s not clear.) The CEUS-SSC catalog-construction scheme is described here relative to what USGS did, and this seems to be causing some confusion. A more straightforward description/separation might be clearer. 3.2.2 p 3-4, paragraph 1: Should be “for each earthquake” or “for earthquakes” in last sentence? 3.2.3 p 3-4, paragraph 2: “Boatwright” misspelled? “McCulloch” misspelled? 3.2.4 p 3-4, paragraph 4: Provide reference (web?) for the NEIC Mining Catalog? Should be “Section 3.2.3”, not “3.2.4”? 3.3.1.1 p 3-5, paragraph 5: Clarify the location of the added blue square? Is it +0.2 mag units? 3.3.1.3 p 3-6, paragraph 2: Should be “a coda”, not “an coda”? 3.3.1.3 p 3-6, paragraph 2: Should be “albeit”, not “abet”? 3.3.2.2 p 3-7, paragraph 2: Should be “reasonably”, not “reasonable”? 3.3.3.1 p 3-7: Provide a definition/description of M_N? 3.3.3.1 p 3-7, paragraph 5: should be “particular”, not “particulars” 3.3.3.1 p 3-7, paragraph 5: I think “sources” means catalogs, but the text after eqn 3.3.3-1 suggests that earthquake location also matters. So for an earthquake in southern Canada with Canadian-network M_N, both Z_{CAN} = 1 and Z_{NE} =1, right? Clarify this? 3.3.3.2 p 3-8: Provide a definition/description of m_{bLg}? Should m_{bLg} be included in the acronym table? 3.3.3.3 p 3-8: Provide a definition/description of m_b? The text & eqn refer to m_{bLg}? Ordinate label is cropped in Fig 3.3.3-3? 3.3.3.4 p 3-8: Provide a definition/description of M_L? Discuss truncation level (Fig 3.3.3-4)? Ordinate label is cropped in Fig 3.3.3-4? 3.3.3.1–3.3.3.4: Simple offsets between **M** and M_N (Fig 3.3.3-1) and **M** and m_{bLg} (Fig 3.3.3-2) do not fit the smaller-magnitude data (~ magnitude 3) very well. M_N, m_{bLg}, m_b, and M_L will begin to saturate somewhere in the magnitude 6–7 range, and will fall below **M** at greater magnitudes; the simple offsets do not account for this. 3.3.3.5 p 3-9: Provide a definition/description of M_S? The text refers to “local magnitude M_L”? Discuss meaning of very small M_S? Ordinate label is cropped in Fig 3.3.3-5?	Discussions of response to review comments included Typos corrected Sources of USGS corrected, Discussion of ISC problems included, Confusing sentence corrected (it is our purpose) Section 3.3 rewritten, incorporating comments as appropriate The simple offset model is used in the magnitude range where conversions are occurring. At large magnitudes where issues of saturation would be important, the values of **M** are typically coming from special studies

Comment	Summary of Revisions to Report
3.3.3.6 p 3-9: Provide a definition/description of M_C? Discuss truncation level (Fig 3.3.3-6)? Ordinate label is cropped in Fig 3.3.3-6? 3.3.3.7 p 3-9: Should be subscript "D" in heading? Provide a definition/description of M_D? Discuss truncation level (Fig 3.3.3-7)? Ordinate label is cropped in Fig 3.3.3-7? 3.3.3.8 p 3-9: Should be subscript "U" in heading? 3.3.4 p 3-10, paragraph 1: Should be "Sections", not "Section s"? 3.3.4 p 3-10: I suspect that the derivations and applications of the different magnitude measures **M** and **M*** will be confusing to many readers. The discussion in old EPRI is pretty arcane, and many readers will not have access to it anyway. I wonder if the discussion here should be expanded, maybe with an example? How should these magnitudes be used? 3.3.4 p 3-10: How was the sigma "nominal value of 0.1" for instrumental **M** determined? What is the behavior of $sigma_{E[M\|X]}$? Is $sigma_M$ in eqn 3.3.4-4 and eqn 3.3.4-5 and Appendix B the same thing as $sigma_P$ in eqn 3.3.4-3? 3.4 p 3-11: The declustering analysis is interesting. About the EPRI method, you state: "If the rate of earthquakes is significantly higher than the background rate …, then earthquakes are removed until the rate becomes consistent with the background rate." Does this mean that a few earthquakes that would be clearly declared as aftershocks by, say, Gardner & Knopoff remain in the final catalog in order to match the background rate? In other words, is the declustered catalog not strictly a catalog of mainshocks (in, say, the G&K sense)? My recollection is that G&K try to envelope the southern California cluster durations and dimensions, not match averages; comparisons to the EPRI CEUS cluster durations look OK to me, but the G&K time windows look too small (even when multiplied by 1.5). 3.4 p 3-11, paragraph 5: Should be "European earthquakes", not "European earthquake"? 3.4 p 3-11, paragraph 5: Should be "more than the average", not "more the average"? 3.5 p 3-12: Again, the completeness analysis relies on the reader having knowledge of the old EPRI study. What is the rationale for the EPRI completeness regionalization: geology, demographics? The latitude-longitude-bound (blocky) zonation does a poor job of defining some demographic or natural boundaries (for example, along the Atlantic margin). 3.5 p 3-12, paragraph 7: Should be "2009", not "2008"? 3.5 p 3-13: How should TE be used?	The issue of M* is explained in detail Source of uncertainty in reported M explained Expanded explanation of declustering along with comparison with USGS implementation of Gardner Knopoff Expanded explanation of completeness and use of TE
Chapter 4: Conceptual SSC framework Chapter 4 discusses the three attributes that are needed for a conceptual SSC framework, a systematic approach to treat alternatives for spatial distributions of seismicity and seismic sources, an approach to identify applicable data for source models, and a methodology for identifying seismic sources. The master logic tree shows the weights of the major branches for the Mmax and seismotectonic zones. The USGS review team did not understand the impact of the spatial smoothing in the two models on the resultant hazard estimates. In Part 2 there is a more complete explanation of this question. 4.1: NEEDS FOR A CONCEPTUAL SSC FRAMEWORK (1) Concerning section 4.1, the USGS and SSC utilize logic trees to greatly different degrees, and this difference may not be reducible. The appropriate standard of practice for the SSC is the SSHAC procedures, which require inclusion of the range of credible alternatives, which in turn forces the extensive use of logic trees. In contrast, the standard of practice for scientists can be summarized as striving toward using reproducible evidence and its implications to select the one best answer. The two approaches overlap, but they differ fundamentally in focus. Additionally, seismologists' understanding of the geological controls on the physics of rupture nucleation and propagation in the CEUS is rudimentary. The different standards and poor understanding lead USGS scientists to reject most branches of most logic trees, which lead to a few small trees. The USGS does not reject the concept of logic trees entirely. This is evidenced by our summing of weighted hazard from three different choices of lower magnitude	Regarding 4.1 (1): It is true that the goal of the SSHAC process is to develop models that incorporate knowledge and uncertainties (i.e., they represent the center, body, and range of technically defensible interpretations). This means that full logic trees are required. Comparisons with the USGS approach are so noted, but this report does not deal with the differences with the USGS methodology. 4.4 (2): Additional text and discussion is added to make it clear that RLME sources are present as independent sources for all source alternatives; the Mmax zones and the seismotectonic zones are alternative seismic source models for subdividing the distributed seismicity; the logic tree provides the weights for these two alternatives and the justification for the weights. (3) Figures have been revised for consistency.

Comment	Summary of Revisions to Report
limits, and by our representations of epistemic uncertainty in Mmax and in the sizes of large earthquakes at Charleston and New Madrid. However, we should not expect the SSC and USGS to agree much on the preferable number and sizes of logic trees or on some of the conclusions that are based on them. 4.4: MASTER LOGIC TREE (2) Figure 4.4.1–1 appears to be incomplete. The figure and its discussion need revision and expansion to clarify the following matters. The new material would be most effective if it were near the beginning of section 4.4.1, with details later as they are now. First, apparently there are four kinds of zones: RLME sources, seismotectonic zones, Mmax zones, and distributed seismicity zones. The last two are not clearly defined and distinguished from the others. Citations of maps showing the different kinds of zones would help greatly. Second, Figure 4.4.1–1 shows two kinds of conceptual approaches, Mmax zones and seismotectonic zones. There are two source groups, or kinds of Mmax zones: distributed historical seismicity zones (dhs hereafter) and RLME source zones. What is a "source group"? The dhs zones are used as background source zones to the RLME source zones (p. viii). Do the dhs zones exist everywhere including within the RLME source zones, so that the two hazards are summed? Additionally, the label "Mmax zones" implies that both source groups deals primarily with Mmax. If so, then it would be helpful if the link between Mmax and each source group were stated. Third, there are also two source groups of seismotectonic zones: seismotectonic zones and RLME zones. If a conceptual approach has the same name as a source group, each mention of either is likely to be confusing and potentially misleading. The dhs zones "exist in different forms depending on whether the Mmax zones or the seismotectonic branches of the logic tree are being followed" (p. 5–6). However, Figure 4.4.1–1 does not show a dhs source group for the seismotectonic conceptual approach. The quoted sentence implies that the two forms in which the dhs zones exist are similar enough that they should have similar but not identical names in the figure and throughout the text. (3) The captions of Figures 4.4.1.2–2, 6.2–1 and 6.2–2 use "wide" where "narrow' should be, and vice versa. It is advisable to check usage of these two adjectives throughout the report. Usage of Mmax could be similarly checked to make sure that the zone name cannot be confused with the largest-magnitude sense of the term. Finally, different versions of some of the zone names are used in different parts of the text. Examples are in the Executive Summary; there may be other examples elsewhere.	
Chapter 5 : SSC Model: Overview and Methodology Chapter 5 provides a description of the spatial and temporal distribution of earthquakes in the CEUS. The USGS team appreciated the efforts to systematically assess the reasonable maximum magnitudes using the updated SCR catalog for each superdomain and generating a catalog by Monte Carlo simulation. The USGS review team was not clear on details of how the prior and likelihood distributions were constructed. The prior distributions are based on the superdomains and assume that the Mmax distribution is spatially variable. The likelihood distributions assume the largest earthquake in the catalog but do not consider the historical earthquakes (e.g., Charleston). By neglecting the RLME type sources it seems that the model assumes that all RLME sources have been identified. The Mmax distribution seems reasonable if there is a valid reason to throw out the large historic earthquakes.	The large earthquakes are considered RLMEs should not be included in the likelihood because they are modeled by the RLME sources. The Mmax distributions in all cases extend into the magnitude range of the RLME magnitudes and larger
The penalized maximum –likelihood method for smoothing seismicity also seems like a reasonable model as far as we can understand it. However, we do not understand how this method results in such high rates of earthquake exceedances compared to the USGS and COLA models (as described in Chapter 8 commentary). We do not understand all of the details in the penalized maximum approach so it is difficult to assess this methodology. Implementation of the penalized maximum-likelihood approach would be difficult for most of the user community, therefore, we feel that if this approach is maintained in the model the	The method has been refined and made more robust by using magnitude bins instead of continuous magnitudes and the description of the method has been expanded to improve clarity. The two basis elements are the formulation of a likelihood function at the cell level (similar to Weichert's formulation) and the introduction of smoothness between adjacent cells.

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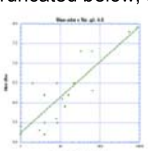
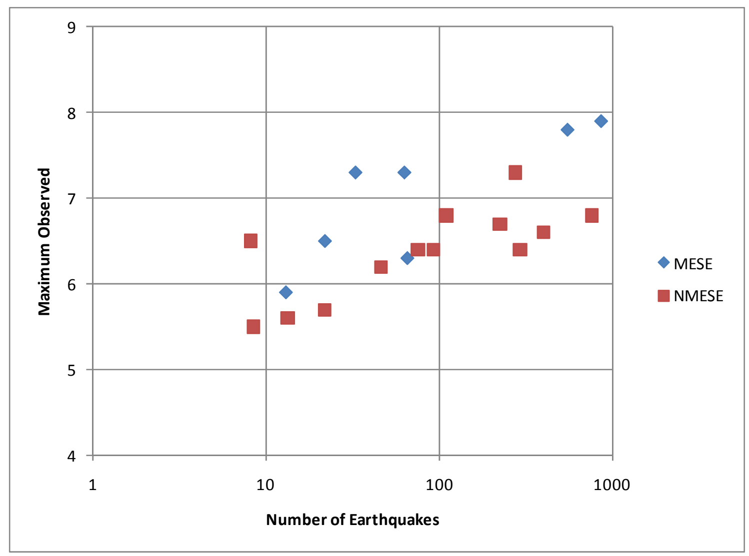
software to calculate the smoothed seismicity should be made available so that further comparisons could be made.	
We applaud the decision not to do time dependence for Central and Eastern sources. While time dependence almost surely characterizes single isolated fault sources, the sources in places like New Madrid and Charleston, and likely other places, are multiple, and if independent, as a group the occurrences would appear Poissonian. If dependent (or "contagious") clustering is rather more likely. We also appreciate the use of likelihood functions for rates, as it more clearly expresses the recurrence rate uncertainties, especially for large events, say, in New Madrid. Discretization of rates is also helpful, inasmuch as rates carry through proportionately in exceedances. For other continuous parameters we expect that Latin Squares for more finally divided discretization is more suitable for exploring hazard uncertainties. We think it helpful to recognize in 5.3.2.4 that more realistic estimates of the uncertainty would be obtained by using bootstrapping from synthetic catalogs derived from alternatives models (even though this was not done).	Tests indicate that the use of eight realizations obtained using the eigenvalue decomposition and Latin Hypercubes captures the uncertainty in hazard. In particular, the 24 realizations from cases A, B, and E capture the mean, median, and 85% hazard curves. We agree that bootstrapping is also a viable approach, but it may require many alternative maps, making it unpractical for PSHA. One possible approach would be to use the bootstrap results to compute the covariance matrix of the recurrence parameters and then use our eigenvalue decomposition plus latin hypercubes to generate eight or so alternative maps
Maximum Magnitude Observed maximum magnitude data should be expected to be strongly rate dependent. The larger the sample derived from a distribution, the larger should be the fractile of the maximum of the sample. A low maximum magnitude from a source with low rates should not be a candidate for a prior to be applied to a source with high rates. As an example, using the data from Table 5.2.1-1 for EC type superdomains, figure A shows a plot of observed maximum magnitude against number of events above magnitude M. There is a clear dependence of maximum magnitude on observed number. To correct for this dependence by the adjustment procedure proposed using equation 5.2.1-2 and illustrated in figures 5.2.1-3, and 4 is certainly reasonable. Thus, of the points in figure A, observed maximum magnitudes with low numbers of events above 4.5, either transform to values in the high 5, low 6 range or give no evidence at all. This suggests in general, that priors for maximum magnitude should have minima in the vicinity of magnitude 6. The examples given in figure 5.2.1-7, and 8 display this desirable quality. (On the other hand, the priors of figures 5.2.1-1 and 2, therefore should be hypothetical and not a typical example.) The two-priors model of section 5.2.1.1.1 seems to us to reflect rather strongly the difference on the rate of earthquake occurrence of the two regimes, even with bias correction. Because prior distributions should not be lower than largest we can expect with no known observed structures, we believe that minima of these distributions should give very low probabilities that true maxima can be lower than 5.8, and should give credible (though low) probabilities for the exceedance of 7.9. The model prior with the lower mean gives around 15 percent probability to the former and 0.003 to the latter. The composite-prior model, on the other hand, seems to us to satisfy our criterion, and be more correct for philosophical reasons, as well. In practice, we notice an unfortunate consequence of the normal model for the prior. There is always some probability of a maximum magnitude in the tails, no matter how extreme. As an example of what happens, figures 7.4.2-2, and 3 show a characteristic double maximum in the posterior. This is coming from the interaction of a strongly spiked likelihood function with the small-magnitude tail of the prior at magnitudes below 5.8. If the lower tails of the priors were truncated below, say, 5.6, this bimodal behavior could not occur.  Figure A. Max-obs versus logarithm of the number of events greater than 4.5 from Table 5.2.1-1 for EC-labeled superdomains. Note the strong correlation.	In an attempt to account for very low seismicity rates was used by eliminating super domains made up of a single domain or having little earthquake data. The data set used shows a much weaker trend with number, which is heavily influenced by two MESE super domains  The updated priors include a lower tail truncation at 5.5. This along with the updated priors has led to more unimodal posteriors

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Concluding General Remarks on Maximum Magnitude Concerning section 5.2, there is a fundamental disagreement about Mmax between the USGS, on the one hand, and EPRI-SOG and SSC on the other. EPRI-SOG made a fundamental assumption that Mmax varies spatially in SCRs. SSC adopts the assumption and implements it with a Bayesian analysis. USGS cannot see any justification for the assumption. However, we also cannot disprove it. Accordingly, USGS sidesteps the matter and makes what it can of the sparse data contained in the global catalog of SCR earthquakes. Accordingly, SSC uses a more elaborate and mathematical methodology than does USGS, although not necessarily a more objective or reproducible one. EPRI-SOG's Mmax values tended to be significantly lower than the USGS values. SSC values appear to be closer to the USGS values. So far, no one proved or disproved either approach. We see no reason to expect that this fundamental disagreement will be resolved any time soon.	NAR
Spatially varying b-values. Inasmuch as b values are almost as dependent on sample size as maximum magnitude values are, and because maximum likelihood b-values are also dependent on the maximum magnitude observed, we doubt that b-values should be allowed to vary spatially in most of the CEUS. In figures 5.3.2-16 and 17, we find that the patterns of changing b-values over the range 0.74 to 0.85 seems to reflect the methodology's allowance of variation of a-value or variation of observed maximum magnitude to affect the b-value determination (notably, in New England, oppositely between the two figures. We are not convinced that a statistical distinction within this small range can be justified by the relatively small number of historical events available.	We disagree regarding the first statement. The distribution of Mmax depends on the highest magnitude observed, while the estimate of b depends on the average magnitude observed (Eq. 5.3.2-5), making it less dependent on the maximum (unless there is only one earthquake). Regarding the issue of constant vs. variable b, the TI team felt that, given the large size of some of these source zones, it was preferable not to adopt a constant b as an a-priori assumption. In the revised results, the objective-smoothing approach arrived at maps with a mild spatial variation in b (except in SLR).
Predictive spatial patterns from observed seismicity. Previous work has shown that alternative subjective interpretation of source zones is a major source of hazard map variability from expert to expert. We commend the use of more objective kernel smoothing methods as a way of setting bounds on likely variability. On the other hand, it is well known that randomly located points in areas show apparent spatial clustering analogous to temporal clustering in Poisson time processes. A predictive procedure that adapts to show detail at the scale of what may be random clustering is ill-advised, unless that clustering is also associated with geologic or seismogenic structure. To us, the rate variations shown in figures 5.3.2-16, 17 and 18 all appear overly detailed. Insufficiently broad spatial smoothing may account for the unanimity seen in alternative recurrence estimates seen in figure 5.3.2-20 through 26, for instance, which we find very unlikely. For this reason, we continue to prefer using multiple-scale constant kernel smoothers with what are apparently larger scale than those of the SSC methodology. On the other hand, the range in values obtained for Minnesota, with reduced M weights (Figure 5.3.2-29 seem to reflect likely uncertainties where there is so little historical information.	We have the following two counter-arguments. (1) We performed calculations for a few source zones using a kernel approach with objectively selected adaptive kernel size (using a completely different objective approach). The resulting map is presented in Section 5.3.2.4. Visual comparisons indicate a very good agreement, except in regions of very low seismicity, where Gaussian kernel approaches are known to be problematic. (2) We performed a test in which a synthetic catalog was generated, under the assumption of a spatially homogeneous rate, a b value of 1, Poisson occurrences, and independent earthquake locations. The rate and duration were selected so that the mean number of earthquakes per cell was comparable to that of the Midcontinent source zone (approximately one event for every three cells). Calculations were performed for one rectangular source zone of dimensions 5 degrees by 5 degrees (containing 100 half-degree cells), using objective smoothing, and using unit weights for all magnitude bins (similar to case A). The methodology produced homogeneous rates and b values, and these values were consistent with the values used to generate the synthetic catalog.
Recurrence determinations Figures 5.3.2-20-23 worry us. Here rates are fit, by maximum likelihood, to data from magnitudes less than 3.75, presumably adjusted for incompleteness. Any uncertainty in the completeness correction produces additional uncertainty in already uncertain historical recurrence rates (and magnitudes). Maximum likelihood fitting strongly depends on the largest	This issue has been addressed in the revised methodology.

