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which are alternatively used when referring to the SSHAC assessment process. The word "study" does not properly communicate the complex activities and processes that constitute the SSHAC Methodology or SSHAC assessment process. These activities together constitute a structured assessment process that involves compilation of the state of scientific and technical knowledge, compilation of datasets, evaluations of state of practice, and finally, assessments that represent the integrated knowledge of the scientific community and the community's knowledge uncertainty as represented in the logic tree of the SSC model. It should be kept in mind that the SSHAC assessment process is accepted by the Nuclear Regulatory Commission (NRC) as the current state of practice for a technical process whereby seismic hazard models are assessed. Thus, it has the same standing as a consensus standard (ASCE Standard 43-05, for example). It is incorporated into the Agency's accepted seismic regulatory procedures (Regulatory Guide 1.208) for demonstrating compliance with the seismic regulation 10 CFR Part 100.23; it also is accepted by the Department of Energy (DOE) as part of the Agency's seismic safety policies and regulatory procedures. We emphasize that it is essential to clearly establish in Chapter 1 that the SSHAC Methodology is an assessment procedure that is accepted by the NRC and the DOE for developing seismic hazard models that are, in turn, accepted as providing reasonable assurance, consistent with these Agency's seismic safety decision-making practice, of compliance with their seismic safety regulations and policies. Reasonable assurance is expressed in the outcome of using the SSHAC Methodology as the representation of the center, body and range of scientific community knowledge. In order to clearly convey the fact that the assessment of the CEUS SSC model has been accomplished through implementation of an accepted structured assessment process, we believe that the terminology "SSHAC Level 3 assessment process" should be adopted and used consistently throughout the CEUS SSC Report, notwithstanding use of alternative terminology in other documents. This would require extensive technical editing. Similarly, a careful edit should be performed, replacing the words "study/studies," which do not properly apply when describing the activities performed in the CEUS SSC Project, with "project" or "assessment," as appropriate. As examples, "LLNL study" and "EPRI-SOG study" are properly "LLNL Project" and EPRI-SOG Project." Although the term "SSHAC Study Level" has been used in past documents, we recommend use of the term "SSHAC assessment process" in order to clearly convey the complex activities performed in the CEUSSSC Project. The word "event" is used confusingly to mean "earthquake" throughout this chapter and the report. While it can be argued that the usage is understood in context, regulatory documents, which are intended to be used for an extended time by many people having differing backgrounds, require clarity. Consider making a blanket change of the word "event" to"earthquake" where appropriate.	
Comments by Section ***Section 1.1*** 1st paragraph: Consider replacing the 2nd sentence with: "As such, the CEUS SSC model replaces regional seismic source models for this region that are currently accepted by the Nuclear Regulatory Commission (NRC) for satisfying the requirements of the seismic regulation, 10 CFR Part 100.23, for assessing uncertainty	Revisions made to text as suggested.

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in seismic design bases. These include the Electric Power Research Institute–Seismicity Owners Group (EPRI-SOG) model (EPRI, 1988) and the Lawrence Livermore National Laboratory (LLNL) model (Bernreuter et al., 1989).” This change would require some additional editing of the paragraph. Note that the proper reference to the EPRI-SOG Project is EPRI (1988). The date should be corrected in the References. Note also, that EPRI (1989) contains hazard computations at the SOG utility’s NPP sites. This report was not submitted to NRC for review. (See also *Comments by Section* for Chapter 3, under *References.*) 2nd paragraph: Consider replacing the 2nd sentence with: “The project used a SSHAC Level 3 assessment process in order to assure compliance with the requirements of seismic regulations that uncertainties in the model have been properly quantified, evaluating the range of views and interpretations of the technical community.” And add to the end of the paragraph: “These models are expected to be adopted as part of the seismic safety regulatory guidance, replacing the EPRI (2004, 2006) models.”	
Section 1.1.1 “Studies” should be replaced with “Projects” here and throughout the report when referring to the EPRI-SOG and LLNL projects.	Revisions made as suggested.
Section 1.1.2 “Studies” should be replaced with “Expert Elicitation Projects.” In the 1st paragraph, consider replacing sentences 4 through 6 with: “These included the EPRI-SOG and LLNL projects.1 Although both of these large projects relied on assessments by multiple experts, there were significant technical and procedural differences between the two, and there were large differences in the hazard results obtained at many common sites compared by the two projects. The formation of SSHAC was motivated by the need to understand these differences and to develop guidance acceptable for meeting the requirements for seismic safety regulation of nuclear facilities for assessing uncertainty in seismic hazard models”. This change would require editing of the subsection as needed to be consistent. Typo: In the first sentence of paragraph 2, change “time if their issuance” to “time of their issuance”	Revisions made as suggested.
Section 1.1.3 Suggested wording change in the first sentence: “just as important as the basis of the technical assessments.” In the subsection heading: “SSHAC Methodology” or “SSHAC Guidance.” At the top of p 1-3, the sentence, “As will be discussed in Section 2.2, the roles and responsibilities that a SSHAC process defines for all project participants must be scrupulously adhered to throughout the process to ensure its success” is overstated. Section 2.2 makes no mention that “scrupulous adherence” is a condition for success. Suggestion: “The roles and responsibilities of participants in the CEUS SSC project were explicitly defined, consistent with SSHAC guidelines for a successful Level 3 assessment project (see Section 2.2), and were diligently followed.”	Revisions made as suggested.

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Section 1.1.4 "Study" should be replaced with "Project" or "CEUS SSC Model"; edit the subsection as needed for consistency. Suggested word change in paragraph 2, line 2: "The CEUS SSC model is based on a comprehensive, transparent, and traceable process, . . ." In the last sentence of paragraph 1, given the purpose of the CEUS SSC project (as described in the following paragraph), it seems strange to mention the DNFSB explicitly but not the NRC in this first general statement. Suggestion: "Standardization at a regional level will provide a consistent basis for computing seismic hazard, which will assist regulators such as the NRC [acronym defined earlier in section 1.1] and the Defense Nuclear Facilities Safety Board (DNFSB) in their oversight of nuclear facilities."	Revisions made as suggested.
Section 1.1.5 In the last line of paragraph 1 on p. 1-3, change "participated or observed the CEUS SSC Project" to "participated in or observed the CEUS SSC Project." Differences from USGS National Seismic Hazard Mapping Project: In the 1st paragraph on p. 1-4, the quoted AFEs should be verified. The national seismic hazard maps and USGS PSHA work is for AFEs in the range of 10-2 to 10-4 (building code maps are developed for an AFE of 4.04 x 10-4), and the CEUS SSC results will provide results for AFEs in the range of 10-3 to 10-6 for design purposes. In the same paragraph, lines 6 and 7, suggested wording change: "critical safety requirements of these facilities" rather than "the robustness of these facilities." [Delete comma preceding period at the end of this sentence.] In the same paragraph, line 11, suggested wording change: "hypotheses and parameter values are included where appropriate" In the same paragraph, line 12, consider changing "witnessed in the paleoseismic record" to "observed in the paleoseismic record"	Revisions made as suggested.
Section 1.2.1 Consider replacing the section heading with "Regional Seismic Source Model that Represents Current Knowledge and Data Uncertainties of the Technical Community" (see Comment **S 1-1**). In paragraph 2, line 1, consider changing "proper" to "appropriate." The last sentence of this section discusses the possibility that local sources can be used to refine the CEUS SSC model for site-specific application. We suggest that this sentence be deleted. Any change to the CEUS SSC model will need to be evaluated in terms of the PSHA distance influence for that change. Thus, what constitutes a local SSC model change versus a regional SSC model change is somewhat vague. The SSC report should recognize that site-specific studies are required but be silent on what happens if these studies indicate an SSC model change. NRC and others will have to decide what to do with any recommended SSC change (the distance extent to which that change must apply) and whether updates to calculations for "regions" are necessary.	Consideration given to suggested revisions; revisions made to text to reflect the intent of the suggestions.
Section 1.2.2 The section heading should be changed to "Conducted Using the SSHAC Level 3 Assessment Process," and edit the section to be consistent with the change (see	Revisions made to text as suggested, except that terminology of SSHAC Study Levels is kept for clarity.

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Comment S 1-1). In paragraph 1, consider replacing the 3rd sentence with: "For regional seismic hazard models intended for use at many sites, the higher assessment levels provide the level of assurance required by the regulators for future use in seismic safety decision-making." In paragraph 2, line 9, suggested wording change: "the success of these assessment levels is the implemented process followed, which . . ." Third paragraph: Time and costs are issues that the regulatory agencies are committed to take into account, but reasonable assurance of safety as required by the seismic safety regulations and regulatory safety practice are primary. This section should be edited to reflect this understanding. Consider replacing the first sentence of this paragraph with: "Selection of a SSHAC assessment level depends on the scope and complexity of the required evaluations and the intended use of the assessed seismic hazard model." At the end of the paragraph consider adding the sentence: "Moreover, after several years experience using the SSHAC Methodology, a Level 3 assessment is now accepted for developing regionally-applicable seismic hazard models intended for use over an extended time as the starting basis for computing PSHAs at multiple sites."	
Section 1.2.3 In paragraph 1, line 3, suggested wording change: "a SSHAC process should not be subject to significant change without new hazard-critical scientific findings." Suggested wording change in paragraph 2, line 2: "Although these findings may lead to" Suggested wording change in paragraph 2, line 3: ". . . it is likely that the assessment will remain viable, avoiding the need for an extensive revision." In paragraph 2, third sentence: The text states, "Longevity means that the model will last for several years before requiring a significant revision or update." The last sentence in the paragraph states, "It is expected that the longevity for studies such as the CEUS SSC Project will be at least 10 years before there will be the need for a significant revision." To avoid confusion, the wording defining *longevity* should be sharpened.	Revisions made as suggested.
Section 1.2.4 The section heading should be changed to "Interface with Ground Motion Models" Use of the words "debate" and "interaction" in the 2nd paragraph, do not properly convey the role of the workshops for implementing the assessment process. Consider replacing the last two sentences of the paragraph with: "The TI Team brought together a panel of ground motion experts constituted of proponents of the range of available models in a series of three workshops, structured to gain a common understanding of the uncertainties in the modeling approaches and to structure the evaluation and assessment process for representing the uncertainty distribution of the technical community." The subsection should make clear that the Expert Panel represented the range of community ground motion modeling knowledge for the CEUS. Suggested wording change in paragraph 2, line 8: "The TI Team interacted with the Expert Panel to . . ."	Revisions made to text as suggested.
Section 1.3 As discussed in Comment S 1-1, the word "study" does not convey the activities and	The term "study region" is commonly used for seismic hazard analyses, while "model region" is not clear and not common. We will continue to use the term

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processes that constitute the SSHAC Methodology. The section heading should be changed to "CEUS SSC Model Region." Regarding the 4th sentence of paragraph 1: Are there any contributing sources that are in oceanic crust? In this same paragraph, the text incorrectly (or at least misleadingly) states that "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." Examination of Figure 1.3.1 shows that the SSC model region extends 200 mi. into Mexico only along the Gulf Coast. It does not generally extend 200 miles into Mexico "on the southwest."	study region when talking about the region depicted on the figure. Change made to sentence regarding boundary.
Section 1.4 "Study" should be changed to "Project" in the section heading (see Comment S 1-1).	Revision made as suggested.
Section 1.4.1 In the section heading, use of the word "Complete" is not clear, and the word "Study" is misleading. Section heading should be changed to "Seismic Source Model Region." Need to introduce the three stages of the SSC Model assessment: In section 1.4.1, the reader should be informed that the SSC Model was developed in three stages—the sensitivity SSC Model, the preliminary SSC Model, and the final SSC Model. This can be done effectively at the end of this section—prior to Chapter 2 where the terms appear for the first time on p. 2-19 unexplained. In paragraph 1 (see line 10), the text states, "sources of repeated large-magnitude earthquakes (**M** ≥ 6.5) earthquakes (RLMEs) are identified . . ." The rationale for selecting the threshold of **M** 6.5 for RLMEs should be explained. In this same paragraph, next-to-last line, change "and the forecast future occurrences" to "and the forecast of future occurrences" Kijko Methodology as "State-of-the-Art": On p. 1-9 in the first paragraph, the text describes "two methods for assessing Mmax: a Bayesian methodology . . . and the Kijko methodology that is state-of-the-art within the technical community." The latter assertion raises questions about the Kijko methodology vis-à-vis the project. If state-of-the-art, then why was the methodology only considered at a late stage of the project (see p. 2-44) and why was it not identified at the USGS Mmax workshop as state-of-the-art? Suggestion for a broad-brush statement needed here: ". . . and a well-founded mathematical procedure that estimates Mmax based on seismic data (where sufficient) only for the source being considered."	"Complete" deleted; term Study Region is specific to the mapped area (see comment response to Section 1.3) Discussion of the three stages added to Section 1.4.4. Explanation added by M6.5 used for RLME. Revisions regarding the Kijko method made as suggested.
Section 1.4.2 In the 3rd line, consider changing "third party" to "future user" In this same paragraph, lines 10–11, consider changing "for a project" to "for seismic hazard analysis at a specific site."	Revisions made to text as suggested.
Section 1.4.4.2 In the 4th sentence, suggested word change: "Where applicable, GIS data layers were developed, and this included new geophysical data compilations developed specifically for the project."	Revisions made to text as suggested.
Section 1.4.4.3 In line 4, change "all events up through 2009" to "all earthquakes through 2008." The	Revisions made to text as suggested.

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project catalog (Chapter 3) extends through the end of 2008. In line 7, suggested word change: “a number of historical earthquakes were reviewed in order to develop reliable moment magnitudes for these shocks.”	
Section 1.4.4.4 In the title of this section elsewhere in the report, *paleoseismicity data* tends to be used loosely as synonymous with *paleoliquefaction data*. Paleoliquefaction data are a subset of paleoseismicity data, which notably include results of geological trenching of active faults, such as for the Meers and Cheraw faults. The report includes varied types of paleoseismic data, and correct terminology is important for clarity. Consider replacing the first sentence of this section with: “Because of the emerging use and significance of paleoliquefaction data in the CEUS, part of the scope of the project was a compilation of these data and development of written guidance for representing uncertainty in evaluations and interpretations of the data to estimate the locations, occurrence times, and magnitudes of causative earthquakes.”	Revisions made to text as suggested.
Section 1.4.4.5 The first sentence of the second paragraph is awkwardly worded. Suggestion: “This report contains an evaluation . . .”	Revisions made to text as suggested.
CHAPTER 2—SSHAC LEVEL 3 PROCESS AND IMPLEMENTATION	
General Comments **G 2-1. (CBR, CC)** This chapter contains generally informative and valuable background information, but it does not adequately achieve the goal of explaining the chapter heading for a number of reasons: (1) the chapter is not organized effectively, with too much discussion of history that, in its present form, distracts from a necessary focus on this project1; (2) there is not enough discussion of what the TI Team did to ensure that they were objective evaluators to “represent the center, body, and range of the technical interpretations that the larger informed technical community (ITC) would have if they were to conduct the study”; and (3) the discussion of the workshops needs to be enhanced to describe what the TI Team did to ensure that (a) the workshops focused on the right issues (completeness), (b) the workshop goals were met, and (c) the experts who attended the workshops were appropriate and sufficient for the purpose of defining the community knowledge and associated uncertainties.	History discussion removed. Additional discussion provided of the activities associated with the evaluation process, including the need for all TI Team members to assume the role of expert evaluators, and the potential for removal of Team members who were not able or willing to assume the evaluator role Additional discussion provided regarding workshops and approaches to ensuring their success, including providing the proponent experts with a list of questions prior to the workshop to ensure that the proponents focused on the hazard-significant issues.
G 2-2. (CC) The discussion regarding a “SHAC Level 3 process” and the concept of the “informed technical community” (ITC) is of great importance for substantiating key claims about the implementation and results of the CEUS SSC project. But, it is marred by imprecise wording that may contribute to confusion or invite argument. Our Comment **S 1-2** (clear communication) applies equally to Chapter 2, and we offer additional specific comments to help strengthen the logic underpinning key claims in this chapter.	Extensive revisions to the discussion of the ITC made throughout to make it consistent with NRC (2011), which will be issued shortly.
Specific Comments **S 2-1. (CC, SSHAC)** *Explaining the Goals of the Chapter* Writing always involves individual choice, and there are different ways to explain the goals of the Chapter at the outset. In the following example text2 an attempt is made to give the reader a road map—intentionally with a regulatory framework in mind:	Most of the suggested passage was included in the revised text. Use of the term “community distribution” is maintained in this discussion because it is specifically defined in the SSHAC guidelines.

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The goals of this chapter are, first, to describe the SSHAC Level 3 assessment process and how it was implemented to assess the CEUS seismic source characterization (SSC) model and, second, to demonstrate that the implementation was accomplished in compliance with the SSHAC guidance. The SSHAC developed guidance for four levels of implementing an assessment, depending on the degree of uncertainty and contention involved and the intended use of the seismic hazard model.3 The SSHC guidance emphasizes that, independent of the implementation level, the goal of a SSHAC assessment is “to represent the center, the body, and the range of the technical interpretations that the larger informed technical community would have if they were to conduct the study” (SSHAC, 1997, p. 21). The “center, the body, and the range” is taken to mean a representation of the uncertainty in the technical community’s knowledge, referred to by the SSHAC as “the community distribution.” The latter, as a representation of the uncertainty in the technical community’s knowledge, can be termed “the community uncertainty distribution.” A proper representation of the community uncertainty distribution appropriately meets the requirements of the NRC’s seismic regulation, 10 CFR 100.23 The SSHAC recommended that a Level 3 or a Level 4 assessment process be used for complex assessments, the products of which have high public importance and attract public scrutiny, such as regional seismic hazard models intended to be used over a sustained time period as base-case models for site-specific PSHAs. Such models require the highest level of assurance that the community uncertainty distribution has been properly represented. For this project, the decision was made to use a SSHAC Level 3 assessment process.4 The CEUS SSC Project arrived at this decision based on experience gained with implementations of the SSHAC guidance, which has shown that a properly executed Level 3 assessment process can provide a level of assurance of meeting the SSHAC goals comparable to that of Level 4, which is much more costly to implement. This chapter begins with a discussion of the fundamental SSHAC goal of representing the center, body, and range of the technical community’s knowledge, including why this goal was developed. This is followed by a discussion of how the SSHAC Level 3 assessment process has been implemented by the CEUS SSC Project, including the roles of key participants, project organization, key activities, and participation of the Participatory Peer Review Panel (PPRP).	
S 2-2. (CC, SSHAC) *“Capture” and the Informed Technical Community* We caution the TI Team that repeated use of the word “capture”—a highly nuanced term as it relates to the center, body, and range (CBR) of the technical interpretations of the ITC—may confound clear thinking. In its 1997 report, the SSHAC most often uses the words “represent” or “a representation of” for actions relating to “the center, the body, and the range of technical interpretations that the larger informed technical community would have if they were to conduct the study” (SSHAC, p. 21).5 In Chapter 2, the dominant action word used for the CBR is “capture,” emphasized, for example, by the headings for sections 2.1.2 and 2.1.2.2. Coppersmith et al. (2010)6 use “capture” (at least 17 times) in the context not only of the CBR but variously in terms of capturing uncertainty, capturing insights, capturing the community distribution, capturing rate of occurrence and randomness, and so on. The problem with *capturing* the CBR of technical interpretations of the ITC, as opposed to	Capture replaced by represent, although both terms are used interchangeably in the SSHAC report. One downside to the use of “represent” is the mistaken notion that the TI Team merely attempts to reflect or mirror the views of the community (i.e., a poll), in the same way that a politician represents his constituency. The “informed” aspect of the definition of the ITC is important, was defined specifically in SSHAC, and is emphasized in the text. All of the discussion of these issues has been made consistent with NRC (2011), which will be issued in the near future.

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<table>
<tr><th>Comment</th><th>Summary of Revisions to Report</th></tr>
<tr><td>representing them, is that it invites critical scrutiny of what may have been left out, not fully preparing the reader for the need to understand important concepts dispersed elsewhere in the report—notably, identification and due consideration of alternative views, allowance not to include views judged to have an insignificant effect on the hazard, and the integration function performed by the TI Team in its role of assessing and representing the CBR of the ITC.</td><td></td></tr>
<tr><td>S 2-3. (CC) Claim that CEUS SSC Robustly Implemented SSHAC Guidance
On p. 2-1, par. 2, the text states:
“These sources, as well as projects conducted prior to the development of SSHAC guidance, offer confirmation that the CEUS SSC process was a robust implementation of both the “spirit” and the “letter” of the law, namely SSHAC.”
It is illogical to say that prior sources “confirm” a later “robust implementation.” And it is misleading to refer to SSHAC guidelines as “the law.” The astute reader will compare the claim made in this introductory part of Chapter 2, with the conclusion eventually reached in section 2.1.2.3 (p. 2-23), where one finds wording such as “addressed adequately,” “preponderance of evidence,” and “reasonable assurance.”
Suggestion:
“These sources, as well as projects conducted prior to the development of SSHAC guidance, provide a basis for concluding that the CEUS SSC assessment process followed in a robust way both the “spirit” and the “letter” of SSHAC guidance. The end result is reasonable assurance that the CEUS SSC final model achieves the primary goal intended by the SSHAC guidelines.”</td><td>Revisions made to merely state that the SSHAC assessment process, as given in the SSHAC guidelines, was followed.</td></tr>
<tr><td>S 2-5. (CC) Historical Context and Evolution of Use of Expert Assessment (Section 2.1.1)
The length of this subsection detracts from this chapter. While this section is informative, Sections 2.1.1 through 2.1.2.2 and Table 2-1 could be moved to an appendix, with a short summary provided here. Also, the text (specifically in Section 2.1.2) and Table 2-1 would be improved if the authors provided their thoughts on how well the experts or expert teams did as evaluators for those projects that were completed at a SSHAC Study Level 4. It is our impression that results are mixed in this regard. If the authors agree, this should be discussed and noted.
In order to completely chronicle the origins of the NRC’s probabilistic seismic hazards program, it should be stated that during the mid to late 1970s, the Advisory Committee on Reactor Safeguards (ACRS) persistently urged the NRC to undertake research aimed at quantifying the uncertainty embodied in SSEs derived following the requirements of the seismic regulation 10 CFR Part 100, Appendix A, which had been adopted in 1973. The ACRS also urged the NRC to undertake a parallel program with the aim of quantifying the margin embodied in the NRC’s seismic design criteria and procedures. In response, the NRC developed and funded a seismic margins research program and, a short time later, a seismic hazard research program, both conducted by the Lawrence Livermore National Laboratory (LLNL). The seismic hazard research program adopted from the decision analysis community the structure and formalism of classic expert elicitation processes.</td><td>The entire section has been removed. A comparable section appears in the NUREG on SSHAC implementation (NRC, 2011), which is a more appropriate venue.</td></tr>
<tr><td>S 2-6. (CC, SSHAC) “Capturing” the Center, Body, and Range (Section 2.1.2)
Consider changing “Capturing” to “Representing” in the section title.
As a lead-in to Section 2.1.2, consider this example text (see also Comment S 2-4):</td><td>Much of the suggested wording has been added in the text.</td></tr>
</table>

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Reasonable assurance is the standard for reaching administrative decisions about public safety across the spectrum of hazards to which the public is exposed. Regulations, regulatory guidance, regulatory review, and administrative hearings all invoke the standard of reasonable assurance. Regulations state the safety requirements, regulatory guides provide guidance for technical methods and procedures that are accepted for demonstrating compliance with applicable regulations, regulatory review provides reasonable assurance that regulatory guidance has been properly implemented, and an administrative hearing determines whether the safety conclusions are supported by preponderance of the evidence developed by the regulatory review process. In this safety decision-making process the SSHAC assessment process is a technical process accepted in the NRC's seismic regulatory guidance for reasonably assuring that uncertainties in data and scientific knowledge (stated by the SSHAC as the center, body, and range of views of the informed scientific community) have been properly represented in seismic design ground motions consistent with the requirements of the seismic regulation 10 CFR Part 100.23.	
S 2-7. (CC) *"Standard of Proof" (Section 2.1.2.1)* Better wording for the title of section 2.1.2.1 would be "The Reasonable Assurance Standard," which is the primary focus of this subsection. The claim made in the fourth sentence of this subsection that, "there is no need for such proof" is out of place (the claim is explained later in the second paragraph). Based on arguments made in our Comment S 2-5, we recommend deletion of the entire first paragraph of this subsection and revision of the remainder. The standard of proof is reasonable assurance, and reasonable assurance is demonstrated by proper implementation of the NRC's regulatory decision-making procedures. In the instance of the CEUS Project reasonable assurance that the CEUS SSC Model represents the center, body, and range of the views (prefer knowledge) of the scientific community is demonstrated by proper implementation of the SSHAC Level 3 assessment process.	The entire section has been removed.
S 2-8. (CBR, SSHAC) *Evidence That CEUS SSC Project Has Captured the Informed Technical Community (Section 2.1.2.2)* Adherence to the SSHAC guidelines is necessary evidence, but it is not sufficient to show that the CBR of the technical community has been represented in the assessment. How can sufficient evidence be obtained? Certainly that is not easy, but sufficiency can be approached by peer review of the report. That is what the review of the draft report by the PPRP, the USGS, and supporting parties is doing. These parties are judging the completeness of the process carried out by the TI Team. The question is, do these reviews achieve the goal of evaluating the results of the process? This will be a subjective appraisal. It would be well for the report to discuss the subjectivity of the evaluation and the role of reviews in the evaluation. This subjectivity is acknowledged in Section 2.1.2.1 [Standard of Proof] in the description of the technical community as a "hypothetical community" and the regulatory use of reasonable assurance. The idea that the technical community is hypothetical is contrary to seismic regulatory principles and practice (see our Comment S 2-5). There is a very real technical community that has developed the evidence and views regarding specific topics that are important to seismic source characterization and assessment in the CEUS. This community does not consider themselves to be hypothetical.	Section has been modified extensively. The remaining section is intended to show that the recommended steps in a SSHAC assessment process (as given in the SSHAC guidelines) have been followed.

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S 2-9. (CC) *PPRP Attendance at the Eight Working Meetings of the TI Team:* The report contains differing statements about the attendance of PPRP observers at the TI Team Working Meetings: "All of the working meetings were observed by one or more members of the PPRP." (p. 2-20) "[The PPRP] participated in many TI Team working meetings to plan and review the process and progress of the project." (p. 2-36) "One to three representatives from the PPRP attended the working meetings in order to observe the deliberation and technical assessment processes." (p. 2-42) For the record, PPRP attendance was as follows: WM # 1 WM # 2 Hinze, Kammerer, Kimball WM # 3 Ake, Petersen WM # 4 WM # 5 WM # 6 Ake, Stepp WM # 7 Ake, Arabasz, Kimball WM # 8 Kammerer	Revisions made to indicate PPRP attendance at seven of the ten working meetings.
Comments by Section ***Chapter 2 (Title)*** In order to emphasize that the CEUS SSC Project implemented an assessment process, were commend the Chapter title be changed to: SSHAC LEVEL 3 ASSESSMENT PROCESS AND IMPLEMENTATION (see Comments **S 1-1** and **S 1-2**). ***Chapter 2 (Introductory Text)*** Spell out PPRP when it is first used in report. ***Section 2.1*** p. 2-2, par. 3, line 3: "the data that applies" (inconsistency: data used as singular here; plural elsewhere in report) ***Section 2.1.1*** par. 1: The text states, "The SSHAC report was written in response to an evolution of expert risk assessment methodologies that had been conducted for purposes of probabilistic risk analyses during the previous three decades." According to the footnote on p. 34, the only identified studies predating the SSHAC report that dealt with **risk** were the WASH-1400 study and the NUREG-1150 study; all the other studies dealt with **hazard**. ***Section 2.1.1.1*** p. 2-5, par. 3, line 1: Change "The EPRI-SOG study" to "In the EPRI-SOG Project" p. 2-7, next-to-last par.: "and offered a prophecy for future guidance:" What exactly is prophesied in the subsequent quoted text? Suggestion: "and future guidance was envisioned" ***Section 2.1.1.2*** p. 2-10, par. 2, line 7: Suggest replacing "third party" with "future user"	All specific comments led to text revisions, except as noted: Sections 2.1.1, 2.1.1.1, 2.1.1.2, 2.1.2, and 2.1.2.1 have been deleted. Comment regarding Sections 2.4.3 and 2.4.4: the workshop summaries are included in the report in Appendix F; the presentations will be made available on the project website after issuance of the final report

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p. 2-11, par. 2, line 3: Suggest replacing "gone up" with "increased"	
p. 2-11, par. 2, second sentence: There is unclear phrasing in the second half of this critical sentence. The difference between the PEGASOS results and the older results were shown to be due to "an appropriate treatment of the ground motion aleatory variability and an error in the calculations in the previous hazard studies (NAGRA, 2004, Section 8.4.2)." Was the treatment appropriate in the older studies or in PEGASOS?	
p. 2-11, par. 2, line7: "to discredit the study" — Clarify which study is being referred to.	
p. 2-11, par. 3, line 11: Change "TI" to "TI Team"	
p. 2-11, par. 3, line 2: Because ESP and COL appear in the list of Acronyms, consider writing, here at their first mention in the text, "Early Site Permits (ESPs) and Combined Construction and Operating License (COL) applications"	
p. 2-11, par. 3: The narrative of what happened in the EPRI (2004) Level 3 process is confusing. The text describes that "A small TI Team was responsible for the assessments and a panel of resource experts/proponents provided their views of the existing ground motion models and their applicability to the CEUS." Subsequent text describes the problem of the experts not taking ownership of the resulting composite model. As written, why would "resource experts/proponents" be expected to take ownership? In the EPRI (2004) Project, the TI Team requested that the Resource Expert Panel endorse the assessed model. The Panel did not challenge the implementation of the assessment process, but persisted in the role of proponent experts, insisting that their proponent model should have more weight. Suggestion: "A lesson learned in the project was that if broad expertise is needed to perform the TI role of representing complex technical views of the informed technical community, then a small TI Team may not suffice. In the case of the EPRI (2004) assessment, the panel of ground-motion experts was not charged with the TI role, but they were asked to review and endorse the assessed ground motion model; individual members of the panel persisted in acting as proponents, advocating higher weighting of their individual proponent models. Subsequent Level 3 . . ."	
p. 2-11, par. 3, last line: Suggest replacing "claim" with "accept:	
p. 2-11, last paragraph, line 6: Suggest deleting "developing"	
p. 2-12, line 1: Typo. Change "significance advances" to "significant advances"	
Section 2.1.2	
par. 1, third sentence: What is meant by "many of the technical issues that drive seismic hazard . . . are rare?" Suggestion: Delete "rare and"	
Section 2.1.2.1	
par. 1: See Comment **S 2-3** regarding the notion of "capturing the informed technical community." If the authors insist on using "capture," for clarity at least describe capturing the *views* or *technical interpretations* of the informed technical community— not the jargon of "capturing the informed technical community."	
p. 2-17, par. 1, last line: Typo. "have the like highest likelihood"	
p. 2-17, par. 3: It will be helpful to clarify for the reader that what is "not yet available" is not the article written by Coppersmith et al. (2010) but rather the NUREG document discussed in Coppersmith et al. (2010). Suggestion: "to develop a NUREG-series	

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document (see Coppersmith et al., 2010)." ***Section 2.1.2.2*** In the discussion of Item 3 (Provide a uniform data base to all experts), mention should be made of the development of the seismicity catalog. p. 2-19, par. 2, last sentence: What "will provide a valuable methodology step for future Study Level 3 projects" isn't "these tables" but rather something like "the structure of these tables." On p. 2-19 near the end of the next-to-last paragraph, the reader encounters, for the first time, "the development of the sensitivity SSC model, the preliminary SSC model, and the final SSC model"—terms which aren't explained until the bottom of p. 2-20. These are fundamentally important for the reader to understand. A good place to introduce the reader to these terms would be at the end of Section 1.4.1, explaining that the SSC model was developed in three stages. p. 2-19, last par.: For complete documentation (useful for future readers) give the dates of the maximum magnitude workshop in Golden, Colorado, and the CEUS workshop in Memphis, Tennessee. p. 2-20, Item 5. *Elicit SSC judgments from experts*: The text describes eight working meetings of the TI Team and goes on to state that "Each working meeting was structured around a particular aspect of the project, as follows:"—but **ten** bullets follow, not eight. To compound the problem, a different list of eight bullets later appears on p.2-41 to describe the focus of the eight meetings. On p. 2-37 under the header TI Team, mention is made of **nine** working meetings. ***Section 2.1.2.3*** Where are the conclusions regarding the selection of the study level—an important part of the process? ***Section 2.3*** par. 3: Change "TI Lead" to "TI Team Lead" consistent with the organizational chart in Figure 2.3-1. p. 2-37, par. 2: To soften jargon, consider replacing "Technical Integrator (TI) Team" with "Technical Integration (TI) Team" ***Section 2.4.2*** par. 1, line 9: Text states, "annual frequencies of interest (e.g., 10–4 to 10–7/yr) for nuclear facilities." Executive Summary states 10–4 to 10–6/yr. ***Section 2.4.3*** The text should describe what was done to identify resource experts for Workshop #1 and the approach used to ensure that the experts who participated in the workshop were appropriate and sufficient. ***Sections 2.4.3 and 2.4.4*** It would be helpful to have more references to the workshop information in the appendices, particularly the workshop summaries and the presentations. ***Section 2.4.4*** The text should describe what was done to identify proponent experts for Workshop #2 and the approach used to ensure that the experts who participated in the workshop were	

