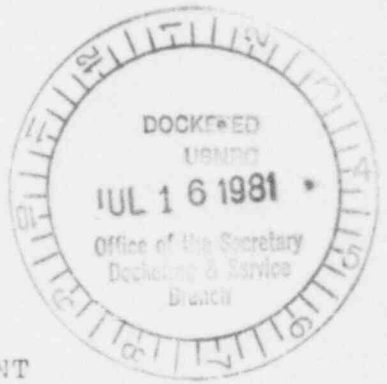
UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING APPEAL BOARD

In the Matter of	)	
PUBLIC SERVICE COMPANY OF NEW HAMPSHIRE, et al.	)	Docket Nos. 50-443 50-444
(Seabrook Station, Units 1 and 2)	)	



NEW ENGLAND COALITION ON NUCLEAR POLLUTION
PROPOSED FACTUAL AND LEGAL FINDINGS ON
REMANDED SEISMIC ISSUES AND SUPPORTING ARGUMENT

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I. Introduction and Statement of the Case

After a myriad of decisions in this proceeding too numerous to detail here, the Appeal Board now has before it a new factual record on which it must judge two narrow issues remanded to it by the Commission. The relevant prior rulings are the Decision of this Board and related dissent and supplemental opinion, Public Service Co. of New Hampshire (Seabrook Station, Units 1 and 2), ALAB-422 6 NRC 33, 54-65 (1977), and ALAB-561, 10 NRC 410-435, 436-a - 436-n (1980), and the Commission's decision to remand on seismic issues, Public Service Co. of New Hampshire (Seabrook Station, Units 1 and 2), CLI-80-35, 12 NRC 295 (1980). In the former, the Appeal Board rejected Dr. Chinnery's methodology for predicting the return periods of earthquakes in the Seabrook area as technically deficient and inconsistent with 10 CFR Part 100, Appendix A, and accepted the Applicant's and

Staff's position that the Safe Shutdown Earthquake ("SSE") should simply be the earthquake of highest intensity in the historical record, Modified Mercalli Intensity ("MMI") VIII. The Appeal Board also accepted the Staff's methodology for correlating vibratory ground motion with the SSE and approved a response spectrum anchor point of 0.25g. In a vigorous dissent, Mr. Farrar argued that the majority had improperly placed the burden of proof on the intervenors and had ignored the mandate to take a conservative approach to the interpretation and application of Appendix A.

The Commission essentially agreed with Mr. Farrar in ruling that a probabilistic approach to estimating earthquake return periods as a basis for determining the SSE is consistent with Appendix A. In so doing, the Commission emphasized both the current lack of understanding of earthquake science, including the lack of a theoretical basis for the Staff's choice of SSE, ar need for conservatism in applying what knowledge and understanding are available. After rejecting the geological conditions on which the Board based its decision as "inconsistent with the intent of Appendix A to provide a conservative approach to determining the SSE in the absence of a theoretical basis for such a determination," the Commission stated,

> As this yet early state in earthquake science, we are not prepared to dismiss an empirical relation on the basis of failure to satisfy criteria, which although they may appear reasonable, imply a greater understanding of the relation between geology, seismology, and earthquakes than is actually available.

* * * * * * * *

> Accordingly, in view of the need for conservatism in this area, we find Dr. Chinnery's methodology is not inconsistent with Appendix A.

Id. at 297.[1]

With conservatism as the watchword, the Commission remanded the case to this Board to consider the following narrow issues:

1. The "factual validity of Dr. Chinnery's hypothesis," and

2. The "consistency of Appendix A and staff's methodology for correlating vibratory ground motion with the SSE."

Id. at 297-98. The Appeal Board further emphasized the narrow scope of the proceeding in its Memorandum and Order of February 12, 1981, in which it ruled that uncertainties and flaws in competing methodologies are not relevant to determining the validity of Dr. Chinnery's methodology. The issue with respect to the Chinnery methodology, therefore, is solely whether, in the absolute sense, it has scientific validity.

In compliance with the Commission's remand, the Board held an evidentiary hearing on April 6-9, 1981. Pursuant to agreement of the parties, the Applicant filed its main brief on May 8, 1981, the Staff filed its main brief and any responses to the Applicant's arguments on June 16, 1981, and the New England Coalition on Nuclear Pollution (NECNP) is now filing its main brief and responses to the Applicant and Staff. The Applicant and Staff have the opportunity to file final briefs, limited to responses

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1/ Since much has been made of the fact that the Commission's remand was a split decision, we note that Mr. Ahearne agreed with Mr. Farrar and the majority on these points. Id. at 299.

to NECNP's arguments, by August 10, 1981.

This brief will demonstrate that:

1. The record requires an SSE of MMI XII, or at least MMI X, unless a probabilistic theory such as that proposed by Dr. Chinnery establishes that a lower intensity is acceptable.

2. The "Chinnery methodology" is valid under the standard by which "validity" must be determined.

3. The Staff's methodology for correlating vibratory ground motion with the SSE is scientifically invalid.

4. A complete re-analysis of the seismic design of Seabrook is required based on the Chinnery methodology and the high probability of exceeding the approved design response spectrum.

5. The pending motions to strike should all be granted.

II. In the Absence of a Probabilistic Justification to the Contrary, the Record Requires an SSE of MMI XII.

Despite the narrow scope of this proceeding, the evidence adduced at the hearing establishes that the SSE for Seabrook must be increased considerably regardless of the validity of the Chinnery methodology.[2/] Indeed, the Chinnery methodology or some other probabilistic approach is the only basis on which to establish an SSE less than MMI XII (or MMI X if a non-conservative approach is taken to the evidence).

According to the Applicant's witness, Mr. Holt, there is a spatial correlation between large earthquakes and geologic struc-

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2/ In light of its public interest responsibilities, the Board must consider all of the implications of this evidence regardless of the particular terms of the Commission's remand. Duke Power Co. (Catawba Nuclear Station, Units 1 and 2), LBP-75-35, NRCI-75/6, p. 626, 654-655 (1975). That is particularly true here, where the Applicant voluntarily presented the evidence in question for its own purposes. The Applicant cannot argue that the Board may consider the evidence to the extent that it is favorable, but must ignore it to the extent that it is unfavorable.

tures known as "intrusions" or "plutons," which are associated with large trending faults. (Holt, Tr. p. 381). This is true both in New England, where the large earthquakes can be related to the Ossipee complex in the White Mountains or to a pluton near Cape Ann (Holt, Tr. p. 381-383), and in the rest of the Eastern United States. In particular, he finds the theory valid for the areas of New Madrid and Charleston, South Carolina, which have experienced earthquakes of MMI XII and MMI X, respectively, in recent history. (Holt, Tr. p. 448-450, Direct testimony, Figures 4 and 5). He also contends that at least the New Madrid intrusions are similar to those in New England. (Holt, Tr. p. 405, l. 4-8). Mr. Holt's view is supported, at least in part, by two United States Geological Survey studies that relate large earthquakes to plutons both in the Mississippi embayment and in New England. (Holt, Tr. p. 431).

Although Mr, Holt and the Applicant did not take the narrow position that the New England plucons controlled large earthquakes in the area and thereby argue for an even smaller SSE (Holt, Tr. p. 430, l. 11-23), Mr. Holt firmly believes that the large earthquakes and the plutons are related. Indeed, he believes that the evidence is strong enough to allow him virtually to predict the occurrence of the MMI XII New Madrid event, assuming that it had not already occurred. (Holt, Tr. p. 447-450, esp. p. 450, l. 10-16).

With respect to the Seabrook area, in particular, Mr. Holt testified that that the pluton that correlates with the Cape Ann earthquake is approximately 15 miles from Seabrook. (Holt, Tr.

p. 390-391, 420, l. 13-16). He also testified that the pluton itself is about 10 kilometers wide and that it could generate an earthquake 15 miles away. (Holt, Tr. p. 461). Therefore, the pluton could generate an earthquake of MMI XII intensity on the Seabrook site. Mr. Holt attempted to mitigate the impact of this information by noting that the earthquake - pluton relationship relates to the existence of trending faults, which in the case of Cape Ann do not trend in the direction of Seabrook. (Holt, Tr. p. 461). However, there is absolutely no evidence that those faults have been active in any way in recent geologic history, even when the 1755 Cape Ann earthquake occurred. Furthermore, no historical evidence indicates that earthquakes correlated with the pluton have occurred only along the trending faults.