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rates observed, and these must be the lowest magnitude. Reducing the M weights mitigates some of this, but we would think that predicted rates which are double the observed rates at magnitude 4 should be considered disqualified. Later in the report are shown hazard curves at some locations which are double the exceedance probability of those both of the USGS and of the [COLA] models. Is there perhaps some maximum likelihood model that penalizes a fit that falls outside the error bounds of the observed data?	
Section 5.1.2. The sentence,, "With time and study the earthquake research community in the WUS has developed information that suggests that the assumption of an exponential distribution of magnitudes may not be appropriate for individual faults." is probably the basis for considering only characteristic sources for the RLMEs, rather than considering branching which includes the truncated exponential distribution with Mmin in the neighborhood of M6.5 and Mmax at Mchar. The exclusion of models having truncated exponential source frequencies on faults such as the Cheraw fault in southeast Colorado has a strong effect on hazard. The above view is embodied in the California A-fault model, on which just big events are believed to occur. However, for Quaternary faults in all the rest of the western US, and for b-faults in California, the NSHMP hazard model continues to give the GR branches 1/3 to 1/2 weight, that is, weight on par with the characteristic source model. It looks like the current SSC has weighed in on identifying all faults in the CEUS as A-faults, in the California sense, even though not much is known about CEUS faults compared to California A-faults. It is not clear that the methodology is considering the possibility that some CEUS faults may be more like b-faults, where small M events may be somewhat more likely than large M events. The effect on hazard is explored in the Harmsen review of the new EPRI Cheraw RLME, for example. When not much is known about seismogenesis on specific faults in the CEUS, it is hard to believe that smaller sources simply cannot occur, or can only occur at frequencies that make them immaterial to the seismic hazard when compared to larger events. The use of a truncated GR distribution for Mmax and distributed zone seismicity where essentially nothing is known may also be considered questionable. Such an assumption essentially denies the possibility that other RLME source zones may exist, as yet undiscovered. The turning on and off of known RLME source zones should give the authors pause, that is, other zones may exist but may be at this moment in an out-of-cluster mode. Great benefit to society would be accrued by discovering such a zone, if such a discovery included proof that it will turn on in the next 50 years. The promulgation of models that essentially deny the existence of such zones may discourage the search for as yet undiscovered RLMEs.	The text makes it clear that all RLME sources are treated in the same manner whether they are faults (e.g., Meers) or local zones (e.g., Charleston). The magnitude of the RLME is assessed with its uncertainty from various lines of evidence and the associated recurrence data are used to represent the recurrence rate of those earthquakes. There is no size-distribution model (e.g., characteristic, exponential) assumed for the RLME sources. Each RLME sits within a seismic source zone and the recurrence rates for those source zones are derived from the observed seismicity, thus resulting in an exponential distribution of magnitudes. As noted in the text, the RLME recurrence is independent of and adds to the recurrence rate from observed seismicity. As noted in the text, there is no assumption that all RLME sources have been identified and included in the SSC model. This is easily seen by comparing the broad distributions of Mmax for the seismic source zones and comparing with the magnitudes assessed for the RLME sources. Essentially any of the source zones has magnitudes in its Mmax distribution that are as large as the RLME magnitudes. Clearly, this does not mean that such magnitudes are likely to occur, but they are not precluded.
Section 5.2.1. The second paragraph on p. 5–7 notes that the global SCR catalog contains earthquakes that occurred in RLME sources, whereas the Bayesian method is being applied only to CEUS sources that are not RLME sources. The suggestion is made that using global RLME sources might lead to overestimation of Mmax in the CEUS non-RLME sources. Why would Mmax be potentially overestimated? Again concerning the second paragraph, RLME sources lack recognized geologic records of large cumulative Quaterary deformation. This implies that probably the RLME sources have not generated RLMEs throughout geologically long periods of time. Absent a clear preference for RLMEs to be in specific geologic settings, the likelihood that presently active RLMEs have geologically short lifetimes suggests that RLME sources turn on and off at unknown intervals. The SSC report lists ten RLME sources, two of which are in parts of the craton that are sparsely seismically active to aseismic. Five of the ten RLME sources were not known 5-10 years ago. Thus, it seems likely that more RLME sources exist in the CEUS but are presently inactive or undiscovered. It is seems likely that RLMEs can occur anywhere within the CEUS. Furthermore, prehistoric and historical surface ruptures are abundant and weakly clustered spatially in much of the Australian SCR. The diffuse clusters of Australian scarps could be regarded as RLME sources. This reasoning suggests that RLME sources worldwide may be	

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appropriate estimators of the value of Mmax, but not its frequency, in the CEUS outside RLMEs.	
Section 5.2.1.1. In the SSC Bayesian estimation of Mmax, a bias correction deals quantitatively with the likelihood that Mmax exceeds the largest observed magnitude. We are not aware of any published test of the bias correction. The choice of a test is not straightforward because the bias correction estimates the median of the range of Mmax instead of a single value. This difficulty does not reduce the need for a test to increase confidence in the bias correction. An interesting example test could be presented, say for the Wabash area, where there is a limited amount of historical seismicity, but some paleoestimates of large magnitudes.	A test of the Wabash area is subject to a small sample size. The observation is 14 earthquakes with **M** ≥ 4.5 and a max_obs of 5.52. The bias correction based on the median would be 5.8, smaller than the estimated paleomagnitudes of 6.2 to 6.3. This one test does not match perfectly. However, if one considered probability levels near the median (say in the range of the 30^{th}% to 70^{th} %) then one could get a bias correction to reach 6.2 from the observed of 5.5. The bias correction is intended to work on average, not for every case.
Section 5.2.1.1. Pages 5–9 summarize the pooling of domains into superdomains according to shared values of geologic and seismological variables. C.A. Cornell, in proposing the pooling, listed eight variables that might be used, including those used in pages 5–9. None of Cornell's variables were justified or explained. Of the variables, only crustal type and crustal age have been observed to be associated spatially with SCR earthquakes of M7.0 or larger worldwide. What evidence supports use of any of the other variables? Statistical tests are unlikely to help because there are 246 ways of choosing two or more of the eight variables and 154 ways of choosing two to four of the variables. Given this many possibilities, it is probable that chance alone would cause a statistical test to identify spuriously several of the possible choices as significant.	That is why we limited the variables to just age, stress, and favorable orientation and did not use all of the other variables originally tested in Johnston et al (1994)
Sections 5.2.1.1. Pages 5–10 through 5–12 present a multi-step process for identifying variables that are associated from step to step with an increasing ability to distinguish groups of superdomains according to their mean values of maximum observed magnitude. It is not clear from the text whether the analysis was done in an exploratory sense, without formal testing of hypotheses, or in the standard statistical way of formulating a testable hypothesis and the corresponding null hypothesis. If it were done in an exploratory sense, then probably it would be valid to use the p-values as descriptors, although some statisticians might disagree. However, tests of hypotheses and conclusions about significance would be invalid. In contrast, if the analysis were done in the usual way, then the hypotheses to be tested would have to be determined before examining the data. Otherwise the tests of significance would be invalid. Which was done should be stated clearly at the outset to avoid misinterpretation and confusion about validity. Additionally, most of the p-values are not stated. Providing them would help the reader follow the argument. Other matters also could be stated more clearly. (a) In standard hypothesis testing, is it necessary to select a significance value, such as 0.05, before performing the test? (b) Was the null hypothesis rejected or not? (c) Other tests can give the probability that the alternative hypothesis is true; what were their results? (d) Some of the samples were so small that the t-test might not have had much power to detect differences that exist; then the true p-value may have been smaller than 0.14, which would weaken the conclusion. Did a test less sensitive to sample size give similar results to the t-test? (e) For small samples the t-test is sensitive to non-normal distributions. Did another test less sensitive to small sample size give similar results to the t-test? The analysis involves a family of similar statistical tests. The similarity creates the problem of multiple comparisons, by which a single test with a significance level of 0.05, repeated n times in similar fashion, increases the probability of at least one spuriously significant result to approximately n(0.05). Therefore, to avoid one or more spuriously significant results, the significance level must be reduced. Determination of the exact amount of reduction is a complex problem, but the simplifying Bonferroni inequality suggests reduction to about 0.5/n, or in this case about 0.01. The reduction further weakens the interpretation of the p-value of 0.14. The other p-values in the analysis are not presented; were any p-values less than about 0.01 obtained at the end of the analysis?	The process used to select the priors is explained in greater detail. It was not performed in a formal manner, but as an exploratory method. The expectation was that extension would be important, but the data do not strongly favor it. That is why there is significant weight given to the model that it does not matter. In the end, the results of the t-tests were only used to suggest what might be the best separation of the SCR regions. It was not used formally to assign a weight to two versus one prior models

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Section 5.2.1.1.5. This section is insufficiently clear. It can be interpreted as saying that instead of using only the historical maximum magnitude, a distribution of maximum magnitudes is calculated by simulating 100,000 sets of magnitudes and taking the maxima, binning these to make a distribution, and then applying the bias correction procedures and developing a likelihood function for each 0.1-unit magnitude in the distribution, and getting a posterior for each, the posterior being weighted in accordance with the distribution. It seems to say, then, that observed maximum magnitude no longer plays a role, and it implies that the observed number of historical events alone controls the maximum magnitude distribution. Is this correct? Please make the section more specific.	The process is only incorporating the uncertainty in estimating the size of the historical earthquakes. Additional explanation added to the section
Section 5.2.1.2. We applaud the effort to compare the Kijko with the Bayesian approach. The result seems to be that we can have confidence in the method when it more or less agrees with the Bayesian. Although a rule is given for assigning 0 weight to the Kijko approach, it seems that, in general, one would never be secure in adopting this approach when the number of observed events is moderate, without also attempting a Bayesian approach as a check. This suggests that a Bayesian approach should always be preferred. Is this a fair assessment? From your experience is it possible to provide other rules of thumb for using/not using the Kijko method?	We do not have other rules. The concept we have implemented seems to represent Kijko's own statement about the confidence to be applied to estimates given his method
Section 5.3.2.1.2. It is not completely clear to us from the *details* of how the MCMC procedure described here is related to the usual uses of MCMC, namely sampling from posteriors of distributions whose joint discription would be too complex, or exploring the joint distribution of parameters compatible with some objective criterion, like minimal square error of predicted vs observed data, as in Bayesian inversion. Please provide an *overview* of the procedure.	The first example given by the reviewer is exactly the situation here: the joint penalized likelihood function for nu and beta at all cells, after suitable normalization, may be viewed as the joint distribution from which we want to generate multiple realizations using MCMC. We have revised the text to make this more clear.
Section 5.3.2.3.3. Typo. Page 5-33, first paragraph, line 4, Figure 5.2.3-16 should be 5.3.2-16.	Typo corrected. Thank you.
Section 5.3.2.3.3. Last paragraph. "Although there are no events within this area, the model predicts activity rates that are comparable to those obtained for Miami." Thanks for pointing this out, but you have no comments as to whether this is good or bad. From a structural point, one should not expect similarity. From a statistical point, since, for Poisson occurrences, when 1 event is expected, the probability of observing 0 is the same as the probability of observing 1, the process may be respecting this.	The point that we wer trying to make is that one of the advantages of this approach is that it provides a natural floor for the rate, which takes into account the information provided by the lack of events.
Section 5.3.2.4. Typo. Page 5-33, last paragraph, last line, Figure 5.2.3-1 should be 5.3.2-1	Typo corrected. Thank you
Section 5.3.3.2. The last paragraph on p. 5–38 states that the coefficient of variation α of the interevent time t in CEUS RLME sources is estimated from data that had been used to estimate earthquake probabilities in the San Francisco Bay area. The paragraph explains that this was done because data are few from the CEUS RLMEs. Presumably most of the larger datasets used in the San Francisco Bay study are from active continental regions and plate boundaries. Most faults in those areas have slipped farther and faster than CEUS faults, so that the former are likely to be the more mature. What evidence or reasoning supports the use of active-area data to estimate α of CEUS RLME sources? Are CEUS data sufficient to tell whether or not they are consistent with α from active areas? The same question arises in section 5.3.3.4 with respect to assuming that the aleatory uncertainty of the magnitude of an RLME is 0.25.	The data from the two sequences in the CEUS show relatively low values of α. But the sample is very limited and that is why the assessment of the working group based on a larger data set was used. The question of applicability of this model is addressed by the down-weighting of it compared to the Poisson model.
Section 5.3.3.3 discusses uncertainties in ages, some of which were represented as normal distributions. Laboratory dates are commonly given with standard deviations of the analytical uncertainty. However, the geological uncertainty of the age that the date constrains may	These complexities were not incorporated. The stated uncertainties in the ages of liquefaction features were used. Given that the sample sizes for number of intervals are typically small, the result is a wide uncertainty in estimated recurrence rate. Adding the effect of deviations from

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commonly be larger, comprising uncertainties in the various structural and stratigraphic relations seen in a trench. The geologic uncertainties may have distributions of various shapes, some of which may be asymmetrical. How were these complexities incorporated?	normal or moderately wider uncertainties on individual sample ages is not expected to have an appreciable effect on this distribution
Section 5.3.3.4. (See the comment at the end of the one on section 5.3.3.2.)	The uniform distribution over range of ½ magnitude unit is considered to be a reasonable variability under the assumption that the RLME's are similar in size. Given that the estimate of the average size typically has a wide distribution, the resulting composite assessment of hazard is expected to cover minor variations from a uniform.
6.1.1: CHARLEVOIX RLME SOURCE (1) Figure 6.1.1–1, which shows the Charlevoix RLME source, shows the M of the 1663 earthquake as 3.71. Also, the M of the 1925 earthquake is actually 6.3, not 6.2. Bent's 1992 paper lists M6.2 but it also lists the moment as 3.1 x 10^{25}. The moment gives M6.3. She and I were not able to determine where the 6.2 came from. She advised me to always check M by calculating it from the moment.	Corrected label.
6.1.2: CHARLESTON RLME SOURCE (2) Section 6.1.2.1 uses the example of the Eastern California shear zone and its clear geomorphic expression to argue that the Charleston seismic zone, which lacks such an expression, may have a variable recurrence rate. The point is well taken, but the example is not the best. The Eastern California shear zone is in a dry climate. Additionally, SHmax orientations indicate that the causal fault or faults of the Charleston seismic zone may be largely strike-slip. Is there a better example from a humid climate? (3) Section 6.1.2.3.1 states that a northeast strike is assumed for future rupture zones in the Charleston Local source. Figure 6.1.2–4 shows the gravity and aeromagnetic expressions of the north edge of the broad South Georgia rift trending easterly across the northern part of the Local source. The rift contains the Mesozoic rocks that are widespread beneath the Charleston source zones. State geologic maps show that eastern seaboard Mesozoic basins have short faults of many orientations, but long faults parallel to the basin boundaries. Should not some weight be given to east-striking rupture zones? The same question applies to the Charleston Regional source of section 6.1.2.3.3. (4) Section 6.1.2.3.2 explains that the Charleston Narrow source is truncated at the north end because the strength of the evidence for its existence decreases from south to north. Which specific features show that the zone should be truncated where shown in Figure 6.1.2–4?	(2) Slight revised text for clarity. Eastern California shear zone comparison was retained, however, because of well-constrained slip rates. (3) No revisions. The Charleston Local source envelops numerous postulated faults in the 1886 meizoseismal zone that strike northeast. Some northwest-striking faults have been proposed in this area, but the dimensions of these typically are insufficient to produce Mmax earthquakes. The Charleston Regional source configuration, which completely envelops both the Local and Narrow sources, allows for both northeast- and northwest-striking ruptures. (4) Text revised for clarity. The northeast extent of the Narrow source is defined largely by fault mapping from Dura-Gomez and Talwani (2009) and Talwani (2009). Marple and Talwani's (2000) depiction of the southern segment of the East Coast fault system extends northeast beyond the Narrow source configuration. However, according to Dura-Gomez and Talwani (2009) and Talwani (2009), evidence for the existence and activity of the East Coast fault system is greatest in the south (within the Narrow source configuration) and decreases northeastward (beyond the Narrow source configuration). Regardless, the northeast and southwest boundaries of the Narrow source are leaky, such that ruptures that initiate within the zone are allowed to extend beyond the zone boundaries in those directions.
6.1.3: CHERAW FAULT RLME SOURCE (5) "Average and Maximum Displacement" in section 6.1.3, paragraph 1, reports displacement for the three ruptures as 1.5 m, 1.1–1.6 m, and 0.5–1.1 m, listing the oldest first. After noting that the second two ruptures might have been one, the discussion lists estimates of displacements differently as 1.1 m, 1.5 m, and 1.9 m. These sum to 4.5 m, whereas the other three displacement estimates sum to only 3.1–4.2 m. Please explain the origin of the three final displacement estimates. (6) In section 6.1.3 "Rupture Area" states that magnitude was estimated from rupture length, fault dip, and fault width. However fault displacement is generally preferred by paleoseismologists because length can be severely censored by erosion. What effect would use of displacement have on the final magnitude distribution? (7) Section 6.1.3.4, paragraph 6 gives recurrence intervals of 2–5 centuries for the out-of-cluster alternative. Is this an error?	(5) The three values used for D/E of 1.1, 1.5, and 1.9 are not equivalent to the estimates for three specific events. Rather they represent estimates for the average D/E value that can be used with empirical relationships to estimate Mmax. The text will be modified to clarify what average (3.2 to 4.1 m/2-3 events) and maximum D/E at this site are based on available data. Text modified to note that average slip per event at this site would range from 1.1–2.1; the maximum slip during a single event would be on the order of 1.6 m to as much as 2.6 m. 6) Table 6.1.3-1 added to show range of estimated magnitudes based on maximum and average displacement per event estimates relative to estimates based on length and area relationships. 7) typo corrected. Years will be changed to kry.
6.1.4: MEERS FAULT RLME SOURCE (8) Section 6.1.4.4 states that rupture length is preferred to displacement as a magnitude estimator because length requires fewer assumptions. Displacement is subject to uncertain interpretations of structural and stratigraphic relations in a trench. It is not clear how large these uncertainties might have been. However, scarp length is recognized as being subject to severe censoring by erosion over time. The ends of the 26-km section of the fault	1) Uncertainty in the scarp length is accounted for in the range of characteristic magnitudes and is described throughout the section (i.e., 26 km vs. 37 km vs 67 km). Moderate changes (+/- several km) in length will not significantly impact the estimated magnitudes 2) It is difficult for us to judge what displacement measurements are more "accurate" (i.e., Swan et al. vs Crone and Luza). As indicated in the text, we used the Swan et al. (1993) because they