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appropriate and sufficient. ***DRAFT*** Installment 2, PPRP Review Comments, page 2-11 ***Section 2.4.8*** A short summary of the purpose of the Data Summary and Data Evaluation tables and the use that was made of them would be informative here. ***Section 2.4.9.1*** The HID is a valuable document. It would be useful here to expand on its purpose and to note specifically that this document is meant for the analyst—providing clarity about the model to be implemented and obviating the need to distill the model from the full report. This document helps assure that implementation of the model (which is sometimes challenging) is as intended. ***Section 2.4.9.2*** First sentence: This sentence appears to be the objective of the report. Suggest that it be moved forward or reappear in an appropriate place in Chapter 1. ***Table 2-2*** Under "Other Technical Experts . . ." there are duplicate entries for Al-Shukri and Mueller To avoid confusion about the listing of names in this table, delete "Other" in "Other Technical Experts" because some of the experts are also listed in the first two categories of the table.	
CHAPTER 3 — EARTHQUAKE CATALOG	
General Comments **G 3-1. (NAR)** This chapter summarizes the project approach to developing the earthquake catalog for use in the CEUS seismic source model. The process followed in this project is similar to many others in that it consists of three basic elements: (1) assembly of available, relevant sources of earthquake data into a single, magnitude-consistent earthquake catalog; (2) identification of dependent events; and (3) evaluation of catalog completeness.	No response required.
G 3-2. (NAR) Chapter 3 is arranged logically as it describes the goals for earthquake catalog development (Section 3.1), the compilation of available data from continental and regional scale catalogs as well as special studies (Section 3.2), development of various relationships to convert all earthquake size estimates to moment magnitude (Section 3.3), catalog declustering (Section 3.4), and catalog completeness (Section 3.5).	No response required.
G 3-3. (NAR) It is appropriate to emphasize that, the comments below notwithstanding, the catalog that has been developed for this project represents a major achievement and is a real step forward for the entire seismic hazard community. It is a major improvement over previous catalogs in that it incorporates more regional catalogs and has developed moment magnitude estimates for all the earthquakes. The efforts of the TI Team, together those of collaborators from the USGS and the Geological Survey of Canada (GSC), are to be commended. The detailed and thorough approach followed has led to a product that will be widely used. The TI Team, USGS, and GSC staff should consider producing something in the open literature that documents this work. The development of a specific catalog for non-tectonic events in this region may not seem like an interesting product,	Listing of non-tectonic events included in Appendix B

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but for practitioners in this field it will be very useful (especially if it is maintained over time). Having said the above, in order to achieve a clear and complete description of the efforts that went into developing the catalog and of the results, Chapter 3 needs to be improved, as we proceed to explain.	
G 3-4. (CC) The text and explanation of figures in Section 3.3 are too terse. The knowledgeable practitioner may be able to "read between the lines" or infer the meaning of unexplained dashed and dotted lines on many of the figures, but the documentation for this project report must be clear and complete for all readers.	Section 3.3 greatly expanded
G 3-5. (CC) This chapter would be enhanced by a description of the problems associated with obtaining useful focal depths in the region, limitations on focal-depth resolution, and general observations or conclusions regarding the depth of earthquake foci in the CEUS.	Section on focal depth data added
Specific Comments **S 3-1. (CC)** *Non-PPRP Review Comments* Section 3.1 documents the emphasis placed on the earthquake catalog as it provides the basic earthquake rate information that "drives" the seismic hazard model for most of the CEUS. This section describes the process of compiling the relevant catalogs and data sources and summarizes the rationale for returning to the basic data sources for magnitude or intensity data. A brief synopsis on review of the catalog by other interested and experienced seismologists is contained in Section 3.1.3. However, no mention is made of any results, comments, or changes due to those reviews (hence uncertainty whether suggested changes were implemented in the final catalog). Will those review comments (particularly those of the USGS) be part of the project documentation in any form? They do not appear as an Appendix. Will they be documented in project files in a form that could be retrieved by interested individuals?	Section 3.1.3 added to describe main review comments and actions taken as a result
S 3-2. (CC) *Clarity and Completeness in Figures* The meaning of different line symbols is incompletely explained on several of the magnitude conversion figures. On Figures 3.3.1-1 and 3.3.1-2, the addition of an added point to extend the regression to lower values needs more explanation and justification. On Figure 3.3.4-1, the labeling in the *Explanation* of "CEUS dependent catalog" makes the content on the figure ambiguous. The text on p. 3-11 states that "the catalog of earthquakes" is shown on the figure—but two sentences later, the text states, "Therefore, dependent earthquakes (foreshocks and aftershocks) must be identified . . . ." So "dependent catalog" can be read as the catalog of dependent events.	Text greatly expanded in Section 3.3 to clarify figures.
S 3-3. (CC) *Corrected Moment Magnitudes from Atkinson* Section 3.3 provides the summary of the development of the various conversions of earthquake size measures (instrumental magnitude or macro-seismic observations) to moment magnitude. This step is essential to ensure consistent earthquake counts and compatibility with modern ground motion prediction equations. Section 3.3.1.1 describes the first of the specific instrumentally determined moment magnitude studies utilized (Atkinson, 2004). To make it clear to the reader how the conversion was carried out, additional detail should be added to 3.3.1.1. This additional discussion will ensure that the other 3.3.1.x sections are clear. For instance, for events that are used from Atkinson's study, our understanding is that her estimated **M** values are "corrected" to moment magnitudes consistent with the results of waveform inversion studies for those events. If this is not what was done, considerably more detail must be supplied as the correction process is not clear to the PPRP.	Additional discussion added to explain process

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S 3-4. (CC) *Approximate vs. Instrumentally Determined Moment Magnitudes* In Section 3.3.1, second paragraph, the text notes that some “moment magnitude estimates were obtained from three studies that determined **M** by approximate methods . . . .” As part of the project documentation, it would be helpful to identify these earthquakes in a table (presumably, the number involved is manageable). Also, to aid future users of the catalog, and for transparency, instrumentally determined moment magnitudes in the Earthquake Catalog should be flagged—ideally in Appendix B, or in files available to interested parties.	Listing of approximate moment magnitudes added to Appendix B
S 3-5. (DMM, U, CBR, CC) *Sensitivity of Recurrence or Hazard to Choice of Declustering Method* Section 3.4 provides a discussion of the approach used to perform declustering of the magnitude-corrected earthquake catalog. Because the PSHA formulation used for area source zones relies on the assumption of earthquake occurrences following a Poisson process, it is necessary to identify any dependent events in the catalog and remove them prior to performing any rate calculations. A number of different approaches have been used in the past to perform declustering analyses in major seismic hazard studies. The work of Gardner and Knopoff (1974), Reasenberg (1984), and Reasenberg and Jones (1989) have been widely used. The Gardner and Knopoff technique, as well as similar region-specific methods (Urhammer, 1986; Gruenthal, 1985), rely on removing events within fixed magnitude-dependent time and distance windows about a “main” earthquake. The method developed by Reasenberg defines variable space-time windows for individual event clusters using statistical tests and related to a particular model of aftershock occurrence. In contrast, the approach that has been used in the CEUS-SSC study is a stochastic approach developed in the mid-1980s as part of the EPRI-SOG Project. Section 4.3 cites EPRI (1988) as the source document for this approach to declustering, this reference is missing from the reference list (see note on EPRI references below). The EPRI approach begins by treating each earthquake as a main event and then evaluates the rate of earthquake occurrences within a “local window” about the main event and compares that rate to that within an “extended window,” i.e., one larger in space-time dimension. If the rate of earthquakes within the local window is significantly higher (based on an un-specified statistical test) than within the extended window, then smaller events are removed within the local window until the rate approaches the extended window (“background”) rate. However, in regions of low seismic activity, stable estimates of rate in the larger window can be problematic and hence lead to bias due to the unwarranted removal of events. The PPRP has several specific concerns related to the approach taken to declustering of the catalog used in the CEUS SSC Project: 1. The lack of clear documentation. The discussion of declustering in Section 3.4 is less than one page long. The discussion and development of the EPRI declustering algorithm contained in EPRI (1986, Vol. 1, Pt. 2, Sections 3 and 4) runs to more than 20 pages and is not trivial to follow. EPRI (1988) contains a thorough discussion of the various declustering approaches and the assumptions associated with each. The EPRI declustering method was designed to minimize the number of assumptions required about the clustering process. The description of the adopted declustering methodology in Chapter 3 needs to be significantly expanded. 2. Given that the declustering fundamentally alters the number of earthquakes in the catalog for calculations of recurrence—and thus hazard, more discussion is warranted	1. Explanation of declustering method expanded 2. The effect of the declustering method is examined by including a comparison with results obtained by Gardner Knopoff as implemented by USGS. Differences are small. 3.More discussion added to declustering section 4. That is correct, there is variability in cluster length as a function of magnitude. The “classical” methods ignore this effect while the EPRi approach recognizes it 5. There are assessments/conjectures in the literature for very long aftershock sequences for earthquakes in the CEUS in the literature, but these do not enjoy widespread support in the technical community. The comparisons shown in the revised report indicate similar results to the Gardner-Knopoff approach

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about associated uncertainty. What are the implications if a different method were used (e.g., the Gardner and Knopoff method, which reportedly produces 15 percent fewer dependent events and thus more main events)? In the case of the EPRI-vs.-Gardner and Knopoff comparison, were smaller magnitude bins systematically more affected? This issue of uncertainty associated with declustering methodology could be addressed in one of two ways: (1) sensitivity studies displaying the impact that this assumption has on recurrence relationships or hazard results, or (2) explicit consideration of alternative declustering models each with an appropriate weight. If sensitivity calculations aren't explicitly made, can experience from other PSHAs be used to amplify on uncertainties associated with the choice of the declustering method? Also, because any declustering algorithm is sensitive to the choice of declustering parameters used, some discussion is warranted about the efforts made in the earlier EPRI Project to determine suitable parameters for the CEUS. 3. EPRI (1988) is in the open literature. However, it is difficult to obtain, not widely used outside a small number of individuals, and in the view of the PPRP, not uniquely representative of the CBR of the ITC. If it is the position of the TI Team that in fact the EPRI declustering approach is superior to all other approaches and the only approach that should be considered, then that needs to be more clearly articulated and documented. In point of fact, the EPRI approach has been used only by a few of the teams in the Yucca Mountain PSHA and in updates to the EPRI-SOG seismic source model used for recent COL/ESP applications. The seismic source characterization teams in the PEGASOS project used either the Gardner and Knopoff approach or variants thereof, or a modified version of the Reasenberg approach. Most other seismic hazard studies for critical facilities in the US have used similar approaches to those in PEGASOS. Alternative approaches to declustering should be examined, documented, and if warranted considered for inclusion in the present study to satisfy the goal of capturing the CBR of the ITC. 4. Figure 3.4-1 displays the results using the EPRI (1988) procedure, showing dependent event time and distance windows for events down to about **M*** = 2.5. Are these considered large events? Note: definition of main, large, and independent earthquakes needs to be clearly articulated in this section. If the PPRP is interpreting these figures properly the estimated time windows for many individual small (**M*** < 4) events are significantly longer than time windows for many individual larger (**M*** from 5.5 to 6) events. For **M*** just below 5, the time window ranges from 4 days to about 6.5 years. The PPRP questions if that range would be endorsed by the broader community of observational seismologists. Based on the information provided, it is not clear whether these outcomes are unique to the model selected, and whether the model properly models the uncertainty associated with identifying dependent events.	
S 3-6. (DMM, CC, U) *Catalog Completeness* Section 3.5 describes the approach used to assure catalog completeness in the CEUS SSC Project. The methodology used for catalog completeness is that developed in the EPRI-SOG Project and works with the uniform magnitude, **M***. The EPRI approach defines spatially discrete zones that have uniform levels of magnitude completeness and defines magnitude specific probabilities of detection (PD) in each. For the CEUS SSC Project, the TI Team augmented the completeness regions used in the earlier EPRI study slightly to address additional catalog information and to properly cover the current study region.	Additional description of the completeness approach is provided. Discussion of the alternative approaches is included

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Many of the same comments made regarding Section 3.4 can be made regarding Section 3.5. The lack of detail and clarity make a proper evaluation of this section virtually impossible. The sole reliance on reference to the EPRI documents as the technical basis fails to meet the standard of documentation required in a study of this scope. It is not discussed in this section, but the probability of detection thresholds defined and shown in Table 3.5-1 were derived by simultaneously maximizing the log likelihood functions for PD as well as the "a" and "b" values in the earlier EPRI approach. Based on our reading of Section 5 it is not clear if the same approach was used in the current study. As with the discussion of declustering, there are alternative methods for performing completeness assessments in the literature and those should at least be discussed and evaluated. The PD and equivalent time period of completeness methodology used is quite powerful as it maximizes the number of events used from the declustered catalog. However, it needs to be more completely described and evaluated against alternative methodologies if it is to be the sole approach used.	
Comments by Section ***Entire Chapter*** The word "study" should be replaced with "project" throughout the chapter where used in as part of the designation of an integrated assessment project; e.g., "EPRI-SOG Project", "this project", and so on. ***Section 3.1*** Suggestion: The reader would find a summary preceding this to be helpful. ***Sections 3.1.1 through 3.2.2*** Numerous acronyms are unexplained and do not appear in the list of acronyms. These include: SUSN, NEIC, PDE (p. 3-1), ISC, ANSS (p. 3-3), CERI (p. 3-4), NEDB (p. 3-5) ***Section 3.1.1*** p. 3-1, par. 2, line 1: Change "CGS" to "GSC" ***Section 3.1.3*** line 3: Typo. "Therefore, and an important part of the catalog development process was review by seismologist seismologists with extensive knowledge . . ." line 7: Affiliation for Martin Chapman as "Virginia Technological University" is incorrect. The school is called either Virginia Tech or Virginia Polytechnic Institute and State University (see http://www.vt.edu/). ***Section 3.2*** p. 3-3, 1st paragraph: It would be helpful to give an example of the numbering scheme as it is not entirely obvious how the scheme will appear in the summary catalog. ***Section 3.2.1*** p. 3-3, 1st paragraph, line 3: Typo. Change "and primary earthquake listing" to "and the primary earthquake listing") p. 3-3, 2nd paragraph: EPRI (1988) reference is missing. (Please see comment on EPRI references below.) ***Section 3.2.3*** p. 3-4, 1st paragraph: Typo in line 3? ("locations and/or depths"?); in line 6, change "Boatwrigth" to "Boatwright" p. 3-4, 2nd paragraph: Typos. Change "catalog" to "catalogs"; "are area" to "an area"); "The second is" to "the second was" (for consistency with tense in preceding sentence). ***Section 3.2.4*** p. 3-4, 3rd line: Reference to Section 3.2.4 should be to 3.2.3	Correction made 3.1 Brief summary added at beginning of chapter 3.1.1-3.2.2 Acronyms explained as introduced CGS fixed Fixed Numbering scheme explained Section rewritten Fixed Scheme explained.

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Section 3.2.5 The scheme for assigning order of preference to events located south of the US-Canada border is not clear. We assume that all the regional networks have equal weight and events located near New Madrid would default to CERI or St Louis University, and if in New Jersey would default to Lamont Doherty. If not, this needs to be made clearer. ***Section 3.3.1.1*** line 5: Typo. Change "over estimates" to "overestimates" ***Section 3.3.1.3*** Typos. In line 2, change "an coda wave technique" to "a coda wave technique"); in line 4, change "abet" to "albeit" ***Section 3.3.2.1*** Define fN, and FN ***Section 3.3.2.2*** 5th line and equation 3.3.2-3: Missing word and typo. "The Johnston (1996) relationship is reasonably consistent with the project data. Also, is Equation 3.3.2-3 the Johnston (1996) relationship, and is that what was actually used? Not clear as written. ***Sections 3.3.3.1 and 3.3.3.2*** Unclear whether the locally-weighted least-squares fit or a constant offset model was used in the conversions between MN and mbLg to moment magnitude **M**, as shown on Figures 3.3.3-1 and 3.3.3-2. ***Section 3.3.3.2*** Add a sentence after the equation indicating the variables ZCAN and Z1995 are as defined in Section 3.3.3.1. ***Section 3.3.3.3*** Suggestion: "The A third mb body-wave magnitude scale (mb) is also more commonly used in the US than in Canada . . . ." Also note that mbLg is used in this section when it should be mb. Perhaps add a reference for robust regression. ***Section 3.3.3.5*** Typo in first sentence. Should be surface-wave magnitude (MS) not "local magnitude ML"; the same error is in equation 3.3.3-5. ***Section 3.3.3.8*** The discussion of unknown magnitude (MU) is not clear. For any given earthquake, how was the decision made as to which conversion should be used? ***Section 3.3.4*** p. 3-10, line 3: Typo. Change "Section s" to "Sections" p. 3-10: Following equation 3.3.4-1, the reference to σE[M|X] should perhaps indicate this is illustrated by the confidence interval for the mean shown on Figures 3.3.1-1, 2, 3 etc. for example. We suggest that equations 3.3.4-2 and 3.3..4-3 be double checked as comparison with equations 3-8 and 3-9 in Vol.1. Pt.2 of the EPRI-SOG report indicates some discrepancies. Since the corrected magnitudes are ultimately used to derive the "b-value" one may wish to comment on the sensitivity (or hopefully lack thereof) to the "b-value" used in equation 3.3.4- 3. In equation 3.3.4-4 the σ2 M|M instrumental is not clear. Is it the 0.1 value assigned to the instrumentally determined values referenced in the paragraph above equation 3.3.4-1? p. 3-10, last paragraph: The text states, "As discussed in EPRI (1988) uncertainty in the magnitude estimates and its propagation through the magnitude conversion process introduces a bias in the estimated earthquake recurrence rates." It would be helpful to the general reader to add some explanatory detail, rather than placing the burden on the reader seek another publication to understand the purpose or basis of the information that	Section 3.3 rewritten to address comments and add additional clarification

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follows. ***Section 3.4*** p. 3-11, par.1,line 6: The text states, "The standard method of creating a catalog of independent earthquakes developed by Gardner and Knopoff . . . ." It is misleading to describe the Gardner and Knopoff procedure as "the standard method." Researchers in earthquake statistics outside the U.S. would likely use Ogata's well-established epidemic-type aftershock sequence (ETAS) model as the basis for declustering. p. 3-11, par. 1, next-to-last sentence: In the report, "large" earthquakes are defined as **M** ≥ 6.5, so it is confusing to write "and distance interval about a large earthquake." Suggestion: "and distance interval about a relatively large earthquake." p. 3-11, par. 1, last sentence: The text states, "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 clearly be declared as aftershocks, say by Gardner and 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 main shocks? p. 3-11, par. 2, second sentence: For clarity (because Figure 3.4-1 contains two plots), consider writing, "The data points in the two plots represent the length in days of individual clusters and the maximum distance between earthquakes assigned to a cluster, respectively." p. 3-11, par. 3, first sentence: Typo. Change "European earthquake" to "European earthquakes" p. 3-11, par. 3, last sentence: The narrative describing that the EPRI procedure identifies about 15 percent more dependent events may confuse readers examining Figure 3.4-1. For clarity, consider cautioning the reader not to confuse numbers of dependent events with the number of data points for dependent-event parameters associated with individual clusters on Figure 3.4-1.	Section rewritten 3.4 to better explain the EPRI method and address comments
Section 3.5 First sentence: Typo. Change "EPRI SOG" to "EPRI-SOG" p. 3-12, par. 2, line 6: Could not find Figure 8-1 in Report; what is the basis for the boundaries of the completeness regions? For example, how were the boundaries of Region 15 defined, which is one of the new regions? Is there a rationale for including both the Gulf of Mexico and Florida offshore in the same completeness region? p. 3-12, 4th paragraph: The terms PENB, PENA and WEDT are not defined. p. 3-12; 6th par., line 2: The text states, "in the time period 1995 to 2008" but in Table 3.5-1 the limiting year is 2009.	Section 3.5 rewritten to address comments and add greater explanation
Figures Labeling of page numbers on pp. 3-31, 3-32, and 3-33 needs to be corrected.	
Figures 3.3.1-1 through 3.3.1-3 Add more detail to the figure captions, and indicate the 1:1 line and the 90% confidence interval for the mean. Typo in Figure 3.3.1-2: (1994) not (19944). ***Figure 3.3.2-1*** Lots of lines on the figure with no explanation in the figure caption. What exactly is approximate **M** in this figure?	Figures for section 3.3 have been redone
Figure 3.3.4-1 Is the map of epicenters south of Florida complete to the shown boundary of the study region? If not, explain justification for neglecting these. Was the Caribbean seismicity catalog accessed to determine earthquakes in the study region?	Caribbean seismicity not included
Figure 3.4-1	

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The text should comment on the very large disparity in cluster duration and spatial dimension for similar magnitudes. Virtually all readers will be left with distrust of the methodology based on these results, absent any additional discussion. ***References*** EPRI (1988) is missing from reference list. EPRI reports need to be properly referenced (see next page). **This is how the EPRI reports are referenced in the CEUS/SSC report:** Electric Power Research Institute (EPRI), 1986, Seismic Hazard Methodology for the Central and Eastern United States: Volume 1, Part 2, Methodology (Revision 1): Final Report, EPRI-NP-4726-A-1(1). Electric Power Research Institute (EPRI), 1989, *Probabilistic Seismic Hazard Evaluations at Nuclear Power Plant Sites in the Central and Eastern United States: Resolution of the Charleston Earthquake Issue*: EPRI Technical Report EPRI NP-6935-D. **The references below are how the EPRI Project documents are referenced in the PEGASOS report.** EPRI-SOG 1986: Seismic Hazard Methodology for the Central and Eastern United States, Electric Power Research Institute NP-4726A, Volumes 1-11. EPRI 1989: Probabilistic Seismic Hazard Evaluations at Nuclear Power Plant Sites in the Central and Eastern United States, Electric Power Research Institute NP-4726, 9 v. **The PPRP suggests the proper reference is the following**: EPRI-SOG 1988: Seismic Hazard Methodology for the Central and Eastern United States, Electric Power Research Institute NP-4726A, Revision 1, Volumes 1-11. The EPRI-SOG Project was completed and submitted as "EPRI NP-4726" in 10 volumes to the NRC for review as a topical report. The review was completed in 1988. The report number designation "4726-A, Revision 1" identifies that the report has been revised in response to NRC's review and that it is accepted by NRC for future use for licensing submittals and contains the NRC's Review Report and Acceptance Letter. Volume 11 is the NRC's requests for additional information and EPRI's responses. The above noted inconsistency is indicative of the problem with just broadly referencing the EPRI documents within this chapter of the report and the attendant issues with transparency and availability. The PPRP has two systemic recommendations regarding utilization of methods from the EPRI-SOG Project and citations. First, be much more specific when referencing the EPRI studies (i.e. volume, section etc.). Second, the TI Team should strongly consider reproducing and expanding the discussions and developments in the EPRI-SOG report in the CEUS-SSC report. This will enhance clarity and transparency and facilitate utilization of some of the methods by the broader community. ***Other references either missing from Chapter 3 and/or that probably should have been cited*** Gardener, J.K. & Knopoff, L. 1974: Is the sequence of earthquakes in Southern California, with aftershocks removed, Poissonian? Bull. Seism. Soc. Am. 64, 1363-1367. Grünthal, G. 1985: The up-dated earthquake catalogue for the German Democratic Republic and adjacent areas – statistical data characteristics and conclusions for hazard assessment. *In:* Proceedings 3rd International Symposium on the Analysis of Seismicity and Seismic Risk, Czech. Ac. Sc., Prague, 19-25. Reasenberg, P.A. 1985: Second-order moment of central California seismicity. J. Geophys. Res. 90, 5479-5495. Reasenberg, P., and L. M. Jones (1989), Earthquake hazard after a mainshock in California, *Science* **243,** 1173–1176.	EPRI reference fixed to 1988 Additional references cited as appropriate

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Stepp, J.C. 1972: Analysis of completeness of the earthquake sample in the Puget Sound area and its effect on statistical estimates of earthquake hazard. Proceedings of the International Conference on Microzonation 2, 897-910.	
CHAPTER 4—CONCEPTUAL SSC FRAMEWORK	
General Comments **G 4-1. (CBR, CC)** Chapter 4 describes the Conceptual SSC framework. This chapter is generally well-written, organized in a logical format, and responsive to early PPRP recommendations for creating a structured systematic approach to SSC, including the establishment of criteria for defining seismic sources. However, it is incumbent on the TI Team to document how these criteria were used to define seismic source zones. While the PPRP appreciates the role that informed judgment has on assessing weights for various branches of the logic tree, these weights must have a documented basis. In response to the PPRP April 7, 2010 letter, the TI agreed that project documentation must provide a detailed (emphasis added) discussion of the criteria that were used to identify seismic sources and a justification for all logic tree branches and weights.	To aid in the description of the criteria for identifying seismic sources, Table 4.1.3-1 has been added. Also, text has been added in Section 4.1.3 stating where the detailed descriptions are given of the technical bases for identifying each source (in Chapters 6 and 7). Also, each discussion of alternatives in the logic tree and their associated weights has been reviewed to ensure that it provides ample detail for the reader to understand the technical bases for the branches and weights of the logic tree.
G 4-2. (NAR) The development of Data Evaluation and Data Summary Tables has been extremely important with respect to making the seismic source characterization process more transparent and complete (see detailed comments on these tables). These types of tables represent a foundation upon which future SSC seismic hazard evaluations can be efficiently built. This is particularly true for seismic source characterization projects that have a broad regional extend. The TI Team is to be commended for taking the time to create these tables. The tables include an unprecedented level of information that external reviewers can use to understand the assessments that have been made and represented in the logic trees. An important point that was developed in Section 4.2.2 was that the Data Summary and Evaluation Tables are not intended to replace the documentation of the SSC effort but to supplement it.	No revisions necessary.
Specific Comments **S 4-1. (CC)** *Terminology* The nuanced words "study," "capture," and "event" are used throughout Chapter 4, contributing to a lack of clarity. We recommend replacing with words that convey the specific contextual meaning: that is, replacing "study" with "project" or "assessment" as appropriate; "capture" with "represent" as appropriate; and "event" with "earthquake" as appropriate.	Revisions made to text, as appropriate.
S 4-2. (CBR) *Master Logic Tree and Representing the Community Distribution* The assessment of a conceptual tectonic framework is ultimately represented in the master logic tree as the weights applied to branches of this logic tree (major alternatives related to the overall tectonic framework). Interactions with the broad scientific community in Workshops #1 and #2, and the scientific knowledge base developed through these interactions, informed: (a) the TI Team's assessments for the conceptual tectonic framework, (b) the TI Team's evaluations of the hazard significance of various seismic source characterization issues (Section 4.3.2), and (c) development of criteria for defining seismic sources (Section 4.3.3). For assessment of SSC models of this regional extent, it is now clear to the PPRP that it	Evidence that the data evaluation process developed for this project and the methodology for identifying seismic sources can result in a full SSC model is the CEUS SSC model itself. Linkages are made (or enhanced) between the use of the data tables to assist in the evaluation and integration process, as well as the linkage between the seismic source criteria and their application for the sources identified in the assessment. The PPRP is the fundamental group in a SSHAC assessment process to provide feedback on all technical and process issues, including whether or not the conceptual SSC framework advanced in this project is reasonable. Importantly, the SSC process is a hazard-related activity and, in addition to the PPRP, the TI Team received

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could have been useful to have additional feedback of the conclusions discussed in Section 4.3.2 and the criteria discussed in Section 4.3.3 to enhance confidence that this information can be used (i.e., appropriately represents the CBR of the ITC) to create a detailed SSC logic tree. From a generic perspective, this should be considered a lesson learned, recommended for Level 3 assessment projects of broad regional extent, to directly link the overall development of the seismic source assessment logic tree with a broader segment of the ITC. The TI Team is strongly encouraged to consider whether additional feedback with a targeted group of subject experts is warranted.	valuable input from the USGS regarding potentially applicable data and approaches to identifying seismic sources.
S 4-3. (SSHAC) *Level 3 Assessment Process* In the first paragraph of Chapter 4, the text states that the justification for the use of a SSHAC Level 3 assessment process is given in the CEUS SSC Project Plan. While the project plan did discuss the selection of the assessment level, this project report must demonstrate that execution of this assessment level is appropriate, resulting in a high quality product consistent with the requirements for seismic regulatory decision-making. We suggest that this sentence be deleted.	Sentence has been deleted, as suggested.
S 4-4. (CBR, CC) ***"Generic" Data Evaluation (Section 4.2.1)*** The development of Table 4-3 and the discussion of this table are beneficial to this report. The text would be strengthened if at the end of this section the TI Team discussed how they developed the numbers in the table. Specifically past PSHA experience, results from Workshop #2, and discussions with a wide range of people who are part of the ITC were all used to make these assessments (See Comment S 4-2). Finally, there should be discussion of how the numbers were or were not used to guide the weights ultimately assigned on the logic tress.	Additional discussion added to clarify the dual purposes of data identification and evaluation. The generic table is described more fully and its role of helping to document the identification of data based on potential indicators of seismic sources. The bases for the weights are discussed and they provide a basis for documenting the current thinking regarding the relative importance of potential indicators and the relative usefulness of various types of data to address the indicators. They also provided a means of prioritizing the data compilation efforts toward those data that have the highest potential usefulness in the SSC process. They are not used in any quantitative sense, nor do they have a direct relationship to the weights given in the logic trees.
S 4-5. (CC) ***Logic-Tree Branches and "Credible" Alternatives (Section 4.1.1.1)*** In Comment S 1-2, caution was raised about the use of particular wording that may lead to confusion or invite argument. We offer a similar caution here about declaring that only "credible" alternatives are included in the logic tree. Having to defend the assertion of zero credibility in the case of excluded alternatives can become a red herring. The nature of the TI Team's assessment of a representation of the views of the ITC is explained at great length in Chapter 2. Allowance is made for excluding an alternative view or parameter based on the judgment that its relative weighting would lead to an insignificant effect on the hazard. When discrete probability distributions are used to represent the center, body, and range of a continuous distribution, it is recognized that the distributions have tails of low-to-zero probability. Instead of having to assess exactly where the zero bounds are, acceptable practice allows representing the significant mass of the distribution. We recommend removing "credible" from the section title.	"Credible" is not used in the section title. The term "non-credible" is replaced by "alternatives that are not technically defensible." The discussion of the exclusion of non-credible branches of the logic tree is motivated by problems that have emerged in PSHA projects where it was felt that including very low-weighted alternatives would provide a means of handling outlier and controversial models and parameter values. In one such project, the total number of end branches exceeded 10^{27}, thus leading to excessive run times for calculations (~one month) and limited sensitivity analyses. Analysis of the hazard significance of the branches allowed for the vast majority of them to be "pruned" or "pinched" based on a lack of hazard significance. It is felt that, rather than trim branches after the fact, making an attempt to eliminate non-credible alternatives could lead to similar reductions in scale during the development of the trees.
S 4-6. (CC) *Methodology for Identifying Seismic Sources (Section 4.3)* This section would be improved if there were a discussion how Workshop #2 was used to guide the TI Team in terms of developing a methodology for identifying seismic sources.	Text added to explain usefulness of WS2 to source identification process.
S 4-7. (CC, SSHAC) *Hazard-Informed Approach: Section 4.3.1* In the last paragraph on page 4-10, the following statement is very confusing, seemingly in conflict with SSHAC guidance, and likely to create controversy: "Rather, it reminds us that the purpose of the CEUS SSC Project is to develop a seismic	The point of the sentences is to indicate that the SSC process is part of a hazard analysis and is not a mechanism for answering research questions. First sentence was modified and second sentence deleted.

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source model to be used in a seismic hazard analysis, and not to attempt to answer or even capture the larger technical community's questions about SCR earthquake causative mechanisms. The exceptions are those cases where a hypothesis might have profound implications on the geometry, Mmax, or recurrence for a seismic source such that it would affect the hazard results." Perhaps the intent is to convey the fact that the CEUS SSC Project is an assessment based on existing knowledge rather than an attempt of advance knowledge or resolve competing arguments. The two sentences could be removed without loss of continuity. In any case, some clarification is essential.	
S 4-8. (CC) *Criteria for Defining Seismic Sources (Section 4.3.3)* It would be appropriate and helpful here to note that geological and geophysical studies of the crust since the 1980s have provided little significant new information about tectonic features and the geological history of the region that may have a bearing on evaluation of seismic hazards. The only possible exception is the improved understanding of the Illinois basin extended zone and its features. However, paleoliquefaction studies have been useful in defining and characterizing seismic source zones.	Sentences added as suggested.
S 4-9. (CBR) *Weights on the Two Conceptual Models (Section 4.4.1)* One of the critical logic tree assessments is the weights on the two conceptual models used to represent classes of seismic sources. Section 4.3.3 establishes criteria for assessing seismic sources while Section 4.4.1 provides a description of the logic tree elements. This section does not develop a strong argument for the weights assigned, particularly the strong preference assigned to the seismotectonic zone branch. Additionally, it is not clear where the TI Team demonstrates that the development of seismotectonic zones leads to hazard significant changes in the model. The text states that the development of seismotectonic zones allows for more relevant information on the characteristics of future earthquakes (the third criteria in the sequence defined in section 4.3.3)—but this seems to be a TI Team judgment, as opposed to a documented evaluation and assessment. Section 4.1 (Item #3) makes the point that a methodology for identifying seismic sources that takes into account defensible criteria is a critical attribute of this project, but the project must demonstrate that the TI Team has properly executed these criteria. Perhaps some type of summary table can be prepared to synthesize how the criteria distinguish between seismic sources. The weight assessed for the seismotectonic branch has increased from 0.33 (August 2009, when three branches were considered) to 0.60 (March 2010) to 0.8 (July 2010 and the draft report). The PPRP notes that these weights could be viewed as somewhat counter to the overall ITC trend that has been documented in the USGS National Seismic Hazard Maps (three cycles including regional workshops) and not necessarily a logical outcome from Workshop #2 of this project. At a minimum, the TI Team needs to bolster their arguments for the weights assigned. The PPRP encourages careful consideration of this issue and the potential need for adjusting the weights toward more parity between the two overall SSC models.	Table 4.1.3-1 has been added, along with associated text, to summarize the criteria that have been used to identify each seismic source. Reference is made to hazard sensitivity studies (which will be included in Section 8) that show little sensitivity to the choice of the "seismotectonic zones" or the "Mmax zones" branches of the logic trees. The comment regarding changes to the weights of the alternative models are misleading and irrelevant. As discussed in the report, refinements to all components and weights in the model were encouraged throughout the project and there was no imposed requirement that weights not change or that alternative branches not be added or removed as the model integration process progresses. The weights on the alternative branches for the conceptual approach are, in fact, a judgment made by the TI Team. Similar approaches have been used by the SSC community for various hazard studies, but the weights are not intended to reflect a poll of what others have done. Rather, the important consideration is the ability of each model to incorporate important seismic source information. Additional text has been added explaining the bases for the weights on these alternative models.
S 4-10. (CBR, CC) *Mmax Zones Logic Tree (Section 4.4.1.2)* The discussion of the magnitude weighting provides no explanation or basis for the weights. The same holds true for the approach to spatial distribution of seismicity rates (smoothing). PPRP comments on these weights are provided in Chapter 5. Once these	These assessments are addressed in the applicable sections of Chapter 5 and applicable cross-references are made.