The only conservative conclusion that can be reached from this evidence is that the Cape Ann pluton is capable of generating an earthquake of MMI XII, or at least MMI X (as at Charleston), at Seabrook. There is no evidence that our understanding of the phenomenon is great enough to allow the conclusion that the pluton cannot generate earthquakes as large as those generated by similar plutons, or that it cannot generate such an earthquake at Seabrook. Undoubtedly, the Applicant and the Staff will argue that the soil-bedrock distinction would lessen the intensity of any earthquake occurring at Seabrook. We discuss the weakness of that argument below at p. 34-36, but it suffices in this context to note that none of the evidence on that point allows the reduction of an earthquake from MMI XII or MMI X to MMI VIII.

In these circumstances, Appendix A requires that the SSE be established at MMI XII, or at least MMI X, unless a probabilistic theory such as Dr. Chinnery's provides a justification for a lower value. Under §III(c) of Appendix A, the Safe Shutdown Earthquake is

> that earthquake which is based upon an evaluation of the maximum earthquake potential considering the regional and local geology and seismology and specific characteristics of local subsurface material. It is that earthquake which produces the maximum vibratory ground motion for which certain structures, systems, and components are designed to remain functional.

(emphasis supplied). On its face, Appendix A does not allow anything less than the maximum earthquake potential to be chosen, regardless of the rarity or return period of the event.

Recognizing "the limited geophysical and geological information available to date concerning faults and earthquake occurrence and effect," (§I), Appendix A dictates that the effort to determine the SSE begins with the identification of the historic earthquake of maximum intensity correlated with tectonic structures and provinces surrounding the site. However, the maximum historic earthquake related to either a tectonic structure or a tectonic province is only the minimum value that may be considered for the SSE. Where "geological and seismological data warrant, the Safe Shutdown Earthquake shall be larger" than the maximum historic earthquake. (emphasis supplied) §V(a)(1)(i) and (iv). Once the correct magnitude or intensity of the SSE is determined, the effect on the site is evaluated based on the tectonic structure or province to which the SSE is related.

The current Seabrook SSE is based on the maximum historic earthquake of MMI VIII which occured at Cape Ann. It is assumed

to occur at the site since at the time the SSE was initially determined, the Cape Ann earthquake was not correlated with a pluton or intrusive structure. Since there was apparently no indication at the time that such a correlation would indicate a higher intensity SSE, this was the conservative approach. Now, however, there is uncontroverted evidence in the record from Mr. Holt that the Cape Ann earthquake correlates with a pluton which could generate an earthquake at the Seabrook site and that the pluton is similar to others that have generated MMI XII and MMI X earthquakes in recent years. Section V(a)(1)(i) of Appendix A requires that such earthquakes be assumed to occur at the site since the pluton or intrusive structure is capable of generating such an earthquake at the site. More importantly, the dictates of Appendix A clearly require that

> [T]he procedures in paragraphs (a)(1)(i) through (a)(1)(iii) of this section shall be applied in a conservative manner.

This is precisely the point the Commission emphasized in remanding the case to the Appeal Board. A conservative approach to this evidence requires a determination that the "maximum earthquake potential" for the Seabrook site, based on available geologic evidence, is a MMI XII.

While this is the inescapable conclusion based on Appendix A and the available geologic evidence, NECNP recognizes that despite the maximum earthquake potential for the site, the likelihood of a MMI XII may be so remote that it should not be considered as the SSE. However, one must find a sound basis to determine that the likelihood of the maximum potential earthquake is so low that the possibility of its occurrence need not be included in any seismic analysis. Only a probabilistic analysis of

the sort proposed by Drs. Chinnery and Trifunac provides a rational scientific basis for making this judgment. At this point, the Chinnery methodology may provide a basis for choosing a MMI X, or possibly a MMI IX, depending upon the analysis of vibratory ground motion for those intensities. Dr. Trifunac's approach may eventually justify a lower SSE, but by his own testimony it is clear that he has not yet undertaken an analysis sufficient to determine whether his results are acceptably conservative. (Trifunac, Direct Testimony, p. 10).

III. The "Chinnery Methodology" for Determining Earthquake Return Periods is Factually Valid, and the Applicant and Staff Have Not Met Their Burden of Proof With Respect to Determination of the SSE.

The first question that the Commission remanded to the Appeal Board for consideration is the "factual validity of Dr. Chinnery's hypothesis." That hypothesis concerns the estimation of earthquake return periods, particularly for earthquakes of large intensities that are not contained within the historical record of an area, or that have occurred so rarely in human history that their occurrences do not provide data adequate to estimate return periods. Dr. Chinnery's hypothesis is not and does not purport to be, a complete methodology for determining the seismic design of nuclear power plants. In terms of Appendix A, it is a methodology that permits a rational evaluation of the likelihood of occurrence of earthquakes equal to or larger than historic earthquakes of greatest intensity, and it provides seismological data that may warrant a choice of SSE larger than the largest historic earthquake. In this case, NECNP believes that it warrants a choice of SSE at MMI IX or MMI X.

The Chinnery methodology has been discussed thus far in terms of four "assumptions," linearity, uniform slope, lack of upper bound, and extrapolation. We will adhere to that approach, although the evidence demonstrates that none is strictly an assumption, and the first two in particular are based on empirical observations of available data. Before discussing the "assumptions" and the issue of factual validity, however, we first address the standard by which factual validity must be judged. Due to the limitations on the scope of this hearing, and the conservative appproach mandated by Appendix A and by our minimal understanding of earthquake science, the "validity" threshold is extremely low. Finally, we will address general criticisms made by the Applicant and Staff.

A. The Standard for Determining "Factual Validity."

The Commission was clear in ruling that the use of Dr. Chinnery's hypothesis concerning earthquake return periods is consistent with Appendix A. However, it provided no criteria by which to judge the "factual validity" of that hypothesis. Rather, it simply stated that "greater exploration on the record is required," Public Service Co. of New Hampshire (Seabrook Station, Units 1 and 2), CLI-80-33, 12 NRC 295, 297 (1980). From that statement, we cannot determine whether the Commission believed the record to be inadequate to support Dr. Chinnery's hypothesis, inadequate to refute it, or simply inadequate to reach a judgment. In this circumstance, the standard by which Dr. Chinnery's methodology is to be judged must be garnered from the more general language of the Commission's decision, the language of Appendix A, the current state of earthquake science, and the particular status of this case.

If there were a platonic ideal of seismic methodology against which the Chinnery hypothesis could be compared, one could judge whether particular uncertainties or assumptions are acceptable or, instead whether they render the hypothesis scientifically invalid. No such ideal exists. Rather, we must work with the minimal information and understanding that we have, which is the primary reason the Commission emphasizes the need for conservatism in addressing these questions. Accordingly, NECNP believes that the Chinnery hypothesis cannot be judged in the abstract, but must be compared to other approaches, all of which are subject to significant uncertainities. Since the Chinnery hypothesis reflects uncertainties that are no greater than those involved in approaches taken by the Staff and Applicant, and since the Chinnery hypothesis alone provides a rational scientific basis for determining seismic risks, it is undoubtedly "comparatively" valid. However, the Board has ruled that this type of comparison is outside the scope of this proceeding, and the standard for decision must be found elsewhere.

First, the Board is making only a "threshold" decision on the scientific validity of the Chinnery hypothesis and not a decision about the acceptability of the Chinnery hypothesis in lieu of other theories. Therefore, the evidence thus far presented need not met this far more severe test. To reject the Chinnery hypothesis, the Board must find that it is virtually impossible for Dr. Chinnery to be correct.

Second, the Board must look to Appendix A's mandate of con-

servatism. If Dr. Chinnery's hypothesis is plausible, a conservative approach demands that it be considered valid and compared with alternative approaches, which NECNP contends (but has been precluded from demonstrating) lack any sound scientific basis for choice of the SSE.

Third, the Board must consider the qualifications of Dr. Chinnery and the sources in which his hypothesis has been published. There has been absolutely no challenge to Dr. Chinnery's qualifications, and his hypothesis has been published in the regular scientific literature, which is subject to peer review in being selected for publication. In the absence of any absolute standard, or any comparative standard of validity, the Board must weigh significantly the evidence that Dr. Chinnery's peers consider his hypothesis to be valid and valuable for the purpose of publication. While this alone does not demonstrate that Dr. Chinnery's methodology should be adopted for Seabrook, it has strong bearing on any threshold determination of its factual vailidity.