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are constrained by the changes in geomorphic expressions into the two adjacent sections. The locations of these geomorphic changes may have uncertainties measured in kilometers. What are the uncertainties in displacement and scarp length, and how much magnitude uncertainty would each estimator produce? (9) Also in section 6.1.4.4, the single-event displacements measured by Crone and Luza were carefully made and well constrained. They may have better accuracy and precision that Swan's measurements of displacement. If so, why were the better displacement measurements not used?	recognized 2 events where Crone and Luza (1990) only recognized 1 event. The identification of only 1 event potentially raises the question as to whether Crone and Luza measured displacements associated with 1 or 2 events. However, the maximum displacement estimated from Crone and Luza (1990) (~5.4 m) does not result in significantly higher magnitudes than that used from Swan et al. (1993) (~5.25 m). We have added text to this discussion to highlight the similarity between the measurements of the two groups.
6.1.5: REELFOOT RIFT - NEW MADRID FAULT SYSTEM RLME SOURCE (10) The fourth paragraph on p. 6–31 discusses alternative locations for the January 23, 1812 New Madrid earthquake. Not mentioned is the 2006 abstract of Cramer and others, which reports that rupture directivity can explain the evidence interpreted as supporting a location in White County, Illinois. (11) Tables 6.1.5–1 through 6.1.5 –3 are missing. (12) Paragraph 3 on p. 6–38 summarizes some observations of Tuttle and others. The paragraph omits their observation that the Bhuj and New Madrid liquefaction fields cover comparable areas. Area is a more robust estimator than is the distance to the single largest feature. (13) Page 6–39 states that equal weights are to be given to the magnitude estimates of Bakun and Hopper, Hough and Page, and Johnston. Some members of the informed technical community might down-weight the Hough and Johnston estimates because they have not been published for others to examine and critique. Additionally, it is not clear whether the low estimates around magnitude 7.0 are consistent with paleoseismic evidence. The most direct way to justify such low estimates would be to compare the sizes, abundance, and geographic extent of liquefaction features caused by an instrumental M 7.0 earthquake with the features attributed to one of the very large New Madrid earthquakes. Tuttle and others made such a comparison for the 2001 Bhuj earthquake of magnitude 7.6, and found the two liquefaction fields to be comparable. Equal weights on the high and low magnitude estimates, and especially on the low estimate, may be hard to defend.	(10) The text has been modified to refer to Cramer et al. (2006) (11) Missing tables will be provided in the final draft (12) The text states that. "…potential analog for the New Madrid earthquakes based on similarities in the tectonic and geologic settings between the two regions as well as on the **extent and scale of paleoliquefaction** that occurred in the Bhuj earthquake relative to the paleoliquefaction features in the NMSZ region " The Tuttle et al. (2002) does not provide a current best estimate for the area of the NMSZ paleoliquefaction features. It states only that the previous estimates of ~10,000 km^2 are likely underestiated. The Bhuj area is cited as ~15,000 km^2. The discussion does not allude to distance to the single largest feature, but rather to the distance to the farthest paleoliquefaction feature. (13) The Hough and Page (in review) manuscript has come back from external review but has not been formally revised and resubmitted. The external review comments according to Hough (10/1/10) are relatively 'benign', but Hough is out of the country for a month and it is not clear when the revised manuscript will be resubmitted. W. Bakun (USGS) also indicated in an email that "Tom Brocher asked me to do a quick internal review of this paper. There is nothing wrong with it. I like the assignment of intensities by a number of qualified practitioners. I think the uncertainties I sent to you earlier pretty much include these lower estimates. That is, I think there is a lot of uncertainty for these events and I don't mind a range of estimates reflecting that uncertainty. The TI team therefore feels that the Hough and Page assessment is a viable one and thus has not elected to change the weights assigned to the three distributions for Mmax.. The Bakun and Hopper estimates and A. Johnston's current preferred values are more consistent with the comparison of the extent of NMSZ and Bhuj earthquake paleoliquefaction features: these two assessments have a combined weight of 0.67 in the overall assessment.
6.1.9: WABASH VALLEY RLME SOURCE (14) Figure 6.1.9–2 lists earthquake epicenters in the map explanation, but no map symbol is shown and no epicenters are on the map. The cited paper does not include a map or list of feature locations. The map does not show any paleoliquefaction features in Kentucky, although Appendixes C and D cite a report of sand dikes by Counts.	Figure 6.1.9-2 will be modified to show seismicity. The reference to cited paper (5) will be corrected to (7). The paleoliquefaction features in Kentucky are outside the area of the figure. These features are shown on Figures 6.1.5-5, 6.1.6-2, and figures in Appendix E. ADD OR CROSS REF TO APPENDIX E FIGURE
6.2.1: CRITERIA FOR DEFINITION OF BOUNDARY (of alternative Mmax zones) (15) In the first paragraph of section 6.2.1, the most inboard of the three Atlantic domains includes the Mesozoic grabens and half grabens that are stated to underlie the continental shelf. However, the Mesozoic extensional basins extend also underlie the Coastal Plain and extend northwestward into the exposed Appalachian crust. (16) The first paragraph of section 6.2.1.1 concludes that the ECC-AM, AHEX and	This entire section was revised in the Final Report to discuss in more general terms criteria that was used to differentiate extended crust of Mesozoic or younger age from older extended nonextended crust. (15) The text has been revised to address comment. (For the Atlantic margin, the domains comprise rifted continental crust of Appalachian origin lying principally beneath the Coastal Plain

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NAP seismotectonic zones should be included in the Mesozoic Narrow Extended zone. The paragraph cites good evidence for the first two, but the sparse cited evidence for the NAP would be strengthened by citations of the 1989 paper and 1993 map and cross section by Stewart. (17) The second paragraph on p. 6–64 includes the GMH seismotectonic zone in the Mesozoic Extended Zone on the grounds of Cretaceous volcanism that has been attributed to the passage of a hotspot. We know that the GMH has unusually abundant low- to moderate-magnitude seismicity. Do we know anything else about the GMH that is pertinent to the occurrence or frequency of larger earthquakes? Adding the GMH to the Mesozoic Extended Zone requires evidence for Mesozoic extension within or below the seismogenic zone. Does the chemistry or mineralogy of the volcanic rocks indicate extension at melting depths? Are any extensional faults known? The hot spot hypothesis is consistent with Mesozoic extension, but consistency does not imply validity or relevance. In what way does the hotspot hypothesis contribute evidence of Mesozoic extension that penetrates well into or through the seismogenic zone?	and continental shelf,…) (16) This text has been deleted from Section 6.2.1. See Section 7.3.3 for a discussion NAP. Reference to Stewart et al., 1989 and cross sections have been added to Section 7.3.3. (17) This text has been deleted from Section 6.2.1. See Section 7.3.2 for a discussion GMH. Eaton et al. (2007) observe variable Vp-Vs ratios and thin crust from results of teleseismic studies in the area of the GMH and interpreted these observations to blind intrusions. The Ottawa-Bonnechere graben appears to have acted as a zone of weakness during the opening of the Atlantic and emplacement of Cretaceous intrusions (Faure, Tremblay, and Angelier, 1996; Faure et al., 2006). West-trending faults located along the northern margin of the Ottawa-Bonnechere graben are associated with Cretaceous extension. Intrusions of alkali rocks in the Monteregian Hills were emplaced along these faults. (Faure, Tremblay, and Angelier, 1996).
6.4: RECURRENCE PARAMETERS (18) Figures 6.4–1 through 6.4–6 show b-values of about 0.8, plus or minus about 0.03. It was worthwhile to demonstrate that variability is so small. Assuming a value of 0.8 everywhere would prune the logic tree. Would the hazard be much affected if a constant value of 0.8 were used? Why should it be used or not used?	There are several elements to this answer: (1) although the spatial variation in b is small, there is a larger epistemic uncertainty in b (see Fig. 5.3.2-23 of revised report, for example) (2) we still need the 8 alternative maps to represent uncertainty in rates.
CHAPTER 7: SSC MODEL, SEISMOTECTONIC ZONES BRANCH The USGS team had trouble understanding the poor fits of the catalog compared to the realizations on several figures (e.g., 7.5.5-11 to 7.5.2-30). Is this the reason that the CEUS SSC model has such high rates of exceedance?	This issue has been addressed in the revised methodology. New results show a much better agreement with data.
7.3.2: GREAT METEOR HOTSPOT SEISMOTECTONIC ZONE (1) Section 7.3.2 attributes the Monteregian Hills alkalic igneous rocks of Cretaceous age to formation of a northwest-trending hotspot track in Cretaceous time. However, the alkalic rocks align east-west. The geologic maps of Ontario and Quebec show several east-striking faults in, south of, and west of the outcrop area of the alkalic rocks. Elsewhere the report summarizes evidence for Cambrian and Mesozoic regional extension to form inner parts of plate-scale passive margins. Why should the west-trending alkalic rocks not be attributed to Cretaceous reactivation of earlier Mesozoic or Cambrian normal faults, instead of to a northwest-trending hotspot track?	West-trending faults and intrusions of alkalic rocks in the Monteregian Hills is consistent with a Cretaceous north-south-oreineted extension, resulting from rotation of the regional NW-SE extensional stress field by the Ottawa-Bonnechere graben (Faure, Tremblay and Angelier, 1996), whereas extension associated with the opening of the Atlantic is oriented east-west and northeast-southwest (Faure et al., 2006). Both authors recognize the role of the Ottawa-Bonechere graben acting as a zone of weakness during both events. Apatite fission track ages from the Adirondack Mountains and New England provide evidence for Late Cretaceous northwest-southeast extension (Roden-Tice et al., 2009) attributed to the Great Meteor Hotspot, consistent with the orientation of Cretaceous regional stress determined by Faure, Tremblay and Angelier (1996).
(2) Section 7.3.2.4 explains why the seismogenic crust in the seismotectonic zone is modeled as 25–30 km thick. This is an unusually thick seismogenic layer for central and eastern North America. Is the unusually thick brittle crust expected in an area that has been uplifted and heated by a hotspot?	Variable Vp-Vs ratios of thinned crust may indicate intrusion of mafic material into more felsic crust. Eaton (2007) propose that seismicity of the Western Quebec seismic zone represents blind intrusions associated with entrapment of mantle-derived melt at the transition from kimberlite dikes to plutons of the Monteregian Hills. These intrusions may cause seismicity either by weakened faults and shear zones due to reheating of the crust by the hotspot track, or by stress concentrations associated with the emplacement of major bodies in more felsic crust (Ma and Eaton, 2007).
(3) Table D–7.3.3 lists four arguments by McHone against the hotspot hypothesis. Have McHone's arguments been addressed in the literature? If not, does other evidence support a separate Great Meteor Hotspot seismotectonic zone? If not, what would be the impact on hazard if the seismotectonic zone were deleted, leaving only its abundant seismicity?	Text added to include more recent models by McHone (2005) and Matton and Jabrak (2009) for mantle convection cells. The geometry of the GMH seismotectonic zone is not drawn based on the entire proposed hotspot track, rather seismicity attributed to thermal modification (Ma and Eaton, 2007), as evidenced by thin crust and variable Vp-Vs ratios (Eaton et al., 2006).See Figure 7.3.2-3. Regardless of the mechanisms for Cretaceous volcanism in the region, crust of the GMH seismotectonic zone warrants separation from the midcontinent based on Cretaceous tectonic events and elevated rates of seismicity exhibiting an anomalous depth distribution.
7.3.4: PALEOZOIC EXTENDED SEISMOTECTONIC ZONE	Data Evaluation Table added.

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(4) Section 7.3.4 cites Data Evaluation Table C–7.3.4, but Appendix C does not contain such a table. (5) Section 7.3.4.1.2 refers to the Chain Lakes massif as “coastal”. The massif is on the Maine-Quebec border, far inland. The same error occurs on page 7–43.	Mention of coastal deleted.
7.3.5: ILLINOIS BASIN EXTENDED BASEMENT SEISMOTECTONIC ZONE (6) The first paragraph refers to “subsequent studies”. What are they? (7) Figure 7.3.5–1 shows an energy center on the Meramec River. The corresponding earthquake is assigned the same magnitude range as the Iona and Elnora earthquakes of the Illinois Basin Extended Basement seismotectonic zone. The first paragraph of section 7.3.5 explains that the prehistoric earthquakes are part of the reason for defining the Illinois Basin Extended Basement Zone. Yet, the Meramec River energy center is excluded from the zone. Please explain the reason for the exclusion, and cite appropriate references. (8) The beginning of section 7.3.5.1 lists five characteristics of the southern Illinois Basin that may influence Mmax and the properties of future earthquakes, and which support delineation of the Illinois Basin Extended Basement seismotectonic zone. The first characteristic is an unusual number of prehistoric and historical earthquakes. The prehistoric earthquakes are known because the southern Illinois Basin has been searched for paleoliquefaction features, whereas no other similarly large part of the Midcontinent has. The other four characteristics are likely results of the basin’s having been much more intensely characterized by exploration for and production of petroleum and coal than most other parts of the Midcontinent. Thus, only the unusually abundant historical seismicity appears to distinguish the southern Illinois Basin from other basins throughout the Midcontinent. Yet in 1988 Coppersmith pointed out that seismicity probably migrates within the central and eastern U.S., at unknown intervals. Migration implies that some factor other than long-lived faults, folds, uplifts, and basins likely controls whether and where most seismicity concentrates. Therefore, the stated reasons that support the first sentence of section 7.3.5.1 may not be valid. Please justify the sentence. (9) As stated in the text, the boundaries of the Illinois Basin Extended Basement Zone are highly uncertain because they are estimated from the geographic distributions of several geologic structural types, stratigraphic sequences, and potential-field anomalies, each of which has its own uncertainty. Section 7.3.5.4 explains how other kinds of uncertainty are incorporated into the hazard-computation model. Please explain how geographic uncertainty in the zone boundary is incorporated.	(6) A reference to Wheeler and Cramer (2002) has been added to the revised text. (7) The Meramec River energy center was recognized and considered during the development of the IBEB source zone configuration. This energy center lies outside the areas of extended basement as shown on Figure 7.3.5-2) and it has been postulated that this paleoliquefaction could be the result of earthquakes originating on structures in the southern Illinois region (e.g., the DuQuoin monocline/Centralia fault that lies within the IBEB) (Tuttle, Chester et al., 1999; Tuttle, 2005b). (8) The discussion of the criteria used to define the IBEB has been revised to more fully explain the basis for defining this zone. As noted in the revised text there is evidence to suggest that the crust in the IBEB is different from the surrounding craton and some of the postulated causes of elevated seismicity in the NMSZ may also apply to the adjacent IBEB. The text also clarifies that given uncertainties in the possible influence of Mesozoic extension in this region the MESE and NMESE Mmax priors are both considered for this zone. (9) Uncertainty in the boundary of the IBEB is handled through the use of the ‘leaky’ boundary.
7.3.6: REELFOOT RIFT SEISMOTECTONIC ZONE (10) Figure 7.3.6–1 shows geologic features in and around the Reelfoot Rift. The steeply dipping rift-bounding faults are dominantly normal, with minor contractional reactivation. Why are they shown with the standard map symbol that identifies shallowly dipping thrust faults? Do other figures contain the same error? (11) The last sentence of the first paragraph on p. 7–41 misstates two of the three kinds of information from which Wheeler defined a boundary between the Reelfoot Rift and the Rough Creek Graben. First, across the boundary, the largest Cambrian normal slip switches from a fault on one side of the rift-graben system to another fault on the other side of the system. Thus, both of the faults with the largest slip and consequently the greatest likely width die out at the boundary. The discontinuity might limit propagation of large ruptures. The amounts of Cambrian normal slip are recorded by the thicknesses of Cambrian sandstones. Fault reactivation after the Cambrian rifting could have reversed part of all of the Cambrian slip, thereby affecting the dip direction of the basement/cover contact within the rift-graben system. However, the thicknesses of the Cambrian strata would have been preserved. Thus, the dip direction of the basement-cover contact is not pertinent to the location of the boundary. Second, the northeast limit of alkalic igneous rocks might indicate the northeast limit of extensional rift faults that are wide enough to sample alkalic melts that were generated by	(10) The symbol used to depict the borders of the Reelfoot rift has been revised in Figure 7.3.6-1, Figure 6.1.5-3, and 6.1.5.2. (11) Text has been revised in response to comments. (12) The magnitude of the two earthquakes is from Bakun et al. (2003) “ We use this model, MMI site corrections, and Bakun and Wentworth’s (1997) technique to estimate M and the epicenter forthree important historical earthquakes. The intensity magnitude MI is 6.1 for the 18 November 1755 earthquake near Cape Ann, Massachusetts; 6.0 for the 5 January 1843 earthquake near Marked Tree, Arkansas; and 6.0 for the 31 October 1895 earthquake. The 1895 event probably occurred in southern Illinois, about 100 km north of the site of significant ground failure effects near Charleston, Missouri.” (from Bakun et al., 2003, abstract)

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extension, and which are therefore wide enough to contain large rupture zones. In contrast, the northeastern limit of Mesozoic strata of the embayment is controlled largely by erosion and by a broad, regional downwarp southwest of the boundary between the rift and the graben. The distribution of Mesozoic strata is not pertinent. The following paragraph repeats the error with respect to Mesozoic strata, and adds an unclear statement about Mesozoic deformation. (12) Section 7.3.6.4 appears to assign M 6 to both the 1843 and the1895 earthquakes. However, intensity VI, VII, and VIII were reported over much larger areas in 1895 than in 1843. There were more reports in 1895 because the region was more densely settled, but density of reports seems unlikely to explain the greater distance of reports from the epicenters. The highest intensity reports from both earthquakes were concentrated along major rivers, so it is not clear that greater amplification of the 1895 shaking might explain the larger reporting areas. What is the evidence that the two earthquakes had the same magnitude?	
7.3.7: EXTENDED CONTINENTAL MARGIN - ATLANTIC MARGIN SEISMOTECTONIC ZONE (13) The last paragraph on page 7–43 states that a topographic high would resist extension. By what mechanism would high topography resist extension, rather than encouraging it as in orogenic collapse? (14) The first paragraph of section 7.3.7.1 reports a “highly significant” finding of a difference between rifted and unrifted crust, and “a statistical basis for separating Mesozoic and younger extended crust to establish a prior distribution of Mmax.” Pages 5–10 through 5–12 appear to present a contradicting statistical assessment of the separation between Mesozoic and younger extended crust, on the one hand, and other crust, on the other hand (although see one of the comments on section 5.2). Please resolve the apparent inconsistency. (15) Section 7.3.7.1 lists three criteria for distinguishing the more seaward northern part of the Extended Continental Crust - Atlantic Margin Zone (ECC-AM) from the onland Northern Appalachian Zone (NAP). First, the multiple phases of NAP reactivation do not seem like a valid criterion because the reactivation is pre-Mesozoic and, therefore, it may be unlikely to have affected the seismic potential of any Mesozoic faults. Furthermore, at least part of the recognition of multiple phases might be caused by the better exposure in the NAP compared to that in the ECC-AM. Second, the absence of exposed Mesozoic rift basins in the NAP does not appear to be a valid criterion, because the greater uplift there could have allowed erosion to destroy the sediments and sedimentary rocks that filled the basins, leaving only the normal faults whose movement created the basins. In fact, the 1991 cross sections of Stewart, which are cited in the reviewed report, show several interpreted examples of NAP faults whose basins presumably have been eroded. If the beheaded faults are of Mesozoic age, then Johnston’s observation would imply potentially similar Mmax in NAP and ECC-AM. Additionally, Figure 7.1–7 shows that the difference between NAP seismicity level and ECC-AM seismicity level is similar to the differences along trend within the ECC-AM. Finally, examination of state geologic maps from Connecticut to North Carolina shows that Mesozoic rift basins contain faults of all orientations, although the longer ones parallel the local basin trend. Thus, except for the largest earthquake ruptures, the difference in structural grain between the NAP and the ECC-AM does not seem likely to produce significantly different preferred orientations of strikes of Mesozoic faults. It seems to be difficult to argue for a difference in the numbers, styles, orientations, or sizes of Mesozoic faults between the NAP and the ECC-AM. Third, absent such differences, it seems equally difficult to argue for a difference in seismicity characteristics or rupture characteristics between the NAP and the ECC-AM. In sum, a convincing argument for the existence of a distinct NAP seismotectonic zone has not been presented. (16) The second sentence of section 7.3.7.3 refers to the Extended Continental	13. The statement regarding high topography resisting extension is summarizing the work of Faure et al (2006). We have modified slightly to state “may have resisted”. 14. The “highly significant” term is summarizing the Johnston et al (1994; page 4-1) observation and not the CEUS reanalysis. The text is clarified by removing “highly” and stating that our CEUS reanalysis also provides a basis for separating Mesozoic and younger extended crust, however the statistical significance is not strong. 15. No revision to text. While the multiple phases of extension observed in the NAP may be due to better exposure, crust of the NAP has a distinct history and involves terranes unique from the central and southern Appalachains accreted during the Acadian and Salinic orogenies. Earthquakes of the NAP occur in the upper 10 km of the crust within these terranes. While the depth distributions for these zones may be similar, focal mechanisms for earthquakes with NAP exhibit a higher proportion of reverse mechanisms. 16. The phrase “highly extended, transitional crust” was modified to read “extended continental crust”.