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comments are addressed these discussions should, at a minimum, refer to specific sections in Chapter 5, and be enhanced to summarize the basis as appropriate.	
S 4-11. (DMM) *Table 4-3 (p. 4-41)* Does “(4) Rift Basins” overlap with “(2) Extended Margins”? Does this include basins formed as a result of regional extension in a Highly Extended Terrain such as the Triassic grabens of the EUS? A comprehensive description of continental rift structures is presented by Olsen and Morgan (Continental Rifts: Evolution, Structure, Tectonics, Elsevier, 1995; Chapter 1). Does “(4) Rift Basins” also overlap with “(5) Failed Rift (Paleozoic and younger)” as in the Oklahoma aulacogen? A failed arm of a rift is a branch of a triple junction that did not develop into an ocean basin. A paleorift that has been reactivated by compressional deformation is an aulacogen, e.g., Oklahoma aulacogen. Does (4) include Precambrian continental rifts that were reactivated in later Precambrian time? Why is “(5) Failed Rift” rated lower than (4) if the Failed Rifts are limited to the Phanerozoic? The Oklahoma failed rift (aulacogen) has the Meers fault, while Recent faulting is not observed on Triassic graben faults, to the best of our knowledge.	No doubt many of the items identified “overlap” and should not be considered as mutually exclusive. The goal of Table 4-3 (now Table 4.1.2-3) is to identify potential indicators of seismic sources (all of those shown have been proposed in the community) for the purpose of identifying the types of data that can address them. Thus, fine-scale definitions of each indicator are not necessary for purposes of defining the applicable data.
Comments on Sections ***Chapter 4 (title and introductory text)*** Consider spelling out SSC in the title of chapter. In the first sentence of par. 1, suggested wording change: “for use in future PSHAs.” In next-to-last line of par. 1: Typo. “how that the framework” On p. 4-1, last sentence: Consider changing “the master logic tree that is the backbone of the SSC model” to “the master logic tree of the SSC model” ***Section 4.1*** In Item #3, line 1: Consider replacing “that takes into account” with “that is based on”; in line 2, consider replacing “takes advantage of” with “incorporates”; in line 3, “identifies” instead of “captures.” ***Section 4.1.1*** To more clearly represent the activities described in this section and in the report as a whole, we recommend changing the title of Section 4.1.1 to “*Logic Tree Approach to Representing Alternatives and Assessing Uncertainties,”* conveying that the alternatives represent the center, body, and range of scientific community’s knowledge and that the assessed uncertainties represent the community uncertainty distribution. On page 4-2, last paragraph, line 3: Consider replacing “identifying” with “representing; also in line 10 of the same paragraph. On page 4-3, 1st paragraph, last line: Consider changing “that express the relative credibility of the alternatives” with “that represent an assessment of the relative credibility of the alternatives” On page 4-3, last paragraph, line 7: Consider replacing “those assessments that are judged” with “those assessed alternatives”; see also Comment **S 4-5**). On page 4-4, first full paragraph, line 1: Consider replacing “considered” with “assessed to be”; in line 2, consider replacing “degree of belief” with “assessment”; in line 7, consider replacing “and not worthy of” with “so did not warrant”; in the last line, consider replacing “assigned” with “assessed.”	Revisions made to text as suggested except as noted here. Section 4.2 (now Section 4.1.2) has been rewritten for clarity. SCR is defined per Johnston et al. (1994)

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On page 4-4, 2nd full paragraph, line 2: Consider replacing “assigned to” with “assessed for”; in line 6, consider replacing “the TI Team considered the available data” with “the TI Team evaluated the alternatives using available data”; in line 13, consider replacing “the weights assigned to” with “the weights assessed for.” On page 4-4, 2nd full paragraph, line 5: When writing that “there is rarely a quantitative basis for assigning these weights,” it should be made clear that this refers to the assessment of subjective probabilities. The CEUS SSC methodology uses five-point distributions to represent quantified continuous distributions of selected parameters. ***Section 4.2*** On page 4-5, 2nd paragraph, line 1: Consider changing “an attempt was made to provide more structure and transparency” with “more structure and transparency has been provided”; in the next-to-last line, replace “study” with “evaluations and assessments of the TI Team” ***Section 4.2.1*** First paragraph, line 3: Consider replacing “as the technical community evolves its thinking regarding” to “as the knowledge of the technical community evolves regarding” On page 4-5, first bullet, line 7-8: Consider replacing “which is an SCR” with “which geologically is constituted of SCR crust” ***Section 4.2.2*** On page 4-7, last paragraph, 2nd paragraph: The text states that “errors in the data generally exceed the signal” (data referring to geodetic data). It is suggested that this be changed to “errors in the data may exceed the signal.” ***Section 4.3*** First paragraph, line 2: Consider replacing “three decades in SSC” with “three decades in assessing SSCs”; in line 3: consider changing “community” to “scientific community”; in line 4, consider replacing “a regional PSHA that can be applied” with “a regional SSC assessment that can be applied; in line 5: consider replacing “requires that a methodology include” with “requires that the assessment include”; in the last line, consider replacing “across the study region” with “throughout the regional SSC model.” In the last paragraph on p. 4-8, Regulatory Guide 1.208 is mentioned with respect to guidance for commercial reactors. ANS Standards 2.27 and 2.29 provide similar guidance for other nuclear facilities, and this should be recognized. In the first paragraph on p. 4-9, the message conveyed by the first sentence is not clear. Consider replacing the word “intuitive” with “subjective” or “common practice.” ***Section 4.3.1*** The meaning of the first sentence is not clear and it seems to be inconsistent with the content of the paragraph. It could be deleted, as the following sentence seems to properly introduce the content of the paragraph. Add Pa to List of Acronyms. ***Section 4.3.3*** On page 4-14, next-to-last paragraph, line 3: Consider replacing “captured by” with “obtained from”; in the last paragraph, line 7, replace “reasonable assessment” with “reasonable interpretation” On page 4-15, 3rd full paragraph, line 4: Replace “PSHAs” with “seismic hazard models”	

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On page 4-16, first partial paragraph, lines 4-5: Consider replacing "Because the CEUS SSC Project is a regional study and not a site-specific study," with "Because the CEUS SSC Project developed a regional SSC model rather than site-specific one," ***Section 4.4.1*** It would be useful to start this discussion with recognition that RLME sources are identified based on well defined evidence for Late Quaternary or Holocene direct evidence of repeated large magnitude earthquakes. Also when discussing the 8th node of the logic tree, the discussion needs to be enhanced, consistent with the information shown on the logic tree figure. ***Section 4.4.1.1*** On page 4-18, the text refers to Table 4.4.1.1-3, which is not included in the report. Rather it appears to be labeled Table 4-6, which is included in the body of the text of the chapter. We suggest that this numbering be corrected, that the tables be numbered in a consistent manner, and that a List of Tables be included in the report. On page 4-19, 1st paragraph: In the first sentence, make it clear that the issue is "the temporal clustering of large magnitude earthquakes." ***Section 4.4.1.2*** Page 4-20, last paragraph: In the second sentence beginning, "For the CEUS SSC Project . . ." need a connector ("and" or a semi-colon) at the start of the last clause (e.g., "and the prior distributions from that study were reassessed."). On page 4-21, second paragraph, suggest moving up the last sentence "As discussed in Section 6.2…" prior to the sentence that lists the weights assigned to the logic tree branches. Four additional review comments relate to the discussion in the second paragraph on p. 4-21: 1. Two alternative locations of the Mesozoic and younger separation branch are identified: the wide and the narrow. Unfortunately, no map is provided for the location of the narrow zone. Reference is made to Figure 4.4.1.2-3 in line 6, which is presumably this map, but it is missing from the report as well as the List of Figures. 2. Figure 4.4.1.2-2 is labeled as showing the narrow Mesozoic alternative, but instead it shows the wide alternative. 3. Note that the caption of Figure 4.4.1.2-2 is not complete in the List of Figures. All captions in the List of Figures should be checked against those given on the figures. 4. The boundary of the project area shown on Figure 4.4.1.2-2 and subsequent figures of this chapter are not the same as shown in the defining figure of the boundary, Figure 1.3-1, and in Figure 4.4.1.1-2. Apparently the boundary in these figures has been modified to incorporate identified seismic source zones in Canada, which is the northeastern segment of the project area. Inconsistent project area boundaries should be avoided to prevent confusion. ***Section 4.4.1.3*** In the first paragraph, change "shown on Figures 4.4.1.3-2 through 4.4.1.3-7" to "shown on Figures 4.4.1.3-2 through 4.4.1.3-5" ***Tables and Figures*** The order of presentation of text, tables, and figures needs to be standardized in all	

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chapters. In this chapter, the order is different than in preceding chapters. ***Table 4-3*** Page 4-39, last row: Assumed the intended wording is small r recent, not capital R Recent. Perhaps a more definitive and less confusing word could be used—perhaps Phanerozoic? Page 4-42, first row: Suggest replacing “Orientation” with “Fault orientation” Page 4-42, third row: Suggest adding “High-resolution seismic reflection” in third column.	
CHAPTER 5—SSC MODEL: OVERVIEW AND METHODOLOGY	
General Comments **G 5-1. (CC)** Chapter 5 provides an overview of the SSC model and some of the methodologies used within that model. This section is generally well written and provides a good description and summary of a number of the technical elements of the SSC model. The work that the TI Team completed to update the SCR database, performing new statistical analyses, and updating prior distributions is an important contribution to improving assessment of maximum magnitudes. However, some specific elements of the model and/or documentation thereof are problematic in the PPRP’S view. Significant changes or additional justifications may be warranted.	No revisions necessary. Significant changes have been made throughout the chapter.
G 5-2. (CC) In addition to the PPRP review, to ensure thorough review of the many equations contained in the report, the PPRP recommends that all knowledgeable members of the TI Team carefully examine all equations, especially equations in sections that they were not tasked to write.	Reviewed as suggested.
Specific Comments **S 5-1. (DMM, CC)** *Implications of Kafka’s Studies for Spatial Smoothing* Section 5.1 provides a well-written overview of the approach to spatial and temporal models of earthquake occurrence in the current CEUS-SSC model. Section 5.1.1 describes the TI team interpretation that the spatial pattern of observed seismicity provides predictive information about the spatial distribution of future moderate-to-large magnitude earthquakes. The PPRP notes that the studies by Kafka (2007, 2009, and Workshop #2) indicate this is generally (emphasis added) the case. Various versions of the cellular seismology results presented by Kafka suggest that much (55–85%), but not all, seismicity is predicted by the spatial occurrence of past earthquakes. This suggests that the report should at least discuss the possibility of specifying a very high level of smoothing within source zones. This is utilization of subjective rather than objectively defined smoothing parameters that would specifically define a seismicity floor in some regions.	Sentences added to Section 5.1.1 indicating that a range of smoothing parameters has been included in the logic tree in order to represent a range of variations in the spatial distribution of future recurrence rates, including an option that leads to relatively uniform rates throughout the seismic source. It is also noted that the penalized maximum likelihood approach used does not require a floor in rate, unlike the kernel approach.
S 5-2. (SSHAC, CC) *Inconsistency With Principles of Seismic Hazard Model Assessment* In Section 5.1.2, par. 3, second sentence, the statement: “The TI Team has taken a very cautious approach, however.” conveys a clear violation of the SSHAC guidance principals for seismic hazard model assessment; namely the goal to represent the center, body, and range of the community scientific knowledge. An explanation is required. It would be made clearer if “assumed” were replaced with “used” in the last line of this paragraph (and if the awkward sentence were inverted).	The sentence has been removed. Other sentences modified to indicate that distributed seismic source zones are modeled using exponential distribution of magnitudes and Poissonian recurrence behavior.
S 5-3. (DMM, CC) *Inadequate Description of the Assessment Process* In Section 5.1.2, the last paragraph on p. 5-3 (continuing on p. 5-4) is critically important, as it introduces the reviewer to the TI Team’s assessment of temporal clustering,	Revisions made as suggested. Clarification is added to indicated that the TI Team evaluated the data that exists for each RLME and then assessed the appropriate approach to modeling recurrence. Because this section is

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arguably the most uncertain assessment for the CEUS SSC model. For example, the topic is introduced with the weak statement, “consideration was given to” instead of wording such as “assessed,” which links directly to the SSHAC guidance. With similar effect, “considered” is used in line 4, where the word “assessed” would more accurately convey the appropriate action and at the same time connect with the SSHAC guidance. In line 5 continuing on line 6, the physical process would be explained more clearly if the words “based on the concept of” were deleted, leaving the sentence to read: “The physical underpinning of a renewal model is a quasi steady state . . . .” In line 10, it would be clearer to change “concept” to “physical process.” We recommend that this paragraph be rewritten, expanding the discussion to convey the state of scientific knowledge about an earthquake cycle in which strain is released as clustered large earthquakes. The most relevant data appear to be the absence of measurable levels of strain accumulation in the Charleston and New Madrid seismic zones, where the short-term geodetic strain rates are in apparent conflict with interpretations of “in-cluster” rates of occurrence of large earthquakes.	intended to be merely an introduction to key concepts, the reader is referred to the applicable sections regarding RLME recurrence methodology and the specific assessments regarding Charleston and New Madrid RLME recurrence.
S 5-4. (CC) *Weak Support for Conclusion* In the first paragraph of Section 5.1.3, the last two sentences, beginning with “The TI Team reviewed . . .” convey an evaluation and conclusion of the TI Team that is greatly important for the CEUS SSC model assessment. Yet support for the strong conclusion seems general and weak. Consider elaborating on the basis for the conclusion. For example, the last sentence begins with “With a few exceptions . . . .” Describe the data that permitted the exceptions and describe how the data were used in the assessments.	Additional discussion added to indicate that there are no exceptions and the appropriate model given the limited data in the CEUS is recurrence of RLMEs and that of distributed seismicity sources.
S 5-5. (DMM, CBR, CC) *Maximum Earthquake Magnitude Assessment* Section 5.2 describes the methodology for assessing maximum magnitude (Mmax) that was used in the CEUS-SSC Project. The text notes that the maximum magnitude earthquake for any given source zone in regions of low-to-moderate seismicity (such as the CEUS) happens rarely, relative to the period of observation. As a result, the record of historical seismicity provides information, but rarely hard constraints, on the source-specific Mmax value. This fact has led to the investigation of global tectonic analogues to address this issue. The scheme for assessment of Mmax in the CEUS-SSC Project incorporated the uncertainties in both conceptual models and the parameters within models. The approach utilized in the CEUS-SSC Project provides a quantitative and repeatable process for estimating Mmax that is easily updatable if new information becomes available. The discussion of the development of the Bayesian Mmax approach in Section 5.2.1.1 is generally clear and guides the reader through the development of the approach. The PPRP believes that the significant effort invested by the Project in the update and re-investigation of the global SCR database was worthwhile. This refinement represents a significant advancement for the community. However, the PPRP notes there are points that require further clarification and assessments that require additional justification as noted in the following two comments.	No response required.
S 5-6. (DMM, CBR, CC) *USGS Mmax Workshop and Mmax Approaches Considered* In Section 5.2.1, the discussion of the evaluation of alternative approaches to Mmax in the CEUS, lacks any meaningful discussion of the USGS workshop on this topic (Wheeler, 2009), and does not strongly support the TI Team’s selection of Mmax approaches beyond the Bayesian approach. The approach developed by Kijko is not the only viable alternative discussed as part of the USGS workshop. Additionally, the approach developed by Kijko was not given much support in the USGS workshop,	Additional reference made to the Wheeler (2004) to indicate the problems with statistical approaches that rely on large sample sizes. Although the TI Team considered the summary of approaches given in the Wheeler report, the Team was charged with doing more than merely identifying the pros and cons of any given approach. The Team’s conclusion was that there are only two viable approaches: those that rely on analogues and those that use the observed seismicity. The Bayesian approach formalizes the use of analogues, and

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needing additional study before it becomes commonly used in PSHAs. This section should provide more discussion of the USGS Mmax workshop and the Mmax approaches considered by the TI Team, and why they are, or are not, selected for assessment.	provides for updating using observed seismicity. The Kijko approach (the option choosen for application to the CEUS project) uses observed seismicity and an assumption of exponentiality.
S 5-7. (DMM, CC) *"Kijko Approach" — Terminology and Description* Defining how this approach or procedure will be referred to in the report and appropriate attribution for its origin need to be established upon first mention. In section 5.2.1 (pg. 5-6, paragraph 3), two alternative approaches are described for estimating Mmax: the Bayesian procedure and "the Kijko (2004) procedure." Later in section 5.2.1-2 (pg. 5-15), the first sentence states: "The Kijko approach (Kijko and Graham, 1998; Kijko, 2004) . . ." In referring to the "Kijko approach," misleading statements are made. On pg. 5-7 (paragraph 1, last sentence), the text states, "However, the approach relies on the assumption that the distribution of earthquake magnitudes follows a doubly truncated exponential distribution." Later on pg. 5-15 (section 5.2.1.2, paragraph 1, first sentence), the text repeats that the approach is based on the simple assumption that "the distribution of earthquakes in a region follows a doubly truncated exponential distribution." In Kijko (2004, pg. 1) the reader plainly finds: "This paper provides a generic equation for the evaluation of the maximum earthquake magnitude *m*max for a given seismogenic zone or entire region. The equation is capable of generating solutions in different forms . . . . It includes the cases (i) when earthquake magnitudes are distributed according to the doubly-truncated Gutenberg-Richter relation, (ii) when the empirical magnitude distribution deviates moderately from the Gutenberg-Richter relation, and (iii) when no specific type of magnitude distribution is assumed."	The report modified to refer consistently to the Kijko (2004) approach. While many of the concepts were introduced in Kijko and Graham (1998), Kijko (2004) introduces the methodology for developing a distribution for Mmax that was used in the CEUS SSC Project. The PPRP is correct. Kijko indicated that multiple magnitude distribution forms can be accommodated. The report was modified to indicate this and to justify why the selection option was utilized.
S 5-8. (DMM, CBR) *"Kijko Approach" — Justification of Weighting* Adding to Comment **S 5-**7 on the TI Team's use of the "Kijko approach" (Section 5.2.1.1), this is an approach that was not identified in the any of the CEUS-SSC workshops as a potential approach. Further, the approach was not discussed in detail at the 2009 USGS Mmax workshop. The Kijko approach is one that is represented by the form: Mmax = Mmax obs +Δ. At the USGS Mmax workshop this class of methods was given little credence. However, the discussion was mostly focused on models that specified a fixed magnitude increment for Δ (0.5 magnitude unit, for example). Kijko's approach is different in that it utilizes a statistical assessment of seismicity in the region of interest to obtain estimates of Δ (and uncertainties). The approach(es) developed by Kijko have not seen wide usage. The PPRP endorses the utilization of an alternative approach that uses zone-specific data for estimation of this important parameter, but notes that the assignment of equal weights to the Kijko KSB approach and the Bayesian global tectonic analog approach may be inconsistent with the CBR of the ITC. Inspection of the results suggests the Kijko method is only used when it agrees with the Bayesian results. (See also earlier Comment **S 5-5** and Comment **S 5-10** below regarding the justification of the relative weighting of approaches.) The P(mu>8.25) threshold of 0.5 does not seem unreasonable, but it does lead to the question of sensitivity of the final distribution to that choice. If P(mu>8.25) were set to 0.25 or 0.75 what effect would that have on the number of zones for which the Kijko result would be used? The choice of **M** 4.8 for the lower bound of the Kijko approach needs additional discussion. This leads in some cases (see Section 7) to non-zero probability assigned to Mmax branches of **M** < 5.25 in large source areas. The PPRP is not convinced this result is consistent with the ITC. It will certainly provoke discussion and hence should be justified to the maximum extent practicable.	The statement that the Kijko approach is a Max observed plus delta is an over simplification. The Kikjo approach defines a distribution for Mmax. The basic formulation produces equations for the mean of that distribution, which could be considered a max_obs plus delta. However, looking at it in this way, one could also consider the Bayesian approach to be a mean plus delta if one computed the mean of the posterior. The PPRP is correct in that the Kijko approach has not seen wide usage. However, as discussed in the response to comment S-5-10, there do not appear to be many options for a repeatable and readily updatable Mmax assessment method for large source zones. The TI team selected the ones that were judged viable for application in this context. It is true that the weight on the Kijko approach is correlated with the similarity in the Kijko and Bayesian results. This has to do with data. The more data there is in a source zone, the more the Bayesian prior is modified by the likelihood function. That likelihood function has a shape similar to the distribution for Mmax produced by the Kijko method implemented in the CEUS SSC project. Also as the amount of data increases in a source, the Kijko method gets more weight. Therefore, when there is a lot of data, it is not surprising that the two methods produce similar results. The revised model imposed a minimum Mmax of 5.5 for all sources

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S 5-9. (DMM, CBR, CC) *Bayesian Mmax Approach* The discussion of the updated domain dataset analyses in Section 5.2.1.1 (and subsections) is confusing and lacks sufficient information to fully understand what was done. The text states that Table 5.2.1-1 list Mesozoic and younger extended superdomains, yet the table appears to list all ages of extended superdomains (and Table 5.2.1-2 is for all ages of non-extended superdomains). Without listing the actual p-values of the statistical tests it is difficult to appreciate the improvements that are being discussed as you assess subsets of the data. Given that Appendix K only provides tables of the SCR Mmax databases, more specifics should be provided in this section (see also Comments on Appendix K – more detail needs to be provided there also). It is suggested that there be a displayed, on one or more figures, the Mmax-obs distributions for the various classes being compared. Was an assessment made of the impact for using an alternative choice for the lower cutoff of magnitude for each of the domains (such as **M** 5 or **M** 5.5)? Statistical analyses are good, but not necessarily the only basis for assigning weights to the prior distributions. It seems clear that the Mean Mmax between the two-priors is likely to be important from a PHSA perspective (7.1 versus 6.35). The text states that a stronger weight (0.6) is not assigned to the two priors because the statistical significance of the separation is not strong. Assignment of relative weights should consider the seismologic views of the ITC in addition to any statistical significance—based on the text, the TI Team seems to be making the statistics the primary consideration. Discussion at the USGS Mmax workshop and the public workshops held to support the National Seismic Hazard Maps could suggest that the ITC would put more emphasis on the "two-priors" model (the TI Team's intuitive judgment). The Open- File Report from the USGS Mmax workshop should be reviewed in this context, along with pertinent discussion from Workshop #2. A stronger basis for assigning relative weights is needed. The description of the methodology to assess Mmax for all seismic sources contains a discussion of the role of the RLME sources in the assessment. The report suggests that a potential problem is that the global SCR database includes events from RLME sources (e.g., New Madrid) and that the Bayesian approach is being applied to non-RLME sources (p. 5-7). It seems that this methodology assumes that all RLME's have been identified in the current model. Otherwise, the model does not consider RLME's that may be found in the future. The report should explicitly describe how the model accounts for non-identified RLME's that may have maximum magnitudes the size of New Madrid or Charleston.	More details on the development of the priors are provided in the revised report. No impact was made of the choice of lower cutoff for each domain. However, a minimum Mmax is now used for all applications to assess of Mmax The USGS Mmax workshop did not deal explicitly with the issue of the relative weights that should be applied to alternative prior distributions using the Bayesian approach. Nor was there any formal "consensus" in exactly what prior distributions should or could be used with the Bayesian approach. As noted in the text, the lack of strong statistical significance between the separate and the combined priors provides input to the judgment that there is not a strong technical basis for giving either alternative strong weight. The updated distributions all contain the possibility that Mmax values could be as large as 1811,1812 or 1886
S 5-10. (CBR) *Weights for the Alternative Mmax Approaches (Section 5.3.1.3)* Given the TI Team's noted high regard for the Bayesian approach, it is difficult to understand why the Kijko approach was assigned equal weight under any circumstances (large number of larger earthquakes). Discussion at the USGS Mmax workshop and the discussion at the regional workshops to support the National Seismic Hazard Maps would suggest that the ITC gives considerably more weight to the global tectonic analog/Bayesian approach. Beyond the Bayesian approach, there were several potential approaches considered at the USGS workshop, thus it is not clear why the TI Team selected the Kijko approach as the only alternative. The Mmax distributions shown in the report appear to be bi-modal in some cases. The TI Team has not properly discussed and justified the weights assigned to the alternative Mmax approaches.	The TI team is not aware of other quantitative and repeatable procedures for estimation of Mmax other than the Bayesian approach or the Kijko approach other than assigning an arbitrary delta value to the observed Mmax or developing a direct subjective assessment. The Team is not opposed to use of a direct subjective assessment but felt that methods that could readily be updated in the future when new information becomes available are preferable.
S 5-11. (DMM, U, CC) ***Approach to Earthquake Recurrence Assessment***	An effort was made to improve the clarity in section 5.3.2 by providing more

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Section 5.3 describes the approach to earthquake recurrence assessment used in the Project. This section is generally well-written (given the complexity of the topic) but could certainly benefit from the inclusion of additional steps in the derivations and from additional discussion in some places (we elaborate in following comments). A fundamental assumption of the methodology used in the CEUS-SSC Project (and most others as well) is that the magnitudes of earthquakes in the corrected catalog can be represented as exponential variables with a density function $f(m) = \beta * \exp(-\beta(m-m0))$. Lombardi (BSSA, 2003, vol. 93, no. 5, pp. 2082–2088) argues that main shocks (i.e., those in the "corrected" catalog) do not satisfy this assumption. Lombardi suggests a different density function for the use with these main events that depends not only on β but on N (the number of events) as well. In fact, her comparisons utilizing Southern California data suggest much lower *b*-values for main shocks than for all the events in the catalog. The PPRP suggests that some discussion of these alternative assumptions be included in the report—and that the methodology used by the TI Team be checked, vis-à-vis implications of the Lombardi paper, to ensure that there is no systematic bias in the maximum-likelihood estimates of *b*-values.	background. We have also 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.
S 5-12. (DMM, CBR, CC) *Smoothing to Represent Spatial Stationarity (Section 5.3.1)* The argument is presented in this section that the penalized likelihood approach to spatial smoothing of seismicity is superior to other approaches and the only method to be considered in the CEUS-SSC Project. The PPRP does not find this argument to be adequately supported by the report as written in its present form. Keeping in mind that the objective of the SSHAC Level 3 process is to represent the CBR of the ITC, we note that, other than one or two members of the TI team, no other members of the technical community are utilizing the penalized likelihood approach to perform smoothing of observed seismicity. The overwhelming majority of the community is utilizing either a fixed-kernel or adaptive-kernel approach to smoothing. The kernel approaches are conceptually much simpler and easier to implement and, as a result, yield enhanced transparency. The PPRP notes that in Section 5.2.1 (p. 5-7, 2nd paragraph) the report states, "[I]t was decided that for representing the center, body, and range of views of the informed technical community, the assessment would need to include alternative conceptual models for Mmax." The PPRP wonders if one were to replace "Mmax" with "smoothing technique" in this statement, why the argument presented in Section 5.2 would not apply in Section 5.3. The penalized likelihood method, as developed in the EPRI-SOG Project and in the present report, possesses some very positive attributes. Some are briefly discussed in Section 5.3.1 but developed more fully in Section 5.3.2.4. It would enhance clarity to refer the reader to Section 5.3.2.4 in Section 5.3.1. While the TI Team recognizes that the selection of the smoothing option requires expert judgment, the text goes on to note that "The smoothing operation within the distributed seismicity zones results in variations of *a*- and *b*-values over scales that were judged by the TI Team to be reasonable . . . ." The report has not provided an adequate basis for making this statement. The text does not compare the computed smoothing results to other studies, and does not point to any explicit data that indicates that the seismicity parameters fall within a reasonable range.	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. The point of the quoted statement is that the selection of smoothing parameters is subjective and an assessment made by the TI Team. The technical considerations that were part of that assessment are given in Section 5.3.2.4. Calculations were also performed 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.
S 5-13. (DMM, CC) *Penalized Likelihood Function — Differences with EPRI-SOG?* In Section 5.3.2.1 the model for the penalized likelihood function for recurrence parameters is formally developed. Many aspects of the approach appear to be similar to those of the EPRISOG Project. It would be useful to specify the differences in the present	The new Section 5.3.2.5 contains a detailed comparison of the approach used in this study and the penalized likelihood. This discussion covers all the differences identified by the PPRP.