B. Dr. Chinnery's Methodology for Estimating Earthquake Return Periods is Fundamentally Sound and is the Only Method That Provides a Rational Basis for the Choice of a Safe Shutdown Earthquake.

(1) The Factual Validity of Dr. Chinnery's Methodology.

Dr. Chinnery's methodology consists of the following four elements, each of which is addressed in turn below:

a. The empirical observation that earthquake frequency and intensity appear to have a linear relationship.

b. The empirical observation that the linear relationship of frequency and intensity appears to have an essentially uniform slope in the Eastern United States, and perhaps in other areas, including California.

c. The lack of any basis for excluding the possibility that an earthquake of MMI XII can occur in the Seabrook area at some future time.

d. The extrapolation from the historical data on the linear curve of return periods of large earthquakes do not appear in the historical record.

(a) Linearity

The first element of Dr. Chinnery's approach is the examination of earthquake frequency versus intensity data for three areas of the country, which he undertook in two papers. The first was a 1973 study in Earthquake Notes which concerned earthquake statistics in New England (NECNP Ex. 1), and the second was his 1979 paper published in the Bulletin of the Seismological Society of America, in which he compared frequency-intensity data from the Southeastern United States, the Central Mississippi Valley, and Southern New England (NECNP Ex. 2). He concluded that the data demonstrated a remarkably linear relationship between frequency and intensity. His conclusion is consistent with the view of the vast majority of seismologists that a linear relationship exists between frequency and magnitude and with the opinion reached by several investigators that such a relationship exists between magnitude and intensity. (Chinnery, Direct testimony, p. 8-11, Tr. p. 197, l. 1-22).

The Applicant and Staff criticized Dr. Chinnery's use of the data, especially his failure to adopt the more complex relation-

ship indicated by the plots of earthquakes ranging from MMI I to MMI III, and the fact that he did not revise his plot based on recent downgrading of MMI VII earthquakes in the Smith catalogue. However, Dr. Chinnery effectively dealt with these points in his direct testimony. First, he explained that a relatively recent time period must be chosen for the lower intensities, and that even then the data are probably not complete since the lower intensities cannot be felt clearly, and probably have not all been reported. (Chinnery, Direct testimony, p. 8, Tr. p. 59, 155-56). Indeed, even Mr. Holt indicated that it is necessary to adopt a recent time period with respect to lower intensities. (Holt, Tr. p. 373). Second, Dr. Chinnery explained that the number of MMI VII events in the data base was probably unusually high, so that if some or all of these events were downgraded, the plot would become more, rather than less, consistent with the linear hypothesis. (Chinnery, Direct testimony, p. 8, Tr. p. 133, l. 16-20, p. 139, l. 1-7).

Dr. Chinnery concludes, respecting the Boston-New Hampshire zone, that the MMI IV data are probably also incomplete. Therefore, he is left with only two reliable data points, one for a MMI V and one for a MMI VI. As a result, he does not rely on those points to establish linearity for the zone, but on the fact that linearity is established elsewhere and is consistent with what is known of the New England data. (Chinnery, Direct testimony, p. 8).

Mr. Holt also attempts to attack the linearity hypothesis by arguing that Dr. Chinnery incorrectly restricted the time

are consistent with linearity, and provide one of the best linear fits Dr. Chinnery has seen for New England. (Chinnery, rebuttal testimony, p. 7, Tr. p. 110-111). Much the same is true of the Bloom and Erdman data, which the Staff cites to indicate a non-linear relationship. Again, these data are consistent with linearity, and the absence of a justification for a more complex relationship dictates that linearity be accepted. (Chinnery, Rebuttal testimony, p. 6, n. 1, Reiter, Tr. p. 510-512).[3/]

However, vastly more important than the Applicant and Staff's attempt to criticize particular pieces of data, is the fact that the scientific community has accepted the linear hypothesis. This is true not only of those whom Dr. Chinnery cites, but of Dr. Trifunac as well, who testified that reliance on linearity "is a very typical daily process used by hundreds of seismologists world wide and I meant to say that all of us do this and I just wanted to emphasize this." (Trifunac, Tr. p. 751, l. 22-24, 776). Indeed, even Dr. Jackson accepted linearity up to a point, although he would truncate the line.[4/] (Jackson, Tr. p. 573).

[3/] The Staff also attempts to create an argument against linearity based on the fact that the line will begin to curve downward slightly at the highest intensities as it approaches the upper bound, if there is one. (Staff Brief at p. 6). However, the change is very slight, and it has virtually no effect even at the intensity immediately below the upper bound. (Chinnery, Tr. p. 154-155). The more significant point to be drawn from the fact that the line should begin to curve near the upper bound is that there is no evidence in the data to indicate the existence of such a curve. This lack of a downward curve may well indicate that the data do not yet begin to approach the upper bound. This supports Dr. Chinnery's argument that the upper bound in New England is at least MMI X, if not greater.

[4/] Dr. Jackson's differing approach is relevant only to the separate issues of "upper bound" and "extrapolation."

(b) Uniform Slope of 0.57

The second element of the Chinnery methodology is the empirical observation that the linear frequency-intensity curve appears to have a uniform slope for the Eastern United States, and perhaps for the country as a whole. Dr. Chinnery demonstrated this phenomenon in his 1979 paper (NECNP Ex. 2), in which he found that slope values in the Eastern U.S. lie in the range of 0.54 to 0.60 and are consistent with his preferred slope of 0.57. (Chinnery, Direct testimony, p. 11). This appears to be true not only in the East, but in California as well (Chinnery, Tr. p. 186-187, NECNP Ex. 2, p. 765), although various areas of the East and West are quite dissimilar geologically and tectonically. Nonetheless, the data appear to demonstrate such a relationship. (Chinnery, Tr. p. 186-187, 197, l. 1-22).

Again, this hypothesis is widely accepted. For example, McGuire, on whom the Staff relied, proposes a uniform slope for the Eastern United States (Chinnery, Tr. p. 275-277, Reiter, Tr. p. 513-514), and even Dr. Reiter admits that the adoption of a uniform slope may be valid, although he believes it to be subject to substantial uncertainty. (Reiter, Direct testimony, p. 6).

Indeed, the issue of uniform slope is perhaps the least hotly contested in this case. However, the nature of the Staff's criticisms raise serious questions about whether the Staff was attempting to fulfill its mandate to assure a complete record for decision, or whether it was simply picking and choosing information for its own convenience to ensure that the Board would have to

adopt the Staff's position rather than make an independent assessment. In particular, Dr. Reiter cites slope values of 0.24 to 0.76 without giving any indication of the weakness of the claims at the extremes of that range. While he does later admit that the bulk of slope values fall between 0.4 and 0.6, he apparently has done no analysis of, and has provided the Board with no information on whether the narrower range is considered the scientifically significant range and is the one with which the Board should concern itself. As a result, he conveniently, but deceptively, leaves an impression of far greater uncertainty in the range of slope values than actually exists. This by itself is a serious shortcoming. However, it is compounded by the Staff's selective use of the McGuire article. Dr. Reiter apparently found it useful to do an independent determination of the impact of varying slopes, as discussed by McGuire, to use as a tool against Dr. Chinnery. However, at no time did Dr. Reiter undertake any study of the best method to determine the slope; nor did he undertake an independent evaluation of McGuire's preferred slope. (Reiter, Direct testimony, p. 5-6, Tr. p. 513-416). That analysis might well have provided important information to the Board, demonstrating that while there is a range of slopes and while variations in slopes may produce significantly different results, there is also relative agreement on what the slope should be. Dr. Reiter's failure to address that point raises serious questions concerning his credibility and that of the Staff as a whole. Indeed, it indicates that the Staff has not fulfilled its responsibilities in this case to act to increase public knowledge and to serve the public

interest.

Unfortunately, the Staff's gamesmanship with the evidence does not stop there. Rather, Dr. Reiter claims that Dr. Chinnery's estimates of return periods for earthquakes not contained in the historical record are based on only two data points at low intensities. (Reiter, Rebuttal testimony, p. 4). This is a gross distortion of the Chinnery methodology. Far from basing his return period estimates on two data points, Dr. Chinnery relies on data from three different areas of the Eastern United States. His 1979 paper refers as well to consistent slope findings by many other authors. Unlike the Staff, which narrowed its focus to a small area of New England and, in reality, relied solely on the maximum historical earthquake, Dr. Chinnery reached out to the entire seismological community and found significant support. It is simply unfair and inaccurate to characterize his conclusions as an extrapolation from two data points. To the contrary, it is significant that the only two reliable data points in the Boston-New Hampshire zone are completely consistent with the postulated uniform slope.[5/] Since the uniform slope and the reliability of the two data points are independent conclusions, the fact that they are perfectly consistent argues very strongly in favor of this hypothesis, as does the fact that the occurrence of the Cape Ann earthquake, which was not in the data base, is not inconsistent with the slope of 0.57. (Chinnery Tr., p. 265-267).