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Crust - Atlantic Margin Zone as "highly extended, transitional crust". Those words do not describe the onland central and southern Appalachians that comprise roughly half of the zone. The words are better applied to the Atlantic Highly Extended Crust of section 7.3.8.	
7.3.8: ATLANTIC HIGHLY EXTENDED CRUST SEISMOTECTONIC ZONE (17) Section 7.3.7.4 states that the strike expected of future ruptures is taken as the default distribution of Table 5.4–1. The distribution is strongly asymmetrical about N35°E. The strong asymmetry is inconsistent with the little that is known about the Atlantic Highly Extended Crust Zone (AHEX). As the report summarizes, the AHEX is far offshore under the shelf edge, has crust that thins southeastward, is defined by linear potential-field anomalies, is sparsely seismically active, and is defined by the long, thin East Coast Magnetic Anomaly. The anomaly trends approximately N20°E south of New Jersey and about N60°E north of New Jersey. The anomaly is attributed to abundant rift-related volcanic rocks within the AHEX. The most likely properties of any faults in the AHEX can be suggested from the generalized characteristics of the AHEX. Any continental crust is probably highly extended. Large extension tends to produce long faults that strike at high angles to the extension directions, as exemplified by the mapped border faults of Mesozoic grabens from Massachusetts to North Carolina. At least the older of the volcanic rocks may also contain extensional faults. The faults are likely to strike perpendicular to the direction of extension, which would be more or less parallel to the AHEX and approximately N20°–60°E. Emplacement of large amounts of volcanic and perhaps intrusive rocks might disrupt this simple fault geometry. These considerations argue against application of a strongly asymmetric default distribution of fault strikes (Table 5.4–1). If there is a preferred strike, it is likely to be about the same as the mode of the default distribution.	17. The rupture orientation has been modified from the default distribution and Table 5.4-2 and the text have been updated to reflect this modification. We agree with the reviewer that faults are most likely to be oriented parallel to the long axis of the zone, which is nearly perpendicular to the extension direction. The revised distribution is still asymmetric about the overall strike of the zone (N25E in the south and N60E in the north) however we still account for uncertainty in rupture orientation by allowing N-S, E-W and N50W ruptures to occur.
7.3.9: EXTENDED CONTINENTAL CRUST-GULF COAST SEISMOTECTONIC ZONE (18) The third bullet in section 7.3.9.2.1 states that aeromagnetic anomalies are margin-perpendicular in the Extended Continental Crust-Gulf Coast Zone (ECC-GM). However, the 2002 Magnetic Map of North America shows margin-perpendicular aeromagnetic anomalies only in central Texas. Please cite whichever aeromagnetic map supports the bullet, and explain why it is preferred over the 2002 map. (19) On page 7–55, second paragraph, the description of the northern edge of the ECC-GM includes the phrase "The source zone is extended to the Southern Arkansas fault zone …." The preceding sentences indicate that the source zone extends north of the fault zone. (20) Page 7–58 states that the exposures interpreted in the cited papers by Cox do not contain unequivocal evidence of large earthquakes, but gives no reason for this surprising conclusion. Given the recognized quality of Cox's work and its publication in respected journals, it might be wise for the text to state in a sentence the reason for discounting Cox's interpretation. The present reference to Appendix E is not sufficient. (21) The last two lines of section 7.3.9.5 attribute a lack of "additional research conducted to support Quaternary offsets along these faults" to a 2005 paper by Wheeler. The paper cannot reflect any research that has been done since 2005 and it cannot reflect any research that was not found or not published. The citation is wrong and the quotation implies omniscience.	18. The sources supporting this statement are cited in the text (Buffler and Sawyer, 1985; Marton and Buffler, 1994; Sawyer et al., 1991) 19. Changed 20. As stated in the section, the Cox data was subjected to the criteria established as part of the project for identifying earthquake induced liquefaction features. Only one featured identified by Cox met these criteria. It is not appropriate to go through the reasoning or methodology for defining these criteria in this section. 21. Modified the text to try and prevent this perception
7.4: MAXIMUM MAGNITUDE DISTRIBUTIONS FOR SEISMOTECTONIC DISTRIBUTED SEISMICITY SOURCES (22) The text explains how the double-peaked distributions in Figures 7.4.2–2 and 7.4.2–3 resulted from the procedures of chapter 5. However, if I understand the analysis correctly, the explanation does not answer the more fundamental question of whether a double peaked distribution is likely to seem reasonable to the informed technical community.	Maximum magnitude distributions have been revised and now incorporate al minimum value of **M** 5.5

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Figure 7.4.2–2 implies that Mmax 5.0 is just as probable as Mmax 6.0 and Mmax 6.9. Does any substantial portion of the community think that Mmax can be as low as 5.0, when we have seen larger earthquakes occur in very unlikely places? A "yes" answer would require documentation. Additionally, one might ask whether it makes sense to conclude that Mmax 5.4 is less likely than Mmax 5.0 when the majority of the distribution is above M 6.0. However elegant the mathematics, it seems unlikely that Mmax 5.0 would be acceptable to a majority of the informed technical community at any probability.	
7.5: RECURRENCE PARAMETERS (23) The text introduces the figures but does not seem to draw any conclusions from them. For example, Figures 7.4.2–1 to 7.4.2–8 show that seismicity rates are highest where there are the most earthquakes, and that b does not vary much from 0.80. Section 7.5.2 explains the effects of different proportions of small and moderate earthquakes. What conclusions may be drawn from these observations, and how do the conclusions affect the analysis?	We have expanded the revised report. In addition, many of these issues are discussed in Section 5.3.2.
Chapter 8: Demonstration Hazard Calculations Using CEUS SSC Model The purpose of Chapter 8 is to demonstrate the hazard calculations, show the major contributors to the hazard, show the sensitivity of parameters to the hazard, and compare the hazard with the USGS and COLA models. The CEUS SSC model rates are almost always higher, often by a factor of two or more, over a large range of ground motions. The slopes of the hazard curves are similar because they all assume the same ground motion prediction equations. It seems that if Mmax were the only consideration the USGS ground motions might be higher than the CEUS SSC ground motions because they typically have a higher mean value. This higher rate of ground motions compared to earlier models is not clearly explained in the text. This higher hazard indicates that the CEUS SSC model predicts a rate of earthquakes that is considerably higher than the earthquake rate predicted in the USGS and COLA models. These higher rates can be seen in Chapter 5-7 figures (e.g., 6.4-7 to 6.4-16; 5.3.2-22, where the model over-predicts the historical rate. Has the earthquake catalog changed significantly? We examined the catalogs and find that the CEUS SSC catalog is higher than the USGS catalog but probably not by a factor of 2. Is the higher rate related to the decluster or smoothing parameters and methods (a and b-values)? Why doesn't the regression analysis fit the historical seismicity rates better in the figures shown in Figure 7 discussed previously? The USGS model uses a b-value of 0.95 for the CEUS. The b-values for the CEUS SSC model seem to be lower implying more large earthquakes.	The final report, Section 8, has been extensively rewritten, and comparisons to historical seismicity have changed so that fits to historical seismicity are consistent with data. The CEUS SSC earthquake catalog is discussed extensively in the final report
Central Illinois Site: The CEUS SSC model is almost a factor of 2 higher than USGS/EPRI-SOG models. The major contributor is the IBEB (Illinois Basin) zone. The New Madrid (NMFS) RLME is most important at 1 s SA. However, background seismicity dominates at shorter periods. Why does the background hazard from CEUS SSC model give higher rates than were applied in the USGS and COLA models for short periods? At 1 s period the USGS and CEUS-SSC models are much more similar because the NMFS models are much more similar.	As the comment states, the higher hazard at Central Illinois results from the IBEB source, which is not modeled in the USGS source model. That is why the CEUS SSC model gives higher hazard for short periods than either the COLA or USGS models. As stated in the comment, for 1 Hz frequency the hazards are similar because the hazard is dominated by the NMFS, which is modeled similarly in all 3 source models.
Chattanooga Site: The CEUS SSC model hazard is more than a factor of 2 higher in annual frequency of exceedance than the USGS and COLA models. At the Chattanooga site the ground motion hazard at e-3 to e-5 is more than a factor of 2 higher. Background sources contribute most to the hazard. However, the USGS ground motions are higher at 1 hz for exceedances of e-4 to e-6. This result is not explained in the text.	None. It is not part of the project scope to dissect all hazard comparisons, compare parameters from different studies, and determine what causes one curve to lie higher or lower than another curve.
Houston Site: CEUS-SSC model hazard is dominated by GHEX (Gulf of Mexico) which is the zone that encompasses the site. Contributions from other background sources are much lower. Hazard is dominated by background sources at all periods (except for very low ground motions at 1 s SA). The SSC model indicates about a factor of 2 higher annual frequency of	None. It is not part of the project scope to dissect all hazard comparisons, compare parameters from different studies, and determine what causes one curve to lie higher or lower than another curve.

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exceedance than the USGS model frequencies for short periods (10 hz and PGA) but is more similar at longer periods (1 hz). This is probably because NMFS is significant at 1 hz and the USGS and CEUS-SSC models are more similar for NMFS. However, the differences are not explained in the text.	
Jackson Site: For Jackson Site the NMFS is important at all frequencies. Therefore, the CEUS-SSC, COLA, and USGS models are quite similar for pga, 10 hz, and 1 hz.	None. No question asked.
Manchester Site: Similar to the other sites dominated by background hazard the CEUS SSC hazard at the Manchester site is considerably higher than the hazard for the USGS and COLA models. The deaggregation for the Manchester site at 10 hz is dominated by earthquakes with magnitudes less than 6.0 and distances less than 10 km. The CEUS SSC deaggregation for 10 hz at e-4 is similar to that produced by the USGS for pga at 4e-4. The higher rates for the Manchester Site should be explained in the text.	The final report shows that hazard from the CEUS SSC model lies between hazard from the USGS model (highest) and hazard from the COLA model (lowest). So this comment has been superseded by later results.
Savannah Site: For the CEUS SSC model the major contributors to the ground motion hazard are the Charleston RLME source and the ACC_AM background source model. The CEUS-SSC, COLA, and USGS models are quite similar with the CEUS-SSC model showing a little higher ground motions for a large range of exceedances.	None. No question asked.
Topeka Site: The major contributor to the background source is MIDC-A which encompasses the site. The next important contributors are MIDC–B, MIDC-C, and MIDC-D. Background seismicity dominates the hazard at PGA and 10 hz and the NMFS dominates hazard at 1 hz. The hazard curves for the CEUS-SSC, COLA, and USGS and similar, especially at 1 hz. The hazard is typically higher for the CEUS-SSC model with rates almost a factor of two higher for a large range of ground motions. This discrepancy should be explained in the text.	The final report shows that 10 Hz and PGA hazard from the CEUS SSC model is about 25% higher than hazard from the USGS model, so this comment has been superseded by later results.
CHAPTER 9: Use of the CEUS SSC Model in PSHA This chapter is intended to provide information necessary in implementing the hazard model (contained in the Hazard Input Document). The main focus of these sections is on simplifying the logic tree to those branches that are most important and in understanding the level of precision associated with the hazard values. The main comment for this section is that it is hard to justify a +/- 25% uncertainty between experts when the results of Chapter 8 show a factor of 2 difference in rate of exceedance between the USGS and CEUS SSC models at many sites. Section 9.1 gives an overview of the chapter and section 9.2 discusses the Hazard Input Document (HID) that is discussed in Appendix H. Section 9.3 discusses hazard sensitivity studies for different crustal thickness and rupture orientation (dip, direction) at the seven test sites. In general these parameters are shown to have little sensitivity in the hazard results with the exception of a source near the Illinois Basin. Therefore, a single branch of the logic tree is selected rather than maintaining the multiple branches. Sections 9.3.2 and 9.3.3 were not available and will be written later. Section 9.4 discusses the level of precision associated with seismic hazard estimates. For example, how would the hazard vary if different experts made up the TI team. This section discusses three inputs of the hazard analysis: sources, ground motions, and site response. The sources involve source geometry, maximum magnitude, paleoseismic record length, and activity rate given a record. These results may change when NGA-East becomes available and may be specific for these test site and not apply to all sites. The authors make a case for using COV_{WT} of 0.5, 0.3, and 0.7. Would this estimate depend on the number of ground motion equations available? Results generally show that the annual frequencies of ground motion exceedance increase with ground motions, which seems reasonable. However, I do not understand why the COVs decrease in annual frequencies of exceedence greater than 1E-5 on Figure 9.4-53 and 9.4-57. The authors show at the Savannah , Chattanooga, and	The following comment is not clear: *"… it is hard to justify a +/- 25% uncertainty between experts when the results of Chapter 8 show a factor of 2 difference in rate of exceedance between the USGS and CEUS SSC models at many sites."* It's not clear whether the 25% uncertainty is deemed too high or too low, so it's not possible to respond to this comment. Regarding COV_{WT} values of 0.3, 0.5, and 0.7, these are example values chosen to illustrate a range of COVs, and do not depend on the number of ground motion equations. COVs decrease at low ground motions because all ground motion equations produce an exceedance of those ground motions, so the hazard has little uncertainty compared to the hazard for higher ground motions (with depend more on ground motion equation). No revisions proposed. Regarding "cl. Mean COV" being higher than "wts COV", the text explains that this results from the former assuming that all hazard curves are independent, and the latter maintains the symmetry in weights assigned to alternative ground motion equations. Regarding the comment : "*Some members of our group have questioned whether or not experts would all obtain answers within 25% of one another"* It is clearly stated in Section 9.4 that the purpose is to derive a *minimum* measure of imprecision,

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Columbia sites that the term "cl. Mean COV" is quite a bit different from the "wts COV". This was not intuitive and I do not understand this result. Section 9.4.3 discusses the conclusions on the precision in seismic hazard estimates. They conclude that the ground motions can vary by about 25% for ground motions with hazards in the range 10^{-4} to 10^{-6}. Some members of our group have questioned whether or not experts would all obtain answers within 25% of one another, especially in light of the fact that in previous hazard models the difference between the COLA and USGS models have had factor of 2 differences at some sites. The differences in rate of exceedances and the ground motion hazard between the USGS and CEUS SSC model are sometimes more than a factor of two. Editorial Comments: The figure numbers for 9.4-1 are mislabeled. The counting seems to have proceeded up to 9.4-71 even though the last number seems to be 40 greater than the number referenced in the text. Some of the notation in section 9.4 was a little ambiguous. It took me a while to realize that COV_K was for the parameters GEOM, Mmax, RATE, and RECORD. COV_T is probability the same as SRSS. Later in the chapter there are references to COVHAZ and COV_{HAZ} wts COV, and COV_{WT} , cl mean COV and σ_{CL} and σ_H. I think that these terms need to be cleaned up to be more consistent with the equations and figures.	which is a lower bound to what can be achieved. Certain groups may find a level of imprecision that is higher, like ±50% or ±100%. That would not be inconsistent with the results presented in this Section. Figures are renumbered for the final report.
APPENDIX H: EPRI/DOE/NRC CEUS SEISMIC SOURCE CHARACTERIZATION PROJECT- Draft Final Seismic Source Model Hazard Input Document (HID) July 6, 2010 The intent of the HID is to give future users details on how to implement the CEUS-SSC model. It contains the logic tree structure that defines the frequency, locations, and sizes of future earthquakes in this region. The appendix describes how the zones are characterized. A description of why the TI team chose a particular equation, occurrence rate, magnitude, or source geometry, or references is not given in this section of the report. The logic tree does not seem to be trimmed according to Chapter 8 sensitivity studies. It would be helpful to have a logic tree in which only the important branches were included. It would be difficult for most users to implement the variable a and b routines described in previous chapters. Therefore, the process is not open for most users to evaluate that methodology. It seems like the computer codes should be made available for these analyses. Alternatively, the TI team could release the output gridded data. However, this is not the best alternative since most users would not understand how these numbers were generated. A third alternative is for the TI team to revert to the smoothed seismicity kernel that is more intuitive to the user community. Editorial comments: I can't see any differences in Figure 8 and 9. Page H-19 states that an objective approach is used to select the degree of smoothing. It would be very helpful to refer back in the text where this approach is described. I am confused why Tables Charleston HID-2 and HID-3 have identical RLME frequencies even though they both consider 4 events and one table is for a 2000 year period and the other is for a 5000 year period. Steve Harmsen reproduced the 1000 branches of the logic tree for the Cheraw Fault from the HID information. His analysis is described below.	The HID describes the inputs to a hazard analysis. It will include tables of gridded a & b values for use in a hazard calculation. The HID does not describe how to compute these value as they are already available. The question about the availability of the software for computing the gridded a&b values is a separate issue from the HID
Frankel **The Master Logic Tree** First of all, the master logic tree in Fig. 4.4.1-1 is not clear and may be inaccurate. The RLME is not an alternative branch of a logic tree. It is a separate, additive component of the overall hazard "model." For the other component of the model, there should be two branches of the logic tree for the seismicity-parameter calculations based on either 1) Mmax zones or 2) seismotectonic zones. Figure 6.4-1 shows the seismicity parameters for the case of no seismotectonic zones or Mmax zones. I don't see such a branch in the master logic tree. If this case is used, then it needs to be a third branch in the master logic tree. The high weight assigned to the seismotectonic zones is not consistent with the weighting favored by most of the participants of the USGS workshops.	The logic tree displays exactly the case described in the comment. The RLME sources are always an additive component, and that is signified in the tree by the vertical line without a node (as explained in the text). As noted in the comment, the true alternatives are indicated with a node on the logic tree and are the Mmax and seismotectonic zones alternatives. One of the alternatives in the Mmax zones model is that the entire CEUS study region is a single Mmax zone. That is what is shown in Figure 6.4-1. The weights between the Mmax zones and the seismotectonic zones alternatives has been reassessed and is documented in the revised report.
The bottom line is: how much difference in the hazard does it make to use the seismotectonic zones versus the Mmax zones in the seismicity rate calculation?	Plots illustrating the difference in hazard are included in the final report.

