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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)	)	

PROPOSED FINDINGS OF FACT, CONCLUSIONS OF LAW, AND SUPPORTING ARGUMENT OF THE NRC STAFF ON REMANDED SEISMIC ISSUES

I. STATEMENT OF THE CASE AND REFERENCE TO RULINGS

By "Order" issued September 25, 1980,[1/] the Commission acted upon the petition of the New England Coalition on Nuclear Pollution (hereinafter "NECNP") for review of the Appeal Board's determinations concerning "the seismic design of the Seabrook Nuclear Plant." By a divided vote the Commission ordered the Appeal Board to reopen the record, to take further evidence, and to reconsider its opinion with respect to two discrete seismic issues which were part of the Appeal Board's prior seismic determinations on all of the seismic questions litigated below. The two reopened seismic issues were: (1) the "factual validity" (or lack thereof) of Dr. Michael Chinnery's methodology that there is an empirical relationship between earthquake intensity and

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1/ Public Service Company of New Hampshire, et al. (Seabrook Station, Units 1 and 2), CLI-80-33, 12 NRC 295 (September 25, 1980).

earthquake recurrence time; and (2) whether the Staff's methodology for correlating vibratory ground motion (acceleration) is consistent with Appendix A to 10 C.F.R. Part 100. With respect to the second issue the Commission stated that ". . . the parties should provide a discussion of the relation between the mean of the maximum ground accelerations and maximum effective ground acceleration." 12 NRC at 298. An evidentiary hearing to consider the Commission-remanded seismic issues was held in Nashua, New Hampshire before the Appeal Board from April 6 to April 9, 1981. Participating in the proceeding were the Permittee, NECNP, and the NRC Staff.

The April, 1981 hearings on seismic issues were preceded, initially, by hearings conducted by the Licensing Board presiding over the construction permit application. On June 29, 1976, that Licensing Board rendered its initial decision, in which it concluded, inter alia that the seismic design of Seabrook as Modified Mercalli Intensity (hearafter "MMI") VIII, .25g. Regulatory Guide 1.60 was acceptable. 3 NRC at 868-77, 919-22. In so concluding, the Licensing Board rejected a number of challenges to the seismic design of Seabrook, including two matters now at issue. It rejected the theory of Dr. Chinnery that the seismic design of Seabrook must account for the possibility of a MMI IX or greater earthquake in the seismic zone containing the Seabrook site. 3 NRC at 920. The Licensing Board also rejected NECNP's contention that assuming that the Safe Shutdown Earthquake (hereafter "SSE") was a MMI VIII, that the Reg. Guide 1.60 spectrum should be anchored at 0.4g, rather than the Staff approved 0.25g. 3 NRC at 871, 921-22.

On July 26, 1977, the Appeal Board by a divided vote affirmed those findings of the Licensing Board. See ALAB-422, 6 NRC 33, 54-65 (1977) and ALAB-561, 10 NRC 410, 436-a through 436-h (1979). Dissenting Appeal Board member Mr. Farrar set forth his views in summary form at 6 NRC 106, 111-113 and in detail at 10 NRC 411-16, 420-35. The Appeal Board majority concluded that Dr. Chinnery's methodology was "both technically deficient and inconsistent with Appendix A." 6 NRC at 60; see 10 NRC at 436-a to 436-f. It also found the 0.25g design value to be acceptable. 6 NRC at 63; see 10 NRC at 436-b.

The Staff submits that on the basis of the prefiled testimony and the evidentiary hearing convened in Nashua, New Hampshire in April, 1981, that it would be reasonable to conclude: (1) that Dr. Chinnery's methodology lacks validity for current use in determining the seismic design of the Seabrook nuclear power plant; (2) and that the Staff's methodology for correlating vibratory ground motion is consistent with the terms and requirements of Appendix A. The Staff also submits that there is no evidence of record that the present seismic design of Seabrook is other than acceptable, safe, and conservative.

II. STATEMENT OF THE ISSUES PRESENTED FOR REVIEW

As previously stated, the Commission has ordered the record reopened with respect to two discrete seismic issues:

1. Whether the methodology of NECNP witness Dr. Michael Chinnery that there is an empirical relationship between earthquake intensity and earthquake recurrence time has "factual validity," and

2. Whether the Staff's methodology for correlating vibratory ground motion (acceleration) is consistent with Appendix A to 10 C.F.R. Part 100.

III. DISCUSSION

A. The Factual Validity of Dr. Chinnery's Methodology

Dr. Chinnery's methodology may be considered as consisting of four basic assumptions, each of which must be correct assumptions in order for the results of the methodology, or his conclusions, to be accurate (Chinnery, Tr. 90-91). The four basic assumptions which both describe and are inherent in Dr. Chinnery's methodology are:

1. In a given seismic region during a given period of time, there is a linear relationship between epicentral intensity of earthquakes and their frequency of occurrence; and

2. The data are consistent for a uniform linear slope, or "b" value of 0.57 for all regions (both eastern and western U.S.). Even in areas of little data, a uniform slope of 0.57 may be used to construct local frequency-intensity relationships; and

3. There exists no upper bound to earthquake size in any area. To put it another way, Dr. Chinnery believes that there is no satisfactory way to establish the existence of an upper bound to his assumed linear frequency-intensity relationships; and

4. Frequency-intensity data may be extrapolated linearly to predict the probability of occurrence of earthquakes larger than those in the historical record. In particular, the probabilities of occurrence of

Modified Mercalli Intensity IX & X earthquakes in New England may be predicted through linear extrapolation of the 160 years of historical data during which no such events occurred (Reiter 3; Chinnery 8-13; Chinnery Ex. 2 at 757; Chinnery Rebuttal 5-14).

1. Linearity

As to the first assumption, i.e., linearity, there is no basis to accept Dr. Chinnery's position that "he doesn't have to justify the linearity assumption because he believes its an accepted observation" (Chinnery, Tr. 303). Indeed, other valid relationships have been proposed by seismologists to explain deviations of data from Dr. Chinnery's proposed simple linear relationship, particularly at larger magnitudes or higher intensities (greater than MMI VI) (Reiter 4). The proposed relationships, other than linear, include truncated linear, bilinear, and higher order relationships; new forms of such relationships, other than linear relationships, are continually being proposed (Reiter 4-5). Even Dr. Chinnery agrees that the question of whether the Modified Mercalli Intensity scale continues to be linear beyond MMI VIII is a problem. (Chinnery, Tr. 223). On this precise question he testified, "I'm not sure that one can prove anything along these lines but I think one has to take the data" (Chinnery, Tr. 223).

The proposed linear relationship is constructed by Dr. Chinnery by dividing the number of earthquakes for a given intensity in the zone studied by the time-period studied, and plotting such data on semilog paper with the return periods on the abscissa and intensities on the ordinate (Chinnery, Tr. 313-316). This plot is not really a linear plot,

its a log linear plot[2/] and if the same data were to be plotted on linear paper, it would be a very intensively curved line (Chinnery, Tr. 261).

Because Dr. Chinnery believes the linear relationship to be an empirical observation not requiring justification, he has not accounted for uncertainty (Chinnery, Tr. 121-22). Further, inherent within the assumption of linearity, Dr. Chinnery has also assumed that the Modified Mercalli scale is itself linear, and his methodology has not accounted for variations from linearity across the length of that scale. Dr. Chinnery does admit that all frequency-magnitude scales saturate, and that the "net result is that this leads to an apparent curvature at the high-end of the frequency-magnitude curves" (Chinnery, Tr. 123-124). In that regard it was demonstrated at the hearing that if it is assumed, as does Dr. Chinnery, that there is a linear relationship between intensity and frequency of occurrence,that is log Nc = A-BI where Nc = the number of earthquakes of intensity I or greater; if "Im" is an agreed upon maximum earthquake; if one were to make a plot of the probability of an earthquake of a given intensity (Io) occurring, when one looks at cumulative plots, Dr. Chinnery stated that the slope of the curve would not be linear (Chinnery, Tr. 146, 150). The line would ". . . start bending over at the bottom and you would expect to see an indication of an upper bound before you actually reached it" (Chinnery, Tr. 150). He admitted that, at the hignest intensities, the curve isn't linear. If MMI XI were the upper bound, an intensity X earthquake would be in a non-linear region (Chinnery, Tr. 155).