Finally, whether the range is considered to be from 0.24

5/ We note that while two seems a small number of data points, the universe of potentially reliable data points extends, at a maximum, to only four data points, MMI IV to VII.

to 0.76; 0.4 to 0.6; or 0.54 to 0.6, the uniform slope of 0.57 proposed by Dr. Chinnery leads to among the lowest probabilities and longest return periods for large earthquakes. (Holt, Tr, p. 376, Reiter, Tr. p. 517). If the contrary were true, and Dr. Chinnery were arguing the extreme case for high probabilities, the Staff and the Applicant would have a strong argument against him. However, from a large range of slopes proposed by a large number of scientists, Dr. Chinnery proposes one of the lowest probabilities of occurrence of large earthquakes, yet his results are apparently still higher than the probabilities asserted by the Applicant or the Staff.[6/] Dr. Chinnery's position can hardly be characterized as extreme in proposing this uniform slope, and the mandate for conservatism requires that it be considered factually valid.

(c) Upper Bound

As previously discussed, the testimony of Mr. Holt considerably strengthened Dr. Chinnery's assertion that there is no basis for deciding that an earthquake of MMI XII, or at least MMI X, cannot occur in the Seabrook area. Based on Mr. Holt's testimony, it appears that a MMI XII can be generated at the Seabrook site itself. Even in the absence of that evidence, however, there is no basis for adopting an upper bound of less than MMI XII.

As Dr. Chinnery has discussed in detail, it is not possible to demonstrate the existence of an upper bound, and, in particular, there is no geological basis for such a showing. (Chinnery, Direct testimony, p. 12-13, Tr. 101, l. 17-22). Indeed,

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6/ Of course, they avoid a probabilistic approach by simply adopting the largest historical earthquake, although they have no idea what its probability of recurrence may be.

Dr. Jackson agrees with him on the latter point. (Jackson, Direct testimony, p. 6).[7] Mr. Holt attempts to counter with the argument that there is no geologic evidence of large earthquakes in the area, (Holt, Direct testimony, p. 4-5), but he is easily and conclusively refuted by Dr. Chinnery's explanation that glaciers would have obliterated prior evidence and that the existing thin overburden is such that traces of an ancient large earthquake would be very difficult to recognize if they exist at all. (Chinnery, Rebuttal testimony, p. 13).

Perhaps the most telling evidence in the record is the opinion of a portion of the scientific community that extends beyond the advocates in this case. As reflected in Dr. Chinnery's direct testimony, the range of estimates of maximum possible earthquakes in the Cape Ann region shows that of ten experts solicited, five gave "Best Estimates" greater than MMI VIII, while three gave "Best Estimates" of MMI X or greater. When the "High Estimates" are also considered, eight of ten are higher than MMI VIII, and five of ten are MMI X or greater. Dr. Chinnery admits that he is at the high end, which is not a result of deciding that a MMI XII may occur but of an inability to preculde an MMI XII. Mr. Holt, by contrast, is the lowest. (Chinnery, Direct testimony, p. 12-13. Tr. p. 69-72, Holt, Tr. p. 388). As Dr. Chinnery suggests, the only reasonable conservative conclusion that can be drawn from this evidence is that the upper bound, if one exists at all, is at least MMI X, if not higher.

7/ Dr. Jackson does cite some studies of this issue in areas where surface rupture has occurred, but that is not the case in New England.

The Staff, by contrast, never really addresses this issue. Instead, it contents itself with leaving the impression that Dr. Chinnery is unreasonable because he would have all structures built to the largest possible earthquake, and because he does not leave room for engineering judgment to adopt a lower value. (Reiter, Direct testimony, p. 6-8, Jackson Direct testimony, p. 4-5). Again, the Staff grossly distorts Dr. Chinnery's position. Far from the Staff's suggestion, Dr. Chinnery offers a methodology on which engineering judgments can be rationally based, as opposed to a blind acceptance of the maximum historic earthquake in a region where geology provides virtually no useful information. Ultimately, as he must, Dr. Jackson admits that Dr. Chinnery does not propose that all nuclear power plants be built to the upper bound earthquake. (Jackson, Tr. p. 536).

Finally, whatever the upper bound may actually be, there is simply no basis for believing that it is within the historical record. (Chinnery, Rebuttal testimony, p. 4, Tr. p. 17-21) Given the short historical record, it is extremely unlikely on its face that the largest possible event has occurred within that period. It is this obvious fact that apparently leads Dr. Trifunac to conclude that any effort to identify an upper bound earthquake results "from a naive expectation that the past seismicity, geology and tectonics in the area can somehow be employed to compute the largest credible site intensity." (Trifunac, Direct testimony, p. 3). That is precisely Dr. Chinnery's point. This may also be the reason that the Staff has failed to calculate the upper bound

earthquake for tectonic provinces (Reiter, Tr. p. 529, l. 8-11, 541, l. 5-6), although that is the Staff's responsibility in attempting to determine maximum earthquake potential under Appendix A.

(d) Extrapolation Beyond the Historical Record

Extrapolating beyond the historical record is at once the most difficult and the most important task in attempting to determine the seismic risk at the site. The reason, obviously, is that it is difficult, if not impossible, to determine the accuracy of estimated return periods. At the same time, however, it is essential to make the attempt since, as the Staff admits, there may well be earthquakes in New England with return periods longer than the historic period of 300 years (Reiter, Tr. p. 500, l. 1-6), and such earthquakes may not appear in the historical record. (Reiter, Tr. p. 502, l. 8-12). If what Dr. Reiter says is true, and there is no reason to believe it is not, if a MMI IX earthquake has a return period of 500 years, or even 1500 or 5000 years, one could well occur during the life time of the Seabrook facility. Yet the NRC Staff has no method for predicting the return period for earthquakes (Reiter, Tr. p. 501, l. 9-11), and it has not undertaken a study of predictive methods other than Dr. Chinnery's to determine whether they are valid or not. (Reiter, Tr. p. 503, l. 5-24, l. 506, l. 13 - 508, l. 15). Not surprisingly, therefore, the Staff does not know the probability of exceeding the maximum historical earthquake in a given province. (Jackson, Tr. p. 540-541). This is particularly important for Seabrook, since the SSE is based on the implicit premise that the probability of exceeding precisely that earthquake is low

enough to allow it to be chosen as the SSE. (Jackson, Direct testimony, p. 5-6). The Applicant adds nothing to the analysis other than insisting that a MMI VIII cannot be exceeded, although its own geologic evidence indicates that a MMI XII can occur at the site.

We are left, then, in a vacuum, but with empirical observations of linearity and uniform slope, and the inability to establish an upper bound earthquake. These lead directly to the proposition that we can extrapolate from the historical record. Although we clearly cannot prove that the extrapolation is correct, it appears logical, and, even the applicant admits that no physical law precludes it. (Holt, Tr. p. 434-435). Indeed, Dr. Trifunac is more specific in stating that there is no physics that would require a change at higher intensities in what is otherwise a straight line function. While it may be that we do not yet understand the physical law that dictates the straight line relationship throughout the frequency-intensity range, it is always the case that the observation is made before the physical law is determined. To require a physical law at this stage of minimal knowledge of earthquake science would be to ignore the dictates of the Commission's remand and of Appendix A.

Although we cannot expand the historical record to demonstrate the accuracy of the extrapolation, we can work with available data not relied upon in determining the linear function in order to determine whether the data are consistent with the estimates. That is precisely what Dr. Chinnery has done in plotting the Cape Ann earthquake (Chinnery, Rebuttal testimony, Figure 3).

The Applicant argues that the presence of only one MMI VIII in England and Scotland in the historical record, when the Chinnery methodology would have predicted a higher intensity event in the time period, demonstrates that the extrapolation is incorrect. (Holt, Tr. 443-444). That proposition is the purest speculation from Mr. Holt, who provided no detailed plots of British earthquakes, did not reconcile foreign and American earthquake scales, did no work himself, and recalled his convenient conclusion vaguely from articles that he chose not to identify. That testimony simply carries no weight.

