RAS 6848

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September 16, 2003

Rules and Adjudications Branch
Office of the Secretary
U.S. Nuclear Regulatory Commission
Washington, D.C. 20555

SUBJECT: *Filing in MOX CAR Proceeding, No. 70-3098*

Dear Madam/Sir,

Enclosed for filing please find the original and two copies of Georgians Against Nuclear Energy's Opposition to DCS's Motion for Summary Disposition of Contention 3. Please note that the signature page of the supporting Declaration of Dr. Leland Timothy Long has an electronic copy of Dr. Long's signature, which he sent by e-mail. I did not receive his original signed declaration in time for this filing, but will forward it when I receive it.

In addition, please note that the hard copy of Dr. Long's declaration contains a correction to the copy that was e-mailed to you yesterday. In the last line of paragraph 71 of Dr. Long's declaration, "Dr. Long" should be changed to "I."

Sincerely,

Diane Curran

Cc: Service list

Template = SECY-041

SECY-02

September 16, 2003

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

ATOMIC SAFETY AND LICENSING BOARD

Before Administrative Judges:
Thomas S. Moore, Chairman
Charles N. Kelber
Peter S. Lam

In the Matter of	)	
DUKE COGEMA STONE & WEBSTER	)	Docket No. 0-70-03098-ML
(Savannah River Mixed Oxide Fuel Fabrication Facility)	)	ASLBP No. 01-790-01-ML

GEORGIANS AGAINST NUCLEAR ENERGY'S OPPOSITION TO DUKE COGEMA STONE & WEBSTER'S MOTION FOR SUMMARY DISPOSITION OF GANE CONTENTION 3

I. INTRODUCTION AND SUMMARY

Pursuant to 10 C.F.R. § 2.749, Georgians Against Nuclear Energy ("GANE") hereby responds to Duke Cogema Stone & Webster's Motion for Summary Disposition on Consolidated Contention 3 (August 22, 2003) (hereinafter "DCS Motion"). The motion should be denied because, as demonstrated in the attached Statement of Genuinely Disputed Material Facts (Attachment A) (hereinafter "GANE Statement of Disputed Facts") and the Declaration of Leland Timothy Long Regarding GANE Contention 3 (September 16, 2003) (hereinafter "Long Declaration") (Attachment B), it fails to demonstrate the lack of genuinely disputed and material factual issues.

The outstanding question raised by Contention 3, and of which this summary disposition motion attempts to dispose, is whether the probabilistic safety hazard analysis ("PSHA") prepared by DCS contains an adequate analysis of the likelihood of a severe earthquakes at the site of the proposed MOX Facility. The question is important, because the proposed MOX Facility would lie within 120 kilometers (km) of the 1886 Charleston earthquake, a major seismic event. The PSHA examines the likelihood of a repeat earthquake and its effect on the site of the proposed MOX Facility.

GANE does not agree with DCS that its PSHA is robust or conservative. To the contrary, Dr. Long has found that the PSHA is a very poor piece of scientific work, because it unquestioningly relies on outdated studies that were prepared more than twenty years ago by the Lawrence Livermore National Laboratories ("LLNL") and the Electric Power Research Institute ("EPRI"). DCS made no attempt to update these studies with the considerable amount of new information that is available regarding the seismicity of the South Carolina Coastal Plain, the seismic area that is relevant to the design of the proposed MOX Facility. DCS also misused the LLNL and EPRI studies, by applying their general conclusions without refining them through the application of site-specific information.

Moreover, the method used by DCS to check the accuracy of the PSHA, i.e., propagation of an earthquake from Charleston to the Savannah River Site ("SRS"), does not provide a valid test because it fails to take into account the attenuation function of the "Moho bounce," i.e. the reflection of earthquake waves off the earth's core. As a result,

DCS may have significantly underestimated the likelihood of a major seismic event at the proposed MOX Facility.

DCS claims that it did, in fact, take into consideration a spectrum of opinions about the seismic characteristics of the study area, including views that have been developed in the last twenty years. The mere "consideration" of new information is insufficient to demonstrate that it was actually given a quantitative value and taken into account in DCS's mathematical analysis. The relevant question is not whether an opinion was considered, but what mathematical weight it was given.

DCS tries to compensate for its significant errors by claiming that its analysis is conservative. But DCS misapplies the concept of conservatism. As Dr. Long explains in his declaration, a quantitative seismic analysis like the PSHA must be reasonably accurate and rigorous in the first place. It is not possible to add a conservatism to a fundamentally defective analysis, because there is no way to determine how big the conservatism should be, other than to perform the calculation correctly in the first place. The design of a major facility deserves a through analysis and update of pertinent data, not the injection of errors of unknown magnitude that arbitrarily increase the hazard in hopes that they are greater than the effects of the errors and omissions.

II. FACTUAL BACKGROUND

A. Procedural Background

As submitted by GANE in August of 2000 and admitted by the Atomic Safety and Licensing Board ("ASLB") in the fall of 2001, Contention 3 asserts that DCS has not performed a seismic analysis that is either adequate in scope or adequately documented.

LBP-01-35, 54 NRC 403, 444 (2001). In the basis of the contention, GANE raised a number of criticisms of the seismic analysis in DCS's license regarding the lack of documentation in the license application.

During discovery, GANE responded to interrogatories by DCS regarding its position on Contention 3. DCS also spent two full days deposing Dr. Long. During the discovery process, GANE narrowed the scope of the contention by dropping some of issues that DCS had addressed in revisions to its application for construction approval. As a result, the contention now focuses on the adequacy of DCS's analysis of seismicity in its PSHA. At the hearing on Contention 3, GANE plans to offer testimony on Contention 3 by Dr. Long, Professor of Geophysics at Georgia Institute of Technology, who is a leading expert on the seismology of the southeastern United States.

B. DCS's Seismic Analysis

The seismic design of the MOX Facility relies upon probabilistic safety hazards analyses ("PSHAs") that were conducted in the late 1970s and early 1980s by the LLNL and EPRI. The LLNL study summarized expert opinion on seismic zones. The EPRI study folded in opinions on geologic structures that cause earthquakes. These opinions were weighted and reported quantitatively in the report results. Long Declaration, par. 6. DCS assigned a PSHA equivalent to the average of the LLNL and EPRI PSHA at the SRS, and fixed the PSHA peak acceleration at 0.2 g. *Id.*, par 7.

III. ARGUMENT

A. DCS Has Not Met the Standard for Summary Disposition.

Pursuant to NRC regulations at 10 C.F.R. § 2.740, a party is entitled to summary disposition if "there is no genuine issue as to any material fact" and the party "is entitled to a decision as a matter of law." The burden of proving entitlement to summary disposition is on the movant. *Advanced Medical Systems, Inc.* (One Factory Row, Geneva, Ohio 44041), CLI-93-22, 38 NRC 98, 102 (1993). Because the burden of proof is on the proponent, "the evidence submitted must be construed in favor of the party in opposition thereto, who receives the benefit of any favorable inference that can be drawn." *Sequoyah Fuels Corp. and General Atomics Corp.* (Gore, Oklahoma Site Decontamination and Decommissioning Funding), LBP-94-17, 39 NRC 359, 361, *aff'd,* CLI-94-11, 40 NRC 55 (1994). If there is any possibility that a litigable issue of fact exists or any doubt as to whether the parties should be permitted or required to proceed further, the motion must be denied. *General Electric Co.* (GE Morris Operation Spent Fuel Storage Facility), LBP-82-14, 15 NRC 530, 532 (1982).

B. Regulatory Requirements for MOX Facility Seismic Analysis

DCS's seismic analysis must meet the requirements of 10 C.F.R. §§ 70.23(b) and 70.64(a)(2). Section 70.23(b) requires that the design bases of the principal structures, systems, and components of the proposed MOX Facility must provide "reasonable assurance of protection against natural phenomena." In addition, § 70.64(a)(2) provides that the facility design "must provide for adequate protection against natural phenomena with consideration of the most severe documented historical events for the site."

The NRC Staff provides regulatory guidance for compliance with these standards in NUREG-1718, the MOX Facility Standard Review Plan. NUREG-1718 recommends a "risk-informed" approach to evaluation of natural hazards, and recommends consultation of various other NRC guidance documents, including Regulatory Guide 1.165, Identification and Characterization of Seismic Sources and Determination of Safe Shutdown earthquake Ground Motion (1997).

C. DCS's Expert Witness Lacks Sufficient Expertise To Make A Credible Defense of the PSHA.

DCS relies for its Motion on the Affidavit of Dr. Carl Stepp (August 6, 2003) (hereinafter "Stepp Affidavit"). Dr. Stepp, a former employee of EPRI, managed the EPRI study on which DCS relies. According to DCS, Dr. Stepp is "well respected in the scientific community." DCS Motion at 2, note 2. DCS also quotes Dr. Long to the effect that Dr. Long would consider Dr. Stepp's opinion "very highly." *Id.*

Dr. Stepp overstates his qualifications. Long Declaration, par. 11. While Dr. Stepp did substantial computational work some time ago, for the past twenty years his position has been essentially managerial. *Id.*, par. 12. Dr. Stepp's expertise in the technical details of computation probably stopped soon after the early 1970s. *Id.* His affidavit reflects his limited understanding, because it oversimplifies the concept of PSHA, and completely ignores the dynamic nature of the field. *Id.*,pars. 13 and 14. Moreover, at various points in his affidavit, Dr. Stepp makes illogical or incorrect statements that indicate he does not understand the subject matter to which he is testifying. *See, e.g., id.*, pars. 34, 35, 41, 42.

DCS's Statement of Material Facts on Which No Genuine Issue Exists (hereinafter "DCS Statement of Disputed Facts") is also filled with errors, poorly understood scientific concepts and non-sequiturs. Long Declaration, par. 55. This may be a reflection of Dr. Stepp's lack of current expertise. Accordingly, while Dr. Stepp should be admired for his substantial managerial skills, his computational skills in the field of seismology are limited. His limited expertise in current computational issues related to PSHA should be taken into account in considering DCS's summary disposition.

D. DCS Has Failed To Show The Lack Of A Genuine And Material Dispute Of Fact Regarding the Adequacy of Its Seismic Analysis.

In its Motion, its Statement of Undisputed Facts, and the Stepp Affidavit, DCS attacks each element of Dr. Long's criticism of the PSHA, and attempts to demonstrate that it either has been resolved or has no merit. DCS's effort is unsuccessful. GANE's criticisms of DCS's PSHA are supported by scientific logic and the extensive experience of Dr. Long, as well as by professional papers.