2/ The transcript on p. 261 is erroneous; it reads "long linear plot."

There are not only problems with the use of Dr. Chinnery's methodology at the upper end of the MMI scale, but there are problems with the use of the data at the lower end of that scale. Dr. Chinnery admitted that he has excluded data from lower intensities in his analysis without conducting studies to indicate what the sensitivity[3/] of the proposed linear relationship is to the exclusion of such lower intensity data (Chinnery, Tr. 157). He further admitted that he has made no attempt to correct his graphs for this exclusion (Chinnery, Tr. 309).

For New England (and elsewhere), Dr. Chinnery used the data from the Smith Earthquake Catalogue without either re-examining the accuracy of the data (Chinnery, Tr. 54-55) or looking beyond the catalogue to determine whether any of Smith's data had been regraded or changed by subsequent investigation (see, e.g. Chinnery, Tr. 128-133). Even in using the Smith Catalogue for New England, Dr. Chinnery used a time-frame beginning in 1800 as an admittedly "very arbitrary cutoff point" (Chinnery, Tr. 59) in order to exclude from consideration the 1755 Cape Ann event (Chinnery, Tr. 60), the maximum historical earthquake in the region. This earthquake, which Dr. Chinnery has not included in his analysis, was the maximum historical earthquake used by both the Staff

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3/ At this point the transcript is erroneous. The word used is "intensity," rather than the correct word of "sensitivity."

and the Permittee as the "safe shutdown earthquake" under Appendix A to 10 C.F.R. Part 100.[4/]

Dr. Chinnery admits that his selection of varying time frames for comparing different regions of the country in his Exhibit 2 (1979 Article) was both subjective (Tr. 184)[5/] and arbitrary (Tr. 185).

Perhaps the correctness of the linearity assumption can be summarized by the answer to the question put to the Staff on cross-examination by counsel for NECNP, that taking the Bloom and Erdmann article (1980) in which the authors claim to have demonstrated a departure from a linear frequency-magnitude curve, and correcting the data by the quoted error of "0.5 units" the question was whether the corrected data fits a linear curve. The Staff response was that given the same data, one could draw several straight lines with widely varying slopes, as well as a wide number of curves, including higher order curves to the same data (Reiter, Tr. 510-512). The Staff submits that Dr. Chinnery in the final analysis has not provided a convincing explanation as to why his assumption, that a single straight line of a given slope of 0.57, should or could be drawn through those points. Finally, although Dr. Chinnery claimed that "the vast majority of seismologists have accepted the linearity of frequency-magnitude data as a working hypothesis" (Chinnery 10), Dr. Chinnery admitted under cross-examination that his "vast majority" claim was based on references which were not aimed at predicting a maximum earthquake, but were considering other issues where

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4/ See 10 C.F.R. Part 100, Appendix A §§ III(C), V(a)(1).

5/ The "regions" studied by Dr. Chinnery are not uniform in size or shape (Chinnery, Tr. 184). For a general summary of the subjectivity inherent in the methodology, see p. 20 et seq. infra.

the assumption of linearity was not vital to the results (Chinnery, Tr. 66, 69). Indeed, the record is completely barren of any acceptance of Dr. Chinnery's methodology.

2. Uniform Slope or "b" Value

As to Dr. Chinnery's second assumption, a uniform slope or "b" value for all regions of 0.57, different seismological investigations have used different methods to determine b values including simple and weighted least square techniques, maximum likelihood and extreme value methodologies (Reiter 5). Depending upon the data set and region studied, these methodologies may or may not yield similar results for the same data (Reiter, 5). As an example, Algermissen and Perkins (1976) computed b values for 71 regions in the United States, with b values ranging from 0.24 to 0.76, although the bulk of estimated b values was found to lie between 0.4 and 0.6. These differences in b value can be significant (Reiter 6). Thus, while the adoption of a single or universal b value in estimating seismic hazard under certain circumstances could be a valid working assumption for calculating seismic hazard, utilization of the results from such calculations requires a clear understanding that the uncertainty associated with this assumption is large (Reiter 6).

In summary, Dr. Chinnery has not presented a convincing argument that such slopes should be universal, and if a universal value is accepted, that it must be 0.57.

3. No Upper Bound To Earthquake Size

Dr. Chinnery's methodology is also dependent on the assumption that an MMI XII earthquake can occur anywhere in the world (Chinnery, Tr. 77-78; Reiter 6). According to Dr. Chinnery, if one were to strictly

follow the wording of Appendix A, all nuclear power plants would have to be built to withstand a MMI XII earthquake, because he cannot rule out the infrequent occurrence of such a seismic event at any site (Chinnery, Tr. 77-78; Reiter 6). Very few seismologists and geologists would agree with Dr. Chinnery that existing data cannot be used so as to arrive at reasonable estimates of upper bound earthquakes for engineering purposes (Reiter 7). The kinds of information that would be used for that purpose include instrumental and historical seismicity, local and regional tectonic history, geologic structure, stress measurements, and, when available, fault parameters such as dimension and slip rate (Reiter 7). While none of these pieces of information is unequivocal, integrated estimates typical of those employed in the geologic sciences can be made which give reasonable assurance that earthquakes of a given size will not be exceeded during the time period of interest (Reiter 7). Dr. Chinnery in his rebuttal testimony agreed with the Staff that geological, tectonic, and stress information should all go into the estimate of the upper bound for earthquake size (Chinnery Rebuttal 10), but he states "such information is not available for New England." (Id.). Notwithstanding this blanket denial by Dr. Chinnery, recent reports of instrumental seismicity in New England show low seismicity around Cape Ann and do not define a fault or other seismic source zone (Reiter Rebuttal 3). Thus, instrumental seismicity can have significant effects upon the assessment of seismic risk in New England and must accompany any application of numerical probabilistic calculations of earthquake recurrence periods. (Reiter Rebuttal 3).

With respect to Dr. Chinnery's position that geological information relevant to the question of upper bound to earthquake size is not

available for New England, Dr. Chinnery testified that it is his belief that the Boston-New Hampshire region is "low seismicity because all the whole Northeastern U.S. is low seismicity; all areas in the middle of the tectonic plates are low seismicity compared to those on the edges which is, where most of the earthquakes occur." (Chinnery, Tr. 208). Dr. Chinnery argues that "the only [major] events that have been linked to a geological structure in this area [New England] are the 1940 events in New Hampshire, which occurred near the Ossippee Mountain ring dyke complex . . . (Chinnery, Rebuttal 10). Dr. Chinnery admitted that such a geologic linkage is possible (Chinnery, Tr. 127; cf., Tr. 140(18) and Tr. 138). In Dr. Chinnery's 1979 article, he has stated in Table 4 that the largest recorded events in the Boston-New Hampshire zone were three intensity VII events from the selected period of 1800-1959 (Chinnery, Ex. 2, p. 764). If we assume that two of these events are geologically controlled by local geologic structure at Ossippee,[6/] that leaves only one MMI VII event, the October 5, 1817 earthquake (Chinnery, Tr. 128). Dr. Chinnery's use of MMI VII for that event was based on the Smith catalogue which he now "will not stand on" (Chinnery, Tr. 128); i.e., which he wouldn't use now because he believes there now are better catalogues (Chinnery, Tr. 128). Dr. Chinnery was unaware that three independent sources had subsequently downgraded the one MMI VII earthquake to MMI VI, i.e., USGS "Miscellaneous Field Studies Map MI857-1960," Street and Lacroix 1977, and Chiburis Catalogue (Chinnery, Tr. 128-129).

6/ This is a valid assumption (see Chinnery Rebuttal 10-12, Chinnery Tr. 128).

Dr. Chinnery was apparently willing to stipulate to accept such a downgrading. (Chinnery, Tr. 129 (12-13); 133 (13-15). Dr. Chinnery thought it was valid, at one point, that where you have only one or two earthquakes of a given size during the time interval "you probably are better off not to plot the thing." (Chinnery, Tr. 138). Thus, it is Dr. Chinnery's view that removing the intensity VII points from his graph of the Boston-New Hampshire zone "will not do anything at all to the other intensity points on the graph (Chinnery, Tr. 139), even though it appears from Dr. Chinnery's own graphs that the MMI VII points strongly influence the high energy end of the plot for the New England area (see Exhibit 2, Figure 8, p. 766; see also Chinnery, Tr. 272-274). The Staff submits that there is something factually and theoretically questionable with a methodology for predicting the return period of earthquakes, that produces the same result regardless of whether there were three events or one event, or no events of the largest intensity earthquake examined during the period in question. When confronted with this, Dr. Chinnery merely retreated to his view that earthquakes of a given size ". . . occur randomly. It's rather like pulling a handle on one of those one-armed bandits in Las Vegas." (Chinnery, Tr. 139; see also Chinnery, Tr. 142).