Both the Applicant and the Staff leap with glee on the fact that recent ten-year data in New England do not appear to be consistent with Dr. Chinnery's estimated return period from MMI's I-IV. However, Dr. Chinnery explained in his own direct testimony, and on cross-examination, that the recent instrumental seismicity can contribute little to the assessment of seismic risk, precisely because it is recent. He further suggested the recent data, and even data for the last century, may well be an anomalous cycle of low seismicity, particularly since it does not reflect the seismicity of the Cape Ann area, which is historically established. (Chinnery, Direct testimony, p. 2, 4, Tr. p. 320-321, 326-326, NECNP Ex. 1, p. 89) At this point the only testimony on the subject is that the instrumental data would not change Chinnery's conclusions in the absence of further information.

Finally, we return to the most telling points, which are that the data plots clearly demonstrate a recurrence relationship (Chinnery, Tr., p. 197, l. 1-22), that Dr. Chinnery's method of extrapolation is widely accepted in the scientific community (Trifunac, Tr., p. 751, l. 21-25), and that this is the only rational basis for estimating earthquake return periods and seismic risk that has been presented to the Board in this case. Since there is no physical law that prohibits the relationship, the record requires that the Board at least recognize it as a valid hypothesis.

(2) General Criticisms of the Chinnery Methodology

In addition to the points discussed above, the Applicant and the Staff leveled a number of other criticisms against the Chinnery hypothesis as applied to the estimation of the SSE for Seabrook. Each criticism reflects a shallow, and often deceptive, treatment of the underlying facts. Each is invalid.

(a) Dr. Chinnery addresses and admits uncertainties in his methodology more forthrightly and to a far greater degree than either the Applicant or the Staff.

Undoubtedly the greatest attempt on the part of the Staff and the Applicant to warp reality to suit their purposes is in the claim that Dr. Chinnery does not adequately take into account the uncertainties involved in his methodology. To the contrary, recognition of the need to address the uncertainties inherent in the consideration or prediction of seismic risk is at the very core of the Chinnery approach. Far from ignoring uncertainties, he discusses them throughout his work, and he recognizes the need to follow fundamental scientific principles such as Occam's Razor in attempting to draw valid conclusions in light of those recognized uncertainties. By contrast, the Staff and the Applicant utterly fail to address the multitudinous uncertainties involved in their approaches, or to attempt to account for them in reaching their conclusions.

Dr. Chinnery begins with the proposition that none of the available information-seismological, geological, or geophysical-is adequate to allow us to make precise decisions or predictions.

He then examines data sets for three geographic areas, which produce remarkably consistent results, and notes that these results are consistent with work done in other areas of the country, and with widely accepted hypotheses of linearity and uniform slope.[8/] Based on this analysis, he provides not a single falsely precise figure, but an empirical observation of linearity and a range of slopes from 0.54 to 0.6, of which he believes 0.57 to be the best for purposes of analysis, given the consistency of the data. (Chinnery, Direct testimony, p. 11 Tr. p. 302-303). Indeed, he specifically states that his slope of 0.57 is subject to an error of ±3, a point that the Staff ignores. (NECNP, Ex. 1, p. 97). In addition, he demonstrates the consistency of his approach with even wider error bounds, which would still produce nearly the same results. (Chinnery, Direct testimony, p. 14, Figure 4). He also recognizes the

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8/ The Staff suggests that Dr. Chinnery chose the three areas for study "so that the study would yield certain pre-conceived results." (Staff Brief at 21, n. 9). This charge is nothing short of shocking and grossly irresponsible. It is a personal attack on Dr. Chinnery in an area that is crucial to his reputation as a scientist, yet the Staff can provide absolutely no evidence to support its charge of bias. Rather, the Staff has breached the boundaries of acceptable argument by creating a slander from the most tenuous of theories. If anything, Dr. Chinnery is the only witness who had no personal stake in arriving at a particular conclusion. He is not opposed to Seabrook, and he did his analysis before he had any idea that he would testify in this proceeding. There is absolutely no reason to believe that he was doing anything more than attempting to analyze the data in manner that would allow increased understanding of seismic phenomena. By contrast, Mr. Holt is exceedingly well paid to produce theories that favor the Applicant, and the Staff's witnesses are bound to defend the Staff's position. Whether or not they have exhibited actual bias, they have the motive to do so, while Dr. Chinnery does not. In light of its public responsibilities, it is incumbent on the Staff to withdraw this thoroughly unfounded charge and to apologize to Dr. Chinnery.

range of possible upper bound earthquakes, and argues that it must be assumed that an MMI XII can occur in the area because no significant evidence exists to the contrary. Even so, this does not lead him to rigid adherence to that value as an absolute, but rather to the conclusion that the upper bound is at least MMI X, if not higher. (Chinnery, Direct testimony, p. 12-13).

More importantly, Dr. Chinnery clearly explains that the specific numbers that result from his probability analysis cannot be considered accurate in themselves, but represent a range or order of magnitude. This approach, by itself, indicates the likely uncertainty. (NECNP Ex. 2, p. 769, Chinnery, Tr. p. 92-93). The significant point is that a probability approach is an inherently more rational method of approaching an area in which significant uncertainties exist. It serves to give some feeling for possible error, while other approaches give no indication of what the error might be and provide a false impression of precision. (Chinnery, Rebuttal testimony, p. 15, Tr. p. 120, l. 1-6, Trifunac, Direct testimony, p. 1, Tr. p. 732, 776).

The contrast between Dr. Chinnery's openness, which is highly unusual in an adjudicatory proceeding, and the lack of any effort on the part of the Staff or the Applicant to reflect, or even admit uncertainties in their approaches is striking. The Staff claims the authors of Appendix A believed that the likelihood of the maximum historical earthquake is sufficiently low that it can be accepted as the SSE. (Jackson, Direct testimony, p. 5-6). However, the Staff does not know the probability of exceeding the maximum historic earthquake

at Seabrook (Jackson, Tr. p. 538-539), and it admits that the probability can vary from province to province. (Jackson, Tr. p. 540-541). Indeed, the Staff has made no effort to determine how best to calculate slope values for particular regions. Moreover, it has not calculated the parameters of seismic risk for Seabrook, and it has no method for predicting return periods of earthquakes, including those that it chooses as the SSE. (Reiter, Tr. p. 513, 531, 501, Jackson, Tr. p. 514). Finally, the Staff is forced to admit that it follows a mechanistic process leading to results for which it is impossible to determine the uncertainties, (Reiter, Tr. p. 601-603).

The Applicant is in much the same situation as the Staff. While Mr. Holt does not even know whether the data from the Charleston earthquake on which he relies are from aftershocks or independent events, he is prepared to develop a radical theory based on those data. (Holt, Tr. p. 370, Direct testimony, p. 2). In addition, when given an opportunity to provide "High", "Low" and "Best Estimates" for the largest possible earthquake in the Seabrook area, he designates only one value, and then admits to no error, although that value is the lowest of all of the experts participating in the study. (Holt, Tr., p. 387). Unlike Dr. Chinnery's designation of a MMI XII, which is based on a recognition of uncertainties and his inability to be precise, Mr. Holt's choice reflects the view that a single value can be chosen because there are no uncertainties. Mr. Holt's result simply is not credible.

The reality is that only Dr. Chinnery (and Dr. Trifunac as well) addresses and admits uncertainties. The Staff and the Applicant ignore them and attempt to achieve a false precision that is comforting to the mechanics of regulation and to use in adjudicatory argument, but that does not comport with the truth.

(b) Dr. Chinnery fully considers geology to the extent possible.

Dr. Chinnery has been criticized for failing to consider geology in his analysis, or to use the Applicant's term, for failing to admit that geology has a place in the analysis. Again, the truth is ignored.

Dr. Chinnery virtually initiates his testimony with geology, and discusses our inability as yet to develop a detailed correlation of seismology and geology in New England, and the lack of geological evidence of an upper bound to earthquakes in New England. He also notes geological evidence of large earthquakes is meaningless in attempting to determine an upper bound. Since we do not understand earthquake mechanisms in New England, he adds, geology is of little use to our analysis. (Chinnery, Direct testimony, p. 1, Rebuttal testimony, p. 3, l. 10-13, Tr. p. 27, 101, 107, 300). In addition, he specifically notes, as discussed earlier, that the uniform slope hypothesis appears to hold true regardless of geological variations. (Chinnery, Tr. 197-188, 197). With the exception of Mr. Holt's pluton theory, which would establish an SSE of MMI XII, neither the Staff nor the Applicant challenges the proposition that we do not understand the seismology-geology relationship in New England.