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approach relative to the EPRI-SOG Project. The PPRP identifies the following differences (or at least this section of the current report is not clear enough to be sure if these are in fact differences relative to the EPRI-SOG Project): 1. One of the attributes of the EPRI-SOG model was the simultaneous solution of recurrence parameters and incompleteness. On pg. 5-23 the text states the probability of detection (PD) values are calculated in Section 3.3.3 (typo-this should be Section 3.5). This statement plus the remainder of Section 5.3.2.1 give the appearance that PD is calculated independently and no longer simultaneously solved for. 2. The smoothing functions are now analytically determined (objective estimates) as opposed to the general, judgment-based smoothing specified by the expert teams in the EPRI-SOG study. 3. The use of the Monte Carlo-Markov Chain simulation approach to develop alternative maps in the present study as opposed to the parametric bootstrapping used in the EPRISOG study. 4. The use of quarter-degree cells instead of one-degree cells and only using the cells that share sides (4 nearest neighbors instead of 8).	
S 5-14. (DMM, CBR, CC) *Model for the Penalized Likelihood Function — Need for Scrutiny* The development of the statistical approaches used in this Section 5.3.2.1.1 should undergo independent review either using an appropriately qualified member of the TI Team or an outside expert. It is not sufficient to simply provide a description of the approach used. To facilitate a thorough and transparent review, the software developed should be made available for use in the review process. The text implies that selecting a small cell dimension, more cells, is an improvement relative to larger cell dimensions. It is not clear, from a seismologic perspective, considering the short historic record, why this would be the case. Review of the alternative recurrence maps (Appendix J) suggests that there are broad areas where the rates of **M** > 5 are effectively zero, there is wide variation (several orders of magnitude) in rates and *b*-values between alternatives, with generally lower *b*-values (< 0.8). It is not clear how the choice of cell dimension may have impacted these observations. This section has not adequately demonstrated how the method chosen quantitatively compares to other methods such as the kernel approach. While section 5.3.2.4 provides some discussion, it is not sufficient by itself to support the sole use of the method chosen. It appears that the TI Team is using the argument that *b*-values are not constant within a "larger" seismic source. The variation (or lack of variation) of *b*-values is subject to considerable discussion within the ITC. What is the basis for supporting the position that the variation of *b*-values is consistent with the views of the ITC? The weights on the reduced-weight option for the magnitude intervals listed on Table 5.3.2-1 are not properly discussed and justified. Presenting only two figures as a demonstration that the approach is not sensitive to these weights is not compelling. What was the basis for assigning these weights to each of the magnitude intervals? A few additional aspects in Section 5.3.2.1 could certainly be clarified further to enhance readability and understanding: • The reader is challenged to derive 5.3.2-11 from 5.3.2-9. • What is the basis for eight alternative maps as opposed to four or ten? • Section 5.3.2.3 is not clear enough to understand the generation of the alternative maps from the eigenvalues and eigenvectors of the covariance matrix **Sx**.	The software will be made available to the reviewers, as already indicated in the project plan. 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. In the opinion of the TI team, the source-level comparisons shown in Chapters 6 and 7, the comparisons for smaller regions shown in Sections 5.3.2.3, and the comparison to the kernel approach in Section 5.3.2.4 provide sufficient support for the decision to adopt the penalized likelihood approach as the sole method for source-zone recurrence calculations. In addition, the choice of cases A, B, and E samples a broad range of assumptions regarding degree of smoothness or roughness. As indicated earlier, the choice between the penalized-likelihood approach and kernel approach is a choice between statistical tools; the conceptual model for both approaches is the same. The spatial variation of the b value is indeed a topic of much discussion. The TI Lead canvassed several seismologists who have thought about b and its spatial variation, but did not receive any useful guidance in this regard. 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). The choice of magnitude weights has changed: we now use cases A, B, and E. The revised report contains a discussion in Section 5.3.2.2.1 of why other

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	cases were eliminated and how the weights to cases A, B, and E were assigned by the TI Team. Regarding Eqs. 5.3.2-11 and 5.3.2-9 (now 5.3.2-15 and 5.3.2-13), 5.3.2-11 is not derived from 5.3.2-9. The latter represents the likelihood of the recurrence parameters in all cells, while the former represents one of the penalty functions that are introduced to promote smoothness between cells. Regarding the choice of eight alternative maps, the following paragraph was added at the end of Section 5.3.2.1.3: "The initial implementation of this approach required that the number of realizations be a power of 2 because the first few epsilons were sampled using two-point distributions. This number was set to 8 because 4 was considered insufficient and 16 imposed a high computational burden for the hazard calculations. In the present Latin Hypercube implementation, the restriction of a power of 2 no longer exists, but the choice of eight realizations was retained. Tests indicate that 8 realizations, together with Latin Hypercubes, provide an adequate representation of the mean and fractiles of the hazard." Regarding the eigenvalue decomposition, Section 5.3.2.1.3, the text was modified in the hope that it will improve clarity. In essence, the eigenvalue analysis and the Karhunen –Loève expansion are utilized to generate realizations of a random vector with the desired covariance properties. This technique is used in many disciplines.
S 5-15. (CBR, CC) ***Application of the Smoothing Model (Section 5.3.2.2)*** In Section 5.3.2.2, no basis is given for weights on *b*-value priors. The alternatives are shown to be unimportant later, indicate that fact in this section to avoid confusion over the lack of basis for weights.	Those weights were preliminary weights, simply for the purpose of the sensitivity analysis. This potential misunderstanding was removed in the revised report.
S 5-16. (DMM,CBR, CC) *Constant b-value Kernel Approaches* Section 5.3.2.4 discusses the constant *b*-value kernel approaches to smoothing of seismicity. The PPRP believes that significantly more discussion and comparisons are needed to justify the use of a sole unity-weighted branch in the logic tree for this important choice of model. We note that one of the strengths of the penalized likelihood approach, relative to the fixed *b*-value approaches, is the ability to allow for coupled rate and *b*-value behavior within sources. However, the results shown in Figures 5.3.2-3 and 5.3.2-5 suggest the penalized likelihood approach with the CEUS data yields very high smoothing levels on the *b*-value. In other words, the data may be insufficient to make a strong case between variable and fixed *b*-value approaches at the seismic source level—thus significantly reducing one of the strengths and justifications for the penalized likelihood approach. Additional comparisons with the fixed *b*value kernel smoothing approaches are warranted.	As indicated earlier, 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. As indicated in Section 5.3.2.4, the penalized likelihood approach has other advantages over the kernel approach (besides the spatial variation in b). The most important of these are the ability to produce spatially varying estimates of the uncertainty in b and the presence of a natural floor in areas of very low seismicity. A comparison to the kernel approach was added in Section 5.3.2, and it shows a good agreement. Please see the response to 5.5-14 for additional discussion of the issues raised in this comment.
S 5-17. (DMM, CC) *Seismogenic Crustal Thickness* In the title and text of Section 5.4.1.4, the term should be "seismogenic thickness" not "seismogenic crustal thickness." The statement that the focal depth distributions of well studied earthquakes established the basis for the assessment of seismogenic thickness is overly generalized. This section goes on to note that the base of the seismogenic zone is identified as lying near the base of observed focal depths at about the 95th-percentile depth; review of the depths listed in the updated earthquake catalog would suggest that a	The term "seismogenic crust" is commonly used and text is added to indicate that it is synonymous with seismogenic layer or seismogenic zone. The approach used has been modified to be based on the D90 of high-quality focal depths for all seismic sources. Text is added to present the approach and to define its technical basis.

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depth of 13 km may not be consistent with recorded data. If there are specific "well-studied" earthquakes used to establish the TI Team's assessment, these should be listed and summarized. Later in Chapter 6 when discussing the assignment of crustal thickness to specific seismic source zones, the report appears to ignore the stated intent that observed focal depths at about the 95th-percentile depth is an important consideration.	Chapters 6 and 7 will be made consistent with this approach and reported values consistent with those in Table 5.4.1-2.
S 5-18. (CC) *Relationship of Rupture to Source Zone Boundaries* In Section 5.4.1.7, the discussion of strict versus leaky source boundaries is not clear. While it is recognized that TI Team judgment is important here, it seems that some type of systematic approach would be appropriate. It may be important to note that the assumed rupture dimension relationships establish limits that must be explicitly considered in assigning strict versus leaky, and that this constraint is considered on a case-by-case basis. Otherwise it is not clear why some RLME sources move from strict to leaky, given the defined boundary. The same is true for difference between seismotectonic source zones—why are some leaky and some strict?	Explanation added regarding the bases for leaky versus strict boundaries. It is also noted that all sources have sufficient dimensions to accommodate the ruptures consistent with their assessed magnitudes.
S 5-19. (CC) *Assessment of Future Earthquake Characteristic* In Section 5.4, the introduction of Table 5.4-2 invites discussion before the reader has a chance to read the specifics related to each seismic source in Chapters 6 and 7. It is suggested that this table be split into two tables that can be provided as useful summaries at the end of Chapters 6 and 7, respectively, for the sources zones discussed in those chapters. In this way, the reader will have had the benefit of understanding the TI Team's basis for the source-specific weights that are assigned.	Having all of the assessed characteristics for all seismic sources directly in the section where the approach is discussed has value and Table 5.4.1-2 is retained. Chapters 6 and 7 have been structured so that they are consistent in format and discuss the bases for the future earthquake characteristics. All of those discussions now refer the reader to Table 5.4.1-2, for clarity.
Comments by Section ***Entire Chapter 5*** Throughout Chapter 5, we recommend that "event" not be used as a synonym for "earthquake."In order to achieve the needed clarity for a regulatory document, we recommend making a blanket search to replace "event" with "earthquake" where that meaning is the case. Other instances of confusing uses of synonyms are identified elsewhere in the following comments. ***Section 5.1.1*** First sentence: Replacing "led to the belief" with "led to acceptance" would be clearer (note that in line 4 the word "conclusion" is used). On p. 5-2, par. 2, line 9: Suggest replacing "secondary effects" with "liquefaction phenomena associated with them" In the same paragraph, line 10: Suggest replacing "paleoseismic events" with "paleoearthquakes interpreted using the distribution of liquefaction phenomena" In the same paragraph, last line: Replace "studies" with "SSC model assessments" On p. 5-2, par. 3, line 3: Change "EPRI-SOG study" to "EPRI-SOG Project" In the same paragraph, line 8: Suggest replacing "capturing" with "representing" In the same paragraph, line 10: Change "in EPRI-SOG" to "in the EPRI-SOG Project" ***Sections 5.1.2*** In the first paragraph, line: Change "PSHA" to "SSC model assessments" In the same paragraph, last line: Change "PSHAs" to "SSC model assessments" On p. 5-3, par. 2, line 9: Change "CEUS SSC study" to "CEUS SSC Project" ***Section 5.1.3*** In the second paragraph, suggest rewording the first sentence to read: "Another area of ongoing research with potential implications for recurrence behavior relates to geodetic strain rate measurements."	Event changed to earthquake in most cases. Revisions made as suggested.
Section 5.2	Revised as suggested.

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In the first line of the second paragraph: Consider deleting “issue” and change “EPRI-SOG study” to “EPRI-SOG Project”	
Section 5.2.1	
In the first sentence of the first paragraph: Suggest replacing “calls for” with “incorporates”	Revised as suggested.
Section 5.2.1.1	
On p. 5-11, sequential paragraphs describe the results of performing Student’s *t*-test as yielding “a very high probability (*p*-value),” then “a lower *p*-value,” and then “a further reduction in the *p*-value.” But the *p*-values are not given! Finally, the fourth paragraph reports the results of an additional step that “yielded a *p*-value of 0.14.” The other *p*-values also need to be reported and documented for the reader to evaluate whether the extended and non-extended superdomain classifications are statistically significant.	P values given in revised report
Section 5.2.1.1.1	
In the first paragraph, line 4: Consider replacing “known stress” with “known characteristics of tectonic stress”	Revised as suggested.
In the next paragraph, first sentence: Change “study area” to “model region”	
Section 5.2.1.1.2	
In the first sentence: Consider replacing “applicable” with “appropriate”; change “study region” to “model region”	Revised as suggested.
Section 5.2.1.1.3	
In the first sentence: Replace “assigned” with “assessed”	
In the same paragraph, line 3: Consider replacing “an intuitive” with “our subjective”	Revised as suggested.
Section 5.2.1.1.4	
In the second sentence, line 2, delete “likely”; in line 3, change “For this study” to “For this project”	Revised as suggested.
Section 5.2.1.2	
In the last line of the first paragraph: Consider deleting “*possible”* (or explain)	
On p. 5-16, in the second full paragraph, line 4: Consider deleting “relatively” (or explain)	Revised as suggested.
On p. 5-16, in the last full paragraph, line 6: Replace “decided” with “assessed”; in the last sentence of this same paragraph, consider replacing “the following key assumptions are made in the application of” with “the following constraints are placed on the application of”	Revised as suggested.
On p. 5-17, first bullet: Replace “accounted for” with “assessed”	
On p. 5-17, third bullet: Consider replacing “regard for” with “reliance on”	
Section 5.2.1.3	Revised as suggested.
In the first paragraph, line 3: Consider replacing “assigning weights to” with “weighting”	
In the same paragraph, lines 4 and 6: Consider replacing “assigned” with “assessed”	
Section 5.2.1.4	
In the first paragraph, line 5: Consider replacing “assigned” with “assessed”	Revised as suggested.
On p.5-18, in the partial paragraph at the top of the page: Consider replacing “assigned” with“assessed”	
On p. 5-18, first full paragraph, lines 3 and 7: Consider replacing “assigned” with “assessed”	Revised as suggested.
On p. 5-18, second full paragraph, line 3: Consider replacing “assigned to” with “assessed for”	
Table 5.2.1.1	Revised as suggested.
Does the last row contain numbers of earthquakes “Greater than **M** 4.5” or ≥ **M** 4.5?	
Figures 5.2.1-7 and 5.2.1-8	
Typo in legend. Change “Disribution” to “Distribution”	≥ **M** 4.5
	Revised as suggested.

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Section 5.3.1 In the last paragraph on p. 5-19, line 9: Replace “study region” with “CEUS SSC model region”	The term “study region” is common usage and well-understood, so it is retained.
On p. 5-20, first full paragraph, last sentence: Consider replacing: “were judged by the TI Team to be reasonable, given the technical community’s views” to “were judged by the TI Team to represent the technical community’s views”	Considerable additional discussion added on the issue of the community’s views regarding spatial stationarity and smoothing. During the evaluation phase, the larger community’s views were evaluated. During the integration phase, the SSC model was built and that includes the smoothing decisions. So, it is correct to say that the assessment belongs to the TI Team, having given due consideration to the community’s views.
Section 5.3.2.1.1 Regarding m0 and the definition of *v*: Is *v* in fact calculated for $m > m0$ or $m \geq m0$ (e.g., McGuire, 2004; Weichert, 1980)? If calculated as the latter, then corrections should be made to equation 5.3.2-1 (and associated text on pg. 5-20), on pg. 5-29 (paragraph 2, line 2), and perhaps elsewhere.	We use $m \geq mo$ and we corrected the equations and text accordingly. In theory, this is not important for a continuous random variable. Because magnitude are not quite continuous, it has a moderate effect in practice (note: most changes to 5.3.2 were made after Aug. 7 version)
On p. 5-21, third line from the top of the page: Change “This study” to “This project”	Change made in a number of places
On p. 5-22, par. 4, line 2: Consider replacing “one may wish to assign lower weights to lower magnitudes” with “the assessment may result in a lower weight on lower magnitudes”	Change made
In this same paragraph, second sentence: Consider replacing this sentence with “For instance, the magnitude-recurrence law may deviate from exponential, or the magnitude-conversion models or completeness model may be less reliable for lower magnitudes.”	Change made.
On p. 5-22, last paragraph, line 1: Consider replacing “considered” with “incorporated”	Change was considered but it was not incorporated.
On p.5-23, par. 1, line 5: Change reference to “Section 3.3.3” to “Section 3.5”	
On p. 5-25, last full paragraph: Consider replacing “are specified by the expert teams on the basis of judgment” to “are assessed by the expert teams on the basis of their evaluations”	Change made.
On p. 5-26, first text line at the top of the page: Change “study” to “project”	Change made.
On p. 5-26, first full paragraph, line 4: Consider replacing “refer to” with “formulate”	“refer to” was changed to “write”
On p. 5-26, par. 3: In line 1, change “Equation 13” to “Equation 5.3.2-13”; in line 2, consider changing “a characterization” to “an assessment”; in lines 7–8, consider replacing “An additional, practical requirement is that one must represent the epistemic uncertainty by means of a small number of ” with “An additional practical requirement is that epistemic uncertainty must be represented. This can be accomplished by means of a small number of “	Change made using slightly different wording.
On p. 5-27, par. 3, line 4: Change “Equation 15” to “Equation 5.3.2-15”	Change made.

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On p. 5-27, par. 4, lines 7–8: Typo. "maps of to represent"	Change made.
Section 5.3.2.2.1 On p. 5-29, second bullet: Change "in EPRI-SOG" to "in the EPRI-SOG Project" and change "study region" with "SSC model region"	Change made.
Section 5.3.2.2.2 On p. 5-31, first full paragraph, line 6: Replace "assigned" with "assessed"; in line 7, consider deleting "reflected"	N/A. Section was almost entirely re-written.
Section 5.3.2.3 Last line: Consider replacing "small-scale" with "local"	Change made.
The first example used to examine model results in parameter space needs to be more explicit in describing how the expected earthquake counts in the polygons are derived. It would also be helpful to discuss the data error bars for the magnitude bins with no events. The figure captions for these figures need additional information.	Explanations added. Error bars no longer shown for bins with no data.
Section 5.3.2.3.1 On p.5-32, par. 2, line 1: It is an overstatement to claim that Figures 5.3.2-20 and 5.3.2-21 show a "very close" agreement between model and data. In the following paragraph, "good agreement" is claimed between model and data for results shown on Figures 5.3.2-22 and 5.3.2-23. Admittedly, such statements are qualitative, but don't stretch the reader's credulity.	Statements are consistent with revised results.
Section 5.3.2.4 In the first paragraph, first sentence, change "this study considered" to "this project evaluated"; in line 3, change "considered" to "evaluated"; in line 4, change "study" to "project" In the second paragraph, line 2, change "has been specified subjectively" to "has been assessed subjectively"	Change made.
On p. 5-34, next-to-last paragraph, line 6: Consider changing "idea" to "understanding"	Change made.
Section 5.3.3.1 Equation 5.3.3-2 should be checked. The N! in the denominator appears to be an error. Because the normalization procedure used to generate the probability density function for λ isn't explained, it's not evident why the y-axis values are so low (0.00, 0.02, 0.04). Rescaling the x-axes of both plots would be helpful to avoid the awkward labeling of 5e-05, etc., making it easier to read the plots. Checking the five discrete levels on the CDF points to an error in Table 5.3.3-1: The value of cumulative probability in column 1, row 1 can't be 0.304893(other values in the table suggest it should be 0.034893).	Other changes made as suggested.
Section 5.3.3.1.3 First paragraph: In the first sentence, consider replacing "is generally used to represent uncertainty in the inputs" to "is used to represent uncertainty in the SSC model "inputs"; in the last sentence, change "CEUS project" to "CEUS SSC Project"	
Section 5.3.3.1.3 In the section title, consider changing "Estimation" to "Assessment"	

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Section 5.3.3.1.3 On p. 5-39, par. 3, line 1: Note that a 50-year life is stated elsewhere On p. 5-39, last line: Missing word. Insert “on the time before present” ***Section 5.3.4*** In the section title, consider “Assessment of RLME Magnitude Distribution” First paragraph: In the first sentence, consider deleting “are intended to”; in line 6, change “study” to “project; in line 7, consider deleting “set to be”; in the last sentence, consider substituting “is” for “was chosen as”	
Section 5.4 **(and *Tables 5.4-1 and 5.4-2*)** Some additional discussion is required to explain the bases for the development of weights for the characteristics (or improved cross-referencing). On p. 5-41, par. 1, line 5: Consider deleting “a consideration of” On p. 5-41, par 2, line 7: Consider replacing “considering” with “evaluating” ***Section 5.4.1*** First paragraph: In line 3, consider replacing “considered” with “evaluated”; in lines 7–8, consider rewording the last clause to read: “the assessed values in column 2 of the table are based on assessments by the TI Team of the default characteristics that represent the current state of scientific knowledge” ***Section 5.4.1.3*** In the first sentence: Consider rewording to read: “information about the characteristics of earthquake sources, modeled as finite faults in much the same manner as earthquake sources are modeled in the WUS.” In line 6, consider replacing “in light of” with “using” In the last line: Consider deleting “largely” (or explain) and replacing “consideration” by “evaluations” ***Section 5.4.1.4*** In line 2: Consider deleting “upper” (or explain) In line 4: Replace “study” with CEUS SSC Model” On p. 5-43, first partial paragraph at top of page: In line 1, consider replacing “some” with “a high”; in line 2, replace “study” with “CEUS SSC Model” ***Section 5.4.1.5*** In line 5: Replace “capture” with “represent”; in line 6, consider rewording to read: “The relationship used (Somerville et al., 2001)” In the last line: Replace “study” with “assessment” ***Section 5.4.1.6*** In line 2: Consider replacing “a consideration” with “an evaluation” In line 4: Replace “assumed to be equidimensional” to “assessed to be equidimensional” and change “For progressively larger areas” to “For progressively larger rupture areas” In line 6: Consider deleting “it was assumed that” In line 10: Consider deleting “assumed to be” In line 11: The NAGRA approach should be explained, as reviewers are unlikely to have this report. In the last line: Consider replacing “associated with” with “of” ***Section 5.4.1.7*** In line 1: Consider replacing “Assuming” with “For”, and “assumed to have” by “defined	Referencing to Chapters 6 and 7 has been added to indicate that the technical bases for the weights are included in those chapters. Also, Chapters 6 and 7 have been made consistent and each section refers to applicable future earthquake characteristics. “Study region” is common terminology and has been kept. All other revisions made as suggested.

Comment	Summary of Revisions to Report
by" In line 2: Replace "defined" with "represented" In line 5: Replace "assigned to" with "assessed for"	
CHAPTER 6—SSC MODEL: Mmax ZONES BRANCH	
General Comments **G 6-1. (NAR)** The core of the TI Team's assessment of the Mmax zones approach within the CEUS SSC model is described in this chapter. As such it is a critical chapter understanding of the assessment by future users. The TI Team has described an immense amount of data together with its evaluations of these data in characterizing and assessing this branch of the CEUS SSC model; in doing so the TI Team generally has described the assessment in sufficient scope and detail to inform future users of the model.	No revision necessary.
G 6-2. (CBR, U) Chapter 6 is generally well written. The discussion of each of the RLME sources is laid out logically providing a general description of the source, localizing feature(s), geometry, recurrence, and maximum magnitude. However, the basis for some of the assessments is not clearly articulated. Some specific examples are mentioned below, but the PPRP recommends the TI Team review all the subsections with an eye to improving the clarity and strength of the bases for assessments. For example, it is not always clear why one source is using the generic seismogenic crustal thickness assumptions while others are not. The same holds true for differences in assessed weights for clustered behavior. Another example is the empirical relationships used to derive magnitudes given assumed dimensions for seismic sources. To the extent possible, the TI Team needs to clearly establish their overall approach to assessing these weights; in some instances additions to Chapter 5 should be considered to establish the basic approach to how the TI Team decided to modify generic weights, or what generic data (discussed in Workshop #2?) influence the assignment of weights to individual seismic sources.	The bases for all assessments have been reviewed and revised as necessary for clarity. Explanation has been added for the assessed future earthquake characteristics and for assessed weights for clustered behavior. Dimensions of seismic sources have been checked to be sure they are compatible with the dimensions implied by the empirical relationships. The technical bases for all weights have been reviewed to ensure completeness and clarity. There are no "generic weights" for the assessments; all weights must be discussed and supported. Perhaps the comment refers to the "default" future earthquake characteristics; Discussion added to Section 5.4 regarding the use of the default characteristics or source-specific assessments and Table 5.4-2 added summarizing the source assessments.
G 6-3. (CC) In the 3rd paragraph of Section 6.1 the report states: "By identifying the RLME sources and including them in the model, there is no implication that the set of RLME sources included is, in fact, the total set of RLME sources that might exist throughout the study region." This sentence and the remainder of the paragraph make a very important point about a fundamental assumption included in this model. This point needs to be articulated, specifically in Section 4 of the report as well.	The point is made in Section 4 of the report, as suggested.
Specific Comments **S 6-1. (CC, SSHAC)** *Achieving Clarity Necessary for Future User* The importance of Chapter 6 for informing future users of the CEUS SSC model places a heavy demand on the TI Team to clearly document its assessment. As a framework for achieving necessary clarity of documentation, it may be useful for the TI Team to keep in mind the steps involved in implementing the SSHAC assessment process: (1) compiling the community knowledge; (2) compiling the relevant data; (3) evaluating the community's knowledge, understanding the community's uncertainty, and characterizing alternatives for assessment; and (4) assessing weights for the alternatives representing the	Documentation of the technical bases for the assessments has been reviewed and revised as appropriate. Suggested wording for Section 6.1 incorporated as suggested.

Comment	Summary of Revisions to Report
community uncertainty. Generally the TI Team has provided very thorough documentation of steps 1 and 2 in this chapter. Documentation of steps 3 and 4 is often less clear. Much of the lack of clear presentation can be attributed to misuse of terms. This is particularly evident in descriptions of the TI Team's assessments where many different words (define, characterize, modeled, given, constrain, allowed, chosen, assign, assumed …) are used for assessment. In addition to conflicting meanings, the impact of using words with such diverse meanings for the core SSHAC methodology requirement, namely "assessment," is that they undermine the essential discipline that a SSHAC assessment requires. Other instances of misuse of terms coupled with lack of completeness in descriptions detract from the reviewers' understanding of the evaluations performed and weaken the usefulness of the document for future users. Consider as an example the following edited first paragraph of Section 6.1 compared to the original. By definition, RLME sources are the locations of repeated (more than one) large magnitude (**M** ≥ 6.5) earthquakes in the historic and (or) paleoearthquake record. Because of the rarity of repeated large-magnitude earthquakes relative to the period of historical observation, evidence for these earthquakes comes largely from the paleoearthquake record. For example, paleoearthquakes identified by interpretations of paleoliquefaction features and fault displacement (paleoseismic) studies combined with those in the historical record result in the catalog of large-magnitude earthquakes in the central New Madrid region and at Charleston. At Charlevoix, RLMEs are observed in the historical record and are supplemented by the paleoearthquake record. For the Meers and Cheraw faults as well as the Wabash Valley source, there are no large magnitude earthquakes in the historical record. The RLMEs for these sources are characterized by evaluating repeated surface-faulting displacements identified in trenches across the faults and, for the Wabash Valley source, by interpretations of the geographic distribution of paleoliquefaction features.	
S 6-2. (CC) *Improving the link to the Data Summary and Data Evaluation Tables* Prior to discussing specific seismic sources, the reader should be reminded that the information in the Data Summary and Data Evaluation tables provides a comprehensive assessment of the current information related to each seismic source. It is the PPRP's view that external readers and reviewers of the CEUS report need to be at least familiar with those tables prior to objectively commenting on the TI Team's assessment. This section would also benefit from a brief discussion of how the earthquake recurrence for RLME sources was modeled, specifically how the lower-bound magnitude for integration for these sources was established by the TI Team.	Discussion added to Section 6.1 as suggested.
S 6-3. (DMM, CC, U) *Earthquakes of **M** ≥ 6.5 in the Charlevoix RLME* The first paragraph in Section 6.1.1 describes two historical earthquakes of **M** ≥ 6.5 (one of **M** 7 in 1663 and one of **M** 6.5 in 1870). The reader is then pointed to the Charlevoix RLME logic tree (Figure 6.1.1-2) which has branches for the "Events/Data" node that do not appear to include the two historical earthquakes in the stated event count for **M** ≥ 6.5 (e.g., "3 eqs in 9.5– 11.2 kyr"). Section 6.1.1.2 goes on to describe paleoearthquakes, including one "historic" paleoearthquake with "a bracketed age of at least 540 yr BP." These descriptions need to be clarified for the reader to understand the basis of rate information. To appearances, the RLME rate information and calculated uncertainties for Charlevoix in	Text clarified to indicate that the 1663 and 1870 earthquakes are RLMEs. Tuttle's historical paleoearthquake could be either of these. Therefore two prehistoric earthquakes occurring at 5,000 and 10,000 years B.P. are the other two RLME events. Updated and revised recurrence calculation section clarifies how the historical earthquakes are used.

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the HID (Appendix H, Section 5.2) do not account for the two historical earthquakes in 1663 and 1870—only the paleoearthquakes. (For an example of better clarity, see the logic tree and HID tables for the Charleston RLME, where the reader is explicitly informed with labeling such as "1886, A, B, C" that the count includes one historical earthquake and three paleoearthquakes.) Adding to the problem of event counts, the text in Section 6.1.1.2 (first sentence of par. 2) states that "Tuttle and Atkinson (2010) provide evidence for at least three Holocene paleoearthquakes in Charlevoix with **M** ≥ 6.2 . . . ." If 6.2 is not a typo, then an assessment has to be made for how many of those events were of **M** ≥ 6.5 (or explain assumptions).	
S 6-4. (U, DMM, CBR, CC) *Unclear Interpretation Impacting Uncertainty* In Section 6.1.1.2, par. 3, the third sentence states, "Focal mechanisms for earthquakes of magnitude ≥ 3 show reverse faulting, whereas smaller-magnitude earthquakes indicate some strike-slip and normal faulting, suggesting that local stress conditions affect rupture style (Lamontagne and Ranalli, 1997)." This indicates that there is a local source of tectonic stress. If this is the intent, the interpretation would be in conflict with the community's knowledge and would require additional evaluation of uncertainty.	Current literature (Baird et al., 2009) indicates that the Charlevoix RLME is attributed to the interaction of the impact crater and rift faults. Introductory text clarifies the local stress discussion.
S 6-5. (CC, DMM) *Charlevoix—Geometry and Style of Faulting* In the fourth paragraph of Section 6.1.1.2, while discussing the geometry and style of faulting for the Charlevoix RLME, the report indicates that future ruptures for this source are modeled as randomly-oriented thrust faults with dips between 45 and 60 degrees in either direction. Later on p. 6-6 the report indicates the RLME boundaries should be treated as leaky with ruptures permitted to extend beyond the source boundaries. There are a number of questions that arise in interpreting these statements that apply to several other RLME sources as well. The preceding paragraphs of the section describe fault orientations derived from small magnitude earthquakes. Keeping in mind the fact that a RLME source is for large (**M** ≥ 6.5) earthquakes and hence requires large rupture areas, the applicability of these results for small magnitude earthquakes needs to be carefully explained. For the RLME sources it is not clearly explained what assumptions are being made regarding the recurrence model, i.e. is it Mmax ± 0.25 magnitude unit about each of the four identified Mmax values (noted briefly in Section 5)? This would be a "perfectly characteristic" or maximum-moment type model. This represents the epistemic uncertainty in Mmax plus the aleatory variability in the future occurrence of each of the characteristic events. The "interaction" between the lower ranges of magnitudes for the characteristic RLME source that will overlap with the upper end of the truncated exponential distribution being applied for the Mesozoic Extended Mmax source zone needs to be explained. This point is true for all the RLME sources. Since Charlevoix is the first of the RLME sources described, the TI Team should clearly explain these issues in this section.	Text clarified to use reverse for dips between 45 and 60 degrees. Paragraph added at end of section 6.1 to address issue of combining RLMEs with their host seismic source.
S 6-6. (CC) *Charlevoix—Maximum Magnitude* In the last paragraph of Section 6.1.1.3, the discussion of boundary dimensions leading to the TI Team's conclusion that the boundary is leaky requires more discussion. Given the assigned Mmax values, are the boundary dimensions too small to fit these magnitudes fully within the boundaries? To the extent possible quantitative discussion should be provided.	The discussion of the boundary amplified to provide quantitative reasons for assigning them as leaky

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S 6-7. (DMM, CC) *Unclear Logic for Performing Assessment* In the first paragraph of Section 6.1.2.1, the meaning of "time periods of interest" as used is not clear. Is it the projected life of an NPP, the projected life of the CEUS SSC model, a geologic time period? In any case it is not clear how "time periods of interest" influences an assessment of whether tectonic strain release in the Charleston area is in or out of a cluster. Moreover, the TI Team must explain its evaluation, characterization, and assessment of the community's knowledge about tectonic driving forces and the physics of tectonic strain release in a clustered sequence of large earthquakes at about 500-year intervals in the absence of any measurable strain deformation. Otherwise, the reviewer and potential future user of this report will not be able to understand the basis for the assessment.	Text revised for clarity and to delete phrase "time periods of interest".
S 6-8. (CBR, U) *Charleston—Evidence for Clustered Behavior* In Section 6.1.2.1, the TI Team's assessment of "in" or "out" of a cluster requires more justification. While the TI Team appropriately discusses the evidence of long-term versus short-term behavior, the fact remains that there is direct evidence of repeated large earthquakes in the Holocene and little if any direct evidence that we are at the end of a cluster. Perhaps there needs to be some type of generic discussion of this issue in Chapter 5, with Workshop #2 providing the ITC background to characterize and assess this issue. Otherwise the assessment that we are at the end of a cluster seems to come across as somewhat arbitrary versus informed assessment. What is different between Charleston and other RLME sources such as Cheraw?	Generic discussion added to Ch. 5 regarding the issue of assessing clustered behavior.
S 6-9. (CC) *Charleston—Geometry and Style of Faulting* In Section 6.1.2.3, the discussion of boundary dimensions leading to the TI Team's conclusion that the boundary for the three source geometries is either strict or leaky requires more discussion. Given the assessed Mmax values, are the boundary dimensions too small to fit these magnitudes fully within the narrow source boundary relative to the other two source definitions? To the extent possible, quantitative discussion should be provided. The TI Team's assessment of using the default values for seismogenic crustal thickness requires additional justification. While all of the references cited for seismogenic crustal thickness are within the range for the default values, several suggest more preference (higher weight?) for values between about 15 and 20 km. Given this, the basis for assessing a weight of 0.4 to a seismogenic crustal thickness of 13 km is not clear.	Seismogenic crustal thickness distribution revised. Discussion expanded to provide additional rationale and justification for Narrow and Local source configurations.
S 6-10. (CC) *Charleston—Weights for Charleston Narrow and Regional Sources* In Section 6.1.2.3.1, the discussion of the basis for the weight assessed for the Charleston Local Source seems well developed. However, the discussion for the relative weighting of the Charleston Narrow and Regional sources is not clear.	Text revised to provide additional rationale for weights on Narrow and Regional source configurations.
S 6-11. (U, DMM, CBR, CC) *Contextual use of the term "microseismicity"* In Section 6.1.2.3.1, first paragraph, the use of the term "microseismicity" potentially leads to confusion about tectonic processes. "Seismicity" is defined in terms of the spatial and temporal occurrence of earthquakes, a generally accepted measure of space-time tectonic strain release in earthquakes. The term "microearthquake" is now generally accepted to mean an earthquake of **M** ≤ 3. But the PPRP is not aware of a community definition of the term "microseismicity." Consequently, the TI Team needs to explain its use of the term in the context of this evaluation. For example, is "microseismicity" used to	The term "microseismicity" has been removed from section.

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mean "seismicity of microearthquakes," possibly implying a strain cycle process that is different from that implied by "seismicity"? The discussion should clearly convey how the TI Team evaluates "microseismicity" as one of the four observations cited as the basis for assessing the "Charleston Local source zone"?	
S 6-12. (CC, DMM) *Charleston—Recurrence* Given the uncertainty in length and completeness of the paleoliquefaction record and interpreted number of separate episodes, and the very general description of the process used to develop recurrence values contained in Section 5.3.3, the PPRP strongly encourages the TI Team to include a step-by-step example of the application of the procedure used for at least one of the RLME sources. This should include additional figures and text. This will significantly improve clarity and transparency. Consider the following criticisms, some of which apply to recurrence calculations and corresponding HID tables for other RLMEs: In Section 6.1.2.5, the recurrence method is noted to be "based solely on inter-event times estimated from the paleoliquefaction record." What this section fails to communicate clearly to the reader—especially amid the elaborate analysis and description of those inter-event times— is that the methodology used to calculate the annual frequency of earthquakes of **M** ≥ 6.5 (Section 5.3.3.1.2) ultimately uses only the elapsed time since the oldest event in the sequence and the number of events counted. The Charleston RLME logic tree (12th node), for example, points the reader to the HID tables. Referring to those tables, it will not be readily evident to the reader that the key pieces of information are N and the elapsed time since the oldest earthquake in the sequence of N events. Also, given that the oldest earthquakes (Table 6.1.2-1) have an age specified by a range, an explanation is needed whether (or how) that uncertainty was addressed. The unalert reader (or analyst) examining the HID tables for computed annual frequencies for the Charleston RLMEs may potentially be confused by: (1) the inverted order for the 5-point distributions compared to Table 5.3.3.-1, which was used to define the 5-point distribution; and (2) the need to refer to Tables 6.1.2-1 and 6.1.2-2 to discern the elapsed time since the oldest earthquake counted in the sequence. For example, examining "Table Charleston_HID-3," it may escape the reader's attention that the 5-point distribution is not for four events in 5500 years, but rather four events in 1,524–1,867 years (or possibly in 1,569–1,867 years). To reproduce the results in the table (and for virtually all the Poisson-model tables in the HID), there is no explicit information about the exact elapsed time that was used. To add to the confusion, the text does not explain what the age ranges listed in Tables 6.1.2-1 and 6.1.2-2 represent. Do they represent the mean ± 2 sigma from the probability distributions in Figure 5.3.3- 2? As the reader progresses to the BPT renewal model there are terse descriptions of the weighting (without justification of the weights) and cross reference to Section 5.3.3 for methodology—but the text does not provide any discussion of the results. How do the BPT results compare to those for a Poisson model? Do they make sense?	Discussion of these issues added to Ch. 5 on the methodology for assessing recurrence for RMLE sources.
S 6-13. (CC) *Charleston—Time Period for Recurrence* In Section 6.1.2.5.2 the discussion of the completeness period of the paleoliquefaction record (at least the last three sentences) seems equivocal. However, the weight assessed for the shorter completeness period, 0.8, indicates a strong preference; additional	Text revised to provide additional clarity and rationale for relatively high weight on shorter completeness period.