Time and space constraints do not permit GANE to rehearse each of DCS's technical arguments in this response. Below, GANE addresses the major topics addressed in DCS's Motion. GANE's Statement of Material Facts in Dispute, as supported by Dr. Long's Declaration, are the principal documents offered by GANE to demonstrate that GANE has a genuine and material factual dispute with every relevant assertion made by DCS in its Statement of Undisputed Material Facts.

1. DCS's seismic analysis is significantly flawed.

As demonstrated in Dr. Long's declaration, DCS's seismic analysis is significantly flawed. Most importantly, DCS failed to take into account new information

that has been gathered since the LLNL and EPRI studies were published over twenty years ago. These data include many magnitude 3 to 5 earthquakes, the detailed studies of their aftershocks, a better understanding of crustal structures in areas like New Madrid and southeastern Tennessee, and new statistical approaches to understanding seismicity. Long Declaration, par. 9(a)(ii). Furthermore, research has shown that the location of a Charleston type event could be more tightly distributed than was generally thought 20 years ago. Still other studies have shown a sensitivity of local attenuation functions to crustal structure. Long Declaration, par. 9(a)(ii).

All of these factors could affect the computed PSHA. Therefore, they should be evaluated and mathematically compared against the factors that were used in the PSHA. If they are different in any way to suggest a more severe hazard, the PSHA should be reevaluated. *Id,* par. 9(a)(iii). .

In this case, DCS neither computed a locally relevant attenuation function for comparison with those used in the EPRI and LLNL studies, nor has it critically compared the effects of recent data that suggest a tighter distribution of a Charleston type earthquake with the definitions of seismicity used by the experts in the EPRI and LLNL studies. Hence, DCS does not know how its PSHA compares with a true best estimate based on contemporary thinking. *Id.* Moreover, the analysis used by DCS to check the validity of the PSHA is fundamentally faulty, because it fails to take into account the phenomenon of the "Moho bounce." *Id.,* par. 9(c).

2. GANE has a genuine and material dispute with DCS regarding the appropriateness of its unqualified reliance on the LLNL and EPRI studies.

The parties have a major dispute regarding the proper use of the LLNL and EPRI studies in the PSHA for the proposed MOX Facility, and the significance of the fact that those studies were prepared by expert panels whose members expressed a variety of opinions. DCS argues that its PSHA (which incorporates the LLNL and EPRI study methods and results) appropriately "incorporates alternative inputs prepared by multiple experts." DCS Motion at 8.

Dr. Long has no quarrel with the methods used to prepare the LLNL and EPRI studies, i.e., the consideration of multiple expert opinions. Long Declaration, par. 15. As stressed by Dr. Long in his declaration, however, the preparation of the LLNL and EPRI studies involved quantification of those opinions. DCS has made no attempt to evaluate how those quantifications might change in light of new and site-specific information. Long Declaration, par. 9. Nor did DCS perform any analysis to validate the LLNL or EPRI computations. *Id.*, par. 7.

DCS also argues that the LLNL and EPRI studies have been "accepted by the NRC as suitable for developing a site-specific PSHA." DCS Motion at 9, 26-28. This statement is correct only the short distance that it goes. It may be correct that the LLNL and EPRI studies are suitable for "developing" a site-specific PSHA, but it is not true that DCS may place unqualified reliance on them, without conducting any further inquiry. Regulatory Guide 1.165 anticipates that license applicants will update their use of the LLNL and EPRI studies with current site-specific information:

> For sites in the CEUS [central and eastern United States], when the EPRI or LLNL PSHA methodologies and data bases are used to determine the SSE, it still may be necessary to investigate and characterize potential seismic sources that were previously unknown or uncharacterized and to perform sensitivity analyses to assess their significance to the seismic hazard estimate.

Id. Appendix E at 1.165-38 (1997). Regulatory Guide 1.165 further explains that:

> It is necessary to evaluate the geological, seismological, and geophysical data obtained from the site-specific investigations to demonstrate that these data are consistent with the PSHA data bases of these two methodologies [i.e., LLNL and EPRI]. If new information identified by the site-specific investigations would result in a significant increase in the hazard estimate for a site, and this new information is validated by a strong technical basis, the PSHA may have to be modified to incorporate the new technical information.

Id. Moreover, the NRC anticipated that the LLNL and EPRI studies would be revised every 10 years, which hasn't happened. Under the circumstances, it is even more important to update a seismic analysis with current and site-specific information. Long Declaration, par. 31. Thus, Regulatory Guide 1.165 does not permit DCS to rely unquestioningly on the twenty-year-old results of the LLNL and EPRI studies, as DCS has done.

Moreover, even assuming for purposes of argument that the Regulatory Guide allows unquestioning reliance on the LLNL and EPRI studies, a regulatory guide does not have the force of a regulation, and therefore GANE is not precluded from demonstrating that a "prescribed method is inadequate in the particular circumstances of the case." *Louisiana Energy Services* (Claiborne Enrichment Center), LBP-91-41, 34 NRC 332, 354 (1991). Dr. Long has demonstrated that unqualified application of the LLNL and EPRI results was not the intention of the authors at the time the study was prepared. Long Declaration, pars. 9(a)(iv), 32. Moreover, to unquestioningly apply the LLNL and EPRI

studies to the proposed MOX Facility would not be sound scientific practice. *Id.*, pars. 9(a) and (e). Therefore, even if the ASLB finds that DCS's conduct of the PSHA satisfies the guidance of Regulatory Guide 1.165, it should find that guidance to be inadequate to ensure the conduct of a reasonably accurate PSHA.

3. DCS's claims to have taken new information and site-specific information into account are misleading and unsupported.

DCS makes a number of misleading claims to the effect that it did take new and site-specific information into account in the course of preparing its PSHA. First, DCS argues that it took into account "local properties." DCS Motion at 10-11. These "local properties," such as soil column thickness, soil and bedrock shear-wave velocitiy, and soil dynamic properties, relate to the generation of surface level hazard from bedrock level hazard. They are not directly relevant to the evaluation of the seismological hazard. Long Declaration, par. 18.

Second, DCS argues that a number of factors were taken into account because they were, somehow, "considered" by DCS in the course of the PSHA. For instance, DCS argues that it considered new information regarding the potential that a magnitude 7+ earthquake could occur virtually anywhere in South Carolina, as suggested by Kafka (DCS Motion at 30); the possibility of a 7.5 earthquake in southeastern Tennessee (DCS Motion at 33-34); recent paleoseismological work on the South Carolina Coastal Plain ("SCCP") showing more seismic activity in the last 6,000 years, and over a wider area, than previously known (DCS Motion at 35); new information showing a shorter recurrence interval of the Charleston Earthquake (DCS Motion at 39-40); and new information regarding ground motion attenuation (DCS Motion at 44-45). DCS claims

that all of this information was considered in the PSHA and/or the LLNL and EPRI studies. Nowhere in DCS's Motion, or in the affidavit of DCS's expert witness, however, does DCS provide an explanation of how the information was taken into account *quantitatively. See, e.g.,* Long Declaration, pars. 9(a)(iii), 41. As Dr. Long explains, twenty years ago, the experts may have acknowledged these factors, but placed little emphasis on them. Today, if the LLNL and EPRI studies were to be conducted over again, expert opinion would be likely to shift, resulting in greater quantitative emphasis on these factor. *Id.*, par. 43.

4. DCS's claim to have cured its errors through the application of conservatisms is without merit.

DCS also argues that any errors it has made in the PSHA are inconsequential, because they are encompassed by alleged conservatisms introduced into the PSHA. DCS applies this logic to a number of issues, including the validity of DCS's check on the PSHA (DCS Motion at 23), and DCS's approach to ground motion estimates (Stepp Affidavit, par. 37). As Dr. Long explains, it is not possible to make a fundamentally deficient analysis like DCS's conservative, because the failure to correct the errors in the analysis makes the analysis nonconservative under any circumstances. Long Declaration, par. 9(e). *See also id.* par. 29. In addition, DCS incorrectly asserts, in its Statement of Undisputed Facts, that a 7.3 moment magnitude earthquake with an epicenter located 120 km southeast of the MOX Facility site is "appropriate or conservative for modeling the historic 1886 Charleston earthquake ground motions." As discussed in paragraph 56, it is not necessarily conservative to put the MOX Facility 120 km from the Charleston

earthquake, because effects like the Moho bounce increase shaking with increased distance.

IV. CONCLUSION

For the foregoing reasons, DCS's motion for summary disposition of Contention 3 should be denied.

Respectfully submitted,

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September 16, 2003

Attachment A

GANE'S STATEMENT OF GENUINELY DISPUTED MATERIAL FACTS

1. In Statement 3, DCS incorrectly claims that Dr. Long agrees with the assertion that a 7.3 moment magnitude earthquake with an epicenter located 120 km southeast of the MOX Facility site is "appropriate or conservative for modeling the historic 1886 Charleston earthquake ground motions." Dr. Long has not expressed agreement with that statement. *See* GANE's June 28, 2002, response to DCS Interrogatory 3.32; Long deposition transcript at 129:10-15. He agrees only that the Charleston earthquake is the most severe documented historical event that is relevant to the seismic design for the MOX Facility. It is not necessarily conservative to put the MOX Facility 120 km from the Charleston earthquake, because effects like the Moho bounce increase shaking with increased distance. Long Declaration, par. 56.

2. Dr. Long has made an educated guess that the use of the Hermann Crustal Model may produce erroneous ground motions at the MOX Facility from the 1886 Charleston earthquake with an error rate of as much as 50%. In Statement 9, DCS asserts that a 50% error is encompassed by the MOX Spectrum. In order to provide a reasonably reliable PSHA, DCS needs to do the work of re-calculating the seismic hazard, using current and site-specific information. It is not appropriate to guess that DCS has erred by about 50% and then add a so-called conservatism to correct it. DCS must develop a basis for a reasonable level of confidence in the underlying calculation. Long Declaration, par. 57.

3. In Statement 10, DCS asserts that Dr. Long has not identified an alternative model to the Hermann Crustal Model. This is incorrect. Long Declaration, par. 58.

4. Contrary to DCS Statement 11, the EPRI and LLNL studies are not appropriate for site-specific use, because the expert opinion contributions to these analyses are seriously out of date and because regionally responsive attenuation relations were not used in these studies. In addition, the PSHA can be affected, particularly at higher frequencies, by the details of local seismicity. Long Declaration, par. 9.

5. In Statement 12, DCS asserts that NRC Regulatory guide 1.165 explicitly permits the use of the EPRI and LLNL PSHA studies. To some degree, this is a legal question rather than a factual question. Nevertheless, on a factual level, DCS's assertion is contradicted by the following facts:

(a) The authors of the Regulatory Guide contemplated that the LLNL and EPRI studies would be updated every ten years. This has not been done, thus making site-specific updates all the more important.