Nonetheless, the geology argument hinges on the proposition that geology can provide useful information for this analysis. Apparently, the Applicant and Staff believe that if a larger earthquake were possible, they would find geologic evidence to that effect. But even they must admit that is not the case. Although the MMI X earthquake that occurred at Charleston left geological evidence, Mr. Holt testified that there is no geologic evidence of prior large earthquakes in the area. That means that before the MMI X earthquake occurred at Charleston, Mr. Holt and the Staff would have argued that it could not occur. (Holt, Tr. p. 405-406). Similarly, Dr. Jackson, despite valiant attempts to evade the question, is unable to point to geologic evidence in New England of the MMI VIII that the Staff adopted as the maximum historic earthquake for the area. (Jackson, Tr. p. 609-619).

The geology argument is put to rest by the fact that the Staff adopted the maximum historical earthquake as the SSE (Peiter, Tr. p. 525) without the use of geological evidence, and did not increase or otherwise alter the SSE by using geology. This is absolutely consistent with Dr. Chinnery's explanation that we do not know how the geology of New England relates to earthquake generation. That being the case, the Staff could not use geology to increase the SSE. It is ludicrous to suggest, however, that this lack of knowledge in one area means that valid predictions cannot be made using another method, particularly when that method seems applicable regardless of geology.

(c) Dr. Chinnery's probabilities for the Boston-New Hampshire zone must be assumed to apply to the Seabrook site.

The Applicant and the Staff argue that whatever Dr. Chinnery's probabilities may be, they cannot be used for Seabrook because they apply to the entire Boston-New Hampshire region, not to the site itself. This is incorrect as a matter of law since Appendix A requires that once the highest intensity earthquake is determined, it must be assumed to occur at the site. It is also basically incorrect as a matter of fact, as Dr. Chinnery has demonstrated.

The process for determining the Safe Shutdown Earthquake under Appendix A is set out at §V(a)(1)(i)-(v). The first step is to determine the maximum historic earthquake. The second step is to determine whether geological or seismological information indicate that a higher intensity should be chosen than that for the maximum historic earthquake. The third step, if the maximum historic earthquake cannot be correlated with a tectonic structure or a different tectonic province, is to assume that the earthquake (as increased by geological or seismological data) occurs at the site. The only function of the Chinnery methodology is to help determine how much the intensity should be raised under the second step. The third step of assuming occurrence at the site remains the same, consistent with the conservative mandate of Appendix A.

Even if Appendix A did not require assuming the probability at the site, however, it would make little difference. As Dr. Chinnery testified, the occurrence of an MMI X anywhere in the Boston-New Hampshire zone would cause an MMI IX over the entire zone, so that the probability of occurrence of an MMI IX at the site is at least equal to the probability of occurrence of an MMI X anywhere in the area, which is 10^{-4}. (Chinnery, Tr. p. 298-200). Of course, since there is also an independent probability for the occurrence of an MMI IX itself within the province, the actual probability of occurrence of an MMI IX at Seabrook is greater than 10^{-4}. In the absence of a sophisticated calculation of MMI IX probability at the site, which would be substantially more complex than the simple division performed by Mr. Holt (Chinnery, Tr. p. 285-286), it is both reasonable and conservative to accept as valid a probability of 10^{-3}, (or slightly less) for the occurrence of an MMI IX at the Seabrook site.

(d) The record does not support the argument that less damage would occur to a nuclear plant sited on bedrock than on alluvium.

Much of the thrust of Mr. Holt's attack on Dr. Chinnery is to the effect that Dr. Chinnery's data derive largely from earthquakes in alluvium, while equivalent intensity earthquakes would cause less damage at Seabrook since the plant is built on bedrock. In fact, Mr. Holt's evidence is extremely sparse and anecdotal, and he fails to provide any information relevant to the siting of a nuclear reactor.

Mr. Holt's argument is based almost entirely on brief descriptive accounts of damage in different areas as a result of a Canadian earthquake in the St. Lawrence valley. His favorite example alleged to make his case is that a grain elevator on alluvium was severely damaged, while the Chateau Frontenac, standing nearby on bedrock, was not affected. (Holt, Direct testimony, Appendix A). While Mr. Holt attempted on the spur of the moment to dismiss the possible effects of focusing which might explain the difference, in fact he has done no careful study of the question, and he was unable without prompting to remember the relative locations of the faults and structures involved. (Holt, Tr. p. 352, 410-412). More importantly, he provided no information with which the Board can judge whether either the Chateau Frontenac or the grain elevator is structurally comparable to a nuclear power plant. Since a nuclear power plant is a stiff structure that reacts like a low building, it is likely that the Chateau Frontenac does not have the same resonating frequency as Seabrook would have, and therefore would not be subject to the same damage. Without that sort of development, Mr. Holt's testimony on this point is irrelevant.

The soil-bedrock argument becomes particularly weak when it is recognized that nuclear plants are subject to the high frequency end of the specturm. As Dr. Trifunac testified, we can expect higher frequencies, and higher accelerations associated with those frequencies, on bedrock than on

soil. (Trifunac, Tr. p. 783-784). That being the case, the evidence indicates that greater damage can be expected to a nuclear power plant on the Seabrook site than on an alluvium site. Therefore, we should be more, rather than less concerned with the hazard. The most reasonable conclusion at this point is that the evidence is simply too weak to reach any useful conclusions about how the character of the site affects the seismic hazard or risk.

(e) The Staff and the Applicant must undertake a thorough reevaluation of the seismic design of Seabrook based on the return periods estimated by Dr. Chinnery.

Ranging outside the scope of the proceeding as established by the Commission,[9] both the Staff and the Applicant argue that even if the Chinnery hypothesis is valid, it will have no effect on the seismic design of Seabrook. (Reiter, Direct testimony, p. 11-16, Jackson, Direct testimony, p. 9-12, Knight, Direct testimony, Holt, Direct testimony, p. 5-7). These arguments fail because they have not been adequately developed on this record with specific reference to Seabrook, and because they ignore the principle of defense-in-depth that is central to the Commission's approach to protecting the public health and safety. However, we see a glimmer of hope in these approaches, at least as discussed by the Staff, in that they represent an effort at reasoned scientific analysis, as opposed to blind reliance on the maximum historic earthquake and the scientifically baseless contention that the probability of such an earthquake is low enough to

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[9] See argument at p. 47-48, below.

justify its choice as SSE. As such, these approaches should be pursued in full at the conclusion of this phase of the Seabrook proceeding.

The Staff's position on this point is based on four propositions:

1. Probabilities of earthquake recurrence are subject to such uncertainties that they cannot be used directly in determining a maximum intensity earthquake on which to base the choice of a response spectrum anchor point.

2. If the Chinnery hypothesis is to be used, it must be incorporated into a general probabilistic methodology such as that developed in the Site Specific Spectra Program (SSSP).

3. The SSSP method results in a response spectrum that falls below the Seabrook spectrum as now established.

4. In any event, there are such conservatisms in plant design and construction that the plant will be safe even if the design basis earthquake is exceeded.10/

At this point, these arguments are not sufficient to establish the SSE or the response spectrum for Seabrook, particularly in light of Dr. Trifunac's demonstration of the high probability of exceeding the existing response spectrum.

We would generally agree with point 1, but only if it is made in combination with point 2 and in connection with rejection of the mechanistic and scientifically unfounded approach of deriving an anchor point from the maximum historic earthquake. The method that was used to determine the SSE and design response spectrum for Seabrook was simply to adopt the maximum historic earthquake and scale the R.G. 1.60

10/ Points 1 and 2 are in Reiter, Direct Testimony, p. 12-15; point 3 is in Reiter, Direct testimony, p. 15-16 and Jackson, Direct testimony, p. 9-12; point 4 is Knight, Direct testimony. Holt's argument is similar to point 3, but will not be discussed separately, particularly since it completely fails to reflect uncertainties, and was developed by Mr. Holt alone, while the Staff's approach involved canvassing a reasonably broad spectrum of scientific opinion.

response spectrum to an anchor point based on the intensity of that earthquake. To the extent that is the method relied upon to justify the Seabrook design, it is far more rational to choose the maximum earthquake based upon the probabilities established by Dr. Chinnery than on the size of the largest earthquake in an extremely short historic record. That is particularly true in New England, where geology offers no guidance in determining whether a larger earthquake should be chosen. We certainly recognize the uncertainties involved in moving from the chosen intensity to ground motion, but if the Staff method of doing so is considered acceptable, the Chinnery method is superior, and must be adopted. It would then be up to the Board and the Commission as a matter of regulatory policy to establish the acceptable probability level for the earthquake whose intensity governs seismic design. A probability of 10^{-4} would be an absolute maximum given the "incredibility" goal of 10^{-7} and the uncertainties inherent in the remainder of the design process. At Seabrook, therefore, an earthquake of MMI X must be used as the source of the anchor point under the Staff's method of determining ground motion.