Dr. Chinnery then suggested plotting one MMI VII event at Ossippee, (Tr. 142 (4-6) notwithstanding his prior professions of invalidity of plotting a single or highly infrequent event. See Chinnery, Tr. 37 (8-9); Tr. 35 (22-24) ("What I'm saying is that when you have one earthquake, one should not plot this, because it is not telling you a thing" (Chinnery, Tr. 35); (Plotting one earthquake should not be done

because it will change the shape of the graph (Chinnery Tr. 38). Apparently, Dr. Chinnery would advocate plotting a single event when to do so would support his methodology, and would advocate not plotting such an event when it would contradict his methodology.

It is moreover, geologically reasonable to conclude that there are upper bounds to earthquakes over long periods of time, based on geologic observations, including such observations in New England (Jackson, Tr. 580-581; Jackson 8-9; Holt 1-4). In the Eastern United States, the average rate of tectonism is very slow. The absence of surface faults, the subdued topography and relative slow rate of uplift and denudation in this region (with due recognition of the anomalous areas of Charleston, South Carolina and New Madrid, Missouri areas), indicates that strain rates are relatively low, therefore, leading inherently to longer return periods for earthquakes in the Eastern United States (Jackson, 8). Due to the nature and character of faults, certain types of faults just cannot sustain large earthquakes (Jackson, Tr. 544). Moreover, the following geologic parameters also affect maximum earthquake potential: crustal properties, nature of fault gouge material on mineralization, depth of burial with equivalent consideration of temperatures, pressures, and mineral composition (plutonic bodies), and strength of asperities along faults. Based on many geological observations made in the field in New England by Dr. Jackson, including reactor excavations, the relatively old rocks in New England are heavily jointed and heavily cracked.[7/]

7/ It is not known whether this is the case at 10 kilometers (Jackson, Tr. 563).

Rather than having thru-going faults, such rocks are more likely to have a number of joint planes and joint sets on a spacing of every six to eight feet. Thus, there will more likely be small fault areas and small earthquakes occurring in New England, supporting the view, contrary to Dr. Chinnery's assumption, that there is a geologic basis for the existence of an upper bound to earthquakes in New England (Jackson, Tr. 562-63). The question was raised as to "how do we know that we're not going to have a large earthquake somewhere in the Eastern U.S. which we will subsequently find is associated with a geological anomaly that was not discovered before the event," i.e., would we have identified the potential for the large Charleston earthquake prior to its occurrence in 1886 or the potential for the large New Madrid earthquake prior to 1811? The Staff believes that if we imposed the requirements for geologic and seismic investigations such as in Appendix A, we would have identified potentially capable faults in the New Madrid area and we would have identified a high level of seismicity in the New Madrid area as a result of seismic monitoring, and basically, but not as strongly, these conclusions would also apply to Charleston (Jackson, Tr. 581).

In his rebuttal testimony, responding to Staff's comments on the assumption in Dr. Chinnery's methodology that there is no upper bound to earthquake size, Dr. Chinnery testified that mid-plate earthquakes "appear to have higher stress drops than events at plate boundaries and that, in spite of their small fault dimensions, their magnitude [the surface wave magnitude (M_s) of such earthquakes] may be in the range of 7 to 7.5." "This corresponds roughly to maximum epicentral intensity of X." (Chinnery, Rebuttal 12). This observation led Dr. Chinnery to conclude

that the maximum potential earthquake "in the Boston-New Hampshire seismic zone is at least a magnitude 7 (M_s) earthquake." (Chinnery Rebuttal 15), which he estimated would lead to a surface intensity of MMI X (Chinnery Rebuttal 13). These assertions were made by Dr. Chinnery although he admitted that he failed to make a comparative study in support of his conclusion that New England is a "typical midplate region." (Chinnery, Tr. 145).

Dr. Chinnery's methodology for arriving at the size of the largest earthquake that could occur in New England (Chinnery, Rebuttal pp. 11-12) consisted of three essential steps.

1. estimating the size of the maximum fault area that would not lead to surface rupture;

2. utilizing a 1980 study by Liu and Kanamori of five midplate earthquakes which had similar fault rupture system areas as a model to arrive at moments and stress drops;

3. utilizing Fitch and others to convert the observed seismic moments and stress drops into earthquake magnitude and to arrive at what size earthquakes might be expected in New England (Chinnery, Tr. 159).

Dr. Chinnery's prefiled rebuttal testimony was that utilizing this methodology, a maximum epicentral intensity X (approx.) could be expected in New England. However, Dr. Chinnery admitted that in employing the methodology he failed to utilize direct measurements of magnitude for the earthquakes studied that were available in the article by Liu and Kanamori which he was utilizing (Chinnery, Tr. 164). Dr. Chinnery ignored such direct earthquake magnitude values or measurements with respect to the Liu and Kanamori paper, because he "went through too fast to see"

/Chinnery, Tr. 164). If Dr. Chinnery had used the correct data, the data that reflected direct measurements of the earthquakes, the Staff submits that this would drastically change his conclusions by reducing the intensity of the maximum earthquake that might be expected to occur in New England. Dr. Chinnery had testified in his rebuttal testimony that utilizing his methodology, he would expect surface wave magnitude earthquakes ranging from 7-7.5 Ms in New England, which corresponded to a MMI X. The actual surface wave magnitude measurement of the events studied which Dr. Chinnery ignored, was Ms 5.5 to 6.3 (Chinnery, Tr. 160). If Ms 7 to 7.5 earthquakes correspond roughly to MMI X, then Ms 5.5 to 6.3 earthquakes correspond to MMI VII or VIII (see Chinnery, Tr. 161-172, particularly Tr. 169). Whereas Dr. Chinnery had previously estimated that a Ms 7 earthquake corresponded to a MMI X earthquake utilizing for comparison purposes the 1971 San Fernando earthquake (Chinnery, Tr. 166) when he originally made his calculations, the relevant surface wave magnitude range of the earthquakes considered, he admitted, based on actual measurements of surface wave magnitude, should have been 5.5 to 6.3 Ms. The relevant earthquake for making the calculation between surface wave magnitude and MMI was therefore not the 1971 San Fernando earthquake but the 1966 Parkfield earthquake in California which was directly measured using surface wave magnitude as Ms 6.0 (Chinnery Tr. 169, see also Chinnery Tr. 168). The highest Modified Mercalli Intensity associated with that earthquake was MMI VII, according to "Earthquake History of the United States" revised edition through 1970, published by the U.S. Department of Commerce, National Oceanic and Atmospheric Administration (Chinnery, Tr. 168-169). Thus, the Staff submits that if Dr. Chinnery had been more

precise in making his own calculations, and more careful in utilizing the sources which he elected to utilize, then even using his own methodology, the maximum earthquake in New England would be MMI VII or VIII, not MMI X. When confronted with this on cross-examination by the Staff, Dr. Chinnery refused to make the calculation, or even estimation, of converting Ms 5.5-6.6 to MMI, although he had previously made such a conversion in his rebuttal testimony (cf. Chinnery Rebuttal pp. 12-13 with Chinnery, Tr. 161-172). His excuse this time was to try to "blame the whole thing on the people at the California Institute of Technology" (Chinnery, Tr. 171). He later admitted had he actually had some doubts about his specific analysis here (Chinnery, Tr. 235 (4-5)).

The superficiality of Dr. Chinnery's analysis was again demonstrated when although he had concluded that midplate earthquakes appear to have higher stress drops than events at plate boundaries (see Chinnery Rebuttal 12), Dr. Chinnery admitted that he had not undertaken an analysis or calculated stress drops for any earthquake that had occurred in New England. He further conceded that geological information "has to be fed into that particular problem," (Chinnery, Tr. 305) although he was not aware of any studies which had done that (Chinnery, Tr. 305). Dr. Chinnery viscerally "felt" that "we ought to find highest stress drops in areas of older rocks, such as the Eastern U.S." (Chinnery, Tr. 177) but admitted that "evidence has been slow to come along that such things exist" (Chinnery, Tr. 177). In fact, Dr. Chinnery appeared to have little or no evidence to support his theory that New England earthquakes would have high stress drops (Chinnery, Tr. 177). Yet Dr. Chinnery acknowledged that the Street and Turcotte, 1977 study of

32 actual earthquakes in New England indicated relatively low (fifty bars or less) rather than high stress drops in New England (Chinnery, Tr. 178). See Reiter, Tr. 556.