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discussion seems required to justify the strong weighting.	
S 6-14. (U, CC) *Clear Representation of the Community's Knowledge for Characterizing Alternatives and Uncertainties* The discussion in Section 6.1.2.5.3 calls attention to the need for clear representation of the community's knowledge and uncertainty as the basis for characterizing alternatives in the logic tree and for assessing the community uncertainty distribution. We offer the following edited paragraph as an example for comparison with the original paragraph: The ninth branch of the Charleston logic tree represents alternative characterizations of the community's knowledge and the TI Team's assessment of the community uncertainty for recurrence of large earthquakes in the Charleston Seismic Zone, developed as part of the CEUS SSC Project (Figure 6.1.2-1). Alternative interpretations of the distribution of liquefaction features include a total of four large earthquakes in the past approximately 2,000 years and between four and six large earthquakes in the past approximately 5,500 years. The alternative characterizations represented in the logic tree are based on (1) interpreted length of the paleoliquefaction record; (2) interpreted types of constraining ages; and (3) evaluations of the area distribution and interpretations of which prehistoric liquefaction features were caused by large magnitude earthquakes centered in the Charleston area and which were caused by moderate-magnitude local earthquakes. The clarity of this section could be greatly improved by technical editing to better link the descriptions of the current knowledge with characterizations of alternatives in the logic tree and with the assessment of the community uncertainty distribution.	Text revised for clarity as recommended.
S 6-15. (CC) *Cheraw Fault—Evidence for Temporal Clustering* In Section 6.1.3.1, the discussion of weights assigned to in or out of a cluster requires additional discussion given the statements that there is no evidence to indicate that this source is out of a cluster. It is not clear what the differences are for this source relative to other sources, such as Charleston as an example.	Text modified to change weights from 0.8 to 0.9 for 'in cluster' and 0.2 to 0.1 for 'out of cluster' based on the lack of evidence to support a 'out of cluster' behavior.
S 6-16. (CC, U) *Cheraw Fault—Magnitude* In Section 6.1.3.3, p. 6-19, the discussion of relationships used to estimate magnitude from fault area includes "Somerville et al. (2001)." At various places in the Project report the citations for this relationship include Somerville et al. (2001), Somerville et al. (2005), and Somerville and Saika (2000). This needs to be double-checked and a validated reference cited (the Somerville references are in the gray literature and difficult to find, and the basis of the citation was not evident). A verifiable citation and reference need to be included in the Project database.On page 6-20, in the discussion of maximum and average displacement for the Cheraw fault the report notes: "There is insufficient information to establish whether the displacement per event measured at the ***sole trench site*** (emphasis added) along the Cheraw fault represents average or maximum values." In the last sentence of this paragraph, the report concludes the values are maximum values. The conclusion does not seem to follow from the discussion in the paragraph as written.	There is uncertainty regarding whether the displacement/event recorded at the site represents an average or maximum value for the fault as a whole. Revisions to text have been made to clarify uncertainty in the estimated range of average and maximum slip per event at the site. The Mmax distribution assigned to the Cheraw encompasses the range of **M** (6.8-7.2) suggested by displacements suggested by these revised estimates. Table 6.1.3-1 has been added to show the range of estimated magnitudes from different empirical relationships.
S 6-17. (CC) *Meers Fault—Clustered Behavior* In Section 6.1.4.1, the explanation of weights assessed for in or out of a cluster requires additional discussion, given the statements that there is no evidence to indicate that this	Additional discussion was added to Chapter 5 regarding the evaluation of clustering behavior for RLMEs.

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source is out of a cluster. It is not clear what the differences are for this source relative to other sources such as Charleston as an example.	
S 6-18. (DMM) *Meers Fault—Discussion of Potentially Relevant Data* In Section 6.1.4.2, potentially relevant data for the assessment are not discussed. Specifically, the Meers fault is located on the sector of the boundary of the Wichita uplift that has greatest structural relief by a wide margin. The magnitude of the structural relief between the Wichita Mountains and the Anadarko Basin is the source of a very large gravity gradient indicating significant induced stress across the northern Wichita Mountains frontal fault system along this sector. A discussion of these potentially important data should be included for perspective. Also, is "Arbuckle-Wichita-Amarillo uplift" a proper usage? A reference to the source of this usage is needed.	The section in question discusses the "localizing feature" branch of the logic tree. The presence of a gravity gradient was not used to argue whether the Meers-like activity should be localized on the fault or allowed to occur throughout the aulacogen. Also, a gravity gradient does not necessarily say anything about the state of stress on a fault system. The gravity gradient is used in helping to define the geometry of the zone and is discussed elsewhere in the section. A-W-A is a name used in the literature. See references in data summary table (e.g., Perry 1989). There is no need to cite a reference in the text just for this naming convention; the intention and meaning of the name is clear in the existing text.
S 6-19. (CC, U) *Meers Fault—Localizing Feature* In Section 6.1.4.2, it is not made clear in the discussion of the potential for the occurrence of Meers-like ruptures in the Oklahoma Aulacogen why "only one Meers-like structure is active within the aulocogen at a time."	Added "This interpretation is based on the fact that there is no evidence of Quaternary activity on other faults within the aulacogen."
S 6-20. (DMM, CC, U) *Meers Fault—Geometry and Style of Faulting* In Section 6.1.4.3, on page 6-24: When Meers-like earthquakes are allowed to migrate off the fault they are limited to occurring within the OKA. How are the earthquakes within the OKA to be modeled? The next paragraph suggests the strike to be N60W (parallel to the Amarillo- Wichita-Arbuckle uplift) with a dip between 40 and 90 degrees. However (and this comment holds for several of the other RLME sources), it is not clear how the analyst should model this situation. As a series of fictitious parallel faults distributed throughout the appropriate portion of the OKA? If so, how many are appropriate? This answer will clearly be determined by the location of the site of interest relative to the source. What was assumed by the hazard analysts for the demonstration and sensitivity calculations? On pages 6-24 and 6-25: The discussion indicates there is a significant amount of uncertainty in the appropriate H/V values to assign to the displacement observations. It does not seem as if this uncertainty is represented in the final recurrence values for the Meers RLME. Additional clarification seems necessary. On p. 6-24, third paragraph: The assignment of seismogenic thickness for the Meers fault source based on one reference seems to be inconsistent with how this parameter has been assessed for other seismic sources including Charleston. Consistency in assessment of each of the branches of the logic tree is an important consideration. If outside reviewers see inconsistencies in the assessment of weights for the logic tree branches, then their confidence in the overall assessment may be weakened.	Details of the source model implementation are provided in the HID. Recurrence for the Meers fault is based on the record of discrete faulting offsets in the trenches and does not depend on H/V ratios. The methodology for assessing seismogenic thickness for all seismic sources is the same and is discussed in Section 5.4
S 6-21. (CC, U) *Meers Fault—RLME Magnitude* In Section 6.1.4.4, the use of four seismic source dimension relationships to characterize and assess magnitude for this seismic source contrasts with the approach to other seismic sources. It is not clear why the Meers source is any different than other seismic sources to justify these differences. A consistent approach to characterizing and assessing magnitude based on source dimensions seems to be appropriate. There does not appear to be any unique property of the Meers fault that would justify using rupture	Unlike most of the other RLME sources, the Meers fault is a discrete mapped fault, thus allowing fault-specific characteristics to assist in the assessment of RLME magnitude.

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area relationships for the Meers fault but not other RLME sources such as Charlevoix, Charleston, or Cheraw.	
S 6-22. (CBR, CC) *New Madrid—RLME Magnitude* In Section 6.1.5.3, the use of unpublished information (Hough and Page) needs careful consideration. Has the paper been accepted for publication? Additionally, the text discusses the use of the characteristic earthquake recurrence model. Other sections of the text indicate that the characteristic earthquake recurrence model is not being used.	The Hough and Page manuscript has now been published: Hough, S. E., and M. Page (2011), Toward a consistent model for strain accrual and release for the New Madrid Seismic Zone, central United States, J. Geophys. Res., 116, B03311. References to the characteristic model have been deleted from this discussion. The model includes epistemic uncertainty in the average size of the RLME earthquakes, plus aleatory variability in the size of next earthquake of one-quarter magnitude unit. Section 5.3.3 has been modified to clarify this approach.
S 6-23. (CBR, CC) *New Madrid—Recurrence* Section 6.1.5.4 presents an insufficient basis for the assessed weights for the two alternative recurrence models characterized. The text should refer to Workshop #2 for discussion of this topic and present more information to justify the weight assessed for the renewal recurrence model.	Additional discussion of the basis for the weights added to the discussion of recurrence models in Section 5.3.
S 6-24. (CBR, CC) *Reelfoot Rift—Eastern Rift Margin Fault, Evidence for Temporal Clustering: Section 6.1.6.2* In Section 6.1.6.1, for this seismic source, the TI Team has assessed non-clustered behavior with a weight of 1.0. The evidence for this assessment is stated to be insufficient information on the number or timing of earthquakes. This contrast with other RLME sources where the main issue pertained to evidence of short-term versus long-term behavior and the logic that short-term rates cannot extend through extended time frames. That logic also appears to apply to the ERMF. The TI Team needs to develop a consistent approach to assessing clustered versus non clustered behavior.	The text has been revised to state… "The available data regarding number and timing of recent earthquakes and long term slip rates for the ERM sources are not sufficient to evaluate whether the ERM RLME sources exhibit evidence for temporal clustering. Therefore, this branch of the tree is not applicable to the Reelfoot Rift ERM_S and ERM_N RLME sources.
S 6-25. (CBR, CC) *Reelfoot Rift—Marianna Zone, Evidence for Temporal Clustering* In Section 6.1.7.1, the text states, "It also is unclear whether some of the paleoliquefaction features are due to earthquakes on the Eastern Rift Margin (ERM, RLME) source . . . ." Given this statement, it is not clear why this seismic source has a probability of activity of 1.0. The discussion and justification of the weight for temporal clustering need to be strengthened. Similarly, the basis for characterizing the seismic source boundary is "leaky" needs to be improved.	The size and number of features in the Marianna area suggest that most if not all of the liquefaction features are due to a local source rather than a more distant ERM source. It is acknowledged however that some of the paleoliquefaction features in the Marianna area could be related to an earthquake on the ERM. The text has been modified to clarify that there is evidence of a local source. Tuttle (WS #2) and others have suggested that seismicity migrates within the RRZ on a 5-15 kyr time frame. The apparent clustering of events in the early Holocene and lack of recognized events in the late Holocene has been postulated to support this concept (i.e., that the locus of activity is currently in the NMSZ rather than in the Marianna region). A statement regarding this concept has been added to support the 0.5 (in versus out) of a cluster weight assigned to this source. The 'leaky' boundary acknowledges that the location of the source of the earthquakes giving rise to the Marianna features is uncertain.

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S 6-26. (CC) R*eelfoot Rift—Marianna Zone, Geometry and Style of Faulting* In Section 6.1.7.2, last paragraph, the probability distribution on seismogenic thickness is different than the default distribution. Given this, the text should provide more details on the number of well-located earthquakes in this source and how they are used to establish a distribution on seismogenic thickness that is different than the default values.	The seismogenic thickness distribution in the Reelfoot rift is based on recent analysis and relocation of earthquakes using an improved velocity models for the northeastern part of the rift (Shumway, 2008) and along the eastern margin (Chiu et al., 1997). These studies indicate that the best located events lie within the upper 16-17 km of the crust. Therefore, this distribution rather than the default distribution was used.
S 6-27. (CBR, CC) *Reelfoot Rift—Commerce Fault, Evidence for Temporal Clustering* In Section 6.1.8.1, the text notes that the liquefaction and secondary faulting used to document Holocene events may be related to strong ground motion from earthquakes occurring elsewhere in the Reelfoot Rift. Given this statement, it is not clear why this seismic source has a probability of activity of 1.0. The basis for assessing a weight of 1.0 to nonclustered behavior is not clear.	The text has been revised to state… "The available data regarding number and timing of recent earthquakes and long term slip rates for the Commerce RLME source is not sufficient to evaluate whether the Commerce RLME source exhibits evidence for temporal clustering. Therefore, this branch of the tree is not applicable to this source.
S 6-28. (CC) *Reelfoot Rift—Commerce Fault, Geometry and Style of Faulting* In Section 6.1.8.2, last paragraph, the basis for characterizing the northwest and southeast boundaries of the seismic source as fixed and the northeast and southwest boundaries as "leaky" is not clear.	The northwest and southeast boundaries, which are defined by the general limits of the CGL as defined by Hildenbrand are sufficiently wide enough to cover the zone of surface faulting that has been identified by various researchers. There has been less work to define the northeastern and southwestern extent of the zone of Quaternary deformation and the geophysical lineament can be traced in both of these directions beyond the limits of the paleoseismic investigations. Therefore, the TI team judged that leaky boundaries would serve to represent the greater uncertainty in the possible extension of ruptures along the CGL.
S 6-29. (CC) *Wabash Valley—Temporal Clustering: Section 6.1.9.1* In Section 6.1.9.1, the basis for the weight of 1.0 on "in a cluster" needs to be improved and to be consistent with the bases for this assessment for all RLME seismic sources.	The last sentence of Section 6.1.9-1 has been revised to read…"Therefore, this branch of the tree is not applicable to the Wabash Valley RLME source."
S 6-30. (DMM, CC) *Wabash Valley—Future Ruptures* On pages 6-59 and 6-60 there is no specific discussion of how the future ruptures are to be modeled. The text refers to Table 5.4-1 (should be Table 5.4-1 and 5.4-2). But as noted previously, additional guidance for the hazard analyst would be useful.	The appropriate table callout should be Table 5.4-2 (not both Tables 5.4.1 and 5.4-2). The text is modified accordingly. The information provided in Table 5.4-2 and the associated discussion in the Section 5.4 text should be sufficient input for the hazard analyst.
S 6-31. (CC) *Wabash Valley—Alternative Mmax Zones* In Section 6.2, the discussion of alternative Mmax zones only discusses the Bayesian approach to Mmax estimation and its relevance to source zone characterization. The consistency of the results using the Kijko method should be discussed as well.	The results derived from the Kijko approach are now discussed.
S 6-32. (CC) *Criteria for Definition of Boundary—Mesozoic Extended Narrow Zone* In the last sentence of Section 6.2.1.1 on p. 6-64, the text states: "These observations support the weight of 0.8 that this geometry represents crust extended in the Mesozoic." The PPRP does not feel the section make the case well. A series of well written observations are presented, but the relevance of the observations to source characterization and specifically to a weight of 0.8 is not clearly articulated. This same comment applies to the other sections on Mmax zones.	Section 6.2 has been rewritten to better outline criteria used to differentiate MESE versus NMESE crust and more directly relate the weight assigned to the wide versus narrow geometries to these criteria.
S 6-33. (CBR, U) *Comparison of Recurrence Parameters to Catalog*	Two additional cycles of model-building and hazard feedback were conducted

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As discussed in Section 6.4.2, Figures 6.3-7 through 6.3-16 (should be corrected to read 6.4-7 through 6.4.16) show that the recurrence model for the large seismic source zones tends to overestimate the rates for magnitudes 5 or higher. What does this mean to the TI Team? A systematic trend such as the one discussed, should be questioned in detail by the TI Team in terms of evaluating whether all assumptions of the analysis are appropriate. The consistent overestimates of the rates suggest that assumptions related to smoothed seismicity may need to be adjusted to provide a better match between the recurrence model and observed seismicity. The PPRP strongly believes additional discussion and investigation is warranted regarding these results.	following the draft report, thus providing the TI Team the opportunity to question and review the recurrence results, both those related to recurrence plots and to spatial smoothing of recurrence parameters. The PPRP was an observer during these working meetings and briefings. The results are now judged by the TI Team to be reasonable relative to the observed rates from the catalog.
S 6-34. (CR, DMM, U) *Need for TI Team Assessment of Spatial Variation of Rate and bvalues* The results of the recurrence-rate analysis presented in Section 6.4 clearly show that TI Team assessments of priors on rate and *b*-values are required. The derived *b*-values in particular appear to be almost entirely below the range of values supported by studies world-wide over many years. We recommend that the Project arrange to further evaluate this analysis.	As suggested, such analyses occurred and changes were made to the recurrence model. Discussion of the recurrence methodology is given in Chapter 5.3.
Comments by Section ***Chapter 6 (Title)*** Given that 60 of the 70 pages in this chapter deal with RLME sources, the chapter title should be changed to something like, SSC MODEL: MMAX ZONES BRANCH AND RLME SOURCES.	Revision made as suggested.
Chapter 6 (Introductory text) In the introductory paragraph at the top of p. 6-1, after the second sentence, it would be helpful to most readers to repeat a very helpful description that appeared on p. 4-16f in Section 4.4.1: The "Mmax zones" model involves the direct use of observed seismicity by spatial smoothing of distributed seismicity and the inclusion of RLMEs that are defined primarily by paleoseismic evidence. The "seismotectonic zones" model involves the use of additional tectonic data to define the spatial distribution of future events.	Revision made as suggested.
Section 6.1.1 p. 6-2, 2nd paragraph: Regarding "(source IRM in the R model)": we assume this refers to the Canadian study; clarification is needed. p. 6-2, 3rd paragraph: The phrase "investigations undertaken for the . . ." probably should be "investigations evaluated . . . " The PPRP believes only evaluations were performed. ***Section 6.1.1.2*** Note: There are two sections labeled 6.1.1.2—one on p. 6.4 and one on p. 6-6. On p. 6.4, in paragraphs 3 and 4, "thrust" and "reverse" are used inconsistently vis-à-vis the definition provided in the Glossary for "Fault, Thrust" (< 45°) and "Fault, Reverse" (> 45°). On p. 6-6, 2nd paragraph, next-to-last sentence: ". . . favors three events to four based on field observations." A citation would be helpful.	Revisions and clarifications made as suggested.
Section 6.1.2.1 In last sentence of the first paragraph, the reader is referred to a non-existent Section	- Text revised to provide correct subsection call-out (5.1.2).

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5.3.3.6. In scanning Chapter 5, it's not clear that there is a "definition" of the temporally clustered earthquake model. On p. 6-8, 2nd full paragraph: No justification is given for weights on whether the Charleston RLME is "in" or "out" of a cluster. ***Section 6.1.2.5.3*** p. 6-14, last paragraph, line 10: Typo. (see See Appendix E . . .). ***Section 6.1.2.5.4*** The use of "occurrence model" in the section title and text is at odds with "recurrence model" used predominantly throughout the text (easily verified by a global search for "recurrence model," which shows repeated instances of "Renewal vs. Poisson recurrence models") and in the Glossary. There is at least one other appearance of "occurrence model" in the text (Section 4-19, p. 4-20, beginning of second full paragraph). "Occurrence" rates/probability also appears in Section 5.3.3.2 and should be corrected globally. ***Section 6.1.2.4.3*** p. 6-13, 2nd paragraph: "The UCSS magnitudes and weights . . . ." UCSS not defined.	- Generic discussion of in vs. out of cluster to be added to Ch 5. - Typo corrected - Text revised to replace term "occurrence model" with "recurrence model". - "UCSS" is typo. Deleted.
Section 6.1.3.2 p. 6-19, 3rd paragraph: The weights assigned to the two dip cases sum to a value greater than one. ***Section 6.1.3.4*** p. 6-21, second full paragraph, line 3: The term "interval-based approach" is ambiguous and potentially misleading. The data used are the number of earthquakes in a specified time interval (e.g., Figure 6.1.1-2, 7th node), not the interval between earthquakes, as some readers might assume. p. 6-21, fourth full paragraph, line 1: Consider replacing "occurrence rates" with "recurrence rates" p. 6-21, 4th full paragraph: Typo in cited recurrence values: 200, 350, and 500 years, should be k-years.	Change (0.5) to (0.4). The approach used to model recurrence for the Cheraw fault is interval-based. The text and description in this section have been modified to clarify this. Years changed to kyr
Section 6.1.4.5 2nd par., line 4: Typo. Change "500,00 years" to "500,000 years"	Changed
Section 6.1.5 In the Table on the top of p. 6-33: The note for the 1811-1812 earthquakes indicates 138 yr BP ± 100 yr. As written, suggests the uncertainty is 100 years; this needs to be clarified. ***Section 6.1.5.3*** p. 6-39, last paragraph: The text references Table 6.1.5-3 which appears to be missing. ***Section 6.1.5.4*** p. 6-41, first full paragraph, last sentence: Replace "only includes of all three" with "only includes the alternative of all three components" p. 6-41: The paragraph containing equation 6.1.5-1 is not clear. The use of the equation needs to be explained within the source characterization scheme.	Text (and table will be modified to discuss in detail the analysis, data used, and results Table 6.1.5-3 will be included in the final report p. 6-41 (first full paragraph) sentence corrected as suggested. p. 6-41—deleted the last paragraph that included the equation.
Section 6.2.1.2	Section 6.2.1.2 has been rewritten. Typo comment is thus obsolete See

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p. 6-66, 2nd full paragraph line 9: Typo. (** mi)	revised Section 6.2.1.2
Section 6.3 line 4: Typo. “source(described…”	
Section 6.3.1 p. 6-69, first full paragraph, line 9: Reference is made to “the 1882 earthquake”; this event is not in the table on the previous page and there is no context. Adding a short descriptive sentence for clarity would help the reader.	Figure 6.1.1-1 labeled modified. Figure 6.1.6-2 The star represents paleoseismic investigation sites. This will be added to the explanation.
Section 6.4.1 In the first line, change “Figures 6.3-1 through 6.3-6” to “Figures 6.4-1 through 6.4-6”	
Section 6.4.2 In the first line, change “Figures 6.3-7 through 6.3-16” to “Figures 6.4-7 through 6.4-17”	Tables 6.1.5-1, 6.1.5-2, and 6.1.5-3 will be included in the final draft.
Figure 6.1.1-1 Two of the large earthquakes are incorrectly labeled: 1663/2/5 is labeled **M**=3.71 (text in Section 6.1.1 says “**M** 7”; 1791/12/6 is labeled **M** 5.5 (text in Section 6.1.1 says “**M** 5.8). The labeled magnitude for only one of the other three large earthquakes corresponds exactly to the text in Section 6.1.1.	Logic tree labeling of approaches made consistent throughout
Figure 6.1.1-2 In the Charlevoix RLME logic tree, the header for the 10th node should be changed from “Earthquake Occurrence Model” to “Earthquake Recurrence Model” (see comment on Section 6.1.2.5.4).	Clear descriptions of the data used for each RLME recurrence calculations are provided in each section along with tables of the results.
Figure 6.1.2-1 In the Charleston RLME logic tree, the header for the 10th node should be changed from “Earthquake Occurrence Model” to “Earthquake Recurrence Model” (see comment on Section 6.1.2.5.4).	
Figure 6.1.2.4 Figure 6.1.2-4 shows the three zones along with the magnitude and gravity anomalies. It is not clear how these zones were delineated based on these geophysical data.	
Figure 6.1.3-1 In the Cheraw RLME logic tree, under Recurrence Method, the uppermost branch should more correctly be labeled “Earthquake Count in Time Interval” (as for the Charlevoix RLME logic tree instead of “Inter-event Times.”	
Figure 6.1.3-1 In the Meers RLME logic tree, under Recurrence Method, the upper and lower branches should more correctly be labeled “Earthquake Count in Time Interval” (as for the Charlevoix RLME logic tree) instead of “Inter-event Times.” In the corresponding HID tables (Table MEERS_HID-2 and HID-3), information on the data set (N events, T time) should usefully be provided, as in Table Marianna_HID-2.	
Figure 6.1.5-1 In the logic tree for the NMFS RLME source, under Equivalent Annual Frequency, references to the HID tables should be labeled NMFS instead of NMF. Under Events/Data, the labeling of “1811–1812, 1450 AD, and 900 AD” is difficult to relate to the dates in the table presented at the top of p. 6-33 (for example, 900 AD corresponds to 1110 yr BP—but in the table one finds “1,050 yr BP ± 150 yr). Exactly which elapsed time was used in Table NMFS_HID-2? (In that table, information on the data set (N events, T	

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time) should usefully be provided, as in Table Marianna_HID-2. ***Figure 6.1.6.2*** What are the yellow stars on the figure? No explanation in legend or caption. ***Figures 6.4-1 through 6.4-6*** Consider adding a note to the caption explaining what the mean maps are. ***Tables 6.1.5.1, 6.1.5.2, and 6.1.5.3 missing*** Table 6.1.5-1 discussed on page 6-32 is missing. Table 6.1.5-2 discussed on page 6-37 is missing. Table 6.1.5-3 discussed on page 6-39 is missing.	
CHAPTER 7—SSC MODEL: SEISMOTECTONIC ZONES BRANCH	
General Comments **G 7-1. (NAR)** In this chapter, as in Chapter 6, the TI Team has described and evaluated an immense amount of data and information and deserves praise for its efforts. The chapter addresses the "seismotectonic zones" branch of the master logic tree, as developed in Chapter 4 and portrayed in Figure 4.4.1-11 (and companion figures referenced therein). The TI Team's assessment is supported by Data Evaluation and Data Summary tables in Appendices C and D. This conceptual branch of the logic tree splits into two source groups—seismotectonic zones and the independent RLME sources, described in Chapter 6. Chapter 7 deals only with the twelve seismotectonic zones and their seismic characteristics.	No revisions required.
G 7-2. (CBR, CC) A significantly higher weight is assessed for the seismotectonic zones branch relative to the "Mmax zones" branch. As stated in Section 4.4.1 on p. 4-17: "A higher weight (0.8) is assigned to the seismotectonic zones branch than the Mmax zones branch (0.2) because the seismotectonic zones branch allows for more relevant information on the characteristics of future earthquakes to be included in the model." This information is the subject of the majority of Chapter 7. However, no full explanation or validation is presented in the introduction to this chapter to support the decision on the specific weights assessed for the two conceptual approaches at the front end of the master logic tree. A description of the justification of the weights would be an important and useful addition to the chapter.	The discussion of the basis for weights has been bolstered in Section 4.2.1. It is not appropriate to repeat that discussion in Chapter 7, but reference is made back to Section 4.2.1.
G 7-3. (CC, DMM, U) Although the chapter provides an abundance of geological detail, it fails to make a compelling case for identifying many of the seismotectonic zones as separate sources distinct from the larger Mmax zones described in Chapter 6. Considering the weight that is given to this branch (0.8), it is especially important that the definition of each of the seismotectonic zones be very clear and well supported with convincing evidence. Unfortunately, a persuasive case is not developed for the identification of several of the zones described in this chapter.	Each section of Chapter 7 includes a summary of the bases for identifying the seismotectonic zone, and the criteria that define each zone are summarized in Table 4.1.3-1. Each section has been reviewed and revised, as necessary, to ensure that the bases for the seismotectonic zones are adequate and clear.
G 7-4. (CC, DMM) The identification of the zones appears to be made largely on the basis of isolating regions of differing geological and tectonic histories that may have little direct relevance to the SSC characterization criteria that are specified in Section 4.3.3 (p. 4-14). These criteria are : (1) earthquake recurrence rate, (2) maximum earthquake magnitude, (3) expected future earthquake characteristics (e.g., style of faulting, rupture orientation, depth distribution), and (4) probability of activity of tectonic feature(s). The latter criterion was not used in developing the CEUS SSC model (Section 7.1, pg. 7-1), but no	The bases for identifying the seismic source zones in the CEUS SSC model indeed come from the four criteria. The statement that the fourth criterion was not used is incorrect and has been removed. Examples of the application of the criterion are the Meers fault and Cheraw fault. Discussions of the bases for identifying each of the seismotectonic zones have been revised to tie the discussion back to the four criteria, as applicable. As suggested, Table 4.1.3-1 has been added to summarize how the Mmax zones and the seismotectonic

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justification is given for not addressing this criterion. Furthermore, there is no uniform or systematic description of the application of the first three criteria which allow ready identification of the merits of the zones and which permit comparison among zones. Additional information pertaining to how the sources meet the defining criteria and more systematic organization of the content of the description of the zones would increase the rigor of the decisions reached in the report and their presentation. A summary table specifying the critical information that identifies each source zone based on criteria described in Chapter 4 would be helpful in organizing the information and comparing source zones.	zones relate to the four seismic source identification criteria.
G 7-5. (CC) Chapter 7 includes an impressive compilation of information and interpretations representing the range of relevant current knowledge of the scientific community. The scope and detail of this information are important in identifying and characterizing the seismotectonic zones and will be of great value to future users of the CEUS SSC Model. This information is well supported by comprehensive and timely references to the scientific literature. The level of detail is generally consistent throughout the description of the zones, but unfortunately the organization of the descriptions is not consistent. For example, some source zones have initial sections dealing with Background, others with Geologic Evidence, and still others with Basis for Defining Seismotectonic Zone. This lack of consistency in the description of the identified zones is an impediment to the review and comparison of the zones and needs to be corrected. The uneven descriptions appear to be due, in part, to multiple authorship, and some subsections apparently have not been updated since the application of the Kijko Mmax procedure in the Project. Some updating and rewriting appears warranted to alleviate these problems.	The sections have been organized to be comparable section to section.
G 7-6. (CC) The level of detail in this chapter is high, which will be useful in future seismotectonic studies within the CEUS. However, this level of detail will make it difficult for those readers of the report not well versed in the geology and geography of the region or the geologic time scale to comprehend the significance of the detail. Thus, to support the detail it would be advisable to (1) add maps that identify the location of geologic features, (2) provide more geologic terms in the glossary, and (3) accompany the glossary with a geologic time scale. Additionally, the descriptions of the seismotectonic zones should be reviewed to determine if some of the more specialized terminology, e.g., essexite, T-axes, Neoproterozoic, can be eliminated or simplified so that they can be meaningful to the spectrum of users of the report.	Locations referred to in the text have been added to the maps, terms have been added to the glossary, and a geologic time scale has been added to the glossary. Terminology has been simplified wherever possible to avoid unnecessary jargon. Figures 7.3.1-2, 7.3.2-2, -3 added. Labels for places discussed in text added to all relevant figures; deleted mention of specific alkaline rocks, including essexite in Section 7.3.2.1. Deleted mention of T-axes in section 7.3.1.4
G 7-7. (CC, SSHAC) As with previous chapters, this chapter could be greatly improved by a thorough technical edit. There are numerous editorial modifications required to achieve consistency in presentation, remove editorial errors, and improve clarity. Special attention should be given to clearly describing the bases for characterizing alternatives represented in alternative branches of the logic tree. Also, consideration should be given to describing the basis for the assessed weights for alternative characterizations representing the community uncertainty. Finally, care must be exercised to use words in their correct meaning, avoid casual terminology, and use terms that properly convey the essential activities of characterization of alternatives and assessment of the community uncertainty.	All sections have been reviewed and revised relative to describing all branches of the logic tree and the technical bases for the weights assigned to each branch.
G 7-8. (DMM) The Data Summary Tables of Appendix D are an important supplement to the descriptions of the seismotectonic zones. Unfortunately there appear to be omissions in Appendix D so that supporting information is not consistently available for this draft	All Data Summary tables are now included in Appendix D.