If point 2 were added to point 1, and the method used by the Staff to establish the SSE for Seabrook were rejected, not only would we agree, we would strongly urge the Board and the Commission to pursue a general probabilistic methodology such as that used in the SSSP. Similar to the Trifunac

approach, it is attractive in that it incorporates probabilities to provide some rational basis for the design, and it takes into account a broad range of scientific opinion rather than relying primarily on the Staff and Applicant. This would undoubtedly represent progress; we urge that a reanalysis of Seabrook be undertaken using this approach.

The Staff suggests that this analysis has already been done by Lawrence Livermore Laboratories using the SSSP information and that the results demonstrate the acceptability of the Seabrook response spectrum as previously determined. (Reiter, Direct testimony, p. 15-16). While a preliminary reading has apparently been developed, no "report" detailing how the work was done or allowing either the parties or the Board to examine the validity of the result has been presented. The Board simply does not have before it information adequate to support the Seabrook design spectrum based on the SSSP, particularly when the use of an MMI X earthquake to determine the anchor point, as would be done under the current Staff methodology, would clearly result in a design response spectrum exceeding the one previously established. In addition, since, as discussed below, Dr. Trifunac indicates a relatively high probability of exceeding the current response spectrum, the seemingly contradictory SSSP results must be examined very carefully to determine whether conservatism and uncertainties are adequately considered.

Finally, the Staff makes much of the substantial conservatism inherent in the design of Seabrook and argues that the plant will be safe even if the design basis is exceeded. (Knight, Direct testimony). This ignores the entire concept of defense-in-depth. It may well be true that nuclear plants are overdesigned such that in all cases they can withstand earthquakes greater than the design basis. If that is the case, however, the reason is that the uncertainties of seismology and of nuclear technology demand caution. The governing principle of defense-in-depth is that these conservatisms exist once the design basis is determined. However, they may not be considered in determining the design basis itself. Stated otherwise, whether the design basis earthquake is a MMI IV or a MMI X, the conservatisms identified by Mr. Knight are assumed in the design of the plant. (Knight, Tr. p. 687, l. 25-688, l. 3). Only after the design basis is determined do these conservatisms come into play. In this case, they must be ignored until the design basis intensity or response spectrum is arrived at based on seismic information.

IV. The Staff's Methodology For Correlating Vibratory Ground Motion With the SSE Is Scientifically Invalid and Has Resulted In A Design Response Spectrum With A High Probability Of Being Exceeded.

The second issue demanded to the Appeal Board for consideration is the question of whether the Staff's methodology for correlating vibratory ground motion is consistent with Appendix A, with particular reference to "the relation

between the mean of the maximum ground accelerations and maximum effective ground acceleration." 12 NRC at 298. The record establishes that the Staff's methodology is scientifically invalid, and therefore inconsistent with Appendix A, and that there is an unacceptably high probability of exceeding the response spectrum in the frequency range of interest during the life of the plant.

The Staff's methodology has been adequately described in its Brief (Staff Brief at 29-31), and consists of the following:

1. Choice of the SSE,

2. Using the trend of the means of Trifunac and Brady (1975) to provide a peak acceleration anchor point for the design response spectrum,

3. Use of the R.G. 1.60 response spectrum to determine the maximum level of response to ground motion that is assumed for the purpose of the design.

The issue of "maximum effective ground acceleration" appears to have arisen with respect to the second step as a result of the fact that §VI(a)(1) of Appendix A states that the vibratory ground motion of the SSE

> shall be defined by response spectra corresponding to the maximum vibratory accelerations at the elevations of the foundations of the nuclear power plant structures.

(emphasis supplied). The Staff approaches this requirement by arguing that the highest possible accelerations have no effect for various reasons, and that the trend of the means of Trifunac and Brady provides a reasonable approximation of

the maximum effective acceleration; or what the Staff terms the maximum acceleration "of engineering significance." (Reiter, Direct testimony, p. 20-22). The Staff also attempts a comparison of its own R.G. 1.60 spectrum anchored at 0.25g with a purportedly directly estimated mean plus one sigma response spectrum. (Reiter, Direct testimony, p. 23-25).

Dr. Trifunac's testimony establishes that the Staff's methodology is invalid and that significantly more analysis is required to determine the acceptability of the Seabrook design response spectrum. Dr. Trifunac's difficulties with the Staff's methodology begin with the fact that its mechanistic step-by-step process yields a falsely precise result, when our knowledge of the area does not allow such precision. (Trifunac, Direct testimony, p. 1). This leads Dr. Trifunac to conclude that the Staff's approach should be rejected altogether in favor of a probabilistic analysis that takes uncertainties into account. (Trifunac, Direct testimony, p. 3-5).

However, recognizing that the Staff approach may have some utility if R.G. 1.60 is scaled correctly, Dr. Trifunac addressed that methodology as well. Referring to the choice of anchor point, Dr. Trifunac explained that the spectra being anchored were developed based on the use of actual peak accelerations, so that the use of some lower value is "totally inacceptable." (Trifunac, Tr. p. 733-737). He added that neither the concept of "effective peak acceleration," nor the Staff's formulation of "maximum acceleration of engineering

significance" permitted the use of a lower anchor point because the terms are essentially meaningless, avoid the physical basis of the problem, and allow unwarranted freedom for expert judgment. (Trifunac, Direct testimony, p. 4).11/ Indeed, Dr. Trifunac rejected the use of the trend of the means of the data in his own 1975 study as a basis for determining the anchor point. (Trifunac, Tr. p. 797-798).

In addition to challenging the choice of anchor point, Dr. Trifunac strongly criticized the use of R.G. 1.60 itself because it does not have the correct shape. Rather than representing the correct average plus one standard deviation, this spectrum is normalized and is much closer to an average acceleration at the short period end, which is the end of the spectrum of interest for nuclear power plants. (Trifunac, Direct testimony, p. 4-5, Tr. 740-741). The result is that the spectrum is less conservative in the area relevant to reactor design, and that lack of conservatism is componded by improper scaling to a value below the actual peak acceleration on which the R.G. 1.60 spectrum was based.

The staff argues in support of its use of the trend of the means that the absolute maximum actual accelerations clearly do not affect the facility and should not be considered. However, even that argument is weakened by the fact that the Staff did not have any sound theoretical basis for its

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11/ Remarkably, the Staff agrees that the "effective acceleration" concepts are essentially meaningless. (Knight, Tr. p. 670, Jackson, Tr. 694).

approach and simply used the Trifunac and Brady data because they were the only data available at the time. (Reiter, Tr. p. 647, l. 12-15). However, even if we assume that the Staff is correct and the actual peaks should not be used, the conclusion we must reach is not that some lower number chosen virtually at random should be employed, but that the attempt to scale R.G. 1.60 should not be allowed if it cannot be done consistent with the basis on which the spectrum was orginally developed.

Instead, as Dr. Trifunac argues, we must deal directly with the distribution of ground motion that is involved in seismic risk. Remarkably, Dr. Reiter agrees on his crucial point. (Reiter, Tr. p. 693-694). When Dr. Trifunac performed a direct estimation of the design spectrum using a probability distribution, he found that there is a 5% probability of exceeding the Seabrook response spectrum in the frequency range of interest during the life of the plant if the maximum possible earthquake in the area is MMI XII. If the maximum possible earthquake in the area is MMI X or MMI VIII, the probability of exceedence is less than a few percent. (Trifunac, Direct testimony, p. 5-9, Figure 3, Tr. p. (753-755). In fact, when Dr. Trifunac incorporates pessimistic assumptions into his analysis, as he believes is necessary to account for uncertainties and gaps in our knowledge, the result is that the Seabrook spectrum has a 5% chance of being exceeded if the maximum possible earthquake is MMI

VIII, and substantially higher probability if the maximum is higher. (Trifunac, Direct testimony, figure 4). These results lead Dr. Trifunac to conclude that the Seabrook design spectrum "may" be acceptable, but that he would have to do more detailed calculations to determine whether his assumptions are sufficiently pessimistic. (Trifunac, Direct testimony, p. 10).