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It would be informative to have maps that show the mean expected rate of M>=5.5 and M>=6.0 earthquakes, the magnitudes that often dominate hazard in the CEUS (outside of RLME's).	Unfortunately, our schedule did not allow preparation of these maps in time for the report deadline.
I do think there is a need for some areal source zones in the CEUS, based on the classical notion of uniform hazard within a zone. These are needed, for example, in the case of a linear structure that has a cluster of seismicity in only one portion. The SSC study is deficient because it doesn't include these traditional areal source zones in certain areas. I think the Eastern Tennessee (ETN) area requires a traditional areal source zone. I think the fine grained nature of the seismicity rates derived from the report's methodology is not justified as a guide to future seismicity. Also, by not having an ETN source zone, the hazard is underestimated in northeastern Alabama, the site of the Fort Payne earthquake and other recent earthquakes. It appears that this area is on the southwesterly extension of the ETN zone.	Spatial smoothing allows for variation in rate.
The Nemaha Ridge is another example of an area which may require an areal source zone. The M 4.5-5 earthquakes in the 1800's may be associated with the Nemaha Ridge structure. If so, one should connect these epicenters with an areal zone along the Ridge, so that the area between these earthquakes has higher hazard. This is not captured in a smoothed seismicity approach. I would think this source zone deserves some weight in the comprehensive approach advocated in this SSC. The report discusses some possible liquefaction features in the vicinity of the Nemaha Ridge, but then states on page 7-69 that the M5.2 1867 earthquake may "characterize the seismic source in this region." This statement needs elaboration. Could the M5.2 earthquake cause the observed features that may be from liquefaction? Sounds unlikely to me.	There is insufficient information to characterize the Nemaha Ridge as an RLME source. The conclusion stated with regard to the results of the paleoliquefaction study (i.e., the M5.2 1867 earthquake may "characterize the seismic source in this region.") is from Niemi et al. (2004) publication. Their preliminary results did not identify any features that would indicate evidence for prehistoric large magnitude earthquakes. The threshold for paleoliquefaction depends on local site and groundwater conditions. Olson et al. (2005) anchor the low end of their magnitude bound curve at **M** 5.4 based on reported evidence for both liquefaction associated with an historical earthquake of this magnitude and no evidence for liquefaction for another earthquake of similar size. The text has been revised to clarify the source of the statement and to acknowledge that although the source of the earthquake may have been at the threshold size for producing liquefaction as suggested by Niemi et al. (2004) a larger earthquake cannot be precluded.
Spatial smoothing technique to get rate and b-value The procedure for determining seismicity rates and b-values on a grid is extremely complicated, difficult for even an expert to understand, opaque, and not reproducible by others. I wonder if the complexity of the analysis technique is justified by the limited seismicity data and speculative nature of the source zones. A key difficulty is that the programs used to make these seismicity rate and b-value grids have not been released and are not available to scientists outside the SSC project. Therefore, these results cannot be reproduced or checked by other investigators. Given that this method will provide the basis for hazard assessments for nuclear power plants, I don't think this is acceptable from a public policy standpoint. I recognize that there may be some potential advantages of this complicated and turbid method, but I think it also has pitfalls which make its use problematic.	Generally, you need more powerful statistical tools when you have limited data than when you have abundant data. The two basis elements of our statistical model are not that complicated. They are as follows: (1) use of a likelihood function (essentially Weichert's formulation, with the addition of weights) at the cell level, and (2) the introduction of smoothness between adjacent cells by means of penalty functions. The MCMC and the generation of the 8 alternative maps get somewhat complicated, but they are less important than those two basic elements. Compared to the kernel method with arbitrary kernel size, our method has the disadvantage of being implicit. The advantage is that it gives you estimates of uncertainty in rate and b, it is objective, and it is adaptive. If you modify the kernel method to have all of these desirable properties, the difference in complexity is not that large.
The report often uses the term "objective" to describe how the weights for the smoothing were derived for each source zone. I presume that this "objective" aspect is described on page 5-27, but the text is very hard to follow. It is unclear to me how the data are "objectively" constraining the smoothing parameters. I am doubtful that the data do constrain the smoothing parameters. If you're just trying to get a best fit to the earthquake data, then having no smoothing would produce the lowest residuals. It would be very helpful for the authors to provide more details about the penalty method, by showing how it works in some special cases, e.g. a cluster of events in one place and events spread out over an area. The use of the Laplacian is a mathematical construct and doesn't provide insight on physical parameters, such as smoothing distances and the correlation function of the observed seismicity. This appears to be a mathematical exercise that provides	As indicated in the report and in verbal exchanges, there is also a counter-acting sigma term in the denominator of the penalty function, which reduces the value of the penalized likelihood when there is no smoothing (i.e., when that sigma is large). The Laplacina is a generalization of the second derivative to multiple dimensions. The sum of the Laplacian over all cells is a measure of the roughness of the nu or beta surface.

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little physical insight.	
In the USGS maps, we used seismicity models with different low-magnitude cutoffs (M3, M4, M5). However, this was done with the opposite intention to that invoked in the SSC report for the reduced magnitude weights. We wanted to give higher weight to the M4 and M5 earthquakes, to emphasize the hazard for areas that had these larger earthquakes, but had relatively few M3 earthquakes in the catalog. Such areas include New York City and Nemaha Ridge. Our application of models with different magnitude cutoffs addresses possible local variations from a GR recurrence relation. In contrast, the SSC report uses the reduced M weights to create maps with excessively smoothed seismicity rates, which increase the hazard at large distances from the observed M5 earthquakes. The reduced magnitude weight models produce seismicity rate maps that have odd changes within source zones that form almost linear boundaries (e.g., fig. 6.4-6, fig 7.5.2-6). The authors should explain these physically unreasonable patterns and also show the locations of only the M4 and M5 earthquakes that apparently drive these patterns.	The intents are not that different (i.e., give more importance to the higher magnitudes in some branches of the logic tree). In fact the USGS uses larger kernels for the higher cutoffs, which is effectively what the objective approach is doing in cases B and E. One could argue that the objective smoothing increases the kernel too much in cases B and E. After all, it does it on purely statistical considerations. The "almost linear boundaries" that occur sometimes are probably an artifact introduced by the North-South-East-West discretization into cells and are not a cause for concern. On a similar vein, one could argue that "almost circular boundaries" are an artifact of the kernel approach.
It appears that the SSC method systematically overestimates the rate of M>=5.0 earthquakes in the CEUS compared to the earthquake catalog. This is illustrated in figures 6.4-7 through 6.4-16 (for the Mmax zones and in Chapter 7 for the seismotectonic zones). I think the observed rate of M>=5.0 in the CEUS should be a key constraint in a hazard model for the CEUS. We tried to adhere to this constraint for the USGS maps. The LLNL study in the 1990's also had a large over prediction of M>= 5.0 earthquakes. It is important for the report to show the total mean recurrence rates from all the models and compare to the observed rates, for the entire CEUS. If there is a major discrepancy in the observed and predicted (mean) rates of M>=5.0, this should be explained and justified. It is likely that the over prediction of the rate of M5.0 and larger earthquakes from the SSC model is a major reason why the SSC hazard estimates are usually higher than the USGS ones for the sites shown, although I'm not totally convinced that the USGS model was correctly implemented. For St. Paul, the reduced M weight realizations have a mean rate of M>=5.0 of about 0.1 (Fig. 5.3.2-30). So we should expect a M>=5.0 earthquake near St. Paul about every 10 years, according to the mean of these results. Interesting, given that there hasn't been a M>=3.0 in this zone for at least 100 years, according to the plot in the report. I would hope that some of the models based on the occurrence of M5 earthquakes (the reduced M weight models) would give low hazard to a place that hasn't had a M5 historically. This does not seem to be the case and highlights, I believe, a flaw in the methodology.	This issue of overestimation has been addressed in the revised methodology. New results show a much better agreement with data. Regarding Saint Paul, we were trying to demonstrate that our methodology provides a natural floor for the hazard in regions of higher activity. Admittedly, we have not compared our floor to the USGS floor, but we suspect they are not too different.
The reduced M weight models have the most spatial smoothing and help to form the floor of hazard in low-hazard areas. So the weighting of the reduced M weight model (0.333) relative to the full M weight model (0.667) is important to establishing the floor of hazard. **The choice of this weighting is not objective, so the floor of hazard is also not objective.** This is illustrated in Figure 5.3.2-11 for Houston. The reduced M weight model dominates the hazard, so its weight will control the floor of hazard. There is too much fine-grained structure in the seismicity rates determined in the active areas (e.g., fig 6.4-3). As time goes on, we are seeing more M3 earthquakes on the fringes of the active areas. This is to be expected from an ETAS type model of seismicity. Taking such a fine grained approach to the location of future earthquakes is not physically reasonable. It is likely that the areas within about 70 km of the cells with the highest rate values will be filled in by future earthquakes. Again, it is not adequately explained how the "objective" smoothing in this report is achieved. It does not appear that the "objective" smoothing method takes into account the uncertainties in earthquake locations and the change of these uncertainties with time (older events more uncertain in location than more recent ones). It should.	These weights have been changed (along with the nomenclature) and a rationale is provided for the choice of weights for cases A, B, and C. Admittedly, these weights are subjective. Regarding the issue of fine grain, we note that we obtain very similar results for regions of high activity in Figure 5.3.2-42 using an objective adaptive kernel approach (the approach is objective but is very different from the objective approach used with the penalized-likelihood formulation). It is also worth noting that case A (the one with fine-grained structure) gets 0.3 weight. Uncertainty in epicentral location is ignored in this study and may be an issue for case A and in regions where there are many events and rates vary substantially between adjacent cells. In other cases or locations, the rate varies smoothly between adjacent cells anyway. According to the catalog, uncertainty in location has been less than 10 km since the mid 1970's. Because this issue affects active regions only, the effect of uncertain epicentral locations is lessened because a significant portion of earthquakes there are well located and there is a large number of them.
Using the seismotectonic or Mmax source zones creates large, unphysical discontinuities in seismicity rates and b-values across the source zone boundaries (e.g., Fig 6.4-4 for Texas, fig	These source boundaries were added by the TI team based on the geology and geophysics. As a result, one should not be surprised to see discontinuities across some of these boundaries.

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6.4-6 western PA; fig 7.5.2-3 Gulf of Mexico).	The use of alternative geometries (multiple seismotectonic, and Mmax maps, one map with no boundaries) should also help alleviate this concern.
Fig 5.3.2-16 is enlightening because it shows the mean rates and b-values from the highest weight source-zone configuration. It's not reasonable that in Fig. 5.3.2-16 the area off the coast of South Carolina (near the Charleston earthquake sources) has lower seismicity rate than the area offshore of Florida. This seems to be another unintended consequence of the "objective" smoothing method applied in a source zone context. This map also shows the artificial north-south division of seismicity rate in the Gulf of Mexico. Can we resolve the higher b-value found for the Oklahoma aulocogen? How well-resolved are the spatial variations in b-value determined from this method? I would doubt that most of these differences are resolvable, given the number of earthquakes. This level of spatial detail is simply not justified by the data. See my comment in the first paragraph about the views of the experts on this subject in the USGS workshops. Why is the b-value prior different from the b-values listed in Table 5.2.1-4?	See response to above comment regarding boundaries. Regarding OKA, Figure 5.3.2-23 of the revised report indicates that b is not well resolved (hence the high uncertainty). The difference in mean b relative to neighboring MIDC (as depicted in 5.3.2-22, as well as this uncertainty, are taken into account in generating the eight alternative maps. The problem is this: the only way to determine the uncertainty in b at the scale of a source zone or smaller is to assume that b is spatially variable. If we assume a-priori that it is constant for the entire CEUS (except for a few exceptions such as Charlevoix), then we will obtain a ridiculously low estimate of that uncertainty. The objective approach is telling us that we have to apply strong smoothing on a (but not go all the way to constant b). This is consistent with our intuition.
I don't personally subscribe to the view that a region is innocent of having a high Mmax until proven guilty, and neither did the people attending our workshops. I think there needs to be a whole separate branch of the Mmax logic tree that represents this point of view. That way people can see this popular opinion directly in the total model. This also forces the report authors to explicitly assign a weight to the view that regions are guilty of high Mmax until proven innocent, rather than perhaps claiming that it is embedded in the result of the Bayesian procedure.	The TI believes that the revised Mmax distributions encompass a wide range of viewpoints
Why exclude the magnitudes of the RLME's from the Mmax determinations for the rest of the CEUS? In particular how can you not use the RLME magnitudes for the Mmax in the source zone where each RLME resides? For example, how can you have a modal Mmax of 5.6-5.8 for the NMESE_W zone (fig. 6.3.2.-5)? This is too low in my view and the view of our workshop participants. Note that the Meers and Cheraw faults are within this zone. Are the authors of the report telling us that these are unique places within this zone? Given the paucity of neotectonic research in this region, how can this be the highest probability logic tree branch? Why not at least use these RLME earthquakes in the Bayesian Mmax determination for the Mmax zone they are in? Similarly, the MESE-N zone contains New Madrid and Charleston, yet has a modal Mmax of only 6.6. By the way, this is lower than the modal Mmax used for NMSE-N (6.8), which is in non-extended crust. This is counterintuitive. Note that the NMSE-N zone contains Cheraw and Meers, yet has a modal Mmax of 6.8. MESE-W contains New Madrid and Charleston and has modal Mmax of only 7.1. For the seismotectonic zones, the dissonance between the RLME's and the Mmax is sometimes profound. Note that the modal Mmax for the Oklahoma Aulocagen zone is 6.5, yet the Meers fault is within this zone. This is a major disconnect. Again the authors are assuming that all the RLME's in the OKA have probably been found. Another problem: modal Mmax of 6.4 in Reelfoot Rift source zone. This contradicts the Marianna paleoliquefaction data. Presumably this would have been the Mmax distribution used for the Marianna zone if this study had been done *before* the Marianna features were found. If so, the modal value would not have represented the magnitude of the Marianna earthquakes. **The SSC Mmax methodology is basically stating that we have probably found all the RLME's in the CEUS. That is likely to be wrong.** So what makes the CEUS tectonically different from the area of the M7.7 Bhuj, India earthquake or the M>7 paleoearthquakes found in Australia? Note that the aftershock zone of the M7.7 Bhuj earthquake was only about 50 km long. There are likely to be many faults of that length throughout the extended margin of the CEUS.	The TI believes that the revised Mmax distributions encompass a wide range of viewpoints. The Mmax distributions allow for the possibility of large magnitudes in all cases. They also accommodate (although perhaps not in the best way) the concept that Mmax is not high everywhere within a seismic source

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The Bayesian method implicitly assumes that there is some spatially variable nature to Mmax, for a given type of tectonic region, so that a prior distribution is formed based on a collection of observed “Mmax’s” from different regions. This implies that the observation of a M7.7 earthquake in a tectonically-analogous region does not necessarily imply a best estimate Mmax of 7.7 for the region of interest. I think it does, within the uncertainty determination of the magnitude. So the SSC does not consider the tectonic analog approach as implemented by the USGS. I strongly think this causes an underestimation of Mmax’s in this study.	The TI believes that the revised Mmax distributions encompass a wide range of viewpoints
Conversion of Mn and mblg to moment magnitude It has been problematic to develop a moment magnitude catalog for CEUS hazard studies, since there are relatively few determinations of moment magnitudes for mblg 2.5-3.5 earthquakes. The SSC uses a constant offset to go from mblg or mn determinations to M (moment mag). I think this may seriously underestimate the true rate of M>=3.0 earthquakes. Figure 3.3.3-1 shows that the constant offset does not fit the “approximate M” data for mn < 3.5 and figure 3.3.3-2 shows the same thing for mblg. I presume the term “approximate M” means the M determination came from the use of S-wave spectral levels, adjusted using the LS fit in Figure 3.3.1-1 and the figures that follow for Boatwright and Macheridas. The report notes that the constant offset fits the data well for mblg or mn > 3.5. However, it’s my understanding that the seismicity rate calculations use earthquakes with M down to 3.0. So it is important to get the M’s correct for these small events. Why not use the locally-weighted LS fits in figures 3.3.3-1 and 3.3.3-2 to convert mn and mblg to M? The results for M3-3.5 would be substantially different than that found from the constant offset. At least this approach should be given some significant weight. At larger mblg’s there should be a crossover as the corner frequency of the earthquake gets around 1 Hz, say about magnitude 5.5-6. So there should not be a constant offset with all magnitudes applied for mn or mblg to get to M.	There are many problems in fitting the small magnitude data, included truncation of the sample and the possibility that there is a change in scaling. That is why we have used only the larger events above about 3.5 and why we have incorporated downweighting of the magnitudes in the lower range in the assessment of earthquake recurrence rates.
Including speculative RLME’s The existence of RLME’s in the Commerce fault zone is uncertain, but this is not reflected in the logic tree.	There is sufficient evidence for repeated moderate to large magnitude earthquakes at sites along the Commerce fault source to define it as a RLME. Our characterization acknowledges that the size and frequency of such events is highly uncertain. In contrast to the current USGS characterization of repeated large magnitude earthquakes in the New Madrid region, in which large magnitude earthquakes having a recurrence interval of ~500 years, are localized in part along the Commerce fault in low weighted alternatives, the CEUS SSC characterization uses site-specific information to develop a more realistic longer repeat time and Mmax probability distribution
Clustering Allowing for the possibility that a source is out of a time cluster is another way to down weight the hazard from that source, for example, the 0.5 weight for clustering of the Marianna source. Given the long recurrence times for this source region, is a cluster model resolvable?	The cluster model is not clearly resolvable. One can consider the 50-50 weighting to indicate that the ~4000 years of no events is just chance
New Madrid The report does a detailed assessment of the hazard in the New Madrid area. Separate sources are used for the Reelfoot fault, southern and northern NM faults, and the Bootheel lineament. However, I don’t see the possibility that the 1811-12 earthquakes (and the 1450 and 900 A.D. quakes) occurred on none of these features, but on faults nearby. I think this is a possibility that needs to be included. Recent reflection surveys have found many possible faults in the New Madrid area, other than the ones inferred from the current microseismicity lineations.	The source faults (and alternatives) defined for this **regional** study are based on a combination of seismicity, geomorphic, and geologic observations that demonstrate evidence for latest Pleistocene- Holocene activity. The Reelfoot thrust fault, in particular, has well documented evidence for being the source of the 1812 earthquake as well as prehistoric earthquakes. It is recognized that there are numerous other potential faults in the Reelfoot rift that may be potential sources of seismicity. Some of these, which are partly coincident with alternative locations for NMSZ fault sources in the USGS model, have been identified as other RLME sources (Commerce and ERM RLME sources). To some extent future large magnitude earthquakes on other faults not specifically modeled are accounted for by the RR source zone. Site-specific studies could further address uncertainties in the locations of the proposed RLME sources if significant to hazard at a specific site.
Is the Oklahoma Aulacogen treated as an RLME? It is shown in Fig. 6.1-1 where the RLME’s	As described in the Meers RLME section, there is a branch of the logic tree where Meers-like

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are indicated. It is listed in Table 4-6 as an RLME source. Yet from the text it appears to be only treated as a seismotectonic zone.	earthquakes occur randomly in the boundaries of the OKA zone.
How is the b-value and seismicity rate calculated for the continental slop source zone (AHEX)? There are almost no earthquakes in this zone.	The “natural floor,” together with the prior on b control the parameters at AHEX.
I thought there was too much certainty implied in some of the geologic narratives for the seismotectonic source zones. Hypotheses are often presented as facts. For example, on page 7-22: the report states that “a mantle plume initiated Iapetan rifting along the Sutton Mountain triple junction…” Do we really know this is what happened? It’s a hypothesis.	This section has been revised..
I wonder if the USGS model was really implemented properly in some of the comparisons. For our Charleston source zones we aligned the strike of the faults and allowed them to extend outside of the source zones.	For the USGS model, Charleston faults are aligned with the orientation of the source and are allowed to extend outside of the source.