(b) During the past 20 years, there have been significant developments in the collection of data and refinement of theories about the seismicity of the southeastern United States, which warrant updating of the LLNL and EPRI results in this case.

(c) Regardless of what the Regulatory Guide allows or prescribes, it would be professionally irresponsible to rely on a twenty-year old study of seismicity in the South Carolina coastal region, without examining whether it should be updated to account for new information and data regarding local seismicity. Long Declaration, pars. 30, 59.

6. In Statement 14, DCS asserts that the EPRI and LLNL studies included opinions that major earthquakes could occur practically anywhere along the eastern United States. The question is not whether these opinions were included, but what weight they were given. In fact, these opinions were given little weight. Long Declaration, par. 37, 41.

7. In Statement 15, DCS states that Kafka purposefully ignored all known geologic/liquefaction data . This is correct, but irrelevant to the applicability of Kafka's analysis. Long Declaration, par. 36.

8. Similarly, in Statement 16, DCS asserts that the data used by Kafka did not include any earthquakes before 1924, so it necessarily excluded the 1886 Charleston earthquake and all the paleoearthquakes associated with the Charleston Seismic Zone. Again, this observation is irrelevant. Long Declaration, par. 34.

9. Statement 17's observation that Kafka's data set for the Southeastern United States appears to include no earthquakes grater than magnitude 4.8, and only three between magnitude 4.3 and 4.8, is trivial. Dr. Stepp should have advised DCS concerning the natural distribution of earthquakes in a complete catalog. Long Declaration, par. 34.

10. In Statement 18, DCS asserts that "Kafka's theory is not generally accepted in the scientific community." Kafka paper presents a statistically quantifiably observation, not a theory. He has quantified a distribution of epicenters that has been generally known and accepted for over 60 years. Long Declaration, par. 61.

11. In Statement 19, DCS attempts to undermine the value of Kafka's work by noting that Kafka himself described his work as "still exploratory." Obviously, there is always more to be learned in the area of seismology. This does not undercut the value of the data that Kafka has collected. Long Declaration, par. 62.

12. In Statement 20, DCS asserts that I believe "Kafka's theory is a 'pioneer paper.'" Dr. Kafka's paper does not propound a theory, it reports data. Moreover, the fact that a paper is a pioneer work does not mean it is less credible or reliable than others. Long Declaration, par. 63.

13. In Statement 21, DCS argues that Kafka's "position" is contradicted by other articles that I cite, namely Tawani & Schaeffer and Hu *et al.* First, Kafka's paper does not represent a "position" or a "theory," but his observations. Second, the other studies

referenced by DCS suggest a continuing history of seismicity at Charleston and the possibility that major (M>5.5) events occurred in two other locations. That data is entirely consistent with Kafka's observations. Long Declaration, par. 64.

14. In Statement 22, DCS asserts that the EPRI and LLNL PSHA studies did consider the possibility of a 7.5 earthquake in southeastern Tennessee. The fact that a 7.5 earthquake in southeastern Tennessee was considered is neither here nor there. What is relevant is the magnitude of the earthquake's contribution to the PSHA. Long Declaration, par. 37.

15. In Statement 23, DCS asserts that "Southeast Tennessee is an area of frequent earthquakes, but these earthquakes have had a magnitude of no grater than about 5." This statement is incorrect. It should read that "in recent history, the largest earthquakes have been about magnitude 5." There is nothing in the data from southeastern Tennessee seismic zone to preclude larger events. Moreover, there are many similarities between the South Carolina Coastal Plain and to the New Madrid seismic zone which suggest the possibility of a larger event. Long Declaration, par. 38.

16. In Statement 24, DCS asserts that it is the "generally accepted view in the scientific community" that "the geophysical structure underlying the Eastern Tennessee Seismic Zone is very unlikely to support magnitude 7+ earthquakes. While this may have been the conventional wisdom 20 years ago, it is no longer the case. Every seismologist known to Dr. Long who has studied Southeastern Tennessee has expressed a concern over a possible major event in the Eastern Tennessee Seismic Zone. In this context, one should remember that 20 years ago, the number of seismologists worried about the Seattle area was limited. Long Declaration, par. 39.

17. Statement 25 reflects a misinterpretation of Talwani & Schaffer's first sentence. While Talwani and Schaeffer obtained no new data, they did new work on the old data, and came to conclusions that were significant enough to warrant a new publication. Long Declaration, par. 40.

18. In Statement 28, DCS asserts that the two scenarios raised by Talwani and Schaeffer (near Charleston and near Bluffton, Georgetown, and Charleston) were raised a decade earlier and considered in the PSHA. While these observations were *discussed* in the PSHA, they were not factored into the PSHA computation. The question remains as to the magnitude of their quantitative effect on the PSHA. Long Declaration, par. 41.

19. In Statement 30, DCS asserts that GANE has not provided any analysis or data to show that consideration of the earthquake sequences identified in Talwani & Schaeffer would increase the ground motions of the design earthquake for the MOX Facility. The distances to Charleston and Bluffton from the site are in the range affected by the Moho bounce effect, which was not a component of the EPRI or LLNL studies. Inclusion of this effect in the PSHA for these earthquakes, rather than a distribution of ranges (the large

seismic zone defined by some experts) could increase the PSHA. Long Declaration, par. 65.

20. Statement 32 is false. The return interval should be computed from a statistical analysis of the distribution of interval times, not a simplistic average over a total time period which could include missing data. Long Declaration, par. 42.

21. In Statement 34, DCS claims that the return interval proposed by Talwani & Schaeffer is not new information for DCS to consider, because it was referenced in NUREG/CR-5613. This is correct as far as it goes, but the relevant question for purposes of Dr. Long's analysis is whether the PSHA includes that information and in what proportion. Long Declaration, par. 41.

22. In Statement 36, DCS asserts that new information regarding the magnitude of earthquakes causing liquefaction on the South Carolina Coastal Plain does not support a 600 year return interval for magnitude 7 earthquakes, but rather for magnitude earthquakes ranging between 5.3 and 6.8. DCS relies for its position on a Master's thesis that has not gone through the full rigors of the publication review process. Moreover, the author notes the need for more work on the subject. Long Declaration, par. 46, 66.

23. In Statement 37, DCS asserts that GANE has not provided any analysis or data to show that consideration of a shorter return interval for earthquakes along the coast of South Carolina ranging in magnitude between 5.3 and 6.8 would increase the ground motions of the design earthquake for the facility. It is obvious that if a shorter return period is used in a PSHA, the hazard will increase. Similarly, a reduction of the magnitude estimates would decrease the hazard, everything else being constant. However, the Leon thesis on which the revised magnitudes are based has yet to be published and was not complete. Long Declaration, par. 66.

24. In Statement 37, DCS asserts that GANE has not provided any analysis or data to show that consideration of a shorter return interval for earthquakes along the coast of South Carolina ranging in magnitude between 5.3 and 6.8 would increase the ground motions of the design earthquake for the facility. It is obvious that if a shorter return period is used in a PSHA, the hazard will increase. Similarly, a reduction of the magnitude estimates would decrease the hazard, everything else being constant. However, the Leon thesis on which DCS bases its proposed revised magnitudes (Statement 36) has yet to be published and was not complete. Long Declaration, par. 66.

25. In Statement 39, DCS asserts that Hu *et al* is flawed, because the authors did not correct the soil strength data to account for aging. In support of its assertion, DCS cites an unpublished master's thesis. Hu *et al* have not published an erraturm. In any event, the assumptions in Hu et al. are properly stated and the conclusions based on those assumptions should not be affected. Long Declaration, par. 47.

26. DCS claims in Statement 40 that the magnitude of prehistoric earthquakes is decreased to 5.3-6.8 if one corrects for aging. DCS's assertion is based on the Leon thesis, which is not the equivalent of a published academic paper. Long Declaration, par. 47.

27. Contrary to DCS's assertion in Statement 42, Atkinson and Boore is not the only attenuation model on which Dr. Long relies. While it is true that Atkinson and Boore's model more accurately represents the local structure, the attenuation model is also documented in an article by Jones, Long, and McKee. Atkinson and other coworkers have published a number of articles on attenuation, many of which show the effects of reflections off the Moho. Long Declaration, par. 27.

28. In Statement 45, DCS correctly asserts that many earthquakes are located in places where the Moho bounce would not be relevant. The question here, however, is whether the Charleston earthquake is located in a location where the Moho bounce is a significant factor in the seismic analysis. The answer to that question is yes. Long Declaration, par. 9(c).

29. In Statement 46, DCS asserts that GANE has not provided any analysis or data to show that consideration of Atkinson and Boore (1995) or any other model would increase the ground motions of the design earthquake for the MOX Facility. This statement does not make sense. A PSHA and an attenuation model are not directly comparable. Long Declaration, par. 68.

30. In Statement 47, DCS asserts that GANE has not provided any analysis or data to show that consideration of Atkinson and Boore (1995) or any other model would increase the ground motions of the design earthquake for the MOX Facility. Statement 47. This argument is incorrect. If the major contribution to the PSHA is from the Charleston earthquake and from other possible large events at a distance where the reflection from the Moho can cause amplitudes that are anomalously high, then the integrated results will be higher than those where the events are distributed. The Kafka factor, an increased probability of events being in new locations, would increase the resulting PSHA by increasing the probability of events occurring close to the site. These impacts on the PSHA were not evaluated. Instead, DCS has substituted arbitrary errors with bias. This approach constitutes bad science, and does not give confidence that the computed hazard is sufficiently large to overcome errors in data and logic. Long Declaration, par. 69.

31. In Statement 48, DCS states that GANE alleges that the June 2002 U.S. Geological Survey Seismic Hazard Maps show a return period for 0.2g at the MOX Facility site of about 2,500 ears, while DCS fixes the return period for 0.2g PGA at the MOX Facility at 10,000 years. Hence, the hazard reported by the USGS analysis is larger than that assigned to the MOX facility. The statistical relations used would differ only slightly (other than by the factor of 4 difference) between the USGS computation for 2500 years and the EPRI and LLNL computations for 10,000 year return periods. The only possible

difference would be introduced by the truncation of larger magnitude earthquakes by using the characteristic earthquake model. This would result in a slightly lower hazard for the same large time period. Long Declaration, par. 70.