In summary, the Staff submits that the validity of the third assumption of Dr. Chinnery's methodology ("that there is no upper limit to earthquake size affecting the Seabrook site") has not been demonstrated.

4. Linear Extrapolation Beyond Existing Data

In Dr. Chinnery's fourth assumption, he assumed one can continue to extrapolate linearly from the moderate earthquake intensities which have occurred in a given region to predict the "return period" of larger than historical earthquakes which have not occurred. As has been previously indicated, many other non-linear relationships have been proposed to explain deviations from the data at higher intensities (see p. 5 supra).

One problem inherent in the predictive nature of the linear extrapolation is Dr. Chinnery's reliance on existing data points and his uncritical use of the Smith catalogue. Even Dr. Chinnery admits that "[T]he Smith catalogue is indeed questionable in some details" (Chinnery Rebuttal, p. 14). Dr. Chinnery never re-examined the accuracy of the data from the Smith catalogue (Chinnery, Tr. 54-55), and appears to be unaware of studies by others which have re-evaluated or regraded some of the events in the catalogue (Chinnery, Tr. 128-133). Dr. Chinnery's

uncritical use of the Smith data, coupled with his lack of general knowledge of the re-evaluation downward of certain earthquakes by the scientific community and governmental agencies has resulted in the amplification of the perceived seismic risk (Reiter, 8-9). The same amplification of seismic risk also results from the assignment and plotting of high intensity earthquakes that are very much dependent for their high intensity rating upon localized conditions (e.g., landslides, soil liquifaction, etc.) rather than ground shaking (Reiter 9). The simple linear extrapolation from moderate earthquakes that have occurred in a given region to high intensity earthquake that have occurred in other regions also amplifies the seismic risk. For example, MMI IX events have only occurred at four locations in eastern North America (and not in the Boston-New Hampshire zone studied by Dr. Chinnery): the New Madrid Missouri Zone, Charleston, South Carolina, La Malbaie, Quebec and the Grand Banks, off Newfoundland (Reiter 8). Moreover, there are no strong motion recordings associated with MMI VIII on bedrock[8/] anywhere in the United States (Reiter 23).

Even Dr. Chinnery apparently acknowledges that his methodology, as seen in Figure 3 to his direct testimony, has over-predicted (predicted more earthquakes of each intensity or greater) those earthquakes that have actually occurred over a ten-year period as follows:

Intensity II	Over predicts by a factor of 4 or 5
Intensity III	Over predicts by a factor of 3 or 4

8/ The Seabrook site is a bedrock site.

Intensity IV Over predicts by a factor of 5 or 6 (Chinnery, Tr. 319-20).

Finally, the Staff believes it is highly inappropriate to estimate return periods for high intensity earthquakes from only two (or fewer) data points at lower intensities as Dr. Chinnery has done. (See Reiter Rebuttal 4). For example, if this approach were followed utilizing the data set presented by Permittee expert witness Mr. Holt (Holt 17), the b value defined by those two points would be approximately 0.85 rather than 0.57, thereby greatly lengthening the return period of higher intensity earthquakes for MMI VIII earthquakes from 371 to 2200 years; MMI IX earthquakes from 1445 to 16,000 years; MMI X earthquakes from 5623 years to 111,000 years.

All of the above problems are exacerbated by the question of whether the Modified Mercalli Intensity scale continues to be linear beyond MMI VIII. Dr. Chinnery's position, in that regard, appears to be, "I'm not sure that one can prove anything along these lines but I think one has to take the data" (Chinnery, Tr. 223).

5. Overall Assessment Of The Factual Validity of Dr. Chinnery's Methodology

In addition to the four previously discussed assumptions, there are a number of examples of the unexplained arbitrariness or subjectivity contained in Dr. Chinnery's methodology which directly bear on the question of both its "factual validity" and its use. For example, as was previously outlined, Dr. Chinnery appears to have selected rather arbitrary cut-off dates (Chinnery, Tr. 59), and arbitrary time periods for plotting (Chinnery, Tr. 94, Tr. 184-185). The particular confines of

the geographical area or region studied also appear arbitrary[9/] (Chinnery, Tr. 184-185). Dr. Chinnery failed to make a comparative study in support of his conclusion that New England is a "typical midplate region" (Chinnery, Tr. 145). Other examples of arbitrariness in the methodology are that Dr. Chinnery selectively plotted less than all events of a given intensity within the time frames and regions studied (see e.g., Chinnery, Tr. 96-98); he excluded data from lower intensities without conducting studies to indicate the sensitivity of the proposed linear relationship to be excluded data (Chinnery, Tr. 157); he treated regions selected for study differently in terms of plotting all events in some regions and ignoring other seemingly relevant data in other regions (Chinnery, Tr. 277-279); he relied upon data from the Smith catalogue without questioning the accuracy of that data or examining whether that data had

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9/ This is also supported by Dr. Chinnery's admission that the similarity in frequency-intensity data for the four regions studied as depicted in Fig. 9 of his 1979 paper (i.e., Boston-New Hampshire, Mississippi Valley, Southeastern U.S., and Southern New England), "is entirely fortuitous and is simply due to the particular regions chosen for each study . . ." (Chinnery, Tr. 280). This clearly gives the impression that the areas chosen for study were selected so that the study would yield certain pre-conceived results, i.e. equivalent activity in different regions. Although Dr. Chinnery stated during his cross-examination that the similarity in results was only "an interesting little point" (Tr. 280), that statement is not entirely consistent with the inference he attempts to make from that "interesting little point:" i.e., "[H]owever, one is tempted to note that the activity per unit area in the Boston-New Hampshire zone is slightly larger than that in the Southeastern U.S. Is there really any good reason why an event the size of the Charleston earthquake could not occur in the Boston-New Hampshire zone?" (Chinnery, Ex. 2, p. 77).

been updated (Chinnery, Tr. 54-55); and he admitted to "mixing methodologies" (Chinnery, Tr. 285-290). As to the last point, Dr. Chinnery appears to be mixing methodologies or "cream skimming" i.e., by apparently combining selective elements of what he terms the "probabilistic" and the "deterministic" methodologies. He is first, on the basis of rather arbitrary selected seismic source zones and time frames, calculating return periods for larger than historical earthquakes. Having previously ignored the requirements of Appendix A (see Chinnery, Tr. 18; 625-626) Dr. Chinnery utilizes it for purpose of assuming that his probability of recurrence times also apply at the Seabrook site.

Another serious problem as to the validity of the methodology exists with respect to its lack of assessment or correction for errors or uncertainties. This led Dr. Chinnery to state in Exhibit 2:

> We do not pretend that the numbers are very accurate. In fact, because of the subjectivity that has to be used in obtaining the linear relations there is no way to make a realistic assessment of errors. We therefore view the numbers as being a qualitative indication of risk, rather than quantitative. (Chinnery, Ex. 2 at 769).

Inasmuch as Dr. Chinnery's methodology is being proposed for use in the specific pragmatic context of arriving at the seismic design of nuclear power plants, it must be recognized that the range of uncertainty associated with each of Dr. Chinnery's calculations is enormous. Dr. Chinnery testified that precise numbers of risk . . . are "clearly nonsense" (Chinnery, Tr. 92). . . . Chinnery calculates the return period for an MMI IX earthquake in the Boston-New Hampshire zone as 5623 years. What he means is "it's about 5000 years," "it may be three; it may be seven." "It may be four; it may be six. It's an order

of magnitude which is rather important . . ." (Chinnery, Tr. 92-93). Thus, for the pragmatic purpose that such calculations are to be used for in terms of nuclear power plant siting, the Staff cannot apply the results of Dr. Chinnery's methodology without accounting for the errors and range of uncertainties so that a final licensing decision can be made (see Reiter 10). While Dr. Chinnery states that ". . . a good scientist will never quote an observation without also quoting his best estimate of the error associated with his piece of data" (Chinnery, Rebuttal, p. 2), he stated that he has no way of measuring error inherent in the data points he used (Chinnery, Tr. 100). Even Dr. Chinnery believes that he should apply awareness of uncertainties to the essential input parameters in his probabilistic calculations such as maximum magnitude, "b" values, and the linear extension of frequency-intensity relationships (Chinnery, Tr. 118-119). However, Dr. Chinnery has not generally assigned errors to his "estimates" of return period with the exception of Figure Four of Ex. 3 (Chinnery, Tr. 259-260). Moreover, Dr. Chinnery acknowledges, as does Dr. Trifunac (see Trifunac Tr. 764) that the existence of safety factors in the design of a nuclear power the plant, decreases the amount of risk that needs to be put into the selection of the highest intensity earthquake (Chinnery, Tr. 113). Thus, Dr. Chinnery agrees that a decision such as seismic design must by necessity embody engineering judgments (Chinnery, Tr. 112-113).