DNFSB (R. Quittmeyer) Comments

Comment	Summary of Revisions to Report
Chapter 2 of the report emphasizes the project goal of developing a seismic source model whose component probability distributions are consistent with the center, body, and range of distributions from the informed technical community. This goal is subsequently modified, however, through the implementation of a "hazard-informed" approach to seismic source definition and characterization (Section 4.3.1). Uncertainties that are judged to not affect significantly the overall determination of seismic hazard are, in some cases, not included in the seismic source model. While this practical approach is warranted, care must be taken to clearly indicate in the report the cases in which the informed technical community's distribution is being "trimmed" because the uncertainties have a negligible impact on hazard. It is important for potential users of the CEUS SSC to know if a particular characterization represents the Technical Integration team's assessment of the center and full body and range of the informed technical community's distribution or whether that distribution is a hazard-informed, truncated version. Are uncertainties not included in the model because the informed technical community gives them zero credibility or because including them with an appropriate probability has a negligible impact on seismic hazard? There is a sharp contrast between this approach and the previous EPRI-SOG effort in which evaluation of the seismic potential of all credible tectonic feature-based sources was the goal. It is recommended that the "hazard-informed" adjunct to the concept of the center, body, and range of the informed technical community be introduced in Chapter 2 and mentioned in the Executive Summary. Also, the documentation of seismic source characterization should be reviewed to explicitly identify instances in which the hazard-informed criterion is being applied.	The discussion has been revised to clearly indicate the two parts to a SSHAC assessment: evaluation and integration. The consideration of hazard significance is part of the evaluation process and occurs first in WS1 (sensitivity model) and, on the CEUS SSC project, occurred again (preliminary model) when the important issues were identified. These serve to help in the identification of the pertinent data to be compiled and evaluated (Data sheets) and to identify the issues that should be the focus of the evaluation process (alternative models and methods). This is where the consideration of the data, models, and methods of the larger technical community becomes important and our experience with hazard significance helps direct the TI Team towards those issues that have the most hazard significance. The integration process then follows, which is model-building by the TI Team to properly capture knowledge and uncertainties. There is no "truncation" of this distribution. All possible data, models, and methods are considered during the evaluation phase, and the integration phase ends with a complete SSC model. In the case of the CEUS SSC project, the model-building integration process included the preliminary, draft, and final SSC models. The individual components of that model (represented by the branches and associated weights) is documented in the report. This is completely consistent with the SSHAC process.
It is understood that the Repeated Large-Magnitude Earthquake (RLME) sources are defined to facilitate use of paleoseismic data in characterizing earthquake recurrence. Understanding of the RLME source concept would be enhanced by describing its relation to the characteristic earthquake model and the maximum moment model of earthquake recurrence. Is the conceptual model that of a fault or fault system that ruptures only in large-magnitude earthquakes? Or does it produce a range of earthquake magnitudes and the RLME source decouples the consideration of the large, characteristic events from the smaller events whose magnitudes are exponentially distributed and considered as part of an area source? Also, inconsistencies in the report regarding the magnitude criterion for an earthquake to be considered as a possible RLME (e.g., M > 6 in Executive Summary, M > 6.5 in Section 1.4.1) need to be resolved. It is recommended that the RLME source concept be discussed with respect to its relation to the characteristic and maximum moment earthquake models. Also, inconsistencies in the stated magnitude criterion for a RLME should be resolved.	Discussion of the relationship of RLMEs to the other sources is provided at the beginning of Section 6. The recurrence model has similarities to the characteristic and Mmax models, but is in fact a simple maximum uncertainty model given an assessment of the expected RLME earthquake. The text is expanded to discuss this
Approaches to assess completeness, event dependency, and conversion to a uniform magnitude scale for the earthquake catalog are reasonable and well described. However, there is no discussion of whether these approaches represent the center, body, and range of approaches that the informed technical community would use. Other hazard assessments for the CEUS have used alternative or multiple approaches, which suggests that a range of approaches are considered credible by the technical community (although maybe the technical community is not "informed"). There are no Data Summary Forms or Data Evaluation Forms documenting the approaches considered and the basis for the current assessment. It is recommended that discussions be added to the report on how the technical approaches used to develop the earthquake catalog represent the center, body, and range of the approaches that would be used by the informed technical community, or how a hazard-informed criterion was used to limit the approaches implemented. What approaches were considered, what proponent experts consulted, and what interactions facilitated to understand the strengths and weaknesses of different approaches. [Note that this comment also applies to the approach used to develop recurrence parameters.]	Completeness is assessed using an acceptable methodology that has been indorsed by past practice (i.e. EPRI-SOG) and its use for nearly all COLA applications. The approach used in the CEUS SSC project is to use as much of the available information as possible for assessing the distributions of seismicity parameters. The concept that is being used is the "standard" approach – use the observed catalog of earthquakes to assess the frequency and spatial distribution of future earthquakes under the assumption that earthquakes approximately conform to an exponential distribution and the rate of seismicity in general is stationary. The remaining questions are on the tools for doing so. The TI has presented what we believe to be a logical and systematic use of all of the available information.
Given the importance from a regulatory perspective in establishing confidence that the CEUS SSC represents the center, body, and range of the informed technical community (modified by hazard-informed truncations that have a negligible impact on hazard), consider adding subsections to Chapters 3 (Earthquake Catalog) and 5 (SSC	Additional discussion is added to Section 2 based on NRC (2011) to further define and explain the distinction between the *evaluation* phase of the study where the data, models, and methods from the

Comment	Summary of Revisions to Report
Model: Overview and Methodology) that explicitly address how the methodology approaches that are used represent the center, body, and range of the informed technical community.	larger technical community are evaluated; and the *integration* phase of the project where the center, body, and range of technically defensible interpretations are developed by the TI Team (i.e., the logic trees that comprise the SSC model). Thus, the applicable discussion in Sections 3 and 5 is the discussion of the data, models, and methods that were evaluated as part of the evaluation process. Those references and discussions are now included in the applicable sections. The resulting earthquake catalog and elements of the SSC model (including all branches and weights) are also given in Sections 3, 4, 5, 6, and 7.
Three fundamental interpretations underlie the CEUS SSC: (1) earthquake occurrence is spatially and temporally stationary, (2) the rate of occurrence of different size earthquakes is exponentially distributed, and (3) large earthquakes (or earthquake sequences) that have repeated in the past will have the same magnitude when they next occur in the future. These interpretations are used in assessing earthquake recurrence parameters, earthquake catalog completeness, earthquake dependency, and RLME source magnitude. Bits and pieces of a technical basis are distributed through the report. Given their importance, however, consider enhancing the discussion in the report of their technical basis and the assessment that the informed technical community gives negligible credibility to alternative interpretations or that inclusion of alternative interpretations would have a negligible impact on hazard.	Additional discussion has been added regarding the assessments related to spatial stationarity and the use of the observed record for recurrence modeling. The statements made are not a reflection of the actual SSC model, which allows for a broad range of uncertainties in stationarity and future RLME earthquake sizes.
The CEUS SSC is described as being “useful for engineering applications that will entail up to approximately the next 50 years.” This is an important aspect of the conceptual framework for the model, the implications of which should be discussed more completely. To what degree are assessments by the informed technical community on spatial and temporal stationarity and whether RLME sources are “in-cluster” or “out-of-cluster” dependent on the 50-year perspective? It is recommended that, when an interpretation is colored by the 50-year time frame for application, that fact be explicitly identified and discussed as part of the technical basis.	A hazard analysis conducted to focus on low annual frequencies of exceedance (e.g., 10^{-4} – 10^{-7}) and long return periods (e.g., thousands to millions of years) is not representing the hazard over a long forward time period (e.g., thousands to millions of years). The CEUS SSC model is intended for use over relative short time periods (e.g., tens of years) at low annual frequencies of exceedance. The only issue of significance is the potential for changes in the seismotectonic environment, which are possible over long time periods but not short time periods. Therefore, the assessments made in the CEUS SSC model are based an assumption that the forward time periods of interest (i.e., tens of years) are not long enough to entail significant changes in the seismotectonic environment. Text has been added on the significance of the focus on low annual frequencies of exceedance.
In assessing the center, body, and range of the informed technical community, care must be taken to avoid or mitigate the impacts of motivational and cognitive biases. The report is currently silent on this issue. If the center, body, and range of the technical community are biased, are actions taken to compensate in making an “informed” assessment? Also, care must especially be taken when dismissing “preliminary” and “initial” data that while uncertain, challenge status quo interpretations or when taking a “cautious approach.” In such cases a strong technical basis should be provided that indicates anchoring and under-estimation of uncertainty have been avoided. It is recommended that actions taken by the TI team to avoid or mitigate the impacts of motivational and cognitive biases be explicitly discussed in the report.	The TI Team was informed about the potential for cognitive bias from the beginning of their assessment, and this has been added to the documentation in Section 2. Rather than “dismiss” data, this project has developed and implemented the most exhaustive and documented data evaluation process ever conducted. The evaluation phase of the project included the identification of all applicable data, models, and methods that could potential significance to the hazard. During the integration process, those data, models, and methods that do not have the technical support required to merit explicit inclusion into the SSC model are not included. This is not a dismissal or cognitive bias, it is an honest and transparent data evaluation and integration process.
The report discusses use of the CEUS SSC model in nuclear power plant license applications. For a specific site, it discusses the need to evaluate whether any significant local sources exist. However, it does not address the possibility of distant sources of large-magnitude earthquakes that lie outside the study area. For example, large plate boundary earthquakes in the Caribbean may be important to hazard at low structural frequencies for sites in bordering the Gulf of Mexico. It is recommended that discussions on the use of the CEUS SSC model to assess seismic hazard at potential nuclear power plant sites include the need to consider distant sources of large-magnitude earthquakes occurring outside the CEUS study area.	Clarification has been added in Section 1.3 stating that the CEUS SSC model may need to be refined for site-specific use, depending on the location and regulatory requirements.

Comment	Summary of Revisions to Report
The CEUS SSC model in some sense replaces or supersedes the EPRI-SOG and LLNL models from the 1980's, including updates incorporated in COLA and ESP applications. The report, however, does not give explicit guidance on future use of the EPRI-SOG and LLNL models. Are they considered equivalent regional models that should be discarded? Or do their source zone interpretations need to be addressed as potential local sources in future license applications? It is recommended that the report address explicitly the envisioned role of EPRI-SOG and LLNL models in assessing local seismic sources for hazard assessment at a specific NPP site. Ultimately, this clarification will need to come from the NRC.	Section 1.1 clearly states that the CEUS SSC model replaces previous regional seismic source models given in EPRI-SOG and LLNL. It is also stated that this regional model will be subject to local refinements for purposes of site-specific application. The relationship between this project and other projects commissioned by the sponsors (e.g., COLA and ESP applications) will need to be determined by the sponsors, as will potential changes to regulatory documents that refer to the EPRI-SOG and LLNL models.
In discussing specific earthquakes as they relate to seismic source characterization, sometimes a moment magnitude is provided and sometimes magnitude determined on a different scale. This hinders comparison of the size of different earthquakes. It is recommended that, when earthquake magnitude is given in the text for a specific earthquake, the uniform moment magnitude determined for the earthquake catalog be cited. If desired, the originally determined magnitude and scale can also be provided in parentheses.	A consistent listing of the E[**M**] values are provided
The concept of Data Summary and Data Evaluation tables to supplement the characterization summaries in the main report is a good one. However, the description of the purpose of each type of table needs to be enhanced. Also, quality of the implementation is variable. In the evaluation of data, "Discussion of data use" is often a description of the data and/or their quality without indicating how they were used or why they have a certain level of being relied upon. In at least one case, data indicated as of moderate reliance (3) are not even cited in the main body of the report, while in others data discussed in the report are not listed in the data evaluation table. Also, there is no transparency or traceability of the reason that some considered data (i.e., listed in the Data Summary table) were not used (i.e., not included in the Data Evaluation table). The different groupings of data (by characterization purpose in the Data Summary tables and by data type in the Data Evaluation tables) make it difficult to track information between the tables. It is recommended that the Data Evaluation tables be enhanced to meet the expectations raised by the table column headings and discussion in the main report. The Data Evaluation tables should explain for what aspects of the source characterization a particular data source were used and how they were used. Rationales for the reliance level should be addressed. The tables should be integrated and consistent with the characterization summaries in the main body of the report. Use of a single table rather than two tables might solve integration issues between the tables. Alternatively, if the above is beyond the scope of the current study, the purpose of the tables and their intended level of detail should be clarified.	Significant effort has been devoted to clarifying the entries in the data tables and to make them consistent across multiple seismic sources.

J. Hunt Comments

Comment	Summary of Revisions to Report
Section 1.1.5, Differences from USGS National Seismic Hazard Mapping Project – This section is misleading to me. It should not make any difference that the USGS and the nuclear industry are looking at different AFEs. The two groups are looking at the same basic seismological and geological data, and there really is no good reason as to why a consensus cannot reached about the sources, maximum magnitudes, b values, uncertainties, etc. I understand that the RLMEs would probably not impact 10^{-2} to 10^{-3} AFEs like it would much lower AFEs, but this has definitely had an impact on the USGS seismic hazard for the New Madrid and Charleston areas. I had an occasion to compare seismic hazard studies performed at a site by the USGS (with their ground motion attenuation) and COLA (with EPRI ground motion attenuation), and the seismic hazard values were significantly different. The slopes of the hazard curves were drastically different which is a key parameter in implementing the DOE standards and the latest NRC criteria for new nuclear power plants. It is very easy for a lay person to compare the hazard results at DOE and nuclear power plant sites and to ask some very difficult questions as to why are the results so different, particularly when the USGS seismic hazard results are typically higher. I would suggest that this section be revised to identify the specific differences in source zones, maximum magnitudes, etc. and hopefully state that efforts will be made in the future to resolve these differences, or just delete this section. At some point in time, everybody should be using the same information, including the same ground motion attenuation functions. Hopefully the eastern US ground motion attenuation study to be completed in 2014 will get a consensus on the ground motion attenuation functions.	It is agreed that all parties conducting PSHAs are attempting to define the same thing: the seismic hazard defined by nature. It is also agreed that at some point in the future it is highly desirable for all stakeholders to join together in the hazard assessment. In fact, the CEUS SSC and NGA-East project are the first such attempts at a "consensus" seismic hazard project. However, the current reality is that there are basically two groups conducting PSHAs: the utilities/NRC/DOE and the USGS. The USGS has been heavily involved in witnessing the CEUS SSC project and it is hoped that the future USGS hazard analyses will use element of the CEUS SSC model, such as the earthquake catalog. Unlike the CEUS SSC project, which is focused on the lower AFEs and uncertainty treatment necessary for nuclear facilities, the USGS continues to simplify their models and uncertainty treatment because they are not important to their AFEs of interest. That is the point that is being made in the section and additional discussion has been added for clarity.
I would like to review the seismic hazard results for Chattanooga when they are available, since I know more about the east TN area than other areas. ***I sent him the supplement with these results (Larry Salomone)***	Results for Chattanooga were made available in both draft and final form.
The seismic hazard results for the 5 sites presented are very interesting. In general, the CEUS SSC model 2010 gives the highest seismic hazard and the reason given is the M_{max} of the background source (except for Savannah). If this is indeed the case, it is suggested that a special effort be given to come up with a consensus M_{max} with the USGS and COLA people capturing the different opinions with bigger uncertainties or more clearly explain what new information has lead to different M_{max} between the technical people, and whether or not the USGS and COLA agree with the new. For example, what has changed for the Houston site (low seismic area) that results in such different seismic hazard curves.	Mmax distributions have been re-evaluated and fully documented in the HID for each background source, as well as in the methodology description in the final report.
The comparisons of the seismic hazard using the same ground motion attenuation functions are good because this takes out the variability between different ground motion attenuation functions. At the same time, I think if you compared the USGS seismic hazard (with their ground motion attenuation relationships), that the USGS seismic hazard would be greater than any of the results presented. I would suggest that the USGS seismic hazard also be compared with the results presented. I think this would illustrate the need for the eastern ground motion study and the importance with trying to reach a consensus on the ground motion functions to be used in seismic hazard studies by everyone.	Consideration of alternative ground motion functions is outside the scope of this study.
The comparisons also beg the question as to how these new results compare with the original EPRI/LLNL results. I made a rather quick comparison with the nuclear power plants near the sites and it indicates that the seismic hazard has significantly increased in most cases. I suggest that there should be some very focused discussion about how these new results compared to the old and why the seismic hazard has increased, i.e., what specific new information have we obtained that justifies significantly increasing the seismic hazard.	Comparisons with the original EPRI/LLNL hazard results is outside the scope of this study. Calculation of EPRI/LLNL hazard at the 7 test sites would not be possible for the LLNL study because LLNL hazard codes are not available. Further, ground motion models used in the EPRI/LLNL studies are no longer used in hazard calculations in the CEUS.