32. In Statement 49, DCS contends that USGS maps were not developed for nuclear facilities, and are not appropriate for facilities where the applicant is concerned about earthquakes with annual probabilities of exceedance of 10 x 10-4 or lower. Similarly, in Statement 50, DCS argues that the USGS hazard maps s were developed in conjunction with seismic design codes for "ordinary" new buildings. Whether or not these claims are true, it would have no bearing on whether the USGS maps are accurate or reliable. The purpose of a USGS hazards map has no effect on its content. Long Declaration, par. 53.

33. In Statements 51-53, DCS argues that the USGS hazard maps are based on "firm-rock" conditions prevailing in the western United States, and which do not exist beneath the MOX Facility. According to DCS, applying USGS firm-rock assumptions to a hard-rock site overestimates the ground motions at the MOX Facility. This statement is not supported by Dr. Stepp's affidavit. In any event, the question raised by DCS's assertion is not a simple issue. Such conditions would be common for western data. However, the data for the eastern United States would come from sites of considerably higher velocity. Hence, Dr. Long would have to see how the data actually have figured into the computations of site amplitude. If the USGS used different relations for the East and West this could negate the difference in velocity. Long Declaration, par. 71.

Attachment B

September 16, 2003

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

ATOMIC SAFETY AND LICENSING BOARD

Before Administrative Judges:
Thomas S. Moore, Chairman
Charles N. Kelber
Peter S. Lam

In the Matter of	
DUKE COGEMA STONE & WEBSTER	Docket No. 0-70-03098-ML
(Savannah River Mixed Oxide Fuel Fabrication Facility)	ASLBP No. 01-790-01-ML

DECLARATION OF DR. LELAND TIMOTHY LONG REGARDING GANE CONTENTION 3

Under penalty of perjury, I, Dr. Leland Timothy Long, depose and say:

Introduction and Statement of Qualifications

1. My name is Leland Timothy Long. I am Professor of Geophysics at the Georgia Institute of Technology in Atlanta, Georgia.

2. I am a qualified expert on the issue of geophysics, earthquakes, seismology, and seismic wave propagation. I hold a Ph.D. in geophysics from Oregon State University, an M.S. in geophysics from the New Mexico Institute of Mining and Technology, and a B.S. in geology from the University of Rochester. A copy of my resume, including a partial list of publications is attached.

4. I have been a professor of geophysics at Georgia Tech for the past 35 years. As such, I am familiar with all of the research on seismicity in the southeastern United States. My students and I have published papers on earthquakes in South Carolina, Florida, Alabama, and southeastern Tennessee. During the past 30 years, I have been the principal author of most papers on seismology in Georgia. In addition to these publications, I was a co-author on NUREG/CR-5269 titled "Evaluation of Hypotheses for the Cause of the 1886 Charleston Earthquake"

Materials Reviewed

5. I am familiar with the licensing-related filings and correspondence that have been submitted in this proceeding regarding the seismic analysis performed by Duke Cogema Stone & Webster ("DCS") for the proposed Mixed Oxide ("MOX") fuel fabrication facility at the Savannah River Site ("SRS") in South Carolina.

DCS's Seismic Hazards Analysis

6. Dr. Stepp correctly states that the seismic design of the MOX Facility relies upon probabilistic safety hazards analyses ("PSHAs") that were conducted in the late 1970s and early 1980s by the Lawrence Livermore National Laboratory ("LLNL") and the Electric Power Research Institute, Inc. ("EPRI"). The LLNL study summarized expert opinion on seismic zones. The EPRI study folded in opinions on geologic structures that cause earthquakes. These opinions were weighted and reported quantitatively in the report results.

7. DCS assigned a PSHA equivalent to the average of the LLNL and EPRI PSHA at the SRS, and fixed the PSHA peak acceleration at 0.2 g. DCS did not, however, perform any analysis to validate the LLNL or EPRI computations.

Evaluation of DCS Seismic Analysis

8. DCS claims that its analysis for the proposed MOX Facility is "robust." I do not agree. To the contrary, I find that DCS has done a poor job of quantifying the seismic hazard at the SRS. As a result, there is a reasonable chance that the seismic hazard is great enough to warrant further analysis.

9. I have five principal criticisms of DCS's seismic analysis:

a) The EPRI and LLNL evaluations relied on by DCS are based on data limited to that available in the 1970s and 1980s. Consequently, they lack the constraints of more recent seismicity data (for example location and frequency of Charleston type earthquakes) or contain computational bias (for example, as introduced by inaccurate attenuation functions). If the LLNL and EPRI studies were re-calculated, today, using more accurate and up-to-date data, there is a significant chance that they would result in a higher seismic hazard at the Savannah River Site than calculated in the PSHA. This, by the way, would not be a very difficult exercise for DCS. It is merely a question of incorporating more accurate data into the computer programs developed to implement the computation of the PSHA.

(i) The primary objective of a PSHA is to estimate the probability that a given acceleration will be exceeded in some finite time span. The inputs for a

PSHA are seismic source parameters and ground motion models. When the inputs are strictly defined by the existing data, as done in the 1990 USGS maps, the probability represents the historical record. When multiple experts are used to define the inputs, a consensus of scientific thinking at the time of the analysis determines the PSHA. With the LLNL the consensus was based largely on what experts considered viable seismic zones. With the EPRI analysis, expert teams incorporated probabilities for a variety of possible sources. Hence, the LLNL study represents scientific thinking on seismicity and seismic zones, and from another set of experts, attenuation functions. The EPRI gives a consensus on future contribution of many potentially active structures, some not currently active.

(ii) In both the LLNL and EPRI analyses, the experts based their opinion on data available in the 1970's and early 1980's. Since then, there have been significant advances in understanding and knowledge of earthquakes. These data include many magnitude 3 to 5 earthquakes[1], the detailed studies of their aftershocks, and a better understanding of crustal structures in areas like New Madrid and southeastern Tennessee, and new statistical approaches to understanding seismicity.[2] Furthermore, research has shown that the location of a Charleston type event could be more tightly distributed than was generally thought 20 years ago.[3] Still other studies have shown a sensitivity of local attenuation functions to crustal structure.[4]

(iii) All of these factors could affect the computed PSHA. Therefore, they should be evaluated and mathematically compared against the factors that were used in the PSHA. If they are different in any way to suggest a more severe hazard, the PSHA should be reevaluated. In this case, DCS neither computed a locally relevant attenuation function for comparison with those used in the EPRI and LLNL studies, nor critically compared the effects of recent data that suggest a tighter distribution of a Charleston type earthquake with the definitions of seismicity used by the experts in the EPRI and LLNL studies. Hence, DCS does not know how its PSHA compares with a true best estimate based on contemporary thinking.

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[1] See, generally, Southeastern United States Seismic Network ("SEUSSN") bulletins.

[2] See, for example, Chiu, Johnston, and Yang, 1992. Seismological Reserch Letters, 63, 3, pp375-393, Kaufmann, R, and L. T. Long, (1996); A.L. Kafka, Statistica Analysis of the Hypothesis that Seismicity Delineates Areas Where Future Large Earthquakes Are Likely to Occur in the Centarl and Eastern United States, Seismological Research Letters, Vo. 73, p. 992-1003 (Nov./Dec. 2002); Velocity structure and seismicity of southeastern Tennessee, *Journal of Geophysical Research* Vol. 101, No. B4, 8531-8542.

[3] See, for example, Talwani, P., 1999. Fault geometry and earthquakes in continental interiors, Tectonophysics 305, pp 371-379.

[4] See, for example, Gail M. Atkinson and David M. Boore, Ground-Motion Relations for Eastern North America, Bulletin of the Seismological Society of America, Vo. 85, No. 1, pp. 17-30, February 1995.

(iv) It would not be reasonable or conservative to apply the results of the LLNL and EPRI studies to an individual site without also incorporating site-specific data. I participated as an expert in both the LLNL and EPRI studies. During those studies, it was my understanding that the computations were generalized for comparison purposes. It was generally presumed that for a specific site, a more detailed evaluation would be performed. I was fully aware that at close ranges the details of the seismic zones could affect the hazard, particularly if attenuation functions were allowed that properly accommodated shallow earthquakes. At the time of the studies, I was sufficiently concerned to ask the study managers if these were to be used for specific sites and the answer was no, a more detailed study would have to be made for a particular site evaluation.

b) The PSHA is dependent upon the percentage contribution assigned to a floating earthquake. DCS has not assessed what percentage was assigned in the LLNL or EPRI studies. In LLNL, the experts drew seismic zones and decided how many earthquakes were in each zone. All the rest of the earthquakes were put in residual zone. Most seismic zones were determined by having had major earthquakes. For most experts, that regional zone would be a small percentage of the catalog. However, there was at least one expert who suggested the eastern U.S. was one big zone with floating earthquakes. The effect of that expert and the residual events from other experts on the LLNL and EPRI computed PSHA probably would have been a contribution of about 10-15%. What Kafka's analysis suggests is that the percent should be higher – around 30%.

c) The analysis used by DCS to check the validity of the PSHA is fundamentally faulty. DCS checked the PSHA by propagating the Charleston earthquake to the MOX Facility site. Generally, this is a simple and appropriate check on the PSHA: if the Charleston earthquake exceeds the computed PSHA, then there is either an error in the PSHA or the Charleston earthquake is an event with a return period of greater than that specified for the PSHA, namely 10,000 years. In this case, however, the check used by DCS is fundamentally invalid.

(i) One of the factors that must be used to propagate an earthquake is the "attenuation function," which is an equation defining the rate of decay of the amplitude of seismic waves with increasing distance. The attenuation function used by DCS to check its PSHA is technically incorrect, because it fails to fully take account of the nearly total reflection of energy from the Moho at angles greater than the critical angle, a phenomenon also known as "Moho bounce." A correction would likely increase the computed motion of a Charleston earthquake at the Savannah River Site. This increase may be sufficient to exceed the PSHA, indicating possible underestimate of the hazard.

(ii) DCS used the Ou and Herrmann model, which is an adaptation of the Herrmann surface wave model for analysis of attenuation. The velocity models

used by DCS are constant velocity and flat layered. In these models the geometrical attenuation is spherical spreading (proportional to the inverse of the distance of propagation) and is modified by reflection from the layer interfaces. DCS's modified model shares an important characteristic with the original Herrmann model: it introduces an intermediate layer, approximately half way between the ground surface and the Moho. The addition of this factor puts a post-critical reflection for this layer in the distance range of 50 to 90 km. As a result, energy reflected at the interface of the intermediate layer is no longer available for critical reflection from the Moho, which now would arrive at distances beyond 120 km. A consideration of all possible reflections and conversion to other wave types would not significantly change this conclusion because the signals are predominantly controlled by the primary reflections, not the multiples. I do not believe this model is appropriate, because it could deplete the energy available for the post critical reflection from the Moho. In short, DCS's model is not consistent with the actual crustal structure and gives misleading results.