For all of the previously enunciated reasons, including the specific context in which Dr. Chinnery's methodology is being proposed for use in light of the admitted range of error and uncertainty inherent in the methodology, the Staff submits as the proposed conclusion on the first issue that Dr. Chinnery's particular or limited version of a probabilistic

methodology is not presently an acceptable way to proceed for making licensing decisions at the NRC (Reiter, Tr. 574-757; Knight, Tr. 578-79).10/

Any discussion of the validity of Dr. Chinnery's methodology must also consider the risks in the adoption of such a methodology. Heavy reliance upon and premature use of simple extrapolations such as those proposed by Dr. Chinnery can only lead to questionable results and undefendable decisions (Reiter 11). The Staff's view is that it is necessary to do a much fuller investigation than simply seismology in order to come to a reasonable decision on determining the SSE (Knight, Tr. 547). Thus, the validity of Dr. Chinnery's methodology must be viewed in the total seismic design context (Jackson 9) where geologic judgment, and structural engineering judgment as well as seismological judgment must be used in an integrated manner in reaching a conclusion on seismic risk (Jackson, Tr. 520; Reiter, Tr. 554; Knight 6-7). Dr. Chinnery does not factor in such geologic or structural engineering aspects (Id.). The Staff's extensive geological requirements for licensees have in certain instances changed the Staff's initial thoughts on the appropriate SSE value (Jackson, Tr. 551). Indeed a straight seismology review would not necessarily result in the highest g value to

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10/ The Staff's methodology under Appendix A, while not having a quantitative estimate of error, does have a large number of conservative steps which reduce or eliminate errors and uncertainties. For example, the Staff brings the maximum historical earthquake in the province to the site; the Staff requires significant investigations to reduce the uncertainty, i.e. extensive investigations into the locations of earthquake epicenters, of search of the historical records, of trenching investigations. All of these conservative steps are done to reduce the uncertainties so the Staff can make the best estimate of ground motion possible (Jackson, Tr. 599).

anchor the response spectra, as would a combined geological, seismological and structural engineering review (Reiter, Tr. 554-555; Knight, Tr. 555).

To the extent he is qualified to make such judgments, Dr. Chinnery does not believe that the present seismic design of Seabrook of MMI VIII, .25 g, Reg. Guide 160 is either unsafe or insufficiently conservative (Chinnery, Tr. 46, 621). Indeed, using the latest state of the art technology available, the Site Specific Spectra Program ("SSSP") to review the present seismic design of Seabrook,[11/] that design supported the safety, and conservatism of the present seismic design at Seabrook as MMI VIII, .25 g, Reg. Guide 1.60 (Reiter 11-16; Jackson 9-12).

11/ The innovative approach of the SSSP was to canvas expert opinion (including Dr. Chinnery) as to what the choice of these input parameters were, what range they might be expected to assume, and what credibility could be attached to them. Each expert's input was treated separately; response spectra were computed for each expert at each site based upon their own self-ranking. The input parameters covered four areas: (1) the configuration of seismic source zones in the centeral and eastern U.S., (2) the largest earthquake expected in each of these zones, (3) the earthquake activity rate and recurrence statistics associated with each zone, and (4) methods for predicting ground motion in the eastern and central U.S. from an earthquake of a given size at a given distance. Responses were received from 10 of the 14 experts polled. Probabilistic estimates were calculated for 9 sites including 5 in the Northeast. The present seismic design of Seabrook was also compared with the response spectra resulting from the SSSP. The result of that comparison illustrated the conservatism and adequacy of the present seismic design at Seabrook. The design spectrum at Seabrook exceeded the spectrum that would be recommended for the Seabrook site if it were among those sites being considered in the Systematic Evaluation Program. This exceedence is about 25 to 50% in the period range of prime structural interest (0.1 to 0.5 seconds). (Reiter 14-15, Figure 1).

B. Whether The Staff's Methodology For Correlating Vibratory Ground Motion Is Consistent With Appendix A to 10 C.F.R. Part 100.

As was previously stated, the Commission in its September 25, 1980 "Order,"[12/] directed the Appeal Board to reopen the record with respect to two discrete seismic issues, the second of which is the question of whether the Staff's methodology for correlating vibratory ground motion is consistent with Appendix A, to 10 C.F.R. Part 100. With respect to the second issue, the Commission did not comment further in its remand Order, except to state that ". . . the parties should provide a discussion of the relation between the mean of the maximum ground accelerations and maximum effective ground acceleration." 12 NRC at 298. This particular issue emanated in part from the Licensing Board's rejection of NECNP's contention that assuming that the Safe Shutdown Earthquake was a MMI VIII, that the Regulatory Guide 1.60 spectrum should be anchored at 0.4g, rather than the Staff approved 0.25g. 3 NRC at 871, 921-22. In that regard it should be noted that NECNP's contention before the Licensing Board in support of the 0.4g anchor point was based in part upon the testimony of (then) NECNP witness Dr. Mihailo Trifunac.

By unpublished "Memorandum and Order" dated November 6, 1980, the Appeal Board granted NECNP's request that Dr. Trifunac be called to address the second reopened issue as a Board witness because of his present consultant relationship with the Advisory Committee On Reactor Safeguards. As an Appeal Board witness, Dr. Trifunac was permitted to file testimony one-month after witnesses for the other parties had filed

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12/ See note 1 supra.

testimony on the second issue.[13] Significantly, as an Appeal Board witness, Dr. Trifunac never endorsed the .4g anchor point that NECNP had previously advocated. In fact, Dr. Trifunac now appeared to endorse the Staff approved (subsequently also approved by both the Licensing and Appeal Board) .25g anchor point for Reg. Guide 1.60 (Trifunac 10; Figure 3; Tr. 753-755; Tr. 793-796).[14]

In Figure 3 of his prepared testimony Dr. Trifunac presents a plot of his probabistically-derived response spectrum for the Seabrook site which assumes that earthquakes up to and including MMI XII may occur in the Boston-New Hampshire region. As a result of this analysis Dr. Trifunac concluded (with respect to his Figure 3):

> [This curve tells us] . . . that if during the next 50 years the seismicity of this site follows the equations that are consistent with the model . . ., if there is no bound on the maximum intensity during that log,[15] that the probability of exceeding Reg. Guide 160 spectrum [anchored at .25 g] throughout the period range plotted . . . is less than 5% during the next fifty years. (Trifunac, Tr. 754).

13/ "Memorandum and Order," p. 3 (November 6, 1980). Mr. Holt on behalf of the Permittee had only very limited comments on the second issue. Thus, it was the testimony of the three Staff witnesses that Dr. Trifunac primarily had before him when he filed his testimony.

14/ The only caveat to a complete endorsement of .25g is Dr. Trifunac's prepared testimony that the design "may be acceptable;" "may" because a complete endorsement would require additional work which he elected not to do. (Trifunac 10).

15/ The transcript is incorrect, "log" should be "time."

Dr. Trifunac also testified that if the upper bound of maximum intensity in Figure 3 were a MMI X earthquake, the probability of exceeding the Reg. Guide 160 spectrum over the next fifty years at Seabrook is "considerably smaller than five percent" (Trifunac, Tr. 754-55). Dr. Trifunac's opinion was that such results are conservative utilizing a SSE of MMI VIII and still reasonably conservative even if a higher SSE of MMI X were used[16/] (Trifunac Tr. 794-95). Thus, we should approach the question of the Staff's methodology from the perspective that no witness challenged the adequacy, conservatism, or reasonableness of the present seismic design of Seabrook as MMI VIII, Reg. Guide 1.60, .25g.[17/] Nevertheless, we must address the question of the reasonableness and consistency of the Staff's methodology.