The fundamental conclusion from this record is that the Staff methodology is invalid and that further analysis is required to determine an acceptable response spectrum for Seabrook. It does appear that the Staff's R.G. 1.60 spectrum anchored to 0.25g may be in the correct neighborhood, but given the scientifically unsound approach to its development, this seems to be little more than an accident. Nuclear plants are not designed by accident and must not be licensed without a sound scientific and rational basis for judgement that the public health and safety will be adequately protected.

Dr. Trifunac provides a method for making that judgment. His own conclusion, based on what he admits to be cursory analysis that may not be adequately pessimistic, is that there is approximately a 5% chance of exceeding the Seabrook design response spectrum during the life of the plant, which translates into a probability of 5×10^{-2} of an earthquake exceeding the design basis of the plant.[12/] We do not know

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12/ Even the probabilities assuming a maximum earthquake of MMI X or VIII are approximately 1×10^{-2}, a significant probability in the context of determining the design basis of a nuclear power plant.

the basis for Dr. Trifunac's suggestions that this initial analysis indicates that Seabrook's response spectrum may be acceptable, and he provides none. Certainly that is inconsistent with the concept of defense-in-depth, which requires a substantially lower probability in order to achieve an overall likelihood of no greater than 10^{-7} of exceeding the design basis of the plant. In any case, Dr. Trifunac believes that further analysis is necessary to determine whether his figures are correct. We certainly agree. At this point, the record requires the conclusion that the present design spectrum is based on a methodology that has no scientific basis and that Dr. Trifunac's approach may provide an acceptable means of judging its adequacy. However, Dr. Trifunac's analysis must be carried out in full in order to reach a valid conclusion. In addition, the Board and the Commission must address the question of the acceptable level of probability that the response spectrum will be exceeded. Since it constitutes the design basis of the plant, NECNP asserts that the probability must be no greater than 10^{-7} of an accident, and substantial additional complex analysis is required to determine the point at which that probability is reached.

V. The Pending Motions to Strike Should Be Granted

When the Applicant and Staff testimony were offered during the hearing, NECNP objected to various portions on three separate grounds. In order to expedite the hearing, however, NECNP

agreed that the Board should withhold decision pending post-hearing briefs. NECNP now renews those objections and motions to strike.

A. Testimony Discussing the Impact of the Chinnery Hypotheses Is Beyond the Scope of this Proceeding.

The first area in which NECNP objected to Staff testimony is their efforts to address the impact of the Chinnery methodology, if it is assumed to be valid. The objection and motion to strike applies to Question and Answer 9 at page 11 of Dr. Reiter's Direct testimony and to Question and Answer 5 at page 9 of Dr. Jackson's Direct testimony. (The discussion of the objections begins at Tr. p. 467.)

The essence of the objection is that this testimony is beyond the narrow scope of the proceeding as remanded by the Commission and interpreted by the Appeal Board. There are but two narrow questions here, (1) the validity of the Chinnery hypothesis, and (2) the consistency of the Staff's methodology of correlating vibratory ground motion. Once the validity of the Chinnery hypothesis is established, the first question is resolved, and the second question clearly does not extend to this testimony.

NECNP originally believed that the scope of the proceeding extended beyond the validity of the Chinnery hypothesis, at least to the extent that it encompassed the question of whether the Chinnery hypothesis was superior to methods used by the Staff or the Applicant. However, the Staff itself successfully refuted this notion in its arguments to the Board on NECNP's discovery request. It is truly remarkable that the Staff

should at once restrict the scope of the hearing as it applies to NECNP and then expand the scope for its own purposes.

It is doubly remarkable that the Staff should then question whether fundamental fairness requires the exclusion of its testimony. If that question is to be reached, it must be judged from the point of view of NECNP, which is the affected party. NECNP finds itself in the position of having asserted that the comparative merits of various approaches to determining the SSE are relevant to judging the validity of the Chinnery hypothesis. The Board may have viewed the Commission's remand more narrowly, but at least NECNP's argument was related to attempting to make a judgment on the validity of the Chinnery hypothesis. The Staff's testimony objected to here is in no way related to attempting to answer that question. Rather, it assumes the answer and adds to the hearing an additional issue which the Commission did not discuss and of which NECNP had no previous notice. To allow the testimony to stand would constitute a fundamental violation of the NECNP's procedural rights.

B. Appendix A Requires that Earthquake Probabilities for the Province be Assumed at the Site and Prohibits Attempts to Reduce those Probabilities.

NECNP objected to portions of Mr. Holt's testimony (pages 1 and 4 of Direct testimony, see Tr. p. 333 for discussion) and Dr. Reiter's testimony (Question and Answer 2 of Rebuttal testimony, see Tr. p. 488 for discussion, and Tr. p. 518, l. 7-12) on the ground that they represent attempts to avoid the require-

ments of Appendix A by determining earthquake probabilities for particular sites rather than for the tectonic province as a whole. Those portions of their testimony must be stricken for that reason.

As we have previously discussed, p.33-34, Dr. Chinnery's hypothesis provides a means for making a rational choice of the maximum earthquake intensity as required by §V(a)(1)(1) of Appendix A. In effect, it provides a scientifically valid basis for deciding whether the intensity of the maximum historic earthquake or some larger intensity should be chosen for the purpose of analysis under that section. Whichever method is used, however, subsection (ii) requires that where the earthquakes cannot be correlated with particular structures within the province, they must be assumed to occur at the site. That requirement applies to the maximum historic earthquake and to any larger earthquake that may be chosen based on geological or seismological data.

This requirement reflects and is essential to the conservatism of Appendix A. In adopting the regulation, the Commission decided that our knowledge of earthquake science is such that unless we can specifically relate a likely earthquake to a particular structure, we must assume that it will occur at the site. Just as the unknown probability of the maximum historic earthquake was assumed at the site, so identified probabilities of larger earthquakes must be assumed there as well.

C. Mr. Knight's Testimony Concerning Conservatisms Inherent In the Design Basis is Irrelevant To Determining What The Design Basis Should Be.

NECNP's final objection is to the testimony of Mr. Knight, which relates almost entirely to the extent of conservatism in the design of the plant, which is the same as the inherent in any nuclear power plant. Again, this testimony is beyond the scope of the proceeding. The question here is not what conservatisms should be adopted once the design basis is established, but how to establish the design basis itself. Any conservatisms that are inherent in the design are assumed as part of the concept of defense-in depth and may not be considered in deciding what the basis of the design should be.

The Staff virtually admits that Mr. Knight's testimony is beyond the scope of the hearing when it indicates that his testimony was filed because the Staff did not know whether Dr. Chinnery or Dr. Trifunac would challenge the adequancy or conservatism of the design itself. (Staff Brief at 44) Of course, the Staff had no reason to believe that either witness would challenge the conservatism of the design since that issue is not within the scope of the proceeding. Neither the Commission nor the Board mentioned anything like it in their prior rulings, and again, NECNP would be severely prejudiced by having this testimony admitted without adequate prior notice.

CONCLUSION

For the reasons stated above, NECNP urges the Appeal Board to find that Dr. Chinnery's probabilistic hypothesis is factually valid, that the Staff's method of correlating vibratory ground motion with the SSE is scientifically invalid and, therefore, inconsistent with Appendix A, and that NECNP's motions to strike should be granted. These findings will then require a thorough reanalysis of the Seabrook seismic design basis, taking into account both Dr. Chinnery's hypothesis and Dr. Trifunac's methodology for estimating the probability of exceeding the established response spectrum.

Respectfully submitted,

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DATED: July 15, 1981

UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING APPEAL BOARD

In the Matter of	)	
PUBLIC SERVICE COMPANY OF NEW HAMPSHIRE, et al.	)	Docket Nos. 50-443 50-444
(Seabrook Station, Units 1 and 2)	)	

CERTIFICATE OF SERVICE

I hereby certify that a copy of "New England Coalition on Nuclear Pollution Proposed Factual and Legal Findings on Remanded Seismic Issues and Supporting Argument" was mailed first class postage-prepaid this 15th day of July, 1981 to the following:

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