(iii) A modeling of the attenuation function using a flat layered, constant velocity crustal model was the type of analysis performed by DCS. In essence, while the effect of a post critical reflection exists in the analysis, the introduction of an intermediate layer and the use of constant velocity layers has focused the energy at shorter distances and reduced the energy available for the post-critical Moho reflection. The modeling accurately represents wave propagation in a flat layered media with constant velocity layers, but not the wave propagation in the crust.

(iv) Another factor not taken into consideration by DCS in computing the attenuation function is the fact that the real crust is complex. For the Charleston to Savannah River Site path, there is a shallow surface layer, numerous mafic intrusions (or buried extrusive volcanic rocks) Triassic and Jurassic sedimentary basins, and portions of the curst with average velocity. These act as scattering objects for the higher frequencies and modify the velocity for the lower frequencies. The net or averaged effect of such structures is to create a velocity gradient. The influence of a typical crustal gradient on wave propagation is to replace reflection horizons with curved ray paths that focus or defocus energy. The amplitude function with distance is a function of the velocity gradient, not the inverse distance relation implicit in the constant velocity layered model. In particular, gradients typical of the crust exhibit a focusing of energy near the normal position for the post critical reflection. Theoretically, for high frequencies the focusing can cause infinite amplitudes, but wave propagation theory limits the amplification. Focusing can increase amplitudes by more than a factor of 5 (500%), but in my testimony, I suggested a reasonable range of 10 to 50%. It is

undoubtedly this effect that explains the anomalous data commented on in the Atkinson and Boore article.[5]

(v) DCS has argued that I should verify my analysis by performing a calculation regarding the attenuation factor. I do not believe it is necessary for me to perform these computations to understand the consequences of using appropriate crustal models. The Coastal Plain of South Carolina is sufficiently different from the crust of the bulk of eastern North America to warrant a validation of the crustal attenuation function for this region. This is particularly true in the case of the relations utilized for the EPRI and LLNL studies, which relied predominantly on data obtained from the interior of the North American continent.

(vi) I have performed many computations of the attenuation relation in the crust and I am familiar with those factors that influence attenuation. My Ph.D. Thesis was on propagation and attenuation in the crust. Although the specific path from Charleston SC to the site has not been modeled, many similar paths have been studied. An article by Jones, Long and McKee discusses attenuation relations in general for the southeastern United States and points out the existence of a pronounced Moho bounce.[6] The Kean and Long paper also discusses crustal structure in the Georgia and South Carolina Piedmont, and argues strongly for the crust as a single layer. This is contrary to the Herrmann crustal model, which assumes a distinct higher velocity layer in the deeper half of the crust.

d) DCS has discussed many of the recent scientific and technical developments but has failed to provide the analysis that would determine how they affect the earlier EPRI and LLNL studies or the propagation of the Charleston earthquake to the site.

e) DCS misapplies the concept of conservatism. DCS has not done the basic work needed to provide a reliable PSHA. DCS has incorporated the results of the LLNL and EPRI studies, without bringing them up to date or accounting for local phenomena. The resulting errors create too much uncertainty to be compensable by the addition of a so-called "conservatism." A true conservatism is an error purposely added to a reasonably accurate calculation in order to account for unforeseen errors. It is not possible to make a fundamentally deficient calculation conservative, because the failure to correct those errors makes the analysis non-conservative under any circumstances. In this case, DCS could have and should have foreseen and corrected the fundamental errors that are present in the PSHA for the MOX Facility. It is only appropriate to add a conservatism *after* DCS has made a reasonably reliable calculation of the PSHA.

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[5] Atkinson and Boore (1995).

[6] Jones, F. B., L. T. Long, and J. H. McKee (1977). Study of the attenuation and azimuthal dependence of seismic wave propagation in the south eastern United States, Bull. Seismol. Soc. Amer., 67, 1503-1513.

Qualifications of Dr. Stepp

10. I have reviewed the Affidavit of Dr. Carl Stepp, on which DCS relies in support of its motion. I do not believe that Dr. Stepp has sufficient knowledge or experience of the current computational issues involved in seismic hazard analysis to make a credible evaluation of DCS's seismic hazards analysis or credible criticisms of my views.

11. Dr. Stepp is my friend of many years, and my respected colleague. Nevertheless, I believe he overstates his expertise in the area of geophysics in paragraph 4 of his affidavit.

12. During the last 30 years, I have had scientific and editing positions that required me to review much of the scientific literature published during that period in the field of seismic hazards analysis in the eastern United States. I was editor of Seismological Research Letters from 1977 to 1984, and I also served on review panels for Hazards Analysis in the eastern United States during the 1970s and 1980s. In addition, I was an expert on the scientific panels for both the EPRI and LLNL studies. I am familiar with the specific contributions that Dr. Stepp has made to most of the publications on which he is listed as an author. I am aware that early in his career, Dr. Stepp did substantial computational work in the area of seismic hazards analysis. Dr. Stepp's experience in the technical details of computation, however, probably stopped soon after the early 1970s. Since that time, Dr. Stepp's role in projects related to seismic hazards research and analysis has been, overwhelming, as a manager, committee chairman, and editor. He has developed great skills in these managerial areas, especially through his work as manager of the Science Center, Electric Power Research Institute, Inc. (EPRI). As a manager, however, his involvement with actual computational research techniques has been very limited. His comments on my critique of the DCS license application do not reflect deep or extensive familiarity with computational approaches to seismic hazards analysis.

13. Dr. Stepp also oversimplifies the concept of PSHA, and completely ignores the dynamic nature of the field. According to Dr. Stepp, the analytical method used in PSHA:

> uses weighted alternative interpretations, of seismic sources, source parameters (such as magnitude and recurrence frequency) and ground motion models as input for hazard calculation. Because the sources of uncertainty in these inputs are complex, experts may reach different assessments of alternative seismic sources and source parameters and may give different credibility to alternative ground motion models.

Stepp Affidavit, par. 10. Dr. Stepp's characterization of the differences in expert opinion ignores the fact that the field of seismic analysis is one in which new information is constantly being discovered. Expert opinions are constantly changing as they are

influenced by new data and ideas. In fact, as discussed below, one of the most important problems with DCS's PSHA is that it does not address new data and ideas that have been developed in the twenty-plus years since the LLNL and EPRI studies were prepared.

14. Dr. Stepp also oversimplifies the concept of PSHA by stating, in paragraph 11, that "assessments of inputs for a PSHA may be site-specific or they may be done for a large geographic region and applied to many sites of interest." The question of whether inputs for a large region can be used appropriately for a specific site depends on the characteristics of the site and the region. In this case, because of the particular characteristics of the South Carolina coastal plain and their effect on the seismology of the SRS, it would not be appropriate to use only regional inputs.

Dr. Stepp's Characterization of DCS's Seismic Analysis

15. In paragraph 12, Dr. Stepp correctly states that the LLNL and EPRI studies used different methodologies to obtain seismic source inputs. He also states that both methodologies are accepted by the NRC. I do not have a criticism of the methodologies used by LLNL and EPRI. The problem lies in the data put into these programs. Over the last 20 years, significant advances have been made in understanding the seismicity and statistics of the occurrence of earthquakes. If the EPRI and LLNL studies were conducted again using this new information, the range of expert opinion regarding definition of seismic zones and attenuation relations would be much narrower, and the opinions would have a higher degree of accuracy.

16. In paragraphs 13, 14 and 15, Dr. Stepp provides an accurate description of the process by which the EPRI and LLNL studies were prepared.

17. In paragraph 17, Dr. Stepp correctly points out that a 1997 Westinghouse Savannah River Corporation ("WSRC") study on which DCS relies used an average of the EPRI and LLNL bedrock outcrop hazards for the latitude and longitude of the site. In my professional opinion, these computations of bedrock outcrop hazards are not reliable because they used outdated seismicity and attenuation relations. Moreover, while DCS has acknowledged the existence of new information regarding seismicity, it has not evaluated how conclusions from recent seismicity studies would affect the computation. One example of information not considered is refinements to the locations of Charleston type seismicity. For example, the location of earthquakes in the Charleston seismic zone (listed in the Southeastern United States Seismic Network ("SEUSSN") Bulletins) are sufficiently well defined to restrict the possible location of that earthquake to a relatively small area compared to the large zones used by many experts in the LLNL and EPRI studies. It is important to consider these refinements, because the constraints on the location of the Charleston Earthquake and the two other prehistoric epicentral zones from paleoseismic data turn out to be concentrated in the same distance range as the anomalous portion of the attenuation function associated with the post-critical Moho reflections.

This distribution differs significantly from the distributions proposed by the LLNL experts and the EPRI expert teams.

18. In paragraph 17, Dr. Stepp also states that DCS "took into consideration site-specific properties such as soil column thickness, soil and bedrock shear-wave velocity, and soil dynamic properties." I have not verified these results, because they relate to the generation of surface level hazard from bedrock level hazard, a process which is not a subject of my testimony.

19. I have no reason to disagree with Dr. Stepp's assertions in paragraphs 18-22.

20. I also agree with Dr. Stepp's assertion in paragraph 23 that the largest historical earthquake within 200 km of the proposed facility is a check on the validity of the PSHA.

21. In paragraph 24, Dr. Stepp asserts that DCS incorporated the phenomenon of the "Moho Bounce" into its PSHA. I strongly disagree with his assertion. In my professional opinion, as discussed in paragraph 9(c) above, DCS's model is not consistent with the actual crustal structure and gives misleading results.

22. In paragraph 25, Dr. Stepp correctly describes the Moho Bounce as a phenomenon whereby seismic waves are reflected off of the boundary between the Earth's crust and mantle, about 29 km beneath the ground surface in the vicinity of Charleston and the MOX Facility site. His statement that the Moho Bounce results in non-uniform decay of seismic energy in a distance range of 80-120 km, however, is incomplete. Dr. Stepp fails to mention that the non-uniform decay is represented as an anomalously high amplitude in the distance range of between about 80 and 120 kms.

Dr. Stepp's Response to My Analysis

23. In paragraph 30, Dr. Stepp states that the exact location of the Charlestoon earthquake epicenter is unknown. This is not correct. The current understanding of earthquake mechanics coupled with the distribution of aftershocks and rate of occurrence of aftershocks in the Charleston epicentral region place the epicenter within a 10 square km area with a high degree of confidence.