Not only must we recognize that there is no safety-related problem at Seabrook related to the present seismic design, but we start from the premise that both the Staff and Dr. Trifunac agree that the Staff's methodology for correlating vibratory ground motion at Seabrook is consistent with the meaning and intent of Appendix A (Reiter 16) or as Dr. Trifunac stated, such methodology "would be one acceptable way of

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16/ Dr. Trifunac did not advocate the selection of an SSE higher than the design basis earthquake of MMI VIII.

17/ This is not surprising, as "[T]he seismic design of [Seabrook] at .25g and Reg. Guide 1.60 is as high as any seismic design specified for a nuclear power plant in the U.S. east of the Rocky Mountains" (Jackson, 15).

rationally interpreting Appendix A" (Trifunac, Tr. 762). The Staff's methodology which is at issue consists of 3 areas or steps. They are:

1. Definition of the Safe Shutdown Earthquake (SSE);

2. Definition of a peak acceleration associated with the SSE;

3. Specificiation of the response spectrum to be used with that peak acceleration (Reiter 16; Trifunac 761-62).

The final product, the response spectrum, is the most important result since neither the intensity or magnitude of the SSE, nor the peak acceleration alone, is used by engineers in the design and analysis of structures. (Reiter 16).

The first step, definition of the SSE, is accomplished through an evaluation of capable faults, tectonic structures and tectonic provinces. At Seabrook, the Staff determined that the controlling earthquake which defines the SSE would be based upon the MMI VIII Cape Ann Earthquake of 1755 (Reiter 17).

The second step typically followed is to choose a peak acceleration which should be associated with the SSE. This may be done assuming the SSE is specified in terms of magnitude or intensity. In the western United States it is actually chosen assuming a given magnitude earthquake at a given distance (for example the closest distance to a capable fault), while in the east it is usually chosen assuming a given intensity at the site. For the last 5 years the Staff has relied upon the utilization of the trend of the means relationship of Trifunac & Brady (1975). When published, this represented an analysis of the most complete

data set yet available (Reiter, 17).[18/] Thus, the second step typically consists of the selection of an acceleration anchor point or "g" value, which is an acceleration value which is used to anchor a design response spectrum (Jackson, 13).

The third step in the Staff's methodology is choosing the response spectrum. This selection determines the level of ground motion, or more correctly, the maximum level of response to ground motion that is assumed at each frequency in the design process. The response spectrum used at Seabrook is the Regulatory Guide 1.60 spectrum. It is essentially the mean plus one sigma spectral shape derived after normalizing a series of earthquake records to the same peak acceleration or high frequency response (Reiter 18). The Reg. Guide 1.60 spectrum is rather conservative. For example, over much of the frequency range of interest the predicted response is close to twice the response of another widely used spectrum, the Housner spectrum (Reiter, 19). Dr. Trifunac also

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18/ Since the publication of Trifunac and Brady (1975), the NRC has sponsored analysis of new and enlarged data sets. (NUREG's 0143 and 0402). Much of this work was summarized in an article entitled "The Correlation of Peak Ground Acceleration with Seismic Intensity and Other Physical Parameters" at 677 Bulletin of the Seismological Society of America 877 (1979). Using the most general relationship (all the data), this latest study predicts lower mean peak accelerations than the Trifunac and Brady (1975) relationship. For Intensity VIII, for example, the predicted acceleration utilizing Trifunac and Brady (1979) would be approximately 0.25g while the NUREG-0402 would yield approximately 0.15g. The difference in the results are caused by the increased data set and differences in treating the data. Trifunac and Brady (1975) drew a trend line through arithmetic means at the different intensity levels while the more recent study used a least squares regression analysis of the total data set assuming a logarithmic distribution. Because of the difference in data sets and the lower g value results, the Murphy and O'Brien data are still being evaluated by the Staff (Reiter, Tr. 651).

believes that the spectral shape of the Reg. Guide 1.60 response spectrum is adequate for many purposes. (Trifunac, Tr. 760-761). As aforementioned, Dr. Trifunac believes that the Staff's methodology as outlined in the above three steps is one acceptable way of rationally interpreting Appendix A (Trifunac, Tr. 762). There is no opinion evidence of record to the contrary.

As to the Commission's instruction that the parties should include within this issue a discussion of "the relation between the mean of the maximum ground accelerations and maximum effective ground acceleration," the Staff agrees with Dr. Trifunac's characterization of the term "maximum vibratory acceleration" as requiring an engineering interpretation, rather than a literal one (Reiter 21). That is because the very highest frequency peaks on an accelogram are not considered to, and do not, have an effect upon structures like nuclear power plants. Such isolated peaks are therefore not of "engineering significance" (Reiter, Tr. 669; Knight Tr. 719; Jackson 15). These isolated high frequency peaks of ground acceleration are not of concern for purposes of seismic design of nuclear power plants because the amount of energy that would be delivered to the structures and the system's components from such peaks, regardless of the peak value, is not sufficient to cause damage (Knight, Tr. 719; Reiter Tr. 669; Jackson 15; cf. Trifunac Tr. 762).

For example, a peak acceleration of 0.25g was recorded from an earthquake of magnitude 2.7 in South Carolina (Stover and von Hake, 1980) and 0.7g was recorded from a magnitude 4.75 earthquake in California (Morrill and Mattiesen, 1972). Even smaller mine tremors have yielded peaks as high as 12.0g (McGarr, 1978). (Reiter 21). Moreover, structures have been inspected after they have been exposed to

earthquakes with random high peaks and significant high frequency energy and it has been found that there is no damage to the structures (Knight 8). It is for these aforementioned reasons that there have been efforts to accommodate this "engineering interpretation" of analyzing "maximum vibratory acceleration" as a "maximum effective acceleration." The word "effective" in the phrase "maximum effective acceleration" is taken to mean "of engineering significance." (Reiter 21). It should be noted that "effective peak acceleration" is defined in U.S. Geological Survey Professional Paper 1114, "Procedures for Estimating Earthquake Ground Motion" (1980) as the peak ground acceleration after the ground-motion record has been filtered to remove the very high frequencies that have little influence upon structural response (Jackson, 15-16), although questions do exist amongst experts as to how the term should be defined (see Jackson 15-16, Reiter 21-22, Trifunac 4).

It should also be noted that Appendix A to 10 C.F.R. Part 100 does not delineate or specify the manner or the way the acceleration level is to be chosen (for a given SSE) (see, e.g., Trifunac Tr. 759). Moreover, Appendix A gives an engineer, a seismologist, and a geoscientist working under it some degree of freedom to select what seems to be the most rational and reasonable method for developing vibratory ground motion with an SSE (Trifunac, Tr. 759-760; Jackson 17).

Appendix A also expressly contemplates the filtering out of isolated peaks on the accelogram which are without engineering significance by the provision that the characteristics of the SSE shall be designed ". . . to assure that the maximum vibratory acceleration at the site throughout the frequency range of interest is included." 10 C.F.R. Part 100, App. A

SV(a)(1)(iv) (emphasis added). The aforementioned high-frequency peaks are not "in the frequency range of interest" because structures like nuclear power plants do not respond to those high frequency peaks (Reiter Tr. 669, Knight 8, Tr. 669-671). In addition, there is nothing in Appendix A that requires an absolute literal interpreation of the phrase "maximum vibratory acceleration," instead of an interpretation which reflects sound engineering, seismological, and geoscientific practice. Indeed, it was the apparent attempt of the draftsman of Appendix A, in part, to codify such past practice (See Knight, Tr. 671-72). This past practice embodied engineering judgment and did not require an axiomatic application of the highest possible acceleration readings. Moreover, as previously indicated, Appendix A also does not require the seismic design to consider acceleration "beyond the frequency range of interest." 10 C.F.R. Part 100, App. A § V(a)(1)(iv).

The Staff submits that decisions regarding statutory interpretation are useful guides in interpreting a regulation such as Appendix A.