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chapter. This will need to be remedied in revision of the report. Additional comments on Appendix D are given in a review of that segment of the report.	Added key references from text to Date Evaluation tables.
Specific Comments **S 7-1. (CC)** *Suggestion for Rewrite of Introductory Paragraph* The introduction to Chapter 7 could be improved with significant editing. Consider the following as an example. As discussed in Section 4.3, the Conceptual Framework for assessing the CEUS SSC model is characterized by two alternative branches of the master logic tree: the Mmax zones branch and the seismotectonic zones branch. The seismotectonic zones branch, which is assessed a higher weight of 0.8 versus 0.2 for the Mmax zones branch, subdivides the CEUS SSC region according to differences in the seismic source assessment criteria described in Section 4.3.3. A common element of both the Mmax zones and the seismotectonic zones branches is the RLME sources. Because the paleoearthquake data that indicate the presence, location, and size of the RLMEs are essentially independent from data used to assess seismotectonic sources, the RLME branch is present in both models. An overview of the approaches for characterization and assessment of the zones is in Section 7.3.	Revision made using most of suggested example.
S 7-2. (DMM) *Need for Specifics Regarding Geologic Conditions that Affect Mmax* The first paragraph of Section 7.1 (p. 7-1) describes how the seismotectonic zones branch relates to Mmax. The basic premise is that regional differences in characteristics related to Mmax and/or future earthquake characteristics are best dealt with by identifying source zones of uniform properties. A region may possess characteristics that would lead to a different Mmax than adjacent regions, including a different prior distribution or different maximum observed earthquake. Mmax was described in Chapter 5, but it would be helpful to the users of the report for the authors to present examples of specific physical properties of the zones (e.g., thinner crust, lithospheric strength characteristics, aulacogens) and describe why these different conditions might result in different Mmax distributions. This information would help to sharpen the need for, and the significance of, the detailed information in the subsequent text which define Mmax and future earthquake characteristics.	Examples and discussion added to Section 7.1 to illustrate the manner in which the zones were identified and how they differ from one another. The methodologies for assessing Mmax are described in Section 5.2. As discussed therein, the only physical characteristic that is important for the Mmax assessment is whether or not the zones show evidence of Mesozoic or younger extension. Otherwise, the differences in future earthquake characteristics of the various zones are described in Section 5.4 and summarized in Table 5.4-2. There is no explicit connection between these characteristics and Mmax.
S 7-3. (CC) *Description of Charlevoix RLME Source; Section 7.3.1.1.3, pg. 7-6.* In Section 7.3.1.1.3 (p. 7-6), the description of the Charlevoix RLME seismic source (which is assumed to exist as a distinct seismic source) as part of justifying the St. Lawrence Rift (SLR), confuses the understanding of whether the SLR is a distinct seismotectonic zone. Part of the confusion relates to how the project is using historic earthquakes as part of the development of recurrence and maximum magnitudes. Are the historic earthquakes assigned to the SLR, even though they may be located within the boundaries of the Charlevoix RLME source?	Text in Section 6.1.1 and 7.3.1 clarified to indicate which earthquakes are considered RLMEs. Text in Section 7.3.1.1 introduces Charlevoix and other portions of the SLR seismotectonic zone such as the Ottawa-Bonnechere graben, Saguenay graben, and lower St. Lawrence as crust within SLR that exhibits varying rates of seismicity.
S 7-4. (DMM) *Significance of Vp/Vs Ratio* On p. 7-14 of Section 7.3.2, under *Geophysical Evidence*, what is the significance of results from teleseismic receiver functions described in last sentence of this section?	Eaton et al. (2006) do not interpret the result of variable Vp-Vs ratio: "Finally, region 3 is an area of thin crust (<38km) and variable VP/VS ratio. This area is entirely located northeast of the Ottawa–Bonnechere graben, a post-Grenvillian extensional feature that formed during the opening of the Iapetus ocean (ca. 0.7Ga; Kamo et al., 1995). It is interesting to note that region 3 appears to coincide with the Western Quebec Seismic Zone (Fig. 14), an area

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	of relatively intense intraplate seismicity." It could be judged that variable Vp-Vs ratio could be attributed to intrusion of mafic rocks into felsic host rock, but the authors have not made that interpretation.
S 7-5. (DMM) *Evidence for Separating the Northern Appalachian Seismic Zone from the Paleozoic Extended Zone* In Section 7.3.3.2 (p. 7-20), under *Basis for Zone Geometry*: The separation of the Northern Appalachian seismic zone (NAP) from the similar Paleozoic Extended zone (PEZ) to the south appears to be largely based on the location of the Triassic Hartford basin. However, a linear connection of the eastern boundaries of these zones would include only a small segment of the northern extent of the basin as shown in Figure 7.3.7-1 similar to the situation observed farther south along the boundary of the PEZ. Is the termination of the NAP being driven by the studies of Adams et al. in defining the seismic source zones of Canada?	Boundaries of PEZ-N modified on the west to follow the NY-AL lineament and the western boundary of the Hartford Basin. The southern boundary of the NAP seismotectonic zone follows the northern boundary of the Hartford basin.
S 7-6. (DMM) *Future Earthquake Characteristics;* In Section 7.3.3.4 (p. 7-21), under *Future Earthquake Characteristics* for the Northern Appalachian seismotectonc zone, the text notes that all earthquakes with known depths are relatively shallow, but goes on to use the default depth distribution for the seismic source. The basis for assigning the depth distribution for distinct seismic sources, including the NAP, should be based on a common approach to using earthquakes with known depths. Otherwise, assignment of the default depth distribution lacks rigor. Also note that a search of Chapter 5 shows no "default depth" term.	All estimates of seismogenic crustal thickness have been assessed using the same approach, as described in Section 5.4.
S 7-7. (DMM) *Background of the Paleozoic Extended Zone* In Section 7.3.4.1, the text needs to make clear that the Giles County Seismic Zone, the Eastern Tennessee Seismic Zone, and the Clarendon-Linden Fault System, are not unique from a seismotectonic perspective. Otherwise it is not clear why these features are not considered distinct seismic source zones.	Text reorganized to include large regional data and conceptual framework in the background section and specific seismological characteristics of zones of historically elevated seismicity within subsections.
S 7-8. (DMM) *Basis for Western Margin of the Paleozoic Extended Zone* In Section 7.3.4.2 (p. 7-29), under *Basis for Zone Geometry*: A reentrant of the Paleozoic Extended seismic zone extends into the craton in the vicinity of Kentucky, moving the western margin of the zone farther west. There is no support for this feature in the text of the report. The reference in the report that is used most extensively in defining the western margin is Wheeler (1995), but his studies did not indicate this reentrant; rather his margin to this zone in essentially a straight line through this region. A strongly supported description of the cause of this feature is needed or it should be eliminated. No references are cited to provide an indication that this feature is present.	Geometry of PEZ alternatives have been redrawn. The western boundary of PEZ_N follows the NY-AL. Crust northwest of the NY-AL lineament appears to have behaved as a rigid, somewhat coherent block, and its sharp boundary against the anomaly implies the edge of this competent block (Steltenpoh et al., 2010). Crust of the reentrant in the vicinity of Kentucky consists of the Rome Trough and is now included in the PEZ-W alternative geometry. The Rome trough is an Cambrian graben that appears to be related to reactivation of the NY-AL lineament (Stetlenpohl et al., 2010) or the East Continent Rift Basin (Drahovzal, 1997; Stark, 1997).
S 7-9. (DMM) *Basis for Identification of the Illinois Basin Extended Basement Zone.* In Section 7.3.5.1 (p. 7-33), the justification for defining this region as a distinct seismotectonic zone and the discussion in this section are not consistent with the criteria defined in Section 4.3.3 for defining seismic source zones.	The arguments for defining the IBEB as previously stated primarily address magnitude and future earthquake characteristics (i.e., allows for use of both MESE and NMESE Mmax priors; specification of future earthquake characteristics based on analysis of seismicity in southern Illinois, basement and Paleozoic structural trends). The higher rate of seismicity in this region may stem from some of the same

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	mechanisms proposed for the adjacent NMSZ/Reelfoot rift region to the south. This also suggests that the IBEB is different from the surrounding craton regions.
S 7-10. (CC, DMM) *Default Values of Future Earthquake Characteristics in the Eastern Continental Crust-Atlantic Margin; Section 7.3.7.4, pg. 7-48.* In Section 7.3.7.4 (p. 7-48), the text discussing seismicity notes that most well located earthquakes of the Eastern Continental Crust-Atlantic Margin are distributed throughout the upper 13 km of crust. Given this information, the basis for assuming that the seismogenic thickness should be represented by the default values is not clear.	The weights on the default values of 13, 17, and 22 km have been modified to place a higher weight on shallower depths for the ECC-AM. The weights of 0.6, 0.3, and 0.1 result in a slightly shallower mean seismogenic depth than the default weights of 0.4, 0.4, and 0.2.
S 7-11. (DMM) *Additional Basis for Defining the Atlantic Highly Extended Crust* In Section 7.3.8.1 (p.7-49), under *Basis for Defining Seismic Zone*: Canadian seismologists have recognized the zone of weakness at the Atlantic Ocean margin as defined by the continental slope as a zone of potential seismic activity based on the location of the magnitude 7.2 1929 Grand Banks earthquake, which occurred east of the northern tip of Nova Scotia. This earthquake, as well as the Baffin Bay earthquake in Canada, is supportive of the identification of this seismic zone.	We agree that the occurrence of the two mentioned earthquakes could be used to argue for the presence of a zone of weak crust. However, following our methodology for defining seismic zones, the occurrence of the earthquakes, and/or the arguments for a weaker crust, are not criteria for defining zones, so they are not discussed within this section of the report.
S 7-12. (CC, SSHAC) *Clarification of Text Describing the Basis for Mmax of the Extended Continental Crust-Gulf Coast* In Section 7.3.9.3 (p. 7-56), *Basis for Zone Mmax*: The characterization and assessment of Mmax described in this section is unclear. First, use of the term "scenario" (meaning imagined or possible) can convey a lack of disciplined evaluation of the available data for characterizing Mmax for the zone as required by the SSHAC assessment process. Replacing "scenario(s)" with "alternative characterization(s)" would properly convey that the characterizations represent the range of uncertainty based on evaluations of the available data. Second, the third alternative is described as follows: "The largest observed earthquake is the potential paleoearthquake identified from the studies of . . . ." The use of "largest observed earthquake" and "potential paleoearthquake" seems incompatible. In addition, the characterization described here clashes with the strong conclusion stated in Section 7.3.9.5. Elaboration is needed better explaining the evaluations performed supporting the third alternative characterization.	Revised as suggested with the exception of comments regarding paleoliquefaction. There is no strong evidence of repeated earthquakes in the ALM area, but there is potential evidence of one paleoliquefaction event. Also, we don't see the incompatibility in using a potential paleoearthquake as the largest observed earthquake. Including this possibility is part of capturing the uncertainty in the largest observed earthquake. Changed to "alternate characterizations"
S 7-13. (DMM) *Additional Evidence for Defining the Gulf Highly Extended Crust*; In Section 7.3.10.1 (p. 7-59), under *Basis for Defining Seismotectonic Zone*, is there evidence of faulting in this zone as anticipated in a highly extended zone? If so, that would be additional evidence for defining the zone.	There is no evidence of seismogenic Quaternary faulting within this zone.
S 7-14. (DMM) *Evidence Regarding Characterization of the Gulf Highly Extended Crust* In Section 7.3.10.3 (p. 7-60), under *Basis for Zone Mmax*, there are substantive analyses that show the event of February 10, 2006, to have been a landslide. These analyses must be referenced and discussed as part of the data base for characterizing and assessing Mmax for this zone.	Added discussion of this event, but note that there has not been any research published in peer-reviewed journals that demonstrates that the event was a landslide, and all available literature is listed in the data summary tables.
S 7-15. (CC,DMM) *Need to Strengthen the Basis for Defining the Oklahoma Aulacogen as a* *Distinct Seismic Source Zone* In Section 7.3.11.1 (p. 7-62), under *Basis for Defining Seismotectonic Zone*, the text mentions "default future earthquake characteristics." This terminology has not been used	The basis for defining the zone follows from the methodology outlined in Section 4.1.3.3. The description of this methodology has been modified for clarity.

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systematically throughout Chapter 7 (with reference to Table 5.4-1), and in this section it is not clear why these are the primary basis for defining the seismotectonic zone versus the full set of criteria found in Section 4.3.3. While future earthquake characteristics are one of the criteria used to define distinct seismotectonic zones (see Section 4.3.3), there does not appear to be anything profoundly unique about the style of faulting or the strike of ruptures to support defining the Oklahoma Aulacogen as a distinct seismotectonic zone. The basis for defining the Oklahoma Aulacogen as a distinct seismotectonic zone is weak and needs to be improved.	
S 7-16. (CC) *Significance of Statement in Description of Northeast Ohio Seismic Zone in the Midcontinent Seismic Zone* In Section 7.3.12.1.4 (p. 7-68), for the Northeast Ohio Seismic Zone: The third bullet of the second paragraph is meaningless to the reader without additional description of its significance.	Text added to clarify this statement. Additional discussion provided of age and location (significance) of the sand pit exposure.
S 7-17. (DMM) *Effects of Smoothing on Recurrence Parameters* In Section 7.5 (p.7-71), Recurrence Parameters: The objective smoothing results in *b*-values that are low, possibly below the range of values known from world-wide experience. Yet, no alternative is suggested. Additional elaboration of the analyses must be provided to adequately inform future users of the CEUS SSC model.	Recurrence has been reassessed based on the Final SSC model and there is a reasonable match between the observed recurrence and the predicted recurrence based on the objective model.
S 7-18. (DMM) *Full Explanation of the Results Shown in Figures 7.5.2-9 to 7.5.2-42* Many of the data shown in Figures 7.5.2-9 to 7.5.2-42 indicate the poor fits of the realizations to the catalog. This is disturbing and needs to be more clearly explained in the text. Why doesn't the preferred model fit the catalog data better? Only the short text in section 7.5.2 describes these figures. The text should be enhanced to describe the fitting issues, and as a result there needs to be full justification of the rate and *b*-value maps for the seismotectonic zones.	The recurrence based on the Final SSC model shows reasonable fits between the realizations from the model and the observed counts.
Comments by Section ***Section 7.3.1.2*** This section never actually describes why the St. Lawrence Rift should be a distinct source zone. There is some discussion of geometry, but no well defined case for "why" (unless it is simply because the GSC did). ***Section 7.3.1.3*** At least some mention of the implications or importance of the observations to the Kijko model should be provided. This comment applies to all the individual zone sub-sections. Perhaps consider doing it at the beginning of Chapter 7.	SLR seismotectonic crust separates crust initially rifted in the Paleozoic and subsequently reactivated during the Mesozoic into one zone with a maximum magnitude distribution derived from a Mesozoic and younger prior. Text in section 7.3.1.1 better introduces subsections describing portions of the SLR seismotectonic zone that display different geological or seismological characteristics and seismicity rates. Text added giving results from Kijko approach. Discussions of the Kijko approach are a part of section 5.2.
Section 7.3.2 last bullet, p. 7-13: If the hotspot has been tracked farther **to** the northwest, why isn't the seismic source zone extended to the northwest? ***Section 7.3.2.1*** This is one of the few Seismotectonic Zone subsections that actually develop a clear summary for why this should be a separate zone. ***Figure 7.3.2-1*** As on similar maps in the report, Figure 7.3.2.1 should show the magnitudes of the starred earthquakes.	Moved discussion of Ma and Eaton (2007) indicating that seismic portion of the hotspot track corresponds to the transition from kimberlitic dikes to plutons. Also, Figure 7.3.2-2 shows that the hotspot track northwest of the seismic zone is aseismic. Labels added.

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Section 7.3.3.1 This section discusses the basis for proposing the NAP zone. It states: "The basis for defining the NAP seismotectonic zone centers primarily on the concept that terranes of this zone formed outboard of the Laurentian margin after Iapetan rifting and were subsequently accreted to the passive margin." This subsection is weak in terms of developing a basis for defining the NAP as a separate zone. The text focuses on geological arguments that are never specifically tied to the SSC criteria. The reader is left to infer this zone may or may not utilize a different Bayesian Mmax prior than adjacent regions.	Clarified section to illustrate why a Paleozoic and younger Mmax prior does not apply and why a Mesozoic and younger prior does. Further tectonic and seismological arguments are used to distinguish between ECC-AM which also has a Mesozoic and younger prior.
Section 7.3.4 Use of the term "IRM" changes from describing a continental margin in the first sentence of the introductory paragraph of Section 7.3.4 to a seismic zone later in the paragraph. This is confusing. Similarly, note that the labeling of the PEZ in Figure 7.3.4-1 appears to be incorrectly labeled as IRM. ***Section 7.3.4.1.4*** Suggest that the reference to Steltenpohl et al. in Geology, June 2010, v. 38, p. 571-574 be added to the list in the second paragraph. ***Section 7.3.4.1.6*** p. 7-27: At the end of the second paragraph of this section reference is made to "a Class C tectonic feature." It would be helpful to the reader to cite where in this report the classes of the tectonic features are defined and thus the significance of this information to seismic source identification. ***Section 7.3.4.1.6*** p. 7-29, paragraph at top of page: The discussion of a lack of observed paleoliquefaction features should also be used with the appropriate qualification. Specifically, the observation that paleoliquefaction features provides strong evidence for past strong earthquake shaking, should be accompanied with a remark that failure to identify such features does not provide an equally strong a case for the absence of strong shaking.	Restricted usage of IRM as a concept of the Iapetan rifted margin. Discussions based on this reference have been added throughout the section. Statement deleted. Statement added.
Section 7.3.5 p. 7-32: The use of "Basement" in the title of this zone does not appear to be consistent with the titles given to other seismotectonic zones of the CEUS. ***Section 7.3.5.1*** p.7-33, 2nd bullet: In discussing the basis for defining the IBEBZ zone the text states, "The southern part of the Illinois basin is one of the most structurally complex areas of the Midcontinent." How this directly impacts the SSC needs to be more clearly elaborated, or deleted. On the following page in the next bullet the text states: "An extensive series of moderately dipping reflectors is present in the basement, part of which may have been reactivated by the 1968 mb 5.5 earthquake." Are the reflectors then interpreted to be faults? Also, the 1968 earthquake may have occurred in response to reactivation of the reflectors (if they are in fact faults), but not vice versa. ***Section 7.3.5.2*** p. 7-34: Suggest clarification of last sentence in second paragraph with something like: "The margins of the volcanic layered sequences, especially to the south and west, are marked by prominent coincident closed-contour magnetic and gravity anomalies which	p. 7-32: The names applied to the various zones reflect both geographic and geologic information. The IBEB zone is defined in part on its structural and tectonic setting that influenced our characterization of Mmax priors. Evidence for reactivation of extensional structures in the Precambrian basement was considered in this assessment; hence, this was included in the name assigned to the zone. p. 7-33: Additional text has been added to the observations used to define the IBEB as a seismotectonic zone. The mid-crustal reflectors are interpreted to be faults and the revised text clarifies the terminology. p. 7.34 Sentence modified as suggested. p. 7-35: A summary of the article to the Data Summary table for the Illinois Basin-Wabash Valley has been added. The Omaha intrusive mentioned is

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are derived at least in part from mafic volcanic rocks and intrusions . . . ." ***Section 7.3.5.3*** pg. 7-35: In considering the Mmax of this zone it may be useful to consider the presence of numerous late Paleozoic ultramafic intrusions (dikes and sills) into the sedimentary section of this region. See, for example, Sparlin and Lewis in Geophysics, v. 59, p. 1092-1099 (1994).	actually in the WV RLME source zone.
Section 7.3.6.5 **(CC)** ***Develop table for future earthquake characteristics in Reelfoot Rift zone;*** *pg. 7-42.*p. 7-42, text box: The characteristics of future ruptures in the Reelfoot Rift zone listed in the text box at the end of Section 7.3.6.5 should be placed in a numbered table with headings.	This table is being deleted from the revised text. Reference made instead to Table 5.4-2.
Section 7.3.7 p.7-47, first full paragraph, line 5: The text refers to the unlikelihood of a maximum magnitude earthquake of greater than 7 because of the paucity of paleoliquefaction features in the region. Could Mmax be less than 7? ***Section 7.3.7.1*** In the second line of the first paragraph, "large" earthquakes are specified as **M** > 7. Should be **M** ≥ 6.5 to be consistent with the value used elsewhere for the RLMEs.	p. 7-47: The statement reflects the interpretations made by Obermeier and McNulty (1998). It is possible that the paucity of paleoliquefaction data could suggest that the largest mid-late Holocene event could be even smaller than **M**7, but it would more difficult to support that conclusion. 7.3.7.1: In this context, "large" earthquakes refer to the M≥7 events in Mesozoic and younger extended crust observed in the global earthquake catalog compiled as part of the Johnston et al (1994) EPRI study. At the time of that study, the only stable continental earthquakes of M≥7 had occurred in Mesozoic and younger extended crust. The use of "large" was not intended to reflect the same definition of RLME. To avoid any confusion the text is modified and the term "large" is removed.
Section 7.3.9.2.1 p. 7-52, last bullet: The point could be illustrated with reference to the appropriate magnetic anomaly figure. ***Section 7.3.9.2.4*** p. 7-55, first full paragraph: Suggest that the last sentence be modified to something like: "The source zone is extended north of the Southern Arkansas fault zone for several reasons:"	Reference to figure added. Suggested change made.
Section 7.3.10 In the title of this section, for consistency with previously described seismic source zone, suggest the title of this zone be "Gulf Coast Highly Extended Crust."	Revision made as suggested.
Section 7.3.11.3 This subsection is an example case where adding an additional sentence could improve the clarity, consistency and transparency of the document. The Bayesian approach is the only Mmax approach used for this zone. It would be helpful to the reader to note that specifically or state the Kijko approach was not used due to a high *p*-value. Some zones are explicit in describing the two approaches, some are not.	Revision made as suggested.
Section 7.3.12.1.2 pg. 7-65, first full paragraph, line 4: Suggest beginning sentence with, "The deformation during this interval is attributed to" instead of "It is attributed to" ***Section 7.3.12.2***	The text modified as suggested. p. 7-69 This suggestion was not adopted. The Mmax prior is the primary

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p. 7-69, par. 1, line 4: Suggest adding the phrase “and recurrence characteristics” after “maximum magnitude probability”	distinguishing characteristic for delineating this zone.
Editorial Comments and Typographical Errors General Comment: To avoid repetition of editorial comments on repeated issues throughout the text of Chapter 7, the following issues are identified which should lead to necessary revisions throughout the chapter: • The manner of describing compass directions and their hyphenation should be made consistent throughout the report. Note that sometimes the directions are spelled out and in other cases an abbreviation is used. • Geologic time units are not used appropriately throughout the chapter. Ma is used by the scientific community for millions of years before the present and myr is used for millions of years of duration. • Recommended that for each section that presents a different seismotectonic zone, the title include the acronym (e.g., Section 7.3.1 — St. Lawrence Rift (SLR). Some section headings already include the acronym, which is helpful to the reader in referring to maps and figures. • “Aeromagnetic” is not a definitive term. Rather use “magnetic anomaly” and gravity should always be followed by “anomaly,” e.g., gravity anomaly and magnetic anomaly. If there is no adjective before either the gravity or magnetic anomaly, it is assumed that the gravity anomaly is the Bouguer gravity anomaly and the magnetic anomaly is the total intensity magnetic anomaly. Where possible, the type of anomaly should be specified. • Mile should be abbreviated as “mi” without a period at the end, consistent with scientific context. • The first time a term is used that will be identified by an acronym, the complete term should be given followed by the acronym in parentheses. There are numerous acronyms in this chapter that are not listed in the list of acronyms near the front of the report. These will not all be identified in the following comments. • Reference to Adirondacks and Appalachians in place of Adirondack Mountains and Appalachian Mountains, respectively, is not editorially correct. This and similar casual terminology should be removed from the chapter. • Several figures cited in this chapter are neither in the draft report nor in the List of Figures. All cited figures and tables should be carefully reviewed. • Magnitudes of specific earthquakes should be consistent in number of significant figures throughout the text. • Format for dates should be consistent throughout the text. Avoid 10 February 1999 rather use February 10, 1999. • Listing of earthquakes, references, etc. should be in a prescribed order, e.g., date, magnitude, etc. Specific Comments: **E 7-1** Section 7.1 Paragraph 1, line 5 – replace region with seismotectonic source zone **E 7-2** Section 7.1 Paragraph 1, line 7 – replace event with earthquake **E 7-3** Section 7.1 Paragraph 1, line 8 – insert tectonic between particular and province	Revisions made as suggested unless otherwise noted below: **E 7-63:** Text deleted or modified as suggested, replaced with ‘adjacent’ **E 7-67:** Text checked—La Salle anticlinal belt is used throughout **E 7-71:** no change needed. FAFC explained in paragraph 2 of this section **E 7-72:** Sentence revised to add ‘Pleistocene deformation’ after the word ‘late’ **E 7-73:** Reference corrected to Pratt (2009) **E 7-91** no change needed. Discussion is an appropriate term

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E 7-4 Section 7.1 Paragraph 1, line 9 – insert faulting between slip and defining **E 7-5** Section 7.1 Paragraph 2, line 16 – replace eastern with western (?) **E 7-6** Section 7.1 Paragraph 3, line 1 – not all seismotectonic zones represented in Appendices C and D **E 7-7** Section 7.1 Paragraph 3, line 4 – replace provide an indication with specify **E 7-8** Section 7.1 Paragraph 3, line 7 – replace looking at any of the discussions with reviewing the descriptions **E 7-9** Section 7.1 Paragraph 4, line 16 – replace discussion with description **E 7-10** Section 7.1 Paragraph 5, line 6 – replace lie with occur **E 7-10** Section 7.1 Paragraph 6, line 5 – replace called out with identified ***DRAFT*** Installment 3, PPRP Review Comments, page 7-11 **E 7-11** Section 7.1 Paragraph 5, line 9 – replace have been postulated as being with are postulated as **E 7-12** Section 7.1 Paragraph 5, line 11 – replace studies are judged to be too preliminary at the present time with assessments are judged to be without definitive support as a result of the preliminary nature of the investigations **E 7-13** Section 7.3 Paragraph 1, line 3 – replace Mid-Continent with Midcontinent **E 7-14** Section 7.3 Paragraph 1, line 6 – NMESE not in List of Acronyms **E 7-15** Section 7.3 Paragraph 1, line 7 – insert northwest boundary between the and Reelfoot **E 7-16** Section 7.3.1 Paragraph 2, line 2 – separate SCRs and correlate **E 7-17** Section 7.3.1.1.3 Paragraph 1, bullets – capitalize first word of bullets and place period after last bullet **E 7-17** Section 7.3.1.1.4, pg. 7-7, third bullet, separate A and third **E 7-18** Section 7.3.1.1.4 Paragraph 2, first bullet – separate The and oldest **E 7-19** Section 7.3.1.1.4 Paragraph 4, line 6 – remove space after hyphen **E 7-20** Section 7.3.1.1.5 Paragraph 1, line 3 – separate from and the **E 7-21** Section 7.3.1.1.7 Paragraph 1, line 11 – remove s between faults and associated **E 7-22** Section 7.3.1.1.7 Paragraph 1, line 13 – separate which and continued **E 7-23** Section 7.3.1.2 Paragraph 1, line 3 – replace has been with is **E 7-24** Section 7.3.1.2 Paragraph 1, line 6 – remove space after hyphen **E 7-24** Section 7.3.1.2, pg. 7-10, paragraph 1, line 3 and line 8 – separate States and faults **E 7-25** Section 7.3.1.2 Paragraph 1, line 19 – replace asterisks with 250 **E 7-26** Section 7.3.1.1.4 Paragraph 2, line 2 – what is GSC R model?? **E 7-27** Section 7.3.1.1.4 Paragraph 2, line 6 – remove space before Brompton **E 7-27** Section 7.3.1.1.7, pg. 7-9, line 13 – separate which and continued **E 7-28** Section 7.3.1.3 Paragraph 1, line 15 – separate subsidence and within **E 7-29** Section 7.3.1.3 Paragraph 1, line 28 – spell out first time GMH is used **E 7-29** Section 7.3.1.4, pg. 7-11, 1st line - suggest “Earthquakes in Canada are classified	

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….” should be earthquakes in southeastern Canada **E 7-30** Section 7.3.1.4 Paragraph 1, line 13 – 5.8 is 5.75 elsewhere, use care in significant figures, similar problems elsewhere in report that need to be addressed **E 7-31** Section 7.3.1.4 Paragraph 2, line 2 – neither earthquake shown on Figure 7.3.1.1 **E 7-32** Section 7.3.2, Geologic Evidence, Paragraph 1, bullet 2 – why refer to figure here? **E 7-33** Section 7.3.2, Geophysical Evidence, Paragraph 1, line 2 – Figure 7.3.2-3 is missing in report and List of Figures **E 7-34** Section 7.3.2, Evidence for Reactivation, Paragraph 1, several lines – Capitalize Late and Early when part of formal age **E 7-35** Section 7.3.2, Evidence for Reactivation, Paragraph 3, last line – replace / with and **E 7-36** Section 7.3.2.2 Paragraph 2, line 2 – Figure 7.3.2-4 is missing from report and List of Figures **E 7-37** Section 7.3.2.2 Paragraph 2, line 9 – can this information be related to a specific figure? **E 7-38** Section 7.3.2.3 Paragraph 2, line 5 – Figure 7.3.2-5 is missing from report and List of Figures **E 7-39** Section 7.3.2.4 Paragraph 2, line 15 – separate and20 **E 7-40** Section 7.3.3 Paragraph 1, line 1 – remove s from Appalachian **E 7-41** Section 7.3.3 Tectonic Framework, Paragraph 3, line 5 – change to compressional event **E 7-42** Section 7.3.3 Tectonic Framework, Paragraph 7, line 2 – replace million-year with myr **E 7-43** Section 7.3.3 Seismicity Paragraph 1, line 10 – remove period after Ebel **E 7-44** Section 7.3.3 Paragraph 2, line 3 – magnitude of June 1638 earthquake is listed as 6.5 on page 7-19 and 5.67 on page 7-21 **E 7-44** Section 7.3.3, pg. 7-19, Seismicity section - the 1904 earthquake referred to in terms of mblg, shouldn’t moment magnitude be indicated as well? **E 7-45** Section 7.3.3.3 Paragraph 2, line 5 – insert period after al **E 7-46** Section 7.3.4.1.1 Paragraph 1, line 3 – replace valley with rift **E 7-47** Section 7.3.4.1.2 Paragraph 1, line 3 – insert anomaly after gravity **E 7-48** Section 7.3.4.1.2 Paragraph 2, line 7 – remove any **E 7-49** Section 7.3.4.1.2 Paragraph 1, line 9 – replace Valley with rift **E 7-50** Section 7.3.4.1.3 Paragraph 3, line 9 – remove Recent **E 7-51** Section 7.3.4.1.3 Paragraph 4, last line – replace is with are **E 7-52** Section 7.3.4.1.5 Paragraph 1, line 6 – RTG not identified **E 7-53** Section 7.3.4.1.6 Paragraph 5, line 6 – insert space in front of Dineva **E 7-53** Section 7.3.2, pg. 7-13, 2nd line - currently states “This seismotectonic zone is largely defined by moderate seismicity, including …” As written this contradicts the stated position that the model accounts for differences in seismicity by spatial smoothing. It	

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Comment	Summary of Revisions to Report
seems more appropriate to say "This seismotectonic zone is characterized by moderate seismicity,….." **E 7-54** Section 7.3.4.2 Paragraph 1, line 3 – remove unfiltered, add Bouguer gravity before anomaly **E 7-55** Section 7.3.4.2 Paragraph 1, line 5 – replace rise with anomaly gradient **E 7-56** Section 7.3.4.2 Paragraph 2, line 6 – should PEZ be PEZ-W?? **E 7-57** Section 7.3.4.2 Paragraph 3, line 1 – spell out PEZ-N **E 7-58** Section 7.3.4.2 Paragraph 4, last line – replace IRM with PEZ **E 7-59** Section 7.3.4.3 Paragraph 1, line 2 – magnitude **E 7-59** Section 7.3.4.3, pg. 7-30, Paragraph 1 - mixed magnitudes in the section **E 7-60** Section 7.3.4.2 Paragraph 4, line 4 – replace IRM with PEZ **E 7-61** Section 7.3.4.4 Paragraph 4, line 4 – spelling of Pymatning?? **E 7-62** Section 7.3.5 Paragraph 1, line 1 – delete The regions of **E 7-63** Section 7.3.5 Paragraph 1, line 2 – delete more distant, replace presented the with proposed that **E 7-64** Section 7.3.5 Paragraph 1, line 3 – delete concept and change extending to extend **E 7-65** Section 7.3.5 Paragraph 1, line 8 – delete d from indicated **E 7-66** Section 7.3.5 Paragraph 1, line 9 – delete of complexly deformed crust. **E 7-67** Section 7.3.5 Paragraph 4, line 4 – be consistent in use of term for LaSalle anticlinorium **E 7-68** Section 7.3.5 Paragraph 2, line 5 – insert anomaly after intensity **E 7-69** Section 7.3.5 Paragraph 2, line 6 – insert layered between volcanic and sequences **E 7-70** Section 7.3.6.1 Paragraph 1, bullet 1, line 4 – should be plume **E 7-71** Section 7.3.6.1.2 Paragraph 5, line 5 – FAFC, not defined **E 7-72** Section 7.3.6.1.2 Paragraph 8, line 8 – missing words?? **E 7-73** Section 7.3.6. 2 Paragraph 8, bullet 3, line 5 – publication date of Pratt et al. **E 7-74** Section 7.3.7 Geophysical Anomalies, Paragraph 2, line 5 – replace runs with extends **E 7-75** Section 7.3.7 Geophysical Anomalies, Paragraph 2, line 10 – remove separately **E 7-76** Section 7.3.7 Seismicity, Paragraph 4, line 10 – should small be limited?? **E 7-77** Section 7.3.7.2 Basis for Geometry, Paragraph 1, line 16 – BMA, identify **E 7-77** Section 7.3.7.4, Future Earthquake Characteristics, pg. 7-48, - text refers to ECC-AM having the same future rupture characteristics as the AHEX zone. However, the discussion of the AHEX follows the ECC-AM zone. Consider placing description of characteristics in this section **E 7-78** Section 7.3.8.1 Paragraph 2, line 5 – replace runs with extends **E 7-79** Section 7.3.9 Paragraph 1, line 6 – replace represents with is **E 7-80** Section 7.3.9.2.1 Paragraph 1, line 5 – remove any **E 7-81** Section 7.3.9.2.1 Paragraph 1, line 7 – replace think with thin	