24. I agree with Dr. Stepp's statement in paragraph 31 that it is conservative to estimate 7.3 as the moment magnitude for the Charleston earthquake. More precisely, given the estimates of moment magnitude for the Charleston earthquake, moment 7.3 magnitude 7.3 would be on the high side of the distribution.

25. In paragraph 32, Dr. Stepp states that DCS used the Ou and Herrmann (1990) model to take into account the Moho Bounce. As discussed above, the Ou and Herrmann model is a modification of the Herrmann Crustal Model, consisting of constant velocity layers.

26. In paragraphs 34 and 35, Dr. Stepp correctly characterizes my criticism of DCS's use of the modified Ou and Herrmann Crustal Model. He does not, however, provide data to contradict my criticism. Instead, in paragraph 36, he implies that any errors DCS may have made are inconsequential. According to Dr. Stepp, the errors should be ignored because the MOX Spectrum envelopes "dramatically increased ground motions for all frequencies above 0.8 Hz." In other words, according to Dr. Stepp, DCS's PSHA is sufficiently conservative to accommodate any errors DCS may have made.

27. I strongly disagree with Dr. Stepp's analysis. The fact that the test might not work at frequencies below 0.8 Hz provides a serious indication that there is something fundamentally wrong with DCS's analysis, and therefore the PSHA is too low. The test should work at all frequencies, even those that are not important.

28. In paragraph 37, Dr. Stepp states that the MOX Spectrum has an inherent conservatism, because DCS evaluated the ground motions associated with the 1886 Charleston earthquake in conjunction with the SRS PC-3 probabilistic spectrum and both were enveloped to derive PC-3 seismic design basis ground motion. Then, according to Dr. Stepp, DCS took an even more conservative approach and used ground motions from Reg. Guide 1.60. As discussed above in paragraph 9(a), DCS used the very generalized results of the LLNL and EPRI studies, and failed to modify the seismic zones and attenuation functions to fit local conditions and recent developments in seismicity.

29. The significance of DCS's failure to assess the impact of locally relevant developments in attenuation and seismicity is that it adds unnecessary uncertainties to the hazard. It becomes difficult to know whether the errors (i.e. conservatisms) that DCS has intentionally added are sufficient to compensate for these uncertainties in the calculation.

30. In paragraphs 39-40, Dr. Stepp also states that the NRC has a history of using the LLNL and/or EPRI results in site specific applications. He states that NRC Regulatory Guide 1.165 explicitly permits the use of the EPRI and LLNL PSHA studies. I have reviewed the Reg. Guide, and find that it does not permit unqualified reliance on the LLNL and EPRI studies. According to Section 2.1 of the Reg. Guide:

> For sites in the CEUS [central and eastern United States], when the EPRI or LLNL PSHA methodologies and data bases are used to determine the SSE, it still may be necessary to investigate and characterize potential seismic sources that were previously unknown or uncharacterized and to perform sensitivity analyses to assess their significance to the seismic hazard estimate.

Id. at 1.165-5. Appendix E gives further details:

> It is necessary to evaluate the geological, seismological, and geophysical data obtained from the site-specific investigations to demonstrate that these data are consistent with the PSHA data bases of these two methodologies [i.e., LLNL and

EPRI]. If new information identified by the site-specific investigations would result in a significant increase in the hazard estimate for a site, and this new information is validated by a strong technical basis, the PSHA may have to be modified to incorporate the new technical information.

Id. at 1.165-38. According to Appendix E of Reg. Guide 1.165, "[t]ypes of new data that could affect the PSHA results" fall into three general categories: "seismic sources, earthquake recurrence models or rates of deformation, and ground motion models." Here, DCS has failed to consider ground motion models that could have a significant impact on the hazard estimate for the SRS site.

31. Moreover, as also noted in Appendix E, "major recomputations" of the LLNL and EPRI data bases were planned "approximately every ten years." *Id.* at 1.165-38. Obviously, this has not come to pass. In the absence of such regular re-evaluations, it is all the more important to be sure that individual site evaluations are based on up-to-date information.

32. In paragraph 41, Dr. Stepp states that, as the developer of the methodology for the EPRI PSHA, he has "first hand knowledge that the EPRI PSHA outputs were expected to be used for specific sites." I disagree. The experts on the LLNL and EPRI panels were aware that typical sites were being tested, but these were just tests and comparisons of the LLNL and EPRI of the methodologies. They were not actual applications of the EPRI and LLNL data to the sites. As discussed above in paragraph 9(a)(iv), I also obtained clarification that neither the EPRI nor LLNL were intended for site-specific application without input of site-specific data. Moreover, as discussed above in paragraph 30, Dr. Stepp's understanding is inconsistent with the guidance in Regulatory Guide 1.165, which calls for consideration of site-specific information.

33. In paragraph 43, Dr. Stepp states that an equivalent to the floating 7+ earthquake theory was considered in the seismic design of the MOX Facility, by virtue of the fact that the EPRI and LLNL studies included interpretations that major earthquakes could occur practically anywhere along the eastern seaboard of the United States. It is not relevant that such opinions were included in the EPRI and LLNL studies. The significant concept is the relative contribution of these events. The EPRI and LLNL studies gave floating 7+ earthquakes a contribution level of a few percent. The actual contribution could have been 10 to 15%. In this case, I think it is appropriate to give the contribution of a floating earthquake a higher percentage, say 15 to 40%. A 30% contribution of a floating event could have a significant effect. In its PSHA, DCS made no attempt to evaluate whether the contribution of floating events to the seismic hazard should be re-evaluated.

34. In paragraph 44, Dr. Stepp argues that Kafka's work has no applicability to the South Carolina Coastal Plain. He bases his assertion on the fact that Kafka applied his analysis to magnitude 4 and lower earthquakes, and not to larger magnitude earthquakes.

Dr. Stepp's assertions reveal that he either does not appreciate the inherent scale invariance of magnitude statistics, or does not understand that in a scale invariant system, it is not significant whether the largest events are 4.5 or 6.5. The important factor is catalog completeness. That refers to whether all the events have actually been detected. As one goes further and further back in time, only the larger events are recorded. Therefore, catalogs for older data are incomplete at the lower magnitudes.

35. In paragraph 45, Dr. Stepp states that Kafka's paper is not helpful, because he did not study any earthquakes of sufficient magnitude to be of concern for the seismic design of the MOX Facility. The logic of Dr. Stepp's argument is incorrect. Kafka's work is a statistical measure of a geologic parameter, i.e., the relative distribution of earthquake epicenters for new large events. The fact that his method works in areas where magnitude 6+ events are included in the catalog suggests that it should work for large events in the Charleston area. The fact that it should work is implied by the b value, which is generally assumed to represent all magnitudes up to some maximum magnitude and is the basis for much of the computations in the EPRI and LLNL studies. The significance of Kafka's hypothesis is that it gives a statistical estimate of how many events should be assigned to the background zone, the zone of events that could occur anywhere.

36. In paragraph 45, Dr. Stepp also argues that Kafka purposefully ignored all known geologic/liquefaction data associated with the SCCP. This statement is misleading. Kafka selected catalogs of earthquakes that were regionally significant and complete. The earthquakes indicated in liquefaction data are not part of a complete catalog of events.

37. In paragraphs 47 and 48, Dr. Stepp disputes my contention that the EPRI and LLNL studies did not adequately consider that a magnitude 7.5 earthquake could occur in the Eastern Tennessee Seismic Zone. According to Dr. Stepp, "at least one interpretation" in the EPRI and LLNL PSHA studies did consider the possibility of a 7.5 earthquake in that zone. As discussed above in pars. 9(a)(i) and 41, however, the question is not whether a phenomenon was considered, but the weight its contribution was given.

38. In paragraph 49, Dr. Stepp states that southeast Tennessee "is an area of frequent earthquakes, but these earthquakes have never had a moment magnitude greater than about 5." While it is correct that there has been no major earthquake event in the past 150 years, Dr. Stepp has no basis for saying that such an event has "never" occurred in southeast Tennessee. There is nothing in the data from southeastern Tennessee seismic zone to preclude larger events. Moreover, there are many similarities between the Southeastern Tennessee Seismic Zone and the New Madrid seismic zone which suggest the possibility of a larger event. The significance for the PSHA for the MOX facility could be an increase in frequency of occurrence of waves in the low-frequency end of the spectra.

39. In paragraph 50, Dr. Stepp states that in his judgment, the geophysical structure underlying the Eastern Tennessee Seismic Zone is very unlikely to support magnitude 7+ earthquakes, as I have suggested. He asserts that his judgment "is consistent with the views generally accepted in the scientific community regarding this seismic zone." Twenty years ago, it may have been correct for Dr. Stepp to say that his opinion was consistent with the majority view regarding the low likelihood of a major earthquake in the Eastern Tennessee Seismic Zone. During the intervening years, however, a great deal of new information has been gathered. In November of 1973 there was a magnitude 4.5 earthquake near Maryville, Tennessee. After that, the NRC awarded grants to set up monitoring stations in that area. Seismic monitoring in eastern Tennessee began in earnest in about 1980. In 1985-90, a number of papers were published regarding the seismicity of the area, including one by my student, Ron Kaufman. By the 1990s, a number of focal mechanisms (i.e., orientation of all fault planes) had been put together for that area. One of those studies was by my student Karl-Heinz Zelt and published as Long and Zelt, 1991 (Long, L.T. and K.-H. Zelt, A local weakening of the brittle-ductile transition can explain some intraplate seismic zones Tectonophysics, Vol. 186, 175-192, 1991.) Every seismologist I know that has studied Southeastern Tennessee has expressed a concern over a possible major event in the Eastern Tennessee Seismic Zone. In this context, one should remember that 20 years ago, the number of seismologists worried about the Seattle area was limited.

40. In paragraphs 51 and 52, Dr. Stepp disputes my assertion that DCS failed to consider recent paleoseismic work on the South Carolina Coastal Plain, showing more seismic activity in the last 6,000 years, and in more locations, than previously known. In particular, he disputes my reliance on an article published in 2001 by Talwani and Schaeffer which discusses paleoliquefaction along the coast of South Carolina. According to Dr. Stepp, this paper "contains no new work on the SCCP." Dr. Stepp's assertion is incorrect. While the data analyzed by Talwani and Schaeffer was not new, the interpretation of the data presented was indeed new: Talwani and Schaeffer modified their assessments of the likelihood of locations of earthquakes with epicenters other than Charleston. The very fact that the paper was published indicates that Talwani's and Schaeffer's work on the SCCP was new -- papers do not usually get published unless they present new or revised data and/or interpretation.