SR&I Comments

Comment	Strategy for Response	Summary of Revisions to Report
1. The draft report, section 8.2 states: "Calculations of hazard for all three models use the EPRI (2004, 2006) ground motion equations, so the differences in hazard presented here between the three models is attributable to differences in the source models themselves." It is not clear which of the source characteristics (as listed on page 6 of the presentation to SR&I) contribute significantly to the differences in hazard (for each of the sites and soil conditions). What new earthquake data or methodology (which was not factored in the other 2 models) has resulted in the larger hazard prediction using the CEUS Model across the 7 sites? A discussion on this aspect (perhaps in a tabular format) would help the user understand what is different between the 3 models. A study to show the impact of CAV in the comparisons of the three models would be useful, in light of the fact the CAV was used for new plant licensing efforts. Page 5 of the presentation lists a number of technical advancements. Some appear to be for developing the source model and some appear to be for the hazard calculation methodology. Is this correct? For the results presented in section 8, when the hazard was calculated for each of the three source models and compared, was the hazard calculation methodology identical for each of the 3 models? If different, discuss the differences in the methodology, and it's contribution to the differences in hazard between the three models.	The new earthquake catalog developed as part of the CEUS SSC project is extensively described in the final report. The method of deriving the Mmax distributions for background sources is extensively described in the final report. Comparison of specific details of the COLA and USGS models (earthquake rates, Mmax values, source geometries) with the CEUS SSC model is outside the scope of the current project. Calculation of hazard with CAV is outside the scope of the current project. There were no technical advancements developed in hazard methodology for this project per se, the hazard calculation methodology for the three source models was essentially the same (although different computer programs were used because, for example, USGS input files had already been set up to be read by software that is not used for nuclear power plants). Thus there are no differences in hazard calculational methodology to discuss.	The new earthquake catalog developed as part of the CEUS SSC project is extensively described in the final report. The method of deriving the Mmax distributions for background sources is extensively described in the final report

R. Lee Comments

Comment	Summary of Revisions to Report
In the SSHAC report Table 7-1 "List and Description of Standard PSHA Results", required results include: (a) Fractile and Mean Hazard Curves; and (b) Uniform Hazard Response Spectra. Optional results include: ground motion contour maps. These SSHAC requirements and recommendations apply to facility applications; however, the overall purpose of the requirements is to allow the developer and user to make informed decisions on the technical basis, sensitivities, uncertainties and weights used in the PSHA. Therefore, these SSHAC requirements and recommendations apply to this study. There is no documentation that this study included computation of fractile hazard, uniform hazard spectra or hazard deaggregations (M, R, □) for the TI, experts or the PPRP for the purposes of sensitivity analysis for the seven illustration sites. More critically, there is no documentation that mean and fractile hazard contour or gridded hazard analysis or maps were prepared for sensitivity analysis by the TI, experts, or the PPRP. Gridded hazard, when incorporated in sensitivity analyses, provides essential critical information required to understand model sensitivities for a regional hazard study. Mean hazard was computed for seven illustration sites, however the necessary computational results to make hazard informed decisions (gridded mean and fractile hazard) were not provided to the TI team, proponents/experts and the PPRP to adequately perform their assignments. Consequently, from a SSHAC perspective, this study needs significant additional work before the community, users and owners can confidently accept that the study was conducted as a SSHAC Level 3 study. A cost effective approach of this regional SSC was to combine regulatory accepted COLA models into one CEUS model with expert and topical workshops for a TI to incorporate expert opinions to achieve the body center and range of the informed technical community. However, for a model of this importance, the critical missing supporting data necessary to make hazard-informed judgments related to background seismicity and RLME source geometries (and development of corresponding model weights) are of such importance that the report should be revisited and revised prior to this study being used. Because gridded hazard is necessary for both development of the SSC model and is useful for potential applications of the study, it is not sufficient that gridded hazard be developed as a follow-on to this study but rather that the process of model development, weight selection and user buy-in should be revisited with gridded and fractile hazard made available to the TI, experts and PPRP to complete the study.	(1)The project scope was to develop a SSHAC Level 3 seismic source model, not SSHAC Level 3 seismic hazard results at any site. The example results are presented as illustrations, not to be used for design. (2) Uniform hazard spectra and deaggregations are not part of the project scope. Gridded hazard maps are not part of the project scope.. (3) The CEUS SSC model started from original scientific research and references on earthquake causes in the CEUS, not from combining COLA models. The supporting data and references for the background sources and RLME sources is included in the final report. (4) Calculation of gridded hazard maps is not part of the project scope Mean and fractile seismic hazard curves are presented for all 7 test sites in the final report, along with extensive sensitivity studies at all 7 test sites to illustrate the sensitivity of seismic hazard to alternative assumptions
Regional vs Site-Specific There are several areas in the report, including the Executive Summary that appropriately note that the source characterization is regional in nature and not site-specific. The course of action needed for a user to develop a site-specific PSHA is only weakly touched on (see specific comments below) and is generally stated as "The regional seismic source characterization (SSC) model defined by this study can be used for site-specific PSHAs, provided that appropriate site-specific assessments are conducted as required by current regulations with the focus provided by regulatory guidance." This sentence underemphasizes an important and potentially costly caveat that is not adequately treated in the Executive Summary or elsewhere in the report. To make the study site-specific requires an intensive investigation of potential seismic sources within 40-km of the critical facility, an evaluation of potential impacts and possible incorporation of additional sources in the hazard evaluation. This application caveat should be made very clear in the Executive Summary. The report should then state very clearly	The report makes it clear that the regional SSC model coming from this study must be locally refined for site-specific applications, depending on the applicable regulatory guidelines. The applicable regulatory guidance is a function of the agency using the model. For example, NRC-regulated facilities may need to follow RG 1.208 while DOE facilities may be following ANSI/ANS-2.29-2008. Because this study is jointly sponsored by a variety of organizations with their own regulations, it is not appropriate in this report to specify all of the steps that might be required for any particular site-specific application.

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the necessary steps that the user can be expected to take in order to make a site-specific assessment. These steps should be enumerated in the Executive Summary and Introduction. Chapter 9 of the report contains a section on the necessary studies for site specific application of the study. This Chapter 9 section consists of only two sentences. The necessary studies for a site-specific application of the SSC model are described: "local geologic and tectonic interpretations, topographic data, geophysical data, historical seismicity data, micro-seismicity data, paleoseismic studies, and any other data, interpretations, or investigations that might indicate local seismic sources that could affect the site." This discussion should be expanded so that potential users of the study can clearly understand the steps necessary to meet regulatory requirements. The Executive Summary and Conclusions should also summarize and reference this revised section of the report.	
Selected Sites for Hazard Evaluation Mean hazard is computed for seven sites (Topeka, Kansas, Central Illinois, Houston, Texas, Jackson, Mississippi, Chattanooga, Tennessee, Savannah, Georgia, and Manchester, New Hampshire) using the EPRI (2004, 2006) GMPEs. According to the report, the purpose of the hazard evaluation for the seven sites was for (1) "illustration"; (2) "evaluating the significance of various SSC issues and providing that information as feedback to the TI Team"; and (3) for "comparison with other hazard studies." There was no documentation of other hazard assessments or gridded hazard evaluations conducted for this study, other than the seven hazard assessment locations. The documented hazard assessments are inadequate to assess the impacts of the new model or compare to other hazard results including EPRI-SOG, LLNL and the USGS National Map (USGSNM) at sites of interest to the DOE or NRC. Because gridded hazard comparisons are not available for review, DOE, utility and other potential users will not understand the ramifications of the SSC-related hazard unless a site-specific evaluation and related sensitivity studies are completed. DOE and the NRC could complete this portion of the study as a follow-on of the report.	(1)Calculation of gridded hazard is not part of the project scope. Sensitivity studies were provided to the TI team for evaluation purposes, as were comparisons with other hazard results at the 7 test sites. Comparison of hazard with EPRI-SOG and LLNL is not part of the project scope and would not be meaningful because the ground motion equations used in those studies are outdated. This would require a lot of work to conclude that the ground motion models are different. (2)Regarding ramifications of the CEUS SSC model, this comment recognizes that those results are site-specific. It is not part of the project scope to provide hazard results on a gridded basis for the entire CEUS. The final report contains (a) mean and fractile seismic hazard calculations at the 7 test sites, (b) extensive sensitivity comparisons that have been reviewed by the TI team, and (3) extensive comparisons of CEUS SSC seismic hazard at the 7 test sites to hazard calculated from the COLA and USGS (2008) models.
CEUS SSC Longevity There are a number of instances in the report that mention the expected longevity of the study: "It is expected that the longevity for studies such as the CEUS SSC Project will be at least 10 years before there will be a need for a significant revision." Rather than presume such an optimistic assessment, why not develop an annual or semi-annual review mechanism for updating the study? The report should be expanded on this topic to include an annual or bi-annual review mechanism so that the SSC database can be reviewed and updated on a regular basis.	Sponsor's Perspective section revised to respond to this comment and refers to the guidance in the 2011 NUREG covering SSHAC guidance.
CEUS SSC Earthquake Catalog There were considerable resources expended in updating the CEUS earthquake catalog. The report should document the differences between the updated catalog and other catalogs currently used such as the National Map earthquake database. It is a simple matter to identify missing or duplicate events and illustrate differences in magnitude completeness and magnitude bias. This effort would add considerably to the report and provide insights to the user on the stability of earthquake catalogs.	Information on additions/modifications to the USGS catalog are provided in the text and in the master catalog file to be included electronically
Spatial b- and a-Value Determinations The penalized-likelihood approach was developed in this study for the purpose of developing spatial a- and b-value models for background seismicity. Based on the limited comparisons available in Chapter 6 and 7, there appears to be a consistent and significant bias between observed and modeled rates of seismicity, especially for the larger magnitudes (Mw>4). This bias is noted in the report in both chapters: "This is the result of deviations of the data from the	The method has been refined and made more robust by using magnitude bins instead of continuous magnitudes. This, together with a careful re-evaluation of the catalog and better values of the prior value for b, has resolved the problems identified by the reviewer.

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exponential magnitude distribution." It appears that the model predictions for Mw 6 and greater range from 2-6 times the observed mean rates. Also, the rate predictions appear to be too tight as compared to the uncertainty of the data. The "full" and "reduced" magnitude weight rate predictions seem to heavily favor the more numerous lower magnitude earthquakes. The net result seems to be rate predictions that consistently exceed observed data at earthquake magnitudes that drive hazard predictions. A consistent under-prediction of b-values could lead to this result. It appears that the average b-value developed in the model is near 0.8 while the National Map model uses 0.95. A biased-low b-value can result in substantially higher and biased mean hazard. Besides the apparent bias in rates and b-values, the penalized-likelihood approach resulted in disappointingly low variability in both a- and b-values. The zone seismic activity rates indicate that all zones with the exceptions of GHEX, IBEB, OKA and SLR indicate higher predicted rates and lower b-values than suggested from the observed earthquake data. This apparent bias should be resolved by the project and then reviewed before use is made of the study.	
Consideration of Constant b-value Kernel Approaches Although there is some discussion for the rationale of why the constant b-value approaches are not used, there is no definitive documentation of why the constant b-value approach is not credible when a substantial number of the informed technical community uses the constant b-value approach. This decision seems at odds with the SSHAC approach. Also, fully weighting a relatively new and unpublished approach (the penalized-likelihood approach) without direct comparisons of rates and b-values used in other state of practice seismic source characterizations seems contrary to a conservative practice. If the constant b-value approach has no credibility, the report should document that.	Section 5.3.2.4 provides the bases for selecting the approach over the other kernel approaches and reference to it is added to Section 5.3.1. The revised Section 5.3.2.4 includes comparisons to the kernel approach (Figure 5.3.2-42), obtaining very similar results. The TI team felt that, given the large size of some of these source zones, it was preferable not to adopt a constant b as an a-priori assumption. In the end, the objective-smoothing approach arrived at maps with a mild spatial variation in b (except in SLR), which is not too much at odds with the assumption of constant b and is consistent with our intuition. Another consideration is that the only way to determine the uncertainty in b at the scale of a source zone or smaller is to assume that b is spatially variable. If we assume a-priori that b is constant for the entire CEUS (except for a few exceptions such as Charlevoix), then we will obtain a ridiculously low estimate of that uncertainty. The penalized maximum likelihood approach developed for the CEUS SSC project is a refinement of the EPRI-SOG (1986) approach, which is part of an SSC model accepted by the NRC and endorsed in Reg Guide 1.208 and has seen common use throughout the technical community nearly all Combined Operating License applications filed to date.
Executive Summary: 1. Page v, 1st par. Spell out clearly what is meant by "regional seismic source model" as it is critical to the summary 2. Page v, 2nd par., 1st sentence. States "The regional seismic source characterization (SSC) model defined by this study can be used for site-specific PSHAs, provided that appropriate site-specific assessments are conducted as required by current regulations with the focus provided by regulatory guidance." This sentence glosses over an important and potentially costly caveat that is not adequately treated in the Executive Summary or elsewhere in the report. To make the study site-specific requires an intensive investigation of potential seismic sources within 40-km of the critical facility, an evaluation of potential impacts and possible incorporation of additional sources in the hazard evaluation. This should be made very clear in the ES, 3. Page ix, 2nd par. Evaluation of the mean PGA and 1-hz hazard at seven demonstration sites for the purposes of "demonstration" are not adequate for purposes of application to DOE sites and likely potential reactor sites. This issue is discussed above in the context of	1. Revised as suggested. 2. This study has multiple sponsors and multiple users., therefore the regulatory guidance regarding the use of the study will vary for the particular application. It is noted that additional site-specific refinements will be necessary depending on the applicable regulatory guidance. 3. Sufficient seismic hazard calculations were conducted for purposes of developing the CEUS SSC model. 4. Accuracy implies that there is an objective procedure for assessing whether a PSHA is correct or not. Precision can be assessed and is the proper term in this context. 5. The final hazard calculations include fractile and source contribution information. These do not change the conclusions regarding the level of precision in seismic hazard estimates.

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how the TI made hazard informed judgments of the RLMEs, usefulness for reviewers and usefulness for utility and the DOE. 4. Page ix, last par. Discussion of level of precision in seismic hazard estimates seems to be void of consideration of the uncertainty in the hazard as depicted by fractiles. Should the discussion center on precision or accuracy? 5. Page ix, last par. Discussion of level of precision in seismic hazard estimates. "Based on the precision model evaluated, if an alternative assumption or parameter is used in a seismic hazard study, and it potentially changes the calculated hazard (annual frequency of exceedence) by less than 25 percent for ground motions with hazards in the range 10-4 to 10-6, that potential change is within the level of precision with which one can calculate seismic hazard." This assessment is made without the availability of fractile hazard nor source deaggregation evaluation at the seven evaluation sites.	
Chapter 1: 1. Page 1-1, 1st sentence. Be consistent with terminology, change "…a full regional seismic …" to "…a regional seismic …". 2. Page 1-1, second paragraph. "The regional SSC model provided by this study can be used for site-specific PSHAs, provided that the appropriate site-specific refinements- as provided by current regulations and regulatory guidance- are applied." This caveat is provided here and elsewhere in the report and with only one exception, without any discussion of what the caveat entails. The report should state very clearly the necessary steps that the utility or DOE site can be expected to take in order to make a site-specific assessment. These steps should be enumerated in the 1) Executive Summary; 2) Introduction; and 3) Conclusions. 3. Page 1-3, 3rd par. The model will certainty be useful to the utilities and the DOE, however the CEUS SSC report falls short of providing even preliminary hazard results in the vicinity of these facilities using EPRI GMPEs. 4. Page 1-4, 1st par. This paragraph states that the National Map "are focused on AEFs in the range of 10-2 to 10-3/yr…" while this study focuses on the range of 10-4 to 10-6/y. The National Map is a regional study that can be applied to building code requirements using site-specific corrections. The National Map has been previously applied to DOE PC-3 facilities, has been thoroughly documented, and reviewed in public meetings with expert feedback as input on controversial issues. This section could be simply reworded to something like "The National Map is not intended to be used for nuclear power plants". 5. Page 1-6, last sentence of Section 1.2.3. "It is expected that the longevity for studies such as the CEUS SSC Project will be at least 10 years before there will be a need for a significant revision." Rather than presume such an optimistic assessment, why not develop an annual or semi-annual review mechanism for updating? 6. Page 1-10, Section 1.4.4.1. These summaries contribute nicely to the documentation of the report and set the standard for reference documentation for future SSHAC Level 3 studies. 7. Page 1-10, Section 1.4.4.2. The geologic, geophysical and seismological database that will be available on a public website is an excellent idea and will be extremely helpful to future CEUS updates. 8. Page 1-11, Section 1.4.4.4, 1st sentence. Delete phrases like "emerging use" with regard to paleoseismic data as it has had profound impact on EUS seismic hazard since the early 1990s. 9. Page 1-11, Section 1.4.4.5. This section clearly states that the results of this study are not a CEUS PSHA but the source portion of that evaluation. Further, hazard is computed for seven sites using the EPRI (2004, 2006) GMPEs (Topeka, Kansas, Central Illinois, Houston, Texas, Jackson, Mississippi, Chattanooga, Tennessee, Savannah, Georgia, and Manchester, New Hampshire). According to this section, the	1. Revised as suggested. 2. Clarification added that the regulatory framework for any site-specific application must be considered. Because of the variety of potential users and organization, there is no specification of the site-specific steps that will need to be followed. 3. As discussed in Section 8, the purpose of the hazard calculations is to understand the SSC model for a range of source characteristics. Hazard calculations at particular sites will be conducted by users of the SSC model at their particular sites and site conditions. 4. Additional explanation provided related to the different purposes for the two studies. 5. Recommendations for review and updating are not part of the scope of this project. 6. So noted. 7. So noted. 8. Revised as suggested. 9. The conclusion is not correct. The multiple rounds of model-building, hazard calculations, and feedback greatly benefitted the TI Team in their assessments and in understanding the importance of all elements of the SSC model. Chapter 2 1. So noted. 2. This is added to the ES and Chapter 1. 3. As discussed in this section, the hazard information developed as part of this project as well as experience with hazard sensitivity on other projects was used to prioritize the points of emphasis in the SSC model. 4. Reference simply made to the discussion in Section 8. 5. All will be included on the project web site.