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Comment	Summary of Revisions to Report
E 7-82 Section 7.3.9.2.1 Paragraph 2, line 6 – replace reflected with reflects **E 7-83** Section 7.3.9.2.3 Paragraph 2, line 1 – change to In spite of this tectonic interpretation, **E 7-84** Section 7.3.9.4 Paragraph 4, line 5 – change to that formed or were reactivated **E 7-85** Section 7.3.9.5 Paragraph 2, line 4 – replace since with because **E 7-86** Section 7.3.9.5 Paragraph 6, line 1 – insert the after comma **E 7-87** Section 7.3.10 Paragraph 1, line 6 – replace represents with is **E 7-88** Section 7.3.11.1 Paragraph 2, line 1 – replace first sentence with The basis for defining the distinct future earthquake characteristics for the aulacogen is the observation of the characteristics of the Quaternary activity on the Meers fault, a fault within the Frontal Wichita fault system (see Section 6.1.4). **E 7-89** Section 7.3.11.2 Paragraph 2, line 5 – remove any **E 7-90** Section 7.3.12 Paragraph 1, line 2 – insert geologic between two and provinces **E 7-91** Section 7.3.12 Paragraph 2, line 1 – replace discussion with description **E 7-92** Section 7.3.12 Paragraph 2, line 2 – replace discussion with description **E 7-92** Section 7.3.12.1.4 Paragraph 1, line 6 – remove any of **E 7-93** Section 7.3.12.1.4 Paragraph 1, line 9 – replace could not with cannot **E 7-94** Section 7.3.12.1.4 Paragraph 1, last line – delete any of **E 7-95** Section 7.3.12.1.4 Northeast Ohio Seismic Zone, Paragraph 5, bullet 3, line 6 – change to consistent with one expected for a high pore-pressure… **E 7-96** Section 7.3.12.1.4 Northeast Ohio Seismic Zone, Paragraph 6, line 2 – replace very well with favorable **E 7-97** Figure 7.3.4-1 – Indicate 1929 Attica earthquake?? **E 7-98** Figure 7.4.1-1 – scale and size used for displaying mmzx-obs for each seismic source needs to be modified to better illustrate the findings **E 7-99 –** limits of information on all figures (e.g., 7.1-1 and 2) needs to be confined to the limits of the study area	
CHAPTER 8 — DEMONSTRATION HAZARD CALCULATIONS USING CEUS SSC MODEL	
General Comments **G 8-1. (CC)** Chapter 8 is the opportunity for the TI Team to explain differences in hazard obtained using the CEUS SSC model, the USGS seismic source model, and the COLA seismic source models. This has been done to a degree, but more extensive evaluations relating the differences in hazard to elements of the CEUS SSC model would be very valuable for future users. Industry stakeholders and the scientific and technical community will look be looking closely at the demonstration hazard calculations to gain an overall understanding of the CEUS SSC model and whether it yields reasonable results. Figures such as Figures 8.2-5R through 8.2-5T for all test sites together with thorough evaluations of how the TI Team's assessments of smoothing parameters impact hazard would be very informative. Sensitivities to the Team's assessments of weights on the "in cluster" and "out of cluster" characterizations of RLME sources would also be very informative.	Additional discussion of releases for differences at each site

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Comment	Summary of Revisions to Report
G 8-2. (CC, CBR) The CEUS SSC model rates are often by a factor of two or more higher than the USGS and COLA models rates, over a large range of ground motions. The slopes of the hazard curves are more similar because they all assume the same ground motion prediction equations. 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. The basis of these higher rates can be seen in the figures of Chapter 5 to 7 (e.g., 6.4-7 to 6.4-16; 5.3.2-22), where the model realizations over-predicts the historical rate of earthquakes. These differences make one question whether the model encompasses the center, body, and range of the informed technical community.	Hazard curves for CEUS SSC model match other curves better.
Specific Comments **S 8-1. (CC)** *Explanation of CEUS Ground Motion Attenuation Model Application* The TI Team has used the 2004 EPRI ground motion attenuation model to complete probabilistic estimates of ground motion. Chapter 8 should provide a summary of the application steps that were implemented for the 2004 EPRI ground motion attenuation model. It is particularly important that the distance measure be explained. Application of the 2004 EPRI ground motion attenuation model could involve the use of either point source distance measures or extended source distance measures. If both distance measures were used, the text should provide an explanation of the criteria or considerations that resulted in the choice of the distance measure for each of the seismic sources. For those seismic sources that were modeled as extended ruptures, the text should describe what assumptions were made to model the extended rupture and to what extent epistemic uncertainty was considered (alternative extended rupture relationships). Without this explanation the information provided in Chapter 9 regarding the sensitivity to certain logic tree inputs is diminished.	Text modified to include this revision
S 8-2. (DMM, CC) *Questions Regarding Results of Demonstration Hazard Calculations* In the subsection labeled "CENTRAL ILLINOIS SITE" (p. 8-6, 3rd paragraph): It would be informative to know how much higher and over what ground motion range the CEUS SSC model hazard is higher. Also, what characterizations and/or assessments contained in the model contribute to the higher seismic hazard. The CEUS SSC m del 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 significantly 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. In the subsection labeled "CHATTANOOGA SITE" (addendum, 8/18/2010, 3rd paragraph): More complete evaluations and explanations relating the differences to elements of the CEUS SSC model would be very valuable. This comment applies to other sites as well; so, will not be repeated. The CEUS SSC model hazard for the Chattanooga site 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. These results are not explained in the text.	Revised comparisons are described Explained revised comparison

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Comment	Summary of Revisions to Report
In the subsection labeled "HOUSTON SITE": The CEUS-SSC model hazard at the Houston site 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 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. In the subsection labeled "JACKSON SITE": For the 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. In the subsection labeled "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. In the subsection labeled "SAVANNAH SITE": For the CEUS SSC model at the Savannah site, the major contributors to the ground motion hazard are the Charleston RLME source and the ACCAM 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 off exceedances. In the subsection labeled "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.	Explained revised comparison No comment required No comment required No comment required Explained revised comparison
Comments by Section ***Order of Text, Tables, and Figures*** Material needs to be reorganized (including added materials transmitted on August 18, 2010) so that the order of presentation of text, tables, and figures is consistent with other chapters. ***Section 8.1*** 3rd paragraph and elsewhere: The term "hard rock" can lead to confusion because it is unspecific and used in various meanings. Consider defining the term "CEUS Region generic rock," shear wave velocity of 9200 fps, and using this term consistently throughout the chapter. Similarly, using the term "soil" to mean the geologic section above "CEUS Region generic rock" can especially invite confusion because of the well-established use of this term in geotechnical engineering. Consider "stratigraphic column" instead.	Revised accordingly No revision required No revision required

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Comment	Summary of Revisions to Report
4th paragraph: In the first line, would "generalized" or "representative" be more accurate than "hypothetical"? In the last line, would "dynamic response" be more descriptive than "parameters"? ***Section 8.2 [including revised materials distributed on 8/18/2010]*** In the subsection labeled "All site conditions" (p. 8-5): "EPRI-SOG (1989)" should be "EPRISOG (1988)" ***Figures 8.1-4 and 8.1-5*** Are the mean amplification factors independent of the mean AFEs (e.g., at 10–4, 10–5, and 10–6) and the resulting site's mean uniform hazard spectra for hard rock? ***Figures 8.2-5R and 8.2-5T (Manchester Site):*** These figures are very important for understanding how smoothing affects hazard. It would be particularly useful to know the estimated rates of **M** 5 earthquakes compared with estimated *b*-values for the 8 objective smoothing realizations.	No revision required Discuss revised comparisons
CHAPTER 9 — USE OF THE CEUS SSC MODEL IN PSHA	
General Comments **G 9-1. (NAR)** Chapter 9 provides results that are potentially valuable for evaluating whether future new data or evolved knowledge require updating of the SSC model. In addition, the results are potentially valuable for resolving a number of seismic regulatory decision-making issues. The chapter is very well written, providing clear descriptions of the analyses performed and the results—a valuable contribution.	So noted.
G 9-2. (NAR) PPRP review comments on Chapters 1–5 include suggestions that may lead to modification of weights in the Master Logic Tree and hence corresponding changes in calculated hazard results.	Changes in logic trees, such as the weights associated with the seismotectonic zones versus the Mmax zones approach, have been propagated into the seismic hazard results.
G 9-3. (NAR) It is noteworthy that, based on the comparisons provided in Chapter 8, differences with the USGS and EPRI-SOG (COLA) results are significantly larger than the precision defined in this chapter for the CEUS SSC model results at all seven test sites. Indeed, for ground motions in the range of 10-4 to 10-6, the results in Chapter 8 indicate differences sometimes more than a factor of two between the USGS and CEUS SSC models in the rate of exceedances and the ground motion hazard. To avoid confusion, and because it might be argued that all experts have had essentially the same data and knowledge basis for assessing the various SSC models, the report should make abundantly clear how the uncertainty (precision or reproducibility) of the ± 25% should be understood—or not misinterpreted.	Paragraph added at end of Section 9.3
Comments by Section ***Order of Text, Tables, and Figures*** Material needs to be reorganized so that the order of presentation of text, tables, and figures is consistent with other chapters. ***Entire Chapter*** Throughout, change "seismogenic crustal thickness" to "seismogenic thickness." ***Section 9.1*** p. 9-1, par. 1: In line 6, change "Section 2" to "Chapter 2"; in line12, suggest replacing "that capture the community's views" with "that represent the community's views" Because this section is intended to be a useful "overview," in the last paragraph it would help to call the reader's attention more explicitly to the key conclusions presented in Section 9.4.3—at the very end of the chapter and after 96 pages. ***Section 9.2*** In the first paragraph, line 10: Change "components - that is" to "components—that is"	Order is consistent with presentation in Chapters 6 and 7. As discussed in Section 5.4, the use of the term "seismogenic crustal thickness" is consistent with common usage and is maintained. Changes made as suggested.

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Comment	Summary of Revisions to Report
Section 9.3.1	
In the first sentence: "The HIDs describing seismic sources" is confusing. There is only one HID. Suggestion: "In the HID, the specifications for seismic sources . . ."	Revised as suggested
p. 9-3, first full paragraph, third sentence: Because the test sites are extensively referred to in the remainder of this chapter, it would be helpful at the end of this sentence to point the reader to a map of the seven test sites (say Figure 8.1-1).	Revised as suggested
p. 9-3, 3rd full paragraph, last sentence: Suggest replacing "Please refer to Section 9.4" with "See Section 9.4"	Revised as suggested
Section 9.3.1.10	
p. 9-41, par. 2, first sentence: Text should be revised to eliminate reference to internal communications among the TI Team—"outlined in emails from Kathryn Hanson."	Revised as suggested
Sections 9.3.2 and 9.3.3	
No text provided, stated "to be written later."	Others responsible. Text must be written by MACTEC or EPRI for how to ftp access information
Section 9.4.1 and Table 9.4-1	
The text and table contain inadequate documentation insofar as the column of "Available studies" in Table 9.4-1 includes a mix of citations, which can be tracked, and informally referenced studies such as "Charleston: WLA," "New Madrid: Youngs," "PEGASOS study," PEGASOS project."	Citations were corrected to specify the sources
Section 9.4.2	
See Comment **S 9-1** regarding error in referencing figures, beginning in this section.	Figures renumbered.
First paragraph (p. 9-49), last sentence: COV is defined here. Appropriate place to introduce symbols for the standard deviation of hazard (σH) and mean hazard (MH, or some such).	Introduction of symbols was added
p. 9-66, line 2: Change "10-4 to 10-6" to "10–4 to 10–6"	Superscripts changed
Section 9.4.3 and Table 9.4-1	
The abbreviation "SSRS" appearing in Table 9.4-4 needs to be explained in the text. In the figure caption for Figure 9.4-44 one finds "srss" explained as "the square-root sum of squares calculation of the total COV." Neither srss nor SRSS appears in the list of acronyms.	Abbreviation was removed from text
Last paragraph: For clarity, it would be useful to explain where the statement "2/3 of the time" comes from—presumably from a normal distribution.	Statement removed
It is difficult to understand why the COVs decrease in annual frequencies of exceedance greater than 1E-5 on Figure 9.4-53 and 9.4-57.	Revised as suggested
The authors show at the Savannah, Chattanooga, and Columbia sites that the term "cl. Mean COV" is quite a bit different from the "wts COV." Because this is not intuitive, it would be helpful to provide some explanation to the reader.	Revised as suggested

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Comment	Summary of Revisions to Report
APPENDIX A—DESCRIPTION OF THE CEUS SSC PROJECT DATABASE	
General Comments The CEUS SSC Project has assembled and archived a comprehensive suite of data sets of the CEUS that are important to the characterization and assessment of the SSC model of the region by the TI Team and that significantly contribute to the community knowledgebase. Compiling and providing these data sets in a common GIS data format required substantial effort, for which the Project Team is commended. These data, for the entire CEUS SSC model region, as well as for specific subregions of special interest for the characterization and assessment of seismic source zones, have been obtained from existing data bases, digitized maps, data files, and original data. The data have been put into a GIS format to facilitate analysis, employing overlays of various data types, and they have been made available to the TI Team, the PPRP, and others in the project. The data files will be archived on a server that can be accessed in the future via a website. The data include maps of surface, bedrock, and crystalline basement geology, geophysical data (gravity, magnetic, and stress), results of seismic study of the crust, compilations of historic and pre-historic earthquake data, and previous seismic hazard analyses. Workshop #1 was focused on selecting the critical data sets required for the project and identifying the optimum data sets available to the project. Appendix A describes the data included in the Database and the procedures for assembling the data sets and making them available to the project teams. In addition, summary metadata “sheets” are included for 32 of the identified data 72 CEUS data bases. As part of the review of Appendix A, consideration also has been given to the 60 metadata files describing the data sets of the Database. The 60 metadata descriptions are in a separate digital data file which is not part of the final report or its appendices, but has been on the EPRI data server which is no longer in service. Future access to the metadata files via the website needs to be clarified and explained. In general these files are helpful in understanding the source, capabilities, and limitations of the data sets that are important to all users of the CEUS SSC data compilation. The level of detail provided in Appendix A and the metadata files is generally satisfactory, but significant revisions are required to improve the text, update and complete the summary description of the data sets, complete the metadata a sheets for all data sets, synchronize the data sets, the metadata files, and the summary data sheets, and make numerous editorial changes. Suggestions are provided in the following general and specific comments for improving the Database and its description and the metadata files.	Report was revised to clarify that data will be part of future project website for delivery of data/metadata.
Specific Comments for Clarity and Completeness **S A-1.** The Appendix does not describe the future website, or access to it, that will	This will be provided.

make the data sets and the metadata available to future users. This will need to be done to enable the report user to access the data and metadata files.	
S A-2. There are several data sets dealing with gravity, magnetic, and geologic data of the same data type that are of various vintages. Data sets should be eliminated in the Database that have been superseded by more complete and accurate data sets. Including dated, out of date, data sets in the Database will cause confusion in determining which data set was and should be used in analyses. As a result the credibility of the results of the project will be enhanced by removing dated data sets.	Older data sets removed from the database, as suggested.
S A-3. A total of 32 summary metadata sheets are presented in the Appendix for the CEUS SSC model region, but no summary metadata sheets are provided for the remaining 40 data sets listed in Table A-1 for specific subregions. Summary metadata sheets should be provided for all of the GIS layer data sets or an explanation for not preparing metadata for the data sets needs to be provided. Furthermore, there is not an obvious relationship between the summary metadata sheets and the metadata files. In the metadata file there are 60 separate files that do not synchronize with the summary metadata sheets. It is not clear why there are only 60 rather than 72 representing all the data layers as in Table A-1. Note also that the titles of the data sets are not necessarily the same as the titles in Appendix A and the metadata files. This causes confusion in using the files. It would be useful to have a column in Table A-1 that identifies the metadata file(s) of the specific data set as they exist in the metadata file.	Report was revised to provide metadata summary sheets for only the CEUS-scale data. Summary sheets for zone-specific data were not prepared because these data were typically digitized from a published figure.
S A-4. The prose in this appendix is in draft stage and needs clarification, reorganization, and improvement. A technical editor could help improve the appendix so that the resulting description of the efforts and the results associated with the Project Database reflect well on the major investment that was made.	Updated text.
S A-5. All pages of the appendix should be numbered consecutively. **S A-6.** The page size maps of the data sets that are provided as part of the summary metadata sheets are very useful. They provide a view of the data set for use in qualitative analysis by the user of the report. In addition, they assist the user in making a decision about preparing small scale maps of the mapped parameter or in selecting regions of the maps for detailed analysis. Only one of the six magnetic anomaly data sets prepared for this project (Ravat et al., 2009) is shown, and only one of the fifteen gravity anomaly data sets (CEUS SSC, 2010) prepared for this project is illustrated. Please note that referring to the gravity anomaly data sets by CEUS SSC, 2010 as in the summary data sheets may lead to confusion. An alternative suggestion is to cite Keller, 2010, personal communication.	The goal of the summary sheets was to provide an easily-accessible review of the database contents and individual regional-scale data layers rather than a detailed presentation of every derivative of every layer. However, we have developed figures for each of the data layers in the CEUS and their derivatives where there is a metadata summary sheet.
S A-7. Unfortunately a key to the contour interval and symbols used in several of the maps is not provided with the map. This seriously detracts from the usefulness of the maps. In the few maps that show a color bar of the mapped parameter amplitudes, the limits of the range are given to a precision unwarranted in the data set and have limited usefulness for the user. In addition, these color codes are too coarse for most uses of the data.	Explanations, labels, scale bars and other typical map elements have been provided for the figures in the appendix where appropriate. Because of the scale of information presented in the figures not all features can be shown clearly, or exhaustive legends developed.
S A-8. The keywords of the metadata files need further attention. Most data sets do not have keywords, and keywords that are given are not consistent and comprehensive. Keywords are not critical but they can be helpful in directing the data	A keyword dictionary will not be available as part of the metadata for each data layer.

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user to the appropriate data set without laborious, extensive review of all the data sets. Will the user be able to search the data sets by keyword?	
S A-9. "Aeromagnetic" in the title of maps should be changed to "magnetic anomaly." This is the general title that is applied to regional magnetic anomaly maps.	Updated text.
S A-10. Citations in tables are not in consistent format.	Updated text.
S A-11. A data file showing areas where reliable earthquake hypocenter depths are available would be useful. Or is it possible to show range of depths of foci for the CEUS?	
S A-12. Headers for Metadata Sheets: The repetition of "CEUS SSC Project GIS Data Summary" in large point size and bolded is less important to guide the reader's eye than the title of what the sheet contains. Consider reformatting the header information. Example: **Sheet A-1 — CEUS SSC Project GIS Data Summary** **NOAA DNAG MAGNETIC ANOMALY MAP OF NORTH AMERICA**	Updated sheets.
S A-13. Remove bracketed comments in text from previous reviewers.	Updated text.
S A-14. Shaded-relief versions of selected gravity and magnetic anomaly maps (e.g., total magnetic intensity anomaly map, reduced to pole magnetic anomaly map, residual isostatic gravity anomaly map) are a significant aid in the interpretation of the geological sources of the anomalies, particularly the high wave-number components of the anomalies. Several of these shaded relief maps have been prepared, but they are not identified in the data sets. They should be included and the specifications of the azimuth and inclination of the light source used in preparing the maps should be specified on the maps and in the metadata.	Hillshades (shaded relief) were created for the magnetic and gravity anomaly derivatives where possible to provide an added visual aid to the interpretation of these data. Some of the data layers have limited data ranges and were not appropriate for hillshade creation without significant exaggeration. Summary sheet text for gravity and magnetic data, were modified to note that the hillshades (shaded relief) layers exist and their parameters. This information was also included on the figures where a hillshade was displayed.
Comments by Section, Table, and Sheet ***Text: Unlabeled Introduction*** First paragraph: The first two sentences are not clear as to the goals of the database and the method of achieving them. The use of "function" in the first sentence leads to confusion. Suggest a rewrite focusing on goals of the data sets and procedures used to achieve them. Second paragraph: Strongly suggest that the term "aeromagnetic" throughout the titles of data sets be changed to "magnetic anomaly." This is the appropriate title given to regional magnetic anomaly maps. Delete Free-air gravity and remove Bouguer and simply use the term "gravity anomaly." Also, remove DNAG and USGS. These are data sets that have been superseded and should be removed from the Database. The Mesozoic rift basins data base cannot be found as an entity in the Database. Remove the parenthetical phrase in Earthquake Catalog. In the bulleted list, note that there is no metadata file or summary for Mesozoic rift basins which was compiled for this study. Last bullet of second paragraph: Will digital presentations of the crustal scale profiles be available? If not, where can they be obtained for analysis? Last paragraph, last line: The last metadata summary sheet is A-32, rather than A-36.	Updated text. Mesozoic basins have been included as five separate data layers, each corresponding to the interpretation of the source author. Metadata and a metadata summary sheet have been included for these layers. Digital representations of the crustal scale profiles are not included in the database.
Text: Section A.1 Last sentence of first paragraph: "The digital data compiled for the CEUS SSC Project	Updated text.

are available to the public to provide transparency regarding the development of the CEUS SSC database." Transparency does not seem to be an important reason for this. Rather it serves as a repository of data useful for largely regional seismic source zone characterization and assessment in the CEUS. Second paragraph, first line: Figure A-1 is not in the appendix. Second paragraph, second line: For example, some *public-domain* data sets cover… Third paragraph, bullets: Change to "Magnetic anomaly data," "Gravity anomaly data" Add "Mesozoic rift basins within the ECC-AM" to the bullet list in third paragraph. Third paragraph, fifth bullet: Add "data" after "Maximum horizontal compressive stress" to be consistent. Third paragraph: Replace "sources" in first line by "types" or, less desirable, "class." In second line, suggest: "These data layers include the following:"	
Text: Section A-2 First paragraph: Suggest that definitions of data class, theme, etc. be provided or a figure showing hierarchy of data. First paragraph: Spell out FWLA, point out that this server is no longer available First paragraph, top of p. A-3: Instead of "theme," use "type of data"? Fifth paragraph: All project data began at revision 0 (Rev0) and have been updated with consecutive revision numbers and made available via the project web site. Providing a full file name reference allows data to be identified if removed from the organization of the project Database. Sixth (last) paragraph: Add this sentence at end: "This server is no longer in service for this project." Sixth paragraph: Is the "Project GIS Manager" the same as "Database Manager" identified in Figure 2.3-1 and Appendix G? If so, be consistent.	Updated text. Notes added regarding the class/theme as used in database and text. The FWLA server was only for project use, not long-term use. No need to note this in the text as requested in the comment. Title renamed as "Database Manager" as originally noted in the project work plan.
Text: Section A-3.3 Second paragraph: If the steps to review GIS data produced from non-digital data were sequential, it would be better to present the steps using numbers or letters rather than bullets. Second paragraph, fourth bullet: Clarify "Completion of attribute information" Second paragraph , last bullet: Use of the term "topology" appears to be inappropriate here because the term is generally used to describe a branch of mathematics.	Updated text. The term "topology" used in GIS refers to data layers with defined relationships between their component points, lines and polygon features.
Text: Section A-4 Second paragraph, line 2: ** are ?? Second paragraph, line 6: Summary sheet A-22 has no state boundaries. Second paragraph, line 7: Why not "all" rather than "majority"? What criteria were used to omit some? Second paragraph, last sentence: Why are there no metadata summary sheets for data covering specific regions of the study area? If they are important enough to include as a data set, they should be important enough to have a metadata file. Are all data sets included in the metadata file? If not, why not?	Updated text. See above comments about metadata *summary* sheets vs. layer metadata.

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Text: Section A-5 Third paragraph, first line: Were the *original* or *source* data provided? Third paragraph, last line: Typo: "into other coordinate systems."	Updated text.
Text: Section A-6 This section is out of place, place after A-5. First paragraph, 3rd line: Add earthquake [information] to this list Second paragraph, line 5: Typo: "to identify geologic relationships"	Updated text.
Table A-1 Page 1: Where are the citations located? Are they all in the same place in the report? Page 1: Delete Row 1 Page 1: Delete Row 3 Page 1, Row 5: Need more complete description of this database and its preparation or refer to another section of report. Page 2, Row 2: Replace "Geodesy" with "Strain (GPS)" Page 3, Row 3: Why is this map being used, since it was replaced by Reed et al. (2005)? Delete. Page 4, Row 3: Is this the basin map referred to in the data evaluation tables? If so use consistent titles. Page 4, Row 7: Delete, superseded. Page 5, Row 1: Refer to Keller, 2010, personal communication Page 5, Rows 3 and 4: Delete, superseded Page 5, Row 6: How are these tied to references? Where are the metadata for these layers? Page 6, Row 2: This is also referred to as Zoback (2010). Determine appropriate reference and use consistently. Page 7, Row header: Change "Mid-Continent" to "Midcontinent" Page 7, Row 5: Replace "Geodesy" with "Strain (GPS)" Page 7: Why does the numbering of Summary Sheets stop with A-32 (in the last row of page 6)? Page 8, Rows 5 and 6: Need citations Page 9, Row 2: Need citation Page 9, Row 3: Change "Aeromagnetic" to "magnetic anomaly"	Text has been updated. Citations are presented in the report. Citations used in Table A-1 are the same, or same style, as those in the report. Older data that have been superseded with new data in this database have been removed. Data layers that were incorporated into the CEUS paleoliquefaction database have been removed from the rest of the database where earlier presented as separate layers.
Sheet A-1 Delete, superseded ***Sheet A-2*** Replace "aeromagnetic" with "" Contour interval should be given Show page-size maps of six data sets with bar graph for amplitude and in shaded relief if possible Differentially reduced to pole, tilt derivative, etc. may not be known entities to user; suggest a basic reference for each of these for the interested reader ***Sheet A-3*** Delete, superseded	Updated sheets as appropriate.

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Sheet A-4 Increase amplitude at least twice that being shown ***Sheet A-5*** Data description: Needs range of date, also key to map symbols; as throughout report, moment magnitude (**M**) should be bolded; which earthquake catalog is referred to? The raw catalog, the declustered catalog, or ?? Should refer to Appendix B if this is the same catalog. ***Sheet A-6*** Identify symbols ***Sheet A-7*** Brighter colors needed, no extended crust identified ***Sheet A-8*** Need brighter colors ***Sheet A-9*** Need key to colors ***Sheet A-10*** Need key to colors ***Sheet A-11*** How are they keyed to source (reference)? ***Sheet A-12*** Delete, superseded ***Sheet A-13*** Data description is misleading, the dashed line represents the mapped eastern limit of pre- 1600 Ma crust. Why not show all of Figure 2 of this reference? It puts the boundary into the context of the basement terranes. ***Sheet A-14*** Brighten colors and provide key ***Sheet A-15*** Brighten colors and provide key ***Sheet A-16*** Brighten colors ***Sheet A-17*** Needs key ***Sheet A-18*** Needs contour interval, high range is given to 4 decimal points which is much greater than precision ***Sheet A-19*** Needs key ***Sheet A-20*** Legend of figure needs to be checked. What is basement thickness? Unclear. ***Sheet A-21*** Brighten colors and provide key ***Sheet A-22*** Delete, superseded ***Sheet A-23*** Brighten colors, color contour interval needed, show all figures at page size, preferably in shaded relief; suggest for Author that G.R. Keller be identified as the source of the data and derivative anomaly maps . . . as in A-2 for D. Ravat. ***Sheet A-24*** To be consistent, use residual isostatic; color contour interval without range beyond decimal point. ***Sheet A-25*** Delete, superseded ***Sheet A-26*** Delete, superseded ***Sheet A-27*** Tie to references?? Where will the metadata file be accessible? ***Sheet A-28*** Brighten colors ***Sheet A-29*** Needs key; this is also referenced as Zoback (2010) – select appropriate citation	

APPENDIX B — EARTHQUAKE CATALOG	
General Comments Appendix B contains a listing of the earthquake catalog for the CEUS developed as part of this project. The development of the earthquake catalog is a major element of the source characterization and assessment in the project. The Appendix contains a single page of text that identifies the columnar entries in the catalog followed by a 273 page tabular listing of the 9800 earthquakes in the catalog. The table is well laid out and easy to follow. It is evident that monumental efforts were required to compile this catalog, and the Project Team is to be applauded for these efforts. Beyond its use by TI Team members familiar with its contents, careful documentation and explanation is needed for the contents of the catalog to be understood and appropriately used by others.	Greater detail on contents of catalog added to Chapter 3 and to Appendix B
Specific Comments for Clarity and Completeness **S B-1.** *Need for Introductory Text* A brief summary discussion should be added to this Appendix. This discussion should describe what this catalog listing actually is (i.e., final catalog with dependent events flagged). It would also be useful to refer the reader back to relevant sections of Section 3 for a discussion of **M***, etc. Additional notes on depths and how ERH was estimated would also be useful in the introduction to the catalog. A pointer to the appropriate Database entry would be useful. A catalog of non-tectonic events was developed as part of this project (mentioned in Section 3), where will this catalog be documented and maintained?	Summary added, including description of list of non-tectonic event Catalog will be on Project web site. Maintenance in the future is beyond the scope of this project, but discussions with the USGS as a possible repository are in progress
S B-2. *Clarity of Documentation in the Catalog Explanation* For clarity of documentation, attention should be paid to the following: 1. Designation of time in an earthquake catalog should be explicit. Are the times/dates in UTC? Local time? A mix? This is non-trivial if one tries to find the events in another catalog. 2. How should the reader interpret the variable presentation of significant figures in the Earthquake Catalog for latitude, longitude, depth, **M**, and sig**M**? How does one discern available information on precision from the vagaries of spreadsheet display? 3. The meaning of Depth = 0 should be explained. 4. To avoid ambiguity, ERH should be explained as “Horizontal Location Uncertainty (km)”. If correct that the entries for ERH contain both rough estimates and statistical calculations, then ERH is better described as “Estimated Horizontal Location Uncertainty (km)”. 5. After ERH, entries in the Explanation change from having the first letter of all terms capitalized to just the first word capitalized. 6. **M** , **M***, and sig**M** should be bolded in Column 1 of the Explanation 7. In column 1, “Flag” should be written “FLAG” as it appears in the table.	Discussion added of entries
APPENDIX C—DATA EVALUATION TABLES	
General Comments **G C-1. (NAR)** The tables of Appendix C summarize what data were used, how the data were used, and the source, quality, and significance of the data in defining,	Comment noted and appreciated.

characterizing, and assessing the CEUS seismic sources. In addition, the tables specify the availability of the data in GIS format. These tables are a useful supplement to the documentation of the seismic source zone characterization and assessment of both the RLME sources and the seismotectonic source zones. They will be useful to users of the CEUS SSC report, and they will also provide a guide to potential application of various data sets in future evaluations of the CEUS SSC model. In general, the tables are well prepared and presented. However, they are not without problems, as we proceed to explain.	
Specific Comments **S C-1. (CC, DMM)** *Completeness of Tables and Ambiguity About Applicability* Data Evaluation tables have been prepared for many of the identified seismic source zones, but not all. In Section 4.2.2 of the main report, the reader is informed (p. 4-6, first paragraph of the section) that, "Data Evaluation tables were developed . . . and the tables for each source (emphasis added) are included in Appendix C." Comparing a list of the RLME sources, the seismotectonic zones, and the Mmax source zones with the index of tables on the first page of Appendix C will leave the reader perplexed. Further, the treatment of some zones is handled within the Data Evaluation table for another zone (e.g., the Meers fault RLME source is included in the table for the OKA seismotectonic zone). What criteria were used to select which zones were to have Data Evaluation tables? At the top of Table C-5.4, the labeling indicates "Default for entire CEUS SSC." Does this mean that if a table is not given for a specific zone, then Table C-5.4 is the applicable table? (If this is the intent, note that Table C-5.4 is incomplete with regard to several data sets.) Introductory text should be added to eliminate these and similar questions and concerns pertaining to the Data Evaluation tables. All seismic source zones including Mmax zones should have a Data Evaluation table.	Data Evaluation for Meers fault / OKA RLME combined because when Meers is out of cluster, RLME allowed within larger OKA source. The list of tables on the cover sheet for Appendix C was expanded to indicate the applicable table for each RLME and Seismotectonic zone. All seismic sources have an applicable Data Evaluation table. In some cases, the same table is used for multiple sources and the applicable sources are listed at the top of the table. Table C-5.4 is different from the other tables because it is referring specifically to the assessment of future earthquake characteristics (Section 5.4). Thus, the reference to "Default for entire CEUS SSC" is specifically for future earthquake characteristics, as discussed in Section 5.4.
S C-2. (CC) *Facilitating Use of the Data Evaluation Tables* The Data Evaluation tables are explained in the text of the report (Section 4.2.2). However, consideration should be given to adding a short description of the objective, organization (including the keying of the table numbers to the main body of the report), preparation, and uses of the tables in an introductory paragraph to the appendix. This will facilitate the use of the tables. An explanation of the content of the columns used in the tables should be also included in this description for stand-alone reading. Also, all pages of Appendix C should be numbered consecutively, not separately for each table, to enable convenient reference—as opposed to having to point to a specific table and a page number within the table.	Revisions made as suggested.
S C-3. (CC, DMM) *Inconsistencies in the Tables* The Data Evaluation tables have numerous inconsistencies that should be eliminated because they diminish the quality and usefulness of the tables. We note the following: 1. The titles of the tables and the identified source in the notes at the top of each table should be consistent with the nomenclature of the text of the report, and tables should be in the same sequence as the identified source is described in the text (or keyed to a table in the text). 2. Although the majority of the Data Evaluation tables are also in the Data Summary tables (Appendix D), some are not included in the Data Summary tables and vice versa. There is no explanation for this inconsistency among tables in the documentation of the	1. Revisions made as suggested. 2. Explanation added in Section 7.1; a new Table 7.1-1 shows which Appendix C and D table numbers are associated with each of the seismotectonic zones. 3. Modifications made with an objective of achieving greater consistency in the level of information. 4. Modifications made so that all tables have seven columns. "Discuss" is defined as "to consider or examine by argument, comment, etc." and is the

report. 3. The level of information given in the tables is variable. This may be due in part to the information available, or it could be due to the detail that is provided by the individual preparing the table. Greater consistency in the level of information would be desirable. 4. All the tables have seven columns except for Tables 6.1.4 (OK aulacogen) and 7.3.9 (Gulf Coast), which have eight columns. Only seven columns are described in Section 4.2.2 (pages 4-6 and 4-7). Note that the fifth column should be "Description" rather than "Discussion" (there is no oral material here). Throughout the tables, references to "discussions" should be changed to "descriptions." 5. Numbers in the tables are inconsistently spelled out or given in numeric form. Numeric form should be used for data and scoring; otherwise, numbers should be spelled out when referring to counts of ten or less. 6. Geographic (compass) directions are inconsistently given in abbreviated (e.g., NE) and spelled-out form. 7. Some tables have the acronyms for the subdivisions of the seismic source zone identified in notes at the beginning of the tables, others do not. Also several acronyms are not given in the List of Acronyms. 8. Descriptions in cells are variously in sentence and non-sentence form. It may be useful to have both, but an effort to be consistent would be worthwhile. 9. The use of blanks in the tables is inconsistent. Every cell needs to have something in it; if nothing else, N/A for not applicable or some other notation to indicate intention. Otherwise, the meaning of a blank cell will be unclear. 10. There is inconsistency in the title of column 3 among the tables. Is it "data quality" or "data and quality" (as in "Notes on Quality or Data")? 11. Use data as a plural word consistently throughout the tables. 12. Both the terms magnetic and aeromagnetic are used in the tables. The use of the term aeromagnetic should be changed to magnetic throughout. Aeromagnetic simply refers to the method of collecting the majority of the data in the file. Referring to "aeromagnetic" but only to "gravity" is inconsistent. 13. Where no data are available for a particular type of data, the tables deal with this in different ways—sometimes the wording indicates explicitly that no data are available (e.g., Table C-6.1.3, p. 4; r. 3); in other places, data are just not identified. 14. The evaluation of the quality of the data is not consistent; in some cases peer-reviewed publications are referred to and in others simple publications.	formal treatment of a topic in speech or writing. We believe that the term "Discussion", as used in the Data Evaluation tables, is more accurate than the term "Description", so no change has been made. 5.Revisions made as suggested 6. Modifications were made to abbreviate compass directions in the tables. 7. Modifications made for consistency; List of Acronyms has been expanded. 8. Modifications made with an objective of achieving greater consistency in using non-sentence (or notes) form. 9. Modifications made with an objective of achieving greater consistency. 10. Revisions made; the proper heading for column 3 is "Notes on Quality of Data" 11. Revisions made as suggested. 12. Revisions made as suggested. 13. Modifications made with an objective of achieving greater consistency. 14. Modifications made with an objective of achieving greater consistency.
Comments by Table (for Clarity and Completeness) (Notation: pg. = page, c = column of table, r = row of table) ***Table C-5.4*** • pg. 1, descriptor (title) of 5th c: Would "significance" be a more descriptive term than "reliance"? • pg. 4, r. 3; c. 1: Is this the new data set from Zoback? If so, please put a date on it—and put dates in all tables for all the data sets prepared for this project so that in subsequent use there will be no question of date. • pg. 1, r. 4, 5, & 6; c. 6: Add fault to slip • pg. 1, r. 7; c. 3: In the Charlevoix area of the St. Lawrence Rift	Reliance is a better descriptor; "significance" would be some measure of importance, but "reliance" describes how much the TI Team used the information in their assessments. Revisions made as suggested or to clarify.