41. In paragraph 55, Dr. Stepp attempts to discount the significance of the Talwani and Schaeffer study, by stating that the scenario they presented was considered in NUREG/CR-5613 on which DCS relies; and that the EPRI and LLNL studies considered the possibility of a major earthquake anywhere along the eastern seaboard of the United States. Once again, Dr. Stepp oversimplifies. The question is not whether a phenomenon was considered, but the magnitude of the contribution to the PSHA. Dr. Stepp ignores the fact that if one changes the contribution percentage, the results of the analysis could change. He has also failed to provide an analysis of another significant factor introduced by Kafka and discussed indirectly by Talwani and Schaeffer, namely, the influence of the contribution of different possible distributions of prehistoric earthquakes. The

distribution of epicenters relates to the question of floating versus fixed epicenters. *See* paragraph __ above.

42. Talwani and Schaeffer discuss a scenario which calls for seven magnitude seven (or stronger) Charleston events in the last 6,000 years, with a recurrence interval of 600 years. In paragraphs 56-58, Dr. Stepp makes two criticisms of this scenario. He argues that seven earthquakes in the last 6,000 years cannot have an average return interval of 600 years, because 6,000 years divided by seven events yields an average return interval of 857 years. This is not the correct way to compute the average interval time. The return period should be estimated from the distribution of interval times, not by a simplistic division of the total duration by the number of intervals represented in the data. The decrease from old to recent time may be a consequence of undetected events.

43. Dr. Stepp does not account for the importance of techniques for soliciting expert opinion in the EPRI analysis. In an EPRI-type study, the instructions and the data given to the experts can have a profound impact on the outcome of the study. In an interative study like the EPRI study, the opinions of more highly respected experts can bias the opinions of some experts and the treatment of data. If the EPRI study were to be re-done today, and if Kafka were to persuade a panel of fellow experts of the reliability of his data regarding distribution of new large earthquakes, the statistical result of the EPRI analysis could significantly change.

44. Dr. Stepp also argues that the Talwani and Schaeffer scenario is not new information, but was considered in NUREG/CR-5613, on which DCS relies for its site response analysis. As I have discussed above, the question is not whether a phenomenon was considered, but the degree to which it was considered. Moreover, it is clear that DCS unquestioningly accepted the LLNL and EPRI results, which do not consider a return period of 600 years. The experts in the LLNL and EPRI varied widely in their seismic zones and rates of occurrence of earthquakes. The important contribution of recent data to consider is whether the recent data and constraints increase the hazard. This comparison has not been done. Furthermore, as noted above in par. 9(a)(ii), a modern and more representative attenuation function in combination with the better constrains on event locations could combine to increase the PSHA.

45. In paragraph 59-61, Dr. Stepp disputes my reliance on a 2002 paper by Hu, Gassman, and Talwani, which suggests that magnitudes of historical earthquakes in the SCCP may have been much greater than previously considered. Dr. Stepp argues that the paper's conclusions are "not valid" because the authors did not consider how aging affects soil strength. In support of this proposition, Dr. Stepp cites a master's thesis by a student at the University of South Carolina, which concludes that if aging is taken into consideration, the prehistoric earthquakes that occurred during the past 6,000 years and caused paleoliquefaction features in the SCCP have magnitudes ranging from 5.3 to 6.8.

46. Geological aging is a complex issue, dependent on many environmental factors. Leon's thesis work considers many of these, but not all. In Leon's notes for further work she states:

> To develop a greater confidence on the results of the in situ tests, the field results need to be compared to laboratory test results.

Moreover, as summarized in the conclusions, Leon's studies utilized existing liquefaction resistance curves that had not been completely verified for the sands at the specific sites in question. *See id.* at page 159.

47. In paragraph 63, Dr. Stepp ascribes significance to the fact that the Leon thesis is signed by Sarah Gassman and Pradeep Talwani, two of the authors of the Hu et al paper. On a Masters Thesis, the signatures stipulate that the work done is deserving of the award of the Masters degree, not that the work is a complete analysis. For two reasons, the academic standard for acceptance of a master's thesis is less rigorous than for a Ph.D. dissertation: first, the masters' degree program is shorter, and therefore affords the student less preparation time; and second, the student has less education and training to apply to the project. A more appropriate test of whether this thesis has been accepted by the scientific community as a correct and reliable study is whether Hu et al. will be publishing a retraction or revision to their paper, and whether the Masters Thesis is itself published.

48. In paragraph 66, Dr. Stepp correctly observes that many attenuation relations have been published since the EPRI and LLNL studies were published. This development attests to the fact that modeling attenuation relations is not a simple problem, and also demonstrates dissatisfaction with the early attenuation relations used in the EPRI and LLNL studies.

49. As demonstrated in paragraph 67 of his affidavit, Dr. Stepp and I agree that it would be important to take into account "Moho bounce" when modeling a repeat of the 1886 earthquake, which was placed 120 km from the SRS site. I focus on an attenuation relation with a Moho Bounce because the data I have seen for the southeaster U.S. demonstrates a marked post-critical Moho Reflection. See Jones, Long, and McKee.

50. In paragraph 69, Dr. Stepp states that "a" PSHA takes into account multiple earthquakes with multiple distances from a particular location. He also states that it is unclear to him why consideration of Atkinson and Boore (1995) would materially affect the seismic design of the MOX Facility. In paragraph 9 (c) above, I explain the basis for my view that DCS's check on the PSHA failed to take into account the effect of the Moho post-critical reflection or bounce on the magnitude of ground motion at the SRS. While DCS claimed to take the Moho bounce into effect in the test using the Charleston earthquake, in fact it relied on a conceptual crustal model which is an oversimplification

of actual conditions. Atkinson and Boore is relevant because it shows that the reflection is more pronounced than assumed by DCS.

51. In paragraph 71, Dr. Stepp states that the large range of uncertainty in the LLNL and EPRI ground motion attenuation models encompasses the Atkinson and Boore (1995) model that I rely on. The existence and relevance of the Moho bounce, however, is not an uncertainty. It is an actual and significant factor that should be factored into DCS's seismic analysis. Neglecting it has the effect of biasing the results of the PSHA and the test on the PSHA to a lower hazard

52. In paragraph 78, Dr. Stepp asserts that the USGS hazard maps are not appropriate for facilities where an applicant is concerned about earthquakes with annual probabilities of exceedance of 10^{-4} and lower . A probability of exceedance of 10^{-4} is only a factor of 4 longer than the computed probability of exceedance considered and published by the USGS. The techniques and data are so similar that there should be no significant difference in the estimates of PSHA for the same probability of exceedance..

53. In the same paragraph, Dr. Stepp also asserts that the USGS maps were not developed or intended for nuclear facilities. In paragrah 79, Dr. Stepp also states that the USGS maps were developed in conjunction with seismic designs for "ordinary" buildings. The characteristics of the buildings to which the PSHA's are applied have no effect on the applicability of the computations underlying the hazard maps. PSHAs are purely dependent on the seismicity, attenuation relations and related factors evaluated by experts (or by the data in the case of the USGS hazard maps). The ultimate use of a seismic hazard map has no impact on the data or conclusions presented in the map The experts in the LLNL and EPRI studies or the data in the USGS study would have given the same answer with or without nuclear facilities. Why something was done should not affect the results.]

Response to DCS Statement of Material Facts on Which No Genuine Issue Exists

54. In the following paragraphs, I will respond to factual assertions that are not covered by my response to the affidavit of Dr. Stepp.

55. In general, the "Statement of Material Facts on Which no Genuine Issue Exists" is filled with errors, poorly understood scientific concepts and non-sequiturs.

56. In Statement 3, DCS incorrectly claims that I agree with the assertion that a 7.3 moment magnitude earthquake with an epicenter located 120 km southeast of the MOX Facility site is "appropriate or conservative for modeling the historic 1886 Charleston earthquake ground motions." I do not agree with that statement, and I have not said so. As can be seen in the documents cited by DCS, I agree that the Charleston earthquake is the most severe documented historical event that is relevant to the seismic design for the

MOX Facility. *See* GANE's June 28, 2002, response to DCS Interrogatory 3.32; Long deposition transcript at 129:10-15. It is not necessarily conservative to put the MOX Facility 120 km from the Charleston earthquake, because effects like the Moho bounce increase shaking with increased distance.

57. In Statement 8, DCS asserts that I have suggested that the use of the Herrmann Crustal Model may produce erroneous ground motions at the MOX Facility from the 1886 Charleston earthquake with an error rate of as much as 50%. In Statement 9, DCS asserts that a 50% error is encompassed by the MOX Spectrum. As stated at page 428 of my deposition transcript, I do not believe there is a reasonable basis for confidence that a 50% error will be encompassed by the MOX Facility design. As further stated at page 428, I made an educated guess that application of the Herrmann Model had incorrectly depressed ground motion by 10 to 15%. One would have to do certain calculations to determine the actual percentage. It is not appropriate to address such a significant error by introducing a large conservatism. The calculation should be reasonably correct in the first place. The use of the term "conservative" implies that one has a reasonable level of confidence in the underlying calculation, and is adding a margin of error to account for uncertainties. That is not the case here. The design of a major facility deserves a through analysis and update of pertinent data, not the injection of errors of unknown magnitude that arbitrarily increase the hazard (referred to as conservative analysis) in hopes that the intentional errors are greater than the effects of other errors and omissions.

58. In Statement 10, DCS asserts that I have not identified an alternative model to the Herrmann Crustal Model. This is incorrect. At page 428 of my deposition transcript, I have outlined the methodology that DCS should follow to assess the effect of the Moho Bounce.

59. In Statement 12, DCS asserts that NRC Regulatory guide 1.165 explicitly permits the use of the EPRI and LLNL PSHA studies. This is partly a question of legal interpretation. As a seismologist, however, I think it is important to observe that Reg. Guide 1.165 requires the consideration of how new and site-specific information may affect the applicability of the LLNL and EPRI studies. In addition, the authors of the Regulatory Guide contemplated that the LLNL and EPRI studies would be updated every ten years. Given the tremendous growth in available information about seismicity in the southeastern United States over the past twenty years, such an update is long overdue. In my professional judgment, it would be irresponsible to rely on a twenty-year old study of seismicity in the South Carolina coastal region, without examining whether it should be updated to account for new information and data regarding local seismicity.