As the Appeal Board stated in another proceeding;

> In short, in construing statutes, "context and purpose outweigh syntax."[19]

In the same opinion, the Appeal Board quoted from the wisdom of Justice Frankfurter:

19/ The Toledo Edison Company, et al (Davis-Besse Nuclear Power Station, Unit 1), ALAB-323, 3 NRC 331, 337 (1976) citing Kansas Gas and Electric Company et al. (Wolf Creek, Unit 1), ALAB-321, 3 NRC 311 (1976).

> The notion that because the words of a statute are plain, its meaning is also plain, is merely pernicious oversimplification. It is a wooden English doctrine of rather recent vintage to which lip service has on occasion been given here, but which since the days of Marshall this court has rejected, especially in practice.
>
> A statute, like other living organisms, derives significance and sustenance from its environment from which it cannot be severed without being mutilated. (Citations omitted) United States v. Monia, 317 U.S. 424, 431-32 (1943) (Frankfurter, J., dissenting).[20]

The same reasoning, the Staff submits, should be applied to interpreting the phrase "maximum ground acceleration" as utilized in Appendix A. As previously indicated, the scientific environment to which that regulation applies clearly supports the interpretation of the phrase "maximum ground acceleration" as the maximum acceleration of engineering significance, or more pragmatically, the anchor point for a response spectrum.

In considering "maximum vibratory acceleration," another relevant consideration is that the central purpose of Appendix A is the development of a nuclear power plant with structures, systems, and components that have the capability to sustain a very broad range of ground motions and yet have design features that offer minimum conflict with safety requirements needed for normal operation of the plant (Knight 4; cf. 10 C.F.R. Part 100, App. A, § IV). And it is not true that the higher the "g" value, the safer the plant, for a balancing of risks is required. For example, as to the .4g value originally proposed by NECNP (or if such a g value were arbitrarily picked), such a figure represents

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20/ 3 NRC at 335.

a failure to recognize that the end desire is to build a critical structure which will have resistance to very large earthquakes and yet will function in the optimum manner during its lifetime. (Knight, Tr. 661). If .4g were arbitrarily picked as the anchor point at Seabrook, ". . . you have very likely reduced the risk due to seismic events or earthquakes by a miniscule amount, but you have increased the risk of failure in the plant from other reasons by a very large amount. You have had a net loss in safety." (Id).

As one increases the anchor point for the design spectra, the obvious engineering effect is to increase the loads, the requirement for supports and restraints within the plant because of these higher loads. A nuclear power plant is designed to operate through a relative wide range of temperatures so there is thermal motion of the equipment. As a natural consequence of the various power levels of the plant, there are changes in temperature pressure. These cause thermal transients in pipelines and motions from within the system due to thermal expansion. Every time one increases the thickness of a pipe beyond that which is needed, one also increases the thermal stresses. Every time one adds restraints and supports that are not needed beyond those which are reasonable, one increases the stresses under which the system must function as it operates. Every time a device like snubbers or hydraulic devices to absorb motion is added, another active component has also been added into a system. The larger number of such components in a system, the more likely there will be one that could malfunction. The summation of all these activities beyond a reasonable level to provide for the seismic safety of the plant further complicates an already

complicated system which increases the risk of some sort of failure or problem (Knight, Tr. 662-663).

In this regard it should also be noted that when a plant is constructed at a given seismic level, there is capacity well beyond that level; by further increasing that, by building structure to that double value, there would be capacity well beyond the double value. "But you have sharply increased your risk because of all the complications you've added to the plant." (Knight, Tr. 663-664). Indeed, the safety margin extant in the seismic design of Seabrook is already greater than two (Knight, Tr. 678, 720-721; see also Knight, 9-13)).

Thus, in interpreting Appendix A, one must use the scientific (geological and seismological) and engineering judgment[21/] that is available (Jackson, Tr. 694-95).

The staff practice is to view the ground motion associated with the Safe Shutdown Earthquake ("maximum vibratory acceleration") as generally ground motion associated with the mean plus one sigma representation of the free field spectrum (Reiter 22). This has been the practice evolved by the staff in its interpretation of the regulation in light of the following: (i) accelerations associated with an earthquake of a given size

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21/ As to the role the use of engineering judgment under Appendix A, Dr. Trifunac testified that a true assessment of the earthquake resistance of a nuclear power plant must also take into account analytical design and construction techniques and practices (Trifunac, Tr. 764). On the instant subject, Dr. Trifunac testified that engineering judgment should play a very significant role in designing the specific acceleration under the existing Appendix A. (Id.). He explained, in support of this, that it is only the engineer who does the actual calculations who can fully appreciate all the subtleties, assumptions, and all the consequences that are considered (Id.).

can best be represented as a distribution, the absolute maximum of which cannot be presently defined, and (ii) the high accelerations associated with the tails of these distributions generally have been associated with high frequency peaks which have resulted in little or no engineering damage (Reiter 20-22). The mean plus one sigma spectrum may be arrived at by utilizing either peak parameters (such as acceleration) and standardized amplification factors or spectral shapes, or by utilizing a direct estimation of the mean plus one sigma response spectrum at each frequency. Expert witnesses Reiter (Reiter 23-25, Figures 2, 3) and Holt (Holt 7, Figure 10) presented direct estimates of the mean plus one sigma spectra either characterized as MMI VIII or about magnitude 6.0 recorded at close distances. The Seabrook design spectrum was found to exceed all three estimates. The Staff believes that these factors also support the conclusion that the Staff's methodology for correlating vibratory ground motion is consistent with Appendix A.

Whereas Dr. Trifunac testified both to the general acceptability, indeed conservatisms in the present seismic design of Seabrook (Trifunac, Tr. 791-796) as well as to the acceptability of the Staff's methodologies for correlating vibratory ground motion under Appendix A (Trifunac, Tr. 76?), there are a few isolated comments in Dr. Trifunac's prefiled testimony which the Staff would like to address. Dr. Trifunac states in his testimony that in his opinion, "licensing difficulties could be avoided by recognizing that it is sufficient to evaluate the maximum intensities by means of a distribution function, rather than through the selection of a precise yet not necessarily an accurate point estimate" (Trifunac 3). Presumably, Dr. Trifunac is considering, with respect to

these comments, the previous issue in this proceeding over the selection of the "g" value or anchor point. While the Staff would agree that the significance of the "g" value has been overemphasized, and that differences in a "g" value from .25g to .30g would be completely insignificant and immaterial for seismic design purposes (Knight, Tr. 721-22), Dr. Trifunac did agree on cross-examination that in order to design structures, it is necessary to arrive at a point estimate (Trifunac, Tr. 769).

Dr. Trifunac in his filed testimony also appears to have criticized the term "effective peak acceleration" because it has not been precisely defined (Trifunac 4). Dr. Trifunac discusses a number of examples, from past experience with "this expression":[22/] i.e., (1) that it is usually smaller than the recorded acceleration;(2) the expression as interpreted varies amongst experts; and (3) it can "avoid the physical basis of the problem and allows unwarranted freedom for expert judgment." (Trifunac 4). As to the first example, "smaller than recorded accelerations," Dr. Trifunac both at his deposition and at the evidentiary hearing testified that delineation of the SSE under Appendix A does not represent the absolute maximum vibratory acceleration; that such a determination does not represent the largest or highest physically possible level that is recorded (Trifunac 762-763). In his criticism of "effective peak

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22/ The Staff concurs with the recognition of controversy surrounding a precise definition (Reiter 21-22, Jackson 15-16). Indeed a major research project funded by NRC is presently underway to better define effective acceleration and to establish how it may best be estimated (Reiter 22, Jackson 16).

acceleration" Dr. Trifunac is apparently addressing a contention in other proceedings concerning the belief that existing ground records have been modified to eliminate the peaks before the analysis was performed. This is simply not the case at Seabrook, and there is no evidence to the contrary.

Secondly, whereas it is true as with many scientific terms that different experts can interpret the term "effective peak acceleration" in a different manner, Dr. Trifunac's testimony is inconsistent, or at best confusing, on the matter of "unwarranted freedom from expert judgment." This is so because Dr. Trifunac also testified that Appendix A gives an engineer, seismologist, or geoscientist working under the Appendix some degree of freedom to select what seems to be the most rational and reasonable method for correlating vibratory ground motion with the SSE (Trifunac, Tr. 759-60). Dr. Trifunac further testified that engineering judgment should play a "very significant role" in designing the specific acceleration under existing Appendix A (Trifunac, Tr. 764). And Dr. Trifunac recognized that Appendix A does not delineate or specify the manner or the way the acceleration level is chosen for a given SSE (Trifunac, Tr. 759).