• pg. 2, r. 1 & 2; c. 6: Add fault slip • pg. 2, r. 5; c. 6: Could not find where depth as a function of magnitude is described in report	
Table C-6.1.1 • pg. 1, r. 5; c. 6: Incomplete • pg. 1, r. 6; c. 1: Change to magnetic from aeromagnetic, here and elsewhere in tables • pg. 2, r. 2; c. 1: Give date • pg. 3, r. 4; c. 1: Reinecher not in references…this holds true for many of the references cited in the tables…they should be included in Chapter 10 (References)	• Removed reference to specific value from project catalog • changed to magnetic • no date given for gravity dataset; • Replaced reference with Heidbach et al., 2008 as a global change.
Table C-6.1.2 • pg. 2, r. 5 & 6; c. 3: What is the significance of the term "basic"? • 6, r. 1; c. 3: What is meant by "plain sediments"? • pg. 6, r. 5; c. 1: Should be bold and italics • pg. 9, r. 5; c. 1: Should be bold and italics • pg. 11, r. 3; c. 3: Replace to with two	Revised as suggested.
Table C-6.1.3 • pg. 3, r. 2; c. 6: Reference to 2002 article is incomplete (author?) • pg. 5, r. 1; c. 6: Change to "No measurements nearby to the . . ." • pg. 6, r. 2; c. 6: Reference to 2002 article is incomplete (author?)	Revised to address comments
Table C-6.1.4 • Why add an eighth column? Y or N to be used in c. 8 to be consistent with rest of tables. • pg. 1, r. 1; c. 4: How are faults due to hydrocarbon exploration? Change wording. • pg. 1, r. 1; c. 5 and subsequent rows on page: What is OK aulacogen? Background? • pg. 1, r. 2; c. 1: Bold and italics • pg. 1, r. 3 & 4: Delete. These are data sets superseded by the EPRI data set. • pg. 2, r. 2: Delete this data set, superseded by the EPRI data set • pg. 3, r. 3; c. 7: Change to "within the Arbuckle" • pg. 4; r. 2; c. 2: 1990 • pg. 4; r. 4; c. 2: What is BEG? • pg. 7; r. 1; c. 7; "fault slip"	Revised as suggested
Table C-6.1.5 • pg. 1; r. 2; c. 6: Change to …are concentrated…; also …projects to surface.. • pg. 1; r. 5; c. 6: Change to …sequences provides… • pg. 4; r. 1; c. 3: Give map # • pg. 5; r. 5; c. 3: Is relatively short germane? Don't know what short is. This is not used where abstracts are referenced. • pg. 6; r. 2; c. 4: Define abbreviations • pg. 6; r. 5; c. 6: Rationale or geophysical evidence? • pg. 8; r. 1; c. 7: What is significance of ("?")	Revised to address comments

Table C-6.1.6 • pg. 5; r. 2; c. 6: What is RP and ERM-SRP? ; need period after parenthesis • pg. 12; r. 1; c. 6: What is ERRM? ERM	Revised to address comments: RP deleted. ERM-SRP Eastern Rift Margin-Seismic river picks; ERRM, which stands for Eastern Reelfoot rift margin, is an acronym used in a publication
Table C-6.1.7 • pg. 3; r. 5; c. 4; what is EMF_S? not in acronyms	EMF_S has been changed to ERM_S
Table C-6.1.8 • pg. 2; r. 1; c. 6: No CFZ in acronyms • pg. 3; r. 5; c. 6: Explain A and B; replace	Revised to address comments.
Table C-7.3.1 • pg. 2; r. 1; c. 6: Clarify the wording, "A general gradient in amplitude parallels" • pg. 4; r. 8; c. 6: Entries; period at end of sentence • pg. 5; r. 1; c. 6: Capitalize Mechanisms	• Deleted statement. • Entry carries onto next page so no period was added • Entry not capitalized because sentence continues from the previous page.
Table C-7.3.3 • pg. 1: Shouldn't the title be Northern Appalachian zone, without the "s"? • pg. 3; r. 2; c. 3: Parenthesis at end	S deleted from header; Parenthesis added.
Table C-7.3.4 • pg. 1: In notes beneath title, need to identify the acronyms of the subdivisions of the zone • pg. 2; r. 4 & 5; c. 3 & 6: What is CLFS?	Clarified acronyms for source zone; Spelled out Clarendon-Linden fault system
Table C-7.3.9 • pg. 1; r. 3 & 4; c. 7: If considered for defining boundaries, why 0 in column 6?	A "0" was used when a data set was considered but not used
Table C-7.3.12 • pg. 1; r. 5; c. 3: Do not capitalize intensity • pg. 2; r. 1; c. 6: Unfinished sentence • pg. 2; r. 2; c. 6: Belongs in column 6 of row 3; why 2 in column 5 for row 2 and 1 in column 5 for row 3? • pg. 5; r. 1; c. 6: Remove "yet"	Revised to address comments.
APPENDIX D—DATA SUMMARY TABLES	
General Comments **G D-1. (NAR)** The Data Summary tables of Appendix D contain a massive amount of information on references that include data considered by the TI Team in identifying, characterizing, and assessing the CEUS seismic sources. These data include all types of information that have a potential use in achieving these objectives. The tables provide a benchmark of germane data at the time of the Project, which gives transparency to the efforts of the TI Team and which future evaluations can augment with new sources of information. The tables include the citation, the title, and the data included in the reference that are relevant to seismic source identification and characterization. The tables are thorough and, in general, reasonably well prepared and presented. We proceed to point out minor problems needing attention before finalizing the appendix.	No revision necessary.

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Specific Comments **S D-1. (CC)** *Difficulty in Relating the Appendix to the Main Body of the Report* The labeling of the tables is not consistent with the titles and acronyms used in the main body of the report for the source zones, and source zone data summaries are grouped in a manner that makes it difficult to relate the tables to some of the specific zones. For example, the Gulf Highly Extended Crust zone is apparently included in Table D-7.3.9, Gulf Coast Data Summary. Similar situations occur in other tables of the appendix. This makes it very difficult to relate the tables to the source zones in the report and decreases the usefulness of the appendix. This inconsistency needs to be rectified.	The list of tables on the cover sheet for Appendix D was expanded to indicate the applicable table for each RLME and Seismotectonic zone. Modifications have been made to the tables with an objective of achieving greater consistency. Explanation added in Section 7.1; a new Table 7.1-1 shows which Appendix C and D table numbers are associated with each of the seismotectonic zones.
S D-2. (CC) *Facilitating Use of the Data Summary Tables* The Data Summary tables are explained in the text of the report (Section 4.2.2). However, consideration should be given to adding a short description of the objective, organization (including the keying of the table numbers to the main body of the report), preparation, and uses of the tables in an introductory paragraph to the appendix. This will facilitate the use of the tables. An explanation of the content of the columns used in the tables should be also included in this description for stand-alone reading. Also, all pages of Appendix D should be numbered consecutively, not separately for each table, to enable convenient reference—as opposed to having to point to a specific table and a page number within the table.	Revisions made as suggested.
S D-3. (CC) *Inconsistencies in the Tables* The Data Summary tables have numerous inconsistencies which should be eliminated because they diminish the quality and usefulness of the tables. We note the following: 1. The titles of the tables and the identified source in the notes at the top of each table should be consistent with the nomenclature of the text of the report, and tables should be in the same sequence as the identified source is described in the text (or keyed to a table in the text). 2. The level of information given in the third column, Relevance to SSC, is variable. This may be due in part to the information available or it could be due to the detail that is provided by the individual preparing the table. Greater consistency in the level of information would be desirable. 3. Geographic (compass) directions are inconsistently given in abbreviated (e.g., NE) and spelled-out form. 4. Dates are presented in different formats. 5. Some tables have the acronyms for the subdivisions of the seismic source zone identified in notes at the beginning of the tables, others do not. 6. Column 3 descriptors are sometimes in sentences, while others are not. It may be useful to have both, but an effort to be consistent would be worthwhile. 7. The use of blanks in the tables is inconsistent. Every cell needs to have something in it; if nothing else, N/A for not applicable or some other notation to indicate intention. Otherwise, the meaning of a blank cell will be unclear. 8. Both the terms magnetic and aeromagnetic are used in the tables. The use of the term aeromagnetic should be changed to magnetic throughout. Aeromagnetic simply refers to the method of collecting the majority of the data in the file. Referring to “aeromagnetic” but only to “gravity” is inconsistent. 9. The format of the references at the end of each table is inconsistent, and some	1. Revisions made as suggested. 2. Modifications made with an objective of achieving greater consistency in the level of information. 3. Modifications made to abbreviate compass directions in the tables. 4. Modifications made with an objective of achieving greater consistency. 5. Modifications made with an objective of achieving greater consistency. 6. Modifications made with an objective of achieving greater consistency in using non-sentence (or notes) form. 7. Modifications made with an objective of achieving greater consistency. 8. Revisions made as suggested. 9. References have been deleted from the tables; all references are included in Chapter 10 References. 10. Modifications made with an objective of achieving greater consistency. 11. Modifications made with an objective of achieving greater consistency.

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references do not have complete information. 10. The ordering of the citations in the tables is not consistent. Some are listed chronologically, while others are listed alphabetically according to the first letter of the family name of the senior author. 11. Use of bold letters for subtitles in several of the tables is inconsistent. 12. Capitalization of type of feature is inconsistent in the tables. It is suggested that the type of feature should not be capitalized, e.g., Commerce lineament, not Commerce Lineament.	12. Modifications made with an objective of achieving greater consistency.
Comments by Table (for Clarity and Completeness) (Notation: pg. = page, c = column of table, r = row of table) ***Table D-5.4*** • pg. 1, c. 1: Period after et al. on this page and throughout tables • pg. 4: Should Petersen et al. be included?	Revised as suggested. Petersen et al. was not used to assess future earthquake characteristics.
Table D-6.1.1 • pg. 1, c. 3: Spell aulacogens	Revised as suggested.
Table D-6.1.2 • pgs. 2 & 3, c. 3: No difference for Chapman and Beale, 2009 and 2010. Should there be a difference? • pg. 5, c.3, r.2: Should be Appalachian Mountains not Appalachians, similar comment for other geographic features throughout tables. • pg. 15, c.2, r.2: Why is journal listed?	Revised as suggested.
Table D-6.1.3 • pg. 1, c.3, r.2: The abbreviation for miles should be mi without a period (not mi.) — change throughout tables	Modifications were made to the tables to achieve consistency – English units are abbreviated and include a period at the end (e.g., mi. and ft.); metric units do not have a period at the end (e.g., km, cm)
Table D-6.1.4 • pg. 3, c.3, r.1 & 2: Replace further with farther	Replaced
Table D-6.1.5 • pg. 40, c.3, r.2: Blank—similar blanks in other tables	Revised to fill in blanks
Table D-6.1.9 • pg. 4, c.3, r.4: Use of the casual Appalachians and Rockies should be avoided • pg. 12: Has horizontal line between rows missing—this occurs elsewhere in tables	Revised as suggested.
Table D-7.3.1 • pg. 4, c.2, r.2: Misspelled Quebec • pg. 5, c.3, r.2: Is it Sutton Mountain or Sutton Mountains? Both are used in this table.	Revised as suggested.
Table D-7.3.2 • pg. 10: Reference for N.H. Sleep; misspelled mantle	Revised.
Table D-7.3.4 • pg. 9, c.3: No references for two subheadings • pg. 15, r.: Geophysical Investigations should be bold; similar subheading concerns	Entries added. Headings formatted.

elsewhere in tables	
Table D-7.3.7 • pg. 1: Horizontal lines needed between citations • pg. 11: Misspelling of investigate	Modifications made
Table D-7.3.9 • pg. 1 and following: Why () around citations?	Removed
APPENDIX E—CEUS PALEOLIQUEFACTION DATABASE, UNCERTAINTIES ASSOCIATED WITH PALEOLIQUEFACTION DATA, AND GUIDANCE FOR SEISMIC SOURCE CHARACTERIZATION	
General Comments This appendix represents a thorough and well expressed compendium of methodology, data, and guidance related to paleoliquefaction studies in the CEUS. The written content and illustrations present the data and information clearly and with a high degree of technical quality. Generally the documentation of effort encompassed in this appendix supports the related assertions made in the CEUS SSC. This work is notable not only because it represents a new and productive field of study that was not included in the earlier EPRI-SOG and LLNL projects, but also because the effort has brought sets of information and data that were highly varied and inconsistent into a consistent and coherent framework. This appendix is likely to be used as a primer on the topic for future researchers in paleoliquefaction, and the fulfillment of the recommendations provided could significantly improve the understanding of RLMEs in areas of low to moderate seismicity areas in the U.S. and globally.	No revisions required.
Specific Comments for Clarity and Completeness ***S E-1.*** *Incorporation of the Digital Database* It is unclear how the digital database is going to be incorporated into the final report and how it will be accessed in the future. It would be useful to the reader if the location was noted after the sentence, “The database itself is available in digital format.”	Text modified to indicate that the database will be available on the CEUS SSC project website.
S E-2. *Recommendations for Clarification of the Digital Database* Because Section 1.1 (Database Structure) uses many technical terms related to dating that are very well discussed later in the document, it may be useful for many readers who are not well versed on the techniques if a sentence were added at the end of the first paragraph of the section that says, “A discussion of the various dating methods and their uncertainties can be found in Section 2.1.3.” In relation to the description of the database on page 2, a simplified figure illustrating parameters such as SB_THICK, SB_WIDTH, SB_LENGTH, etc. may be helpful to the reader. Similarly, a simple figure illustrating the uncertainty estimates described in the last paragraph of Section 1.1 is not essential, but could be very useful for the reader.	Text modified as recommended. Added two figures illustrating measured size parameters of liquefaction features and age data used to estimate ages and related uncertainty of liquefaction features.
S E-3. *Clarification of Data Contributors* At the beginning of each of the “Data Description” subsections in the discussions of regional datasets in Section 1.2, the authors note that “Paleoliquefaction data have been contributed by . . . .” It is unclear to the reader if the contributors listed represent a complete list of the researchers who have worked in the area or if it is a subset of	Text revised for clarity as recommended. Clarified who contributed directly to the database and new maps produced for the report showing rivers searched. Changed Beta Analytic to Beta Analytic Radiocarbon Laboratory.

researchers who have provided additional information specifically for this project (e.g. by providing 2-sigma data that were not otherwise published). Because this report is likely to be read by researchers not familiar with paleoliquefaction, it may be helpful to refer to Beta Analytic as "Beta Analytic Laboratories" or in similar terms. The way the text reads currently, those not familiar with the topic are likely to understand Beta Analytic to be a process or approach described in Talma and Vogel (1993) or Vogel et al. (1993).	
S E-4. *Missing or Misnumbered Figures* — There is a Figure 11a, followed by Figure 11. Presumably, the second should be Figure 11b. — Figure E28 is missing. — There is a Figure E-39 and a Figure E-39b. Only E-39 is noted in the text. — There is a Figure E-44 and an E-44b. Only E-44 is noted in the text. — On Figure E-50, it would be useful to note what the SL signifies in the description for those not familiar with that notation. — Figure E-51 is sideways. — Figures 53b and 55b are missing.	
S E-5. *Additional Information and Clarification of Seismic Zones* On page 8 in the first paragraph of Section 1.1.2, there is a discussion of a lineament throughout the paragraph. In the next paragraph there is reference to the "Daytona Beach" lineament at the end of the paragraph. It is unclear whether all the discussion relates to a single lineament called the Daytona Beach lineament. If so, perhaps the name should be noted at the start of the discussion. The discussion of the Wabash Valley Seismic Zone should be expanded to make the report more complete. Neither the text, nor the figures, provides any actual dates, with the figure instead indicating "Event A Dates," "Event C Dates." The description of the dataset in the report should discuss these events and their dates rather than expecting the reader to go to the original papers. On page 13, the report notes that "There is no evidence for repeated large earthquakes in the exposures." This statement needs to be further explained. In what way do the data not meet the criteria established by the project? Because this is a hazard-significant finding for sites in the ALM region, the line of evidence that the features do NOT represent seismically-generated features should be made clear. Also, it is unclear how this bullet and the following bullet are different statements. From discussion of the Charleston Seismic Zone, it is unclear from both the text and the figures what the number of events and the dates of those events are. One can only tell that there is a historic event, and at least one other event happened. Clarification as to what the outcomes are in the text would be helpful to the reader.	Daytona lineament: The Daytona Beach lineament is named in the first paragraph of Section 1.1.2. Additional text added. Wabash Valley seismic zone: Text and figures describing the Wabash Valley seismic zone have been revised to provide additional detail. ALM: Text revised for clarity, including brief summary of how the ALM features do not meet the criteria for earthquake-induced liquefaction features. Charleston seismic zone: Text modified to provide cross-reference to Subsection 6.1.2 of the main report, which presents detailed discussions and figures for Charleston earthquake chronology.
S E-6. *Additional Guidance* It would be appropriate to include a bullet point on considerations of completeness in Section 3 on guidance for the use of paleoliquefaction data in SSC.	Text revised as suggested to include bullet about completeness in Section 3.
Minor Editorial Comments and Typographical Errors • TOC: The page number for 1.2.3 St. Louis Region is on the next line • p. 1: There is an EPRI logo embedded on the middle of page	These and other minor editorial issues have been corrected. - Deleted EPRI logo - p. 1, sentence 3 edited.

• Several of the page numbers have “Cited” included before the number • p. 1: Consider changing sentence 3 as follows, “Under this task, a new paleoliquefaction database, including regional datasets, was created and this report was prepared, documentation and illustrating the databases, discussing . . .” • p. 6 and other similar sections: Some sections make reference to “Beta Analytic” and others to “Beta Analytic Beta Analytic” • p. 7, first paragraph: “…that may be capable of large earthquakes (e.g., Eastern Margin and Commerce faults), and migration of seismicity from one part of the Reelfoot Rift…” • p. 5, Sand dikes, last bullet: Typo (“as well we as soft-sediment deformation”) • p. 19, second paragraph: “For the results of a paleoliquefaction study to be most useful in accessing assessing the long-term seismic hazards…” • p. 34, par. 1, line 3: Typo (change “earthquakes parameters” to “earthquake parameters”) • p. 34, last paragraph: The text states that radiocarbon and OSL dating “provide age estimates with uncertainties of one hundred years in the best of circumstances. Dating techniques that provide more precise results would help to improve age estimates of liquefaction features and their causative earthquakes.” In section 2.1.3.2 (p. 24, par. 2), examples are given of reported “precision” of ± 80 radiocarbon years, ± 20 radiocarbon years, and ± 40 radiocarbon years. • The figures start on page E2. Presumably the page numbers will be changed for the final report.	- Deleted “Cited” from footer. - Beta Analytic changed to Beta Analytic Radiocarbon Laboratory. - p. 7. Added references regarding the Commerce and Eastern Reelfoot Rift margin faults being earthquake sources. - p. 5. Changed to “as well as” - p. 19. Corrected typographical error. - p. 34, par.1. “s deleted from “earthquakes” - p. 34, last par. Explained the difference between sample dates and age estimates of liquefaction features and earthquakes responsible for their formation and referenced Figure E-3.
APPENDIX F — WORKSHOP SUMMARIES	
General Comments The summaries of the workshop provided in Appendix F are well-written accounts of the presentations and subsequent discussions that transpired. The workshop summaries, coupled with the agendas, participant lists, and presentations, provide sufficient documentation regarding the content of the workshops.	None required
S F-1. *Added Information for Each Workshop* Information has been described as “what people need and want to know.” Inclusion of the agenda for each workshop would give the reader a useful “road map” for navigating through the dense narratives. Also, the list of attendees for each workshop should be included for complete documentation (Table 2-2, p. 2-47, provides a partial list). As an additional step to help those wishing to review the project in the future, we assume that copies of visual presentations made at the workshops will be included as part of the project report and will become available either as part of this Appendix or on a project Website or in some other conveniently accessible form.	No change. The workshop agendas and lists of participants, as well as copies of all presentations, will be provided on the Project website.
APPENDIX G — BIOGRAPHIES OF PROJECT TEAM	
General Comments This appendix is a straightforward compilation of biographical sketches for members of the CEUS SSC Project. As part of this review, individual members of the PPRP were	None required

asked to carefully examine their own biosketches.	
Specific Comments for Clarity and Completeness **S G-1.** *Correlation and Coordination of Appendix G with Figure 2.3-1* For stand-alone reading of Appendix G, it would be useful to give the reader an overview of the Project Team by either pointing the reader to the CEUS SSC Project Organization diagram (Figure 2.3-1), say by using a footnote on p. G-1, or by reproducing the diagram in this appendix. The inclusion of biographies for the Sponsor Reviewers in Appendix G, as part of the Project Team, implies that their names should also be included in the Project Organization diagram. The presentation of names in Appendix G is a mix of alphabetical and hierarchical ordering. If Figure 2.3-1 is to be a guide for the reader, consider ordering names in Appendix G as they appear in the various boxes on the figure. In both the Project Organization diagram and in Appendix G, the TI Team (and support staff) is arguably a more important component of the "Project Team" than the PPRP. Consider moving the PPRP box on Figure 2.3-1 to the right of the TI Team and, correspondingly, presenting the PPRP names last in Appendix G. A box for the Sponsor Reviewers could be added in the organizational chart to the right of the PPRP (and their biosketches could follow those for the PPRP as in the draft).	New introduction added that points the reader to the CEUS Reference made to the SSC Project Organization diagram (Fig. 2.3-1) and discussion in Sec. 2.3. Sponsor Reviewer box added to Fig. 2.3-1. Fig 2.3-1 names are in hierarchical order for Project management team. For all other boxes, order of names is alphabetical, except for TI and PPRP groups where the lead or chairman is first in box, followed by members of team in alphabetical order. Boxes for TI Team and PPRP rearranged. Appendix G bios reorganized consistent with order of names shown in Fig 2.3-1.
Typographical Errors • p. G-7: Ending period missing in last line at the end of Mark Petersen's biosketch.	Change made
APPENDIX H — EPRI/DOE/NRC CEUS SEISMIC SOURCE CHARACTERIZATION	
PROJECT: Draft Final Seismic Source Model Hazard Input Document (HID), Dated July 6, 2010 **General Comments** **G H-1. (NAR)** 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.	No revisions required.
G H-2. (CC) The elements of the CEUS SSC model are clearly described in enough detail to support future users' implementation of the model for PSHA at any site in the CEUS. Gaps not described in the July 6, 2010 draft should be described in the final revision of the appendix.	Full model including seismicity inputs for distributed sources included
G H-3. (CC) The PPRP's review of the 11 chapters of the main report identified many opportunities to achieve greater clarity in the TI Team's descriptions of the characterizations and assessments represented in the CEUS SSC model by proper and consistent use of terms. These comments apply as well to the descriptions contained in Appendix H.	HID review for consistency with Chapters 4, 6, and 7
Specific Comments **S H-1. (CC)** *Title of Appendix H* Consider changing the appendix title to: "CEUS SSC MODEL HAZARD INPUT DOCUMENT (HID)."	Change made
S H-2. (CC) *Implementing the Variable a- and b-value Routines*	Calculation of the variable a and b values is not part the use of the CEUS

To perform any hazard calculations using the HID, it would be difficult for most users to implement the variable *a*- and *b*-value routines described in Chapter 5. Therefore, the process is not open for most users to evaluate that methodology. It would be desirable that the computer codes 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.	SSC model for application in PSHAs, the purpose of the HID, but rather is an issue for future updates to the model.
S H-3. (CC) *Transparency of HID Tables for Recurrence* The following excerpt is reproduced from PPRP Review Comment **S 6-**12: "The unalert reader (or analyst) examining the HID tables for computed annual frequencies for the Charleston RLMEs may potentially be confused by: (1) the inverted order for the 5-point distributions compared to Table 5.3.3.-1, which was used to define the 5-point distribution; and (2) the need to refer to Tables 6.1.2-1 and 6.1.2-2 to discern the elapsed time since the oldest earthquake counted in the sequence. For example, examining "Table Charleston_HID-3," it may escape the reader's attention that the 5-point distribution is not for four events in 5500 years, but rather four events in 1,524–1,867 years (or possibly in 1,569–1,867 years). To reproduce the results in the table (and for virtually all the Poisson-model tables in the HID), there is no explicit information about the exact elapsed time that was used."	Documentation of calculation process was expanded in Chapter 6, HID revised to be consistent with main chapters and to add pointers to specific sections of the main report
Comments for Clarity and Completeness Figures 8 and 9 appear to be identical figures with different figure captions. p. H-19, *Degree of Smoothing*: The text 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.	References to main chapters added to HID as appropriate
APPENDIX I — PPRP AND USGS REVIEW COMMENTS	
Specific Comments for Clarity and Completeness **S I-1.** *Title of Appendix E* Because this appendix contains both PPRP and USGS review comments, the title of the appendix should be changed.	Title changed
Specific Comments for Clarity and Completeness **S I-2.** *Listing of Letters and Attachments* In the summary of contents for the appendix, the separate listing of Attachments to PPRP Letter 1a as Items 1b and 1c poses a problem of consistency. PPRP Letter 2 (dated August 15, 2008) contains a substantive Attachment A ("Key Issues for CEUS SSC Relevant to Workshop #1) with three labeled enclosures. Also, USGS Letter 1 (dated April 8, 2010) contains five attachments. In the case of these three letters with attachments, one can either spell everything out or simply note that these letters have attachments (perhaps indicating their general nature).	Descriptions of attachments added for PPRP Letters 1 and 2 and USGS Letter 1
S I-3. *Incorrect Date in Correspondence Contents* p. I-2, TI Team Letter 1: Error in labeling the subject of the letter (change "dated August 12, 2008" to "dated August 15, 2008")	Errors corrected (including a typo in the letter heading – 2010 date corrected to 2008)
APPENDIX J — MAGNITUDE RECURRENCE MAPS	
General Comments	No revision necessary.

Appendix J presents the recurrence maps developed for all of the alternative configurations of the distributed seismicity zones. A brief description of the organization of the maps within the Appendix is provided on the title page. Consistent with the care taken in the writing of Section 5.3.2 (*Smoothing Approach*), this appendix is well organized and explained—beginning with the text on the title page that provides helpful guidance to the reader.	
Comments for Clarity and Completeness • Page J-1: Consider adding additional reference to specific figures in Sections 6.4 and 7.5; suggested wording: "Mean maps and magnitude-recurrence for each source zone are shown in Sections 6.4 (Figures 6.4-1 through 6.4-16) and 7.5 (Figures 7.5.2-1 through 7.5.2-42)." • Check: Were rates indeed calculated for **M** > 5 or for **M** ≥ 5? If perchance they were calculated for the latter, then labels on the figures should be changed or an explanation can be added on the title page of the appendix. • In figure caption for Figure J-1, need closing ["] for "no separation . . ." OR simply delete the ["], which doesn't appear in the captions for the following figures. • On Figures J-17 through J-48, the header information incorrectly indicates "MES" vs. "MESE" (the correct acronym, according to the list of Acronyms) written in the figure captions. • On Figures J-49 through J-112, the acronym "RCG" is used for Rough Creek graben vs. "RC" in the list of Acronyms. • Page J-87: Realization 7 for the seismotectonic zone, wide interpretation, Rough Creek Graben in Mid-Continent, full magnitude weights is missing.	Revisions made, as appropriate.
APPENDIX K — SCR DATABASES USED TO DEVELOP MMAX PRIOR DISTRIBUTIONS	
General Comments This appendix provides the database used to develop the Mmax prior distributions. The work done to update and refine data for the global Stable Continental Regions has great value and importance. However, there is no explanatory text provided beyond the Notes and the two tables. To help future users, as well as to enhance transparency, this Appendix could be improved by including additional information and a short description of the content being included in the appendix itself, or to a reference back to the relevant report text. It could also be noted whether or not the database is available in digital form elsewhere.	Short description of process added. Data will be included on project web site.
Specific Comments for Clarity and Completeness **K-1.** *Information that should be considered for Appendix K* Appendix K would benefit from including additional information for the reader to better appreciate where the domains and super domains are, and to better integrate with the text. The TI Team should considering adding the following: • Maps showing domains and superdomains (useful files for the boundaries of these domains should also be included in the Project Database, with a pointer to those files) • Figures displaying the Mmax-obs statistics for each of the superdomains • Summary table of statistical analysis completed on the various superdomain classifications	The digital data will be on project web site along with scanned images of the large plates from Johnston et al. (1994)
K-2. *Clarity of Documentation* For clarity of documentation, attention should be paid to the following: 8. Designation of time in an earthquake catalog should be explicit. Are the times/dates in	Greater explanation added for entries to the tables.

PPRP Comment Response Table

UTC? Local time? A mix? This is non-trivial if one tries to find the events in another catalog. 9. How should the reader interpret the variable presentation of significant figures in Table K-1 for latitude, longitude, **M**, and sig**M**? 10. “Extensive stress” is an unorthodox descriptor for “extensional stress”. (Google the two terms to see how most readers would interpret the first term.) 11. What are the units of “Area” in Table K-2? 12. Neither “Mx_obs” or “N > 4.5” is explicitly explained in Table K-2. 13. Check: Is N > 4.5 indeed the number of earthquakes greater than **M** 4.5? Or perchance is it **M** ≥ 4.5? 14. For the table to be self-contained, an explanation should be given for non-integer values of N > 4.5. 15. The wording used to explain SDNT and SDNC in Table K-2 will trip up most readers. Just add a few words to make it plain English. The acronyms certainly aren’t intuitive, but given that they are what they are, suggestion: SDNT Indicates which Superdomain the domain is assigned to when TYPE is included in the classification SDNC Indicates which Superdomain the domain is assigned to when TYPE is not included in the classification	