60. In Statement 15, DCS states that Kafka purposefully ignored all known geologic/liquefaction data. This statement is misleading. Kafka did not ignore data, he selected data sets that were complete for small and large events. The geologic/liquefaction data do not yield a complete catalog because they cannot detect the smaller events. Moreover, DCS's observation that two or more of the liquefaction

identified event could have occurred outside Charleston actually supports the Kafka statistical analysis. DCS does not appear to understand the concept of scale invariance in the statistics of earthquakes. Scale invariance is implied in earthquake statistics when "b" values are assumed to be constant. The importance is that for scale invariant systems, relationships that hold at small magnitudes should hold at large magnitudes. The most common and obvious example of this is that there are about 10 magnitude 1 events for every magnitude 2, just as there are 10 magnitude 6 events for every magnitude 7 event. One magnitude increase gives the same reduction in number of events in a complete catalog.

61. In Statement 18, DCS asserts that "Kafka's theory is not generally accepted in the scientific community." Kafka paper presents a statistically quantifiably observation, not a theory. He has quantified a distribution of epicenters that has been generally known and accepted for over 60 years.

62. In Statement 19, DCS attempts to undermine the value of Kafka's work by noting that Kafka himself described his work as "still exploratory." Kafka was being modest. Obviously, there is always more to be learned in the area of seismology. This does not undercut the value of the data that Kafka has collected.

63. In Statement 20, DCS asserts that I believe "Kafka's theory is a 'pioneer paper.'" First, Dr. Kafka's paper does not propound a theory, it reports data. Second, the fact that a paper is a pioneer work does not mean it is less credible or reliable than others. When a scientist ventures into an area where others have not gone before and discovers a relationship that suggests disagreement with some conventional thought that should be termed a pioneering type effort.

64. In Statement 21, DCS argues that Kafka's "position" is contradicted by other articles that I have cited, namely Tawani & Schaeffer and Hu *et al.* This is incorrect. First, Kafka's paper does not represent a "position" or a "theory," but his observations. Second, the other studies referenced by DCS suggest a continuing history of seismicity at Charleston and the possibility that major (M>5.5) events occurred in two other locations. That data is entirely consistent with Kafka's observations.

65. In Statement 30, DCS asserts that GANE has not provided any analysis or data to show that consideration of the earthquake sequences identified in Talwani & Schaeffer would increase the ground motions of the design earthquake for the MOX Facility. The distances to Charleston and Bluffton from the site are in the range affected by the Moho bounce effect, which was not a component of the EPRI or LLNL studies. Inclusion of this effect in the PSHA for these earthquakes, rather than a distribution of ranges (the large seismic zone defined by some experts) could increase the PSHA.

66. In Statement 37, DCS asserts that GANE has not provided any analysis or data to show that consideration of a shorter return interval for earthquakes along the coast of

South Carolina ranging in magnitude between 5.3 and 6.8 would increase the ground motions of the design earthquake for the facility. It is obvious that if a shorter return period is used in a PSHA, the hazard will increase. Similarly, a reduction of the magnitude estimates would decrease the hazard, everything else being constant. However, the Leon thesis on which DCS bases its proposed revised magnitudes (Statement 36) has yet to be published and was not complete a complete analysis.

67. Contrary to DCS's assertion in Statement 42, Atkinson and Boore is not the only attenuation model available to demonstrate the Moho bounce. While it is true that Atkinson and Boore's model more accurately represents the local structure, a similar attenuation model is documented in Jones, Long, and McKee. Atkinson and other coworkers have published a number of articles on attenuation, many of which show the effects of reflections off the Moho.

68. In Statement 46, DCS asserts that the LLNL and EPRI PSHAs envelope the Atkinson and Boore (1995) model such that consideration of that model would not materially affect the MOX Facility seismic design. This statement does not make sense. A PSHA and an attenuation model are not directly comparable. I also do not believe DCS has computed a PSHA using the Atkinson and Boore attenuation relation which would be needed to make such a comparison. Even if the Atkinson and Boore attenuation relation is bracketed, does not mean that the PSHA is unaffected. If proper definition of the Moho bounce were used as an attenuation relation, the statistical average for events at distances affected by the Moho bounce would be increased. In other words, the statistics of the attenuation function are too low in the range of the Moho bounce and this bias will suppress the hazard computed with the incorrect attenuation functions.

69. In Statement 47, DCS asserts that GANE has not provided any analysis or data to show that consideration of Atkinson and Boore (1995) or any other model would increase the ground motions of the design earthquake for the MOX Facility. Statement 47. This argument is incorrect. If the major contribution to the PSHA is from the Charleston earthquake and from other possible large events at a distance where the reflection from the Moho can cause amplitudes that are anomalously high, then the integrated results will be higher than those where the events are distributed. The Kafka factor, an increased probability of events being in new locations, would increase the resulting PSHA by increasing the probability of events occurring close to the site. These impacts on the PSHA were not evaluated. Instead, DCS has substituted arbitrary errors with bias. This approach constitutes bad science, and does not give confidence that the computed hazard is sufficiently large to overcome errors in data and logic.

70. In Statement 48, DCS states that GANE alleges that the June 2002 U.S. Geological Survey Seismic Hazard Maps show a return period for 0.2g at the MOX Facility site of about 2,500 ears, while DCS fixes the return period for 0.2g PGA at the MOX Facility at 10,000 years. The statistical relations used would differ only slightly (other than by the factor of 4 difference) between the USGS computation for 2500 years and the EPRI and

LLNL computations for 10,000 year return periods. The only possible difference would be introduced by the truncation of larger magnitude earthquakes by using the characteristic earthquake model. This would result in a slightly lower hazard for the same large time period.

71. In Statements 51-53, DCS argues that the USGS hazard maps are based on "firm-rock" conditions prevailing in the western United States, and which do not exist beneath the MOX Facility. According to DCS, applying USGS firm-rock assumptions to a hard-rock site overestimates the ground motions at the MOX Facility. This is not a simple issue. Such conditions would be common for western data. However, the data for the eastern United States would come from sites of considerably higher velocity. Hence, I would have to see how the data actually have figured into the computations of site amplitude. If the USGS used different relations for the East and West this could negate the difference in velocity. This appears to be a new topic, not addressed in Dr. Stepp's declaration.

Response to Statements in DCS Brief

72. In the following paragraphs, I will respond to factual assertions that are not covered by my response to the affidavit of Dr. Stepp and the Statement of Material Facts.

73. In footnote 2 of its motion, DCS claims that I made substantive changes to the transcript of my deposition. This claim is false. The vast majority of the changes represent corrections and clarifications in syntax and/or word choice. Occasionally I had to change the substance of a sentence in order to make it clear or sensible. It was a full day before the court recorder learned the meaning and spelling of some of the technical words. Sentence structure was often poorly or not at all punctuated. Moreover, many of the questions and answers were recorded in a way that was not accurate, and yielded a nonsensical result. In such circumstances, it was not possible to make a simple spelling or grammatical correction, and I had to make an educated guess at what the correct transcription would have been. Moreover, in response to many questions that were poorly worded or devised to solicit misleading answers, I was required to modify my response mid-sentence in order to go over background and provide qualifications. Considering these handicaps and the limited technical understanding of the DCS counsel, there were remarkable few corrections. It should also be noted that DCS counsel had immediate access to the transcript, which was not available to me.

Leland Timothy Long

September 16, 2003

LELAND TIMOTHY LONG **BIOGRAPHICAL SKETCH**

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Education:

BS	1962	University of Rochester	Geology
MS	1964	New Mexico Institute of Mining and Technology	Geophysics
Ph.D.	1968	Oregon State University	Geophysics

Employment History:

1981-Present	Professor, Georgia Institute of Technology
1972-1981	Associate Professor, Georgia Institute of Technology
1968-1972	Assistant Professor, Georgia Institute of Technology
1964-1969	Research Assistant, Oregon State University, Department of Oceanography
1964	Junior Geophysicist(for summer), Pan American Petroleum Corp.
1962-1964	Graduate Research Assistant, New Mexico Inst. of Mining and Technology
1969-present	Consultant in Geophysics, Professional Geologist in Georgia

Current Fields of Interest:

Dr Long is a seismologist with research experience in earthquake tectonics, wave propagation, seismic imaging, and the environmental impact of seismic vibrations. His principal area of research specialization has been the seismicity of Georgia and the southeastern United States. He has studied the mechanism of reservoir-induced earthquakes. Recent studies include the tomographic inversion of surface waves to image soil structure and the inversion of travel time anomalies to image crustal velocity structure. He has studied and developed a model for the tectonic mechanism for major intraplate earthquakes. In these studies, he has considerable experience in seismic instrumentation and monitoring methods. His theoretical seismology studies include the modeling of seismic coda and the modeling of wave propagation using the finite difference method Dr. Long has experience in estimating the hazards caused by seismic vibrations from by vehicles, quarry blasts, and earthquakes. His gravity studies include regional surveys, the location of sinkholes, the calibration of "g" for sensitive instruments and determination of deflections of the vertical. Educational outreach projects have included the organization of seismicity workshops for k-12 teachers and providing information to the news media following major earthquakes.

Refereed Publications:

Long, L.T., (2002) Group Velocity Inversion Using Synthetic Surface Waves *in* Proceedings of the Symposium on the Application of Geophysics to Engineering and Environmental Problems, The Environmental and Engineering Geophysical Society, February, 2002. (SAGEEP02)

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Location of mine collapse in Alabama from seismic data (for litigation)
Georgia Emergency Management Agency, Assist preparation of Earthquake Video.
DOE, Project evaluation team for bore hole geophysics.
Georgia Emergency Management Agency, Estimation of seismic hazard.
Lawrence Livermore Nuclear Lab, University of California, Expert panel in seismology.
Law Environmental Services, Consultant in seismology.
Law Engineering Testing Co. Consultant in seismology.
U.S. Corps of Engineers, Waterways Exp. Station, Evaluation of maximum earthquake.
Member of LETCO Technical Evaluation Committee in EPRI Seismicity Study.
U.S. Corps of Engineers, Advisory Committee, Seismic design evaluation, Albin Barkley Dam.
Collaborator in Seismology to USGS (formerly NOAA).
Greiner Environmental Services, Environmental impact of seismic road noise.
U.S. Corps of Engineers, Seismic evaluation of Richard B. Russell Dam.
U.S. Corps of Engineers, Seismic evaluation of Strom Thurmond Reservoir.
Georgia Geological Survey, Siting of Nuclear waste depository in crystalline rock.
Georgia Geological Survey, Gravity data evaluation of sinkholes and talc deposits.

CERTIFICATE OF SERVICE

I hereby certify that on September 16, 2003, copies of the foregoing GEORGIANS AGAINST NUCLEAR ENERGY'S OPPOSITION TO DCS'S MOTION FOR SUMMARY DISPOSITION OF GANE CONTENTION 3 were served on the following by e-mail and first-class mail:

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