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purpose of the hazard evaluation for the seven sites was for (1) "illustration"; (2) "evaluating the significance of various SSC issues and providing that information as feedback to the TI Team"; and (3) for "comparison with other hazard studies." Item (1) was achieved, but (2) and (3) were not adequate for the TI or the experts. Chapter 2: 1. This chapter provides a very nice historical perspective on implementation and development of the SSHAC process. 2. Page 2-38, 2nd par., last sentence. "No new data were collected…as part of the CEUS SSC Project." This is an important point and should also be mentioned in the Executive Summary and Introduction. 3. Page 2-38, last sentence under 2.4.2 Identification of Significant Issues. "…throughout the project an effort was made to keep the project "hazard informed" in the sense that highest priority would be given to the issues having the most significance to the hazard results." See general comment above. 4. Page 2-43, 4th paragraph. "It was concluded that that the calculated overall level of precision in mean hazard estimates corresponds to a precision in ground motion of +-8 percent." This precision estimate is stated in terms of hazard in other parts of the report… 5. Page 2-43, last par. I have been unable to locate Workshop #3 CD or its contents on the EPRI website.	Chapter 2 1. No revision required. 2. Revision made as suggested. 3. No revision required. Sufficient hazard information was provided to the TI Team for them to understand the relative importance of the SSC model components. 4. Sentence deleted. The topic is discussed in detail in Section 9. 5. WS3 proceedings can be found on the EPRI website http://my.epri.com/portal/server.pt?Abstract_id=000000000001019220
Chapter 3: 1. Where is the SSC update catalog compared to say the USGS NM catalog? 2. How do the number of earthquakes and final magnitudes compare to other catalogs? This would seem to be an easy comparison and I cannot find it in the documentation. For example, how many earthquakes are missing or double counted in the old vs new catalog and how do the magnitudes compare?	The updated catalog contains moment magnitude as the final size measure. Therefore, comparison of these magnitudes with magnitudes in other catalogs would be expected to show the same magnitude scaling effects as developed for the conversions. The comparison of the number of earthquakes added to the USGS catalog is made in Section 3.1
Chapter 4: 1. This chapter provides a useful and thoughtful introduction to the analysis. 2. Chapter 4, page 4-1. This section states that nearly all PSHAs developed for nuclear facilities in the CEUS have been conducted by members of the TI Team. This is a positive and necessary attribute in the sense that the TI has experience and knowledge of the issues and has knowledge of NRC accepted COLAs. Thus the COLA proponents were well represented. How were other model proponents incorporated in the assessments? There is certain worth in developing SSC models that are in line with COLAs, however the goals of the program seemed higher in that the SSHAC Level 3 effort would capture the body and range of the informed technical community. 3. Section 4.3 Methodology of Identifying Seismic Sources, page 4-9. Criteria for regional vs. site-specific is defined. "Sources within 40 km (RG 1.208) are too site-specific to be included on systematic basis in the CEUS SSC source model". "A more reasonable criterion that was applied…the CEUS SSC model provides the regional	1. So noted. 2. The point of the statement is to indicate that the TI Team is experienced in the process of seismic source characterization. They are not "proponents" of the COLAs or any other PSHA conducted. In fact, not all members of the Team were involved in the COLA models. They were aware of and strived to consider all data, models, and methods within the larger technical community, as per their role on a SSHAC Level 3 project. 3. This concept is captured elsewhere in the report, as suggested. 4. This concept is captured elsewhere in the report, as suggested. 5. The multiple rounds of model-building and hazard calculations, coupled with experience on similar hazard studies, provided ample information to assist with prioritizing the SSC efforts. 6. Table numbering corrected. 7. The point of the 50 yr reference is to place the temporal context of the study in the timeframe of renewal processes. Other facility types, such as nuclear repositories, have a different period of applicability and this would have a

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characterization of sources on a consistent basis throughout the study region, including those special areas that have been the subject of considerable scrutiny in the past. Consideration of site-specific refinement of the CEUS SSC model would be required by current regulatory guidance and would occur only if such refinement would lead to significant differences in the hazard." This criterion is appropriate and acceptable and should be clearly identified in the first sections of the report. 4. Page 4-8 and 4-9, Section 4.3 Methodology for Identifying Seismic Sources, last sentence: "The CEUS model provide the regional characterization of sources on a consistent basis throughout the study region, including those special areas that have been the subject of considerable scrutiny in the past. Consideration of site-specific refinement of the CEUS SSC model would be required by current regulatory guidance and would occur only if such refinement would lead to significant differences in the hazard." Earlier in the same paragraph, "Clearly, local tectonic features that lie entirely within the 8 km (5 mi.) radius site area, and likely the 40 km (25 mi.) radius site vicinity, as defined in RG 1.208, would be too site-specific to be included on a systematic basis in the CEUS SSC source model." These words may be also appropriate in the Executive Summary and Conclusions. 5. Page 4-9 through 4-11, under Hazard-Informed Approach, specifically, page 4-11, 2nd par.: "To further identify and understand the issues of most hazard significance, seismic hazard calculations were conducted using the SSC sensitivity model prior to Workshop #3 for a review of sensitivity cases. The issues identified as having the most hazard significance were as follows:". There are an inadequate number of site hazard evaluations to make a hazard informed opinion of on source geometry sensitivities for RLMEs, source geometries and smoothing for moderate magnitude sources and smoothing and probability of activity for background sources. 6. Page 4-18, 3rd sentence. Table 4.4.1.1-3 should be Table 4-6. 7. Page 4-19, 4th sentence. The reference to the next 50-yr engineering application seems irrelevant to source characterization. 8. Page 4-22, 3rd par. Figures 4.4.1.3-6 and 7 are missing from the report. 9. Page 4-22, 3rd and 4th par. Discussion for second node in Figure 4.4.1.3-1 "Rough Creek Graben Association" is missing.	different effect on the use of a renewal model. 8. All figures are now included. 9. Discussion has been added.
Chapter 5: 1. page 5-3, 3rd par. Why is it tempting to apply non-Poissonian models to non-RLME sources? This paragraph is confusing. Excluding RLMEs, what seismic data support non-Poisonian models? 2. page 5-6, "approaches to Mmax estimation…". Consideration and weighting of both the Bayesian and Kijko approaches to Mmax seems appropriate, but where are the comparisons? There would be an expected difference in how the prior distributions are handled but what are these differences? 3. Page 5-10, reference to Appendix K. It would be helpful to incorporate global maps w/ SCR identified to describe contents of this appendix. 4. Page 5-18, 3rd paragraph. Please provide additional explanation of why the Kijko approach results in a zero weight for the NAP case? I'm confused with what is going on with the magnitude distributions in this case. 5. Page 5-19, Section 5.3 Earthquake Recurrence Assessment. This section is difficult to follow. Because the methodology has not been published the section may benefit from additional graphics or flow charts. 6. Page 5-29, next to last paragraph. Why is the prior b-value (0.81) and spatially determined b-values appear to be so low as compared to b-values used in other studies (0.85-0.95)? Why so different from fixed b-value models such as the USGS National Map?	1. Paragraph has been clarified. All non-RLME sources are Poissonian. 2. Comparisons between the Mmax distributions based on the Bayesian and Kijko approaches are included. 3. The SCR domain maps will be included in the project database on the web site. They are too big for the report 4. The Kijko approach gets zero weight because the probability that Mmax > 8.25 is greater than 0.5 5. A number of explanations and a few graphs were added. 6. This issue has been resolved in the revised report and results. 7. There are several reasons for these differences. The objective approach used in the CEUS SSC study is sensitive to the density and pattern of earthquakes in a source zone, as one would expect Also, the range between high and low smoothing used by EPRI-SOG is rather narrow (they also used full smoothing, which would be off the lower end of the scale). Finally, the conversion of smoothing parameters from one-degree to ¼-degree cells is not perfect, so that the location of the EPRI-SOG equivalent values is only approximate. 8. Color maps are provided for all source zones. 9. This is not feasible, given the current schedule. 10. This issue has been corrected in the revised results. 11. These results have been revised. As one looks at a recurrence at a smaller scale, some of these differences will arise. At the same time, the error bars are

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7. Page 5-30, 2nd and 3rd paragraphs. Explain why the new recurrence formulation results in apparently wildly differing variability on □ (both higher and lower) as compared to EPRI/SOG while variability is larger in □ and also exhibits a bias as compared to EPRI/SOG. 8. Page 5-30, 4th and 5th paragraphs. The provided perspective plots in Figures 5.3.2-6 through 9 are not adequate to read a- or b-values. I recommend that contour plots be provided for both parameters and these be provided for each source zone. 9. Page 5-30. A comparison of average a and b-values rates for each of the zones as compared to EPRI/SOG and USGSNM would be helpful 10. Page 5-31, par 3, reference to Figure 5.3.2-16. Map of mean recurrence parameters suggest very low b-values as compared to the USGSNM… 11. Page 5-31 through 5-33, Section 5.3.2.3 Exploration of Model Results in Parameter Space. Referenced Figures 5.3.2-20 and 21, appears that the derived b-values are too low for E. Tennessee and too high for Nemaha Ridge Area Figures 24 and 25. 12. Figure 5.3.2-19 caption should read "Map of geographic areas considered in the comparison of simulated and observed earthquake counts." 13. Page 5-32, 2nd par. Figures 5.3.2-20 and 21do not appear to show good agreement between model and data as mentioned in text. See comment above. 14. Page 5-32, last par. Change "The penalized-likelihood approach developed here does not have this problem". To read "In a mean sense, the penalized-likelihood approach developed here does not have the problem of low or zero seismicity rates". 15. Page 5-33, 1st par. Figure 5.2.3-16 should be 5.3.2-16. 16. Page 5-33, 1st par. Reference to Figure 5.3.2-26 is comparing earthquake occurrence rates to spatial maps of ground motion hazard. This comparison does not seem appropriate. 17. Page 5-33, Section 5.3.2.4 Consideration of Constant b-value kernel approaches. Although there is some discussion for the rationale of why the constant b-value approaches are not used, the rejection (zero weight, no credibility) to a fundamental modeling approach used in the USGS National Map and other PSHAs for critical facilities appears to be counter to the SSHAC goal of capturing the range of the informed technical community. Also, fully weighting a relatively new approach (the penalized-likelihood approach) without direct comparisons of rates and b-values used in other state of practice seismic source characterizations seems contrary to conservative practice. 18. Page 5-43, Section 5.4.1.5, Fault Rupture Area. SSC model uses only the Somerville model for fault rupture area, because others "leading to very similar results". Can this be documented in the report? 19. Page 5-43, Section 5.4.1.7, Relationship of Rupture to Source Zone Boundaries. If earthquake epicenters are constrained to lie at the midpoint of the rupture length, would that assumption be unconservative for sites lying outside "strict" boundaries?	also becoming broader. Also, these are 1-sigma error bars, so there is a ~ 30% probability of being above or below the error bars, even under ideal circumstances. 12. We appreciate the suggestion. 13. Fit is better in revised results. Also, see response to 11. 14. We appreciate the suggestion 15. Corrected 16. USGS map has been removed. 17. This issue was addressed in the response to an earlier comment.
Chapter 6: 1. Page 6-61, last par. Figures 6.2-1 and 6.2-2 should be reversed, captions stay the same. 2. Page 6-70, 1st and 3rd par. "Figures 6.3-1 through 6.3-6" should be "Figures 6.4-1 through 6.4-6" and "6.3-7 through 6.3-16" should be "6.4-7 through 6.4-16" 3. Page 6-70, Section 6.4.1 and Figures 6.4-1 through 6.4-6. Please explain why the apparent resolution of the rate maps appears to be greater for full magnitude weights vs. reduced magnitude weight cases. Also explain why the b-value maps are much lower (~0.8) as compared to the fixed b-value maps developed by the USGS (~0.95). 4. Page 6-70, Section 6.4.2 and Figures 6.4-7 through 6.4-16. The bias between the	5. No revisions to the Charleston RLME model were made based on this comment. However, the text was revised for clarity and to provide additional rationale for source characterization. Specifically, greatest weight in the model is given to possibility of an onshore source, located at/near Charleston/Summerville (i.e., the combined weight of Local and Narrow sources is 0.8). The Local zone is already weighted highest (0.5) based largely on recent papers by Chapman and co-authors. However, weight is also given to other models because Chapman has yet to demonstrate post-Eocene slip from reprocessed seismic-reflection data. Also, we cannot preclude possible offshore seismic source based on distribution paleoliquefaction features.

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model rates and data is noted in this section with the statement that "This is the result of deviations of the data from the exponential magnitude distribution." It appears that the model predictions for Mw 6 and greater range from 2-6 times the observed mean rates. The rate predictions appear to be too tight as compared to the data. Also the "full" and "reduced" magnitude weight rate predictions seem to heavily favor the more numerous lower magnitude earthquakes. The net result seems to be rate predictions that consistently exceed observed data at earthquake magnitudes that drive hazard predictions. A consistent under-prediction of b-values could lead to this result. 5. Charleston Source: Mmax, recurrence rate models and weights seem appropriate and well justified. The three selected and weighted source zones are not sufficiently justified. Recent Chapman papers suggest greater basis for "local" zone. The "regional" zone is diluted because 1/3 of zone is in the Atlantic with no corresponding historic or paleoseismic basis for doing so other than a single and likely poorly located earthquake on an offshore feature defined by seismic reflection work and is considered inactive (Helena Banks fault zone). The technical justifications of this zone may be risky to the users of the model because if may appear that relatively "weak" data are used to reduce inland hazard and other earthquakes that have historically occurred inland and over Miocene structures are ignored.	
Chapter 7: 1. Section 7.3.1. There is only one figure accompanying the detailed nine page geologic discussion on the St. Lawrence Rift. The section reads as though it was cut and paste with figures omitted from the source. I think this section could be improved by adding descriptive figures. If detailed figures cannot be added, it may be more desirable to remove most of the text and simply reference the source document. 2. Section 7.3.2 Great Meteor hotspot. Same general comment as 1. (Section 7.3.1), except Figures 7.3.2-3 and 7.3.2-4 are referenced but missing from the document. 3. Section 7.3.3 Northern Appalachian. Same general comment as 1. (Section 7.3.1). 4. Section 7.3.4 Paleozoic Extended Zone. Same general comment as 1. (Section 7.3.1). 5. Section 7.5.1 Rate and b-value Maps for single zone and two zones. Same comment as in Chapter 6: explain why the apparent resolution of the rate maps appears to be greater for full magnitude weights vs. reduced magnitude weight cases. Also explain why the b-value maps are much lower (~0.8) as compared to the fixed b-value maps developed by the USGS (~0.95). 6. Section 7.5.1. Comparison of gridded hazard for these rate cases would have been extremely helpful for the TI team and the reader. 7. Section 7.5.2 Comparison of Recurrence Parameters to Catalog. Similar comment as in Section 6.4.2: The bias between the model rates and data is noted in this section with the statement that "This is the result of deviations of the data from the exponential magnitude distribution." It appears that the model predictions for Mw 6 and greater range from 2-6 times the observed mean rates. The rate prediction models appear to be too close as compared to the data. Also the "full" and "reduced" magnitude weight rate predictions seem to heavily favor the more numerous lower magnitude earthquakes. The net result seems to be rate predictions that consistently exceed observed data at earthquake magnitudes that drive hazard predictions. A consistent under-prediction of b-values could also lead to this result.	
Chapter 8: 1. Many figures that are provided are inadequately described: 2. Hazard fractiles should be computed to understand uncertainties. 3. Figure 8.2-1A. What is the "$STUDY_R$" curve in this and other figures? 4. Figure set 8.2-2 for Chattanooga site is missing.	 1.Descriptions of all Section 8 figures are included in the final report. 2.Fractile hazard curves are included in the final report 3. The STUDY_R source is the Study Region source, which is described in earlier sections of the final report. 4. Chattanooga figures are included in final report

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5. Figure set 8.2-5 Manchester site is missing.	5. Manchester figures are included in final report
Chapter 9: 1. The Hazard Input Model (HID) (Appendix H) is a critically needed product of this study and unfortunately the digital files were not available for review. I strongly recommend that DOE critically review the HID when it becomes available. 2. Page 9-1, 1st par. Next to last sentence. "In this sense, the SSC model has been "optimized" to include only those assessments that capture the community's views and that are believed to be significant to the hazard. Once this level of uncertainty treatment was reached, there was no further attempt to optimize or reduce the complexity of the model for purposes of subsequent calculational efficiency." What do these sentences mean and how was this concept implemented? 3. Page 9-1, 2nd par. As mentioned above, the regional hazard model conversion to a site specific model needs serious discussion and guidance. This part of the report would be an ideal point to consider these aspects for the potential user. 4. Page 9-3, 2nd and 3rd par. Sensitivity studies conducted by looking at the impacts on hazard of each of the logic tree branches is an appropriate and necessary methodology, however, judging hazard results/impacts from only seven sites in the entire CEUS is totally inadequate technical basis for the TI team. As suggested by SSHAC, gridded hazard is a necessary ingredient for controversial issues. In addition, for a regional hazard map, it should be a necessary requirement. Suggestions for additional SSHAC minimum requirements for a regional hazard model: (1) gridded 1- and 10-hz hazard for 500, 2500, and 10000 years at 10-km spacing. For sensitivity studies, gridded hazard can be computed using mean based parameterizations to improve computational efficiency. 5. Page 9-3, 5th par. This paragraph states that any (hazard) sensitivities to alternative geometries of the Charleston RLME will be accentuated at the Savannah Site. This statement is not demonstrated in the report and cannot be because of the lack of gridded hazard used in the sensitivity studies. 6. Page 9-3, last par. Rupture orientation may be more sensitive at the Savannah site (but was not demonstrated), however RLME source model orientation sensitivity is certainly not a maximum at the Savannah site. The hazard sensitivities completed for the seven sites is likely inadequate for the conclusions made in this chapter. 7. Page 9-3, last through 9-46. The hazard sensitivities completed for only seven sites may be inadequate to support the conclusions made in these sections. 8. Page 9-47, last par. This section consists of two sentences to describe the necessary studies for a site-specific application of the SSC models including: "local geologic and tectonic interpretations, topographic data, geophysical data, historical seismicity data, micro-seismicity data, paleoseismic studies, and any other data, interpretations, or investigations that might indicate local seismic sources that could affect the site." This section should be extensively expanded so that potential users of the study can understand the necessary efforts to meet regulatory requirements. The regulatory requirements should be identified and summarized in this section. The Executive Summary and Conclusions should also summarize this section and point to this section. 9. Page 9-48, Section 9.4.1 Data available to evaluate the precision of seismic hazard estimates. The estimates of COV derived from other studies are useful, but I don't understand why the COV is not computed directly from the mean fractile hazard evaluations. The contribution to COV from GMPE, background source, RLMEs can be easily resolved. 10. Page 9-53. Figure 9.4-3 should be 9.4-43; all following figure numbers are off by 40. 11. There is quite a detailed section on "precision" in the mean hazard and how close an alternate team of experts might come to the derived mean hazard. The section also	1. HID is included in final report 2. Text has been revised 3. Chapter 9 has been revised 4. Calculating seismic hazard at more than 7 test sites is not part of the project scope. Calculating seismic hazard on a grid of points with 10 km spacing is not part of the project scope. 5. It is not necessary to demonstrate that hazard sensitivity to source geometry is higher if a site is close to that source, and lower if the site is farther away. This is a general statement that reflects experience with hazard results. 6. It is not stated that RLME source model orientation sensitivity is a maximum at the Savannah site. The proximity of Savannah to the Charleston source means that Savannah will be more sensitivity to source orientation than the other 6 test sites, which was why the Savannah site was chosen as 1 of the test sites. 7. Disagree, we believe the sensitivity studies are adequate to support the conclusions presented in the final report. 8. The final report of the CEUS SSC project is not the proper place to propose regulatory requirements. These will depend on the agency involved in regulating whatever facility is being designed. 9. It is not clear what is meant by the "mean fractile hazard evaluations." The purpose of this section is not to summarize what is found in the CEUS SSC study, but what has been the experience with large hazard studies throughout the world. 10. Figure numbers have been resolved in final report 11. Computation of gridded hazard and comparison with the USGS National Map results is not part of the project scope.

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asks the question of how stable the mean hazard may be. An inference could also be made by comparing gridded hazard and COV with the USGS National Map results.	
Appendix C: Provide discussion of table parameters, e.g, quality values and how determined.	Discussion of values is provided in Chapter 4 and reference to that section is now made in Appendix C.
Appendix F: The workshop summaries could be improved by expanding to include copies of all expert presentations and work summaries.	The workshop presentations will be provided on the CEUS SSC Project website.