In addition, Dr. Trifunac in his prefiled testimony appeared to criticize the spectral shape of the Regulatory Guide 1.60 response spectrum because it ". . . does not represent the correct average plus one standard deviation or average spectrum envelope, since prior to analysis for spectral shapes, all spectral amplitudes have been normalized

by the corresponding peak acceleration" (Trifunac 5). However, on cross-examination, Dr. Trifunac testified that the spectral shape of Reg. Guide 160 is adequate for many purposes and that if scaled properly, Dr. Trifunac had no major problem with its use for the design of nuclear power plants in the Eastern U.S. (Trifunac, Tr. 760-761). For Seabrook, Reg. Guide 1.60 has been scaled using the trend of the means relationship of Trifunac and Brady. The Staff's experience with the use of the trend of the means relationship of Trifunac and Brady is that it yields, and has yielded at Seabrook a conservative design response spectrum which is totally consistent with the requirements of Appendix A. (Reiter 23-25, Figures 2-3, Reiter 116-22; Tr. 708; Jackson 13-15).[23/]

Finally, in his prefiled testimony, Dr. Trifunac stated that if Reg. Guide 1.60 were used to calculate ground motion, then the multiplication factor could be by "the average peak ground acceleration" or "the average peak ground acceleration plus one standard deviation" (Trifunac 5). Dr. Trifunac, however, stated that he had no opinion as to which multiplication factor should be used with Reg. Guide 1.60 (Trifunac, Tr. 742-43).

Based upon the above, the Staff submits as the proposed conclusion on the second issue that it has been clearly demonstrated that the

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23/ The mean peak acceleration for MMI VIII earthquakes using Trifunac & Brady was .166g with a standard deviation of .08g. The .25g was the trend of the means. It should be noted that .25g was actually well above the recorded, actual mean of the data. The actual mean of the data was .16g, but the trend line was at .25g (Reiter, Tr. 649).

Staff's methodology for correlating vibratory ground motion with the SSE is consistent with Appendix A to 10 C.F.R. Part 100. The Staff believes that it has been thoroughly demonstrated that the Staff's methodology, as applied to Seabrook, has yielded a seismic design of MMI VIII, .25g, Reg. Guide 1.60, that is consistent with Appendix A, and is safe, and conservative. Moreover, the record shows that increasing the "g" value as proposed by NECNP would detract from the overall safety of the Seabrook plant, which has a seismic design as high as any nuclear power plant in the United States East of the Rocky Mountains.

IV. OUTSTANDING MOTIONS

A. Introduction

The Appeal Board ruled upon all prehearing motions and other motions presented during the course of this remanded proceeding with the exception of motions to strike portions of the Staff's prefiled direct testimony of February 17, 1981, as well as portions of the Staff's prefiled rebuttal testimony of March 16, 1981, which motions were made by NECNP on Wednesday, April 8, 1981. The Appeal Board requested that arguments pertaining to NECNP's motions to strike be included in the proposed findings of the interested parties. At the outset, it should be noted that NECNP did not raise these matters prior to the hearing but waited until all the testimony in question was moved into evidence. Even though NECNP's motions to strike were based, in part, upon the question of the scope of the proceeding, neither the Board, nor any

party, had been informed by NECNP of its objections to the scope of the Staff testimony filed.[24/] In that regard, the testimony filed by the Staff was outlined in summary form on October 17, 1980 in "Memorandum Of The NRC Staff In Response To Appeal Board Order of September 29, 1980 To Identify Nature, Scope, and Timing Of Testimony On Remanded Seismic Issues." That testimony was accepted as offered, subject to further consideration of the matter by the Board in light of the motions to strike of NECNP (Tr. 493). NECNP moved to strike Dr. Reiter's testimony, question and answer 9 (see Tr. 46 et seq.); Dr. Jackson's testimony question and answer 5 (Id, Tr. 469); and all of Mr. Knight's testimony (see Tr. 488 et seq.). NECNP also moved to strike question and answer 2 of Dr. Reiter's rebuttal testimony (Tr. 488).

B. The Factual Setting: The Testimony In Question

NECNP has moved to strike question and answer 5 of Dr. Jackson's testimony on the ground that such testimony "goes beyond the scope of this hearing" (Tr. 468-69). In this regard it is important to note that the testimony contained on pages 9-12 of Dr. Jackson's testimony was also intended to be a continuation of Dr. Jackson's response to the previous question 4, which addressed the factual validity of Dr. Chinnery's methodology (Jackson 3; Jackson Tr. 475). This can be seen from

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24/ A timely opportunity for NECNP to make its objections was at the conclusion of the final conference call, conducted in this proceeding prior to hearing, wherein the Chairman asked the parties if there were any other matters warranting the attention at the Board and the parties prior to the scheduled April 6th hearing. That final conference call was conducted on March 18, 1981 to discuss the request of NECNP for permission to contact and consult with Board witness Dr. Mihailo Trifunac (See unpublished "Memorandum and Order" (March 19, 1981)).

examining the substance of the testimony subject to the motion. On pages 9-10 of his testimony, Dr. Jackson addresses the matter of the scope of Dr. Chinnery's methodology, i.e., the manner in which Dr. Chinnery's methodology fits or does not fit into Appendix A. On page 11 of his testimony, Dr. Jackson discusses Dr. Chinnery's methodology in light of its proferred use by NECNP, i.e., that a SSE greater than MMI VIII must be used for the seismic design of Seabrook. From that point, Dr. Jackson discusses seismic risk in light of Dr. Chinnery's probability of exceeding figure of 10^{-7}. Finally, on page 12 of his testimony, Dr. Jackson discusses current research into probabilistic methods for determining seismic hazard and risk. As to the latter point, it should be recalled that NECNP contends that Dr. Chinnery's methodology is the "state of the art" in seismology. Based upon the legal standards which the Staff submits govern the receipt of such testimony, as discussed in the next section, the Staff believes that NECNP's motion to strike Dr. Jackson's testimony should be denied. Indeed, NECNP's primary quarrel appeared to be with question 5, and not the answer to that question. The question is not evidentiary and, in this instance, could be disregarded from a substantive evidentiary standpoint.

NECNP has also moved to strike question and answer 9 to Dr. Reiter's testimony on the same ground, i.e., that it went beyond the scope of the hearing (Tr. 468). In his answer to question 9, Dr. Reiter first considered the scope of possible use of Dr. Chinnery's methodology (and probabilistic methods in general) by the NRC Staff (Reiter 11-12). Dr. Reiter then estimated the uncertainty associated with Dr. Chinnery's

methodology by comparing it, and the results of its application with another published study which also calculated the intensities associated with earthquakes of different return periods along the eastern seaboard close to Seabrook (Reiter 13-14). Dr. Reiter next compared the results of Dr. Chinnery's methodology at Seabrook with the results of a general probabilistic methodology wherein various experts including Dr. Chinnery were canvassed as to what each expert's choice of input parameters would be for seismic source zones in the Central and Eastern United States (Reiter, 14-15). The results of this study in which Dr. Chinnery participated were then compared with the present seismic design of Seabrook (Reiter 15-16).[25/]

NECNP also moved to strike all of Mr. Knight's testimony (Tr. 488 et seq.). Again, at the time that Mr. Knight's testimony was filed, the Staff did not know whether either Dr. Chinnery or Dr. Trifunac would challenge the adequacy or the conservatism of the present seismic design of Seabrook of MMI VIII, .25g, Reg. Guide 1.60. In that regard, it should be recalled that NECNP had, based upon Dr. Trifunac's prior testimony, previously advocated that the appropriate "g" value anchor point for Reg. Guide 1.60 should be .4g, a view apparently embraced by

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25/ Since the Staff was required to file testimony on this issue simultaneously with Dr. Chinnery, the significance of this comparison (which demonstrated the adequacy as well as the conservatism of the present seismic design of Seabrook) would have been crucial if Dr. Chinnery had testified in this proceeding that the present seismic design of Seabrook, posited upon an SSE of MMI VIII, was something other than adequate and conservative).

Appeal Board member Mr. Farrar.[26] As the assistant director responsible for the conduct of operations of, inter alia, the Geosciences Branch, Mr. Knight first described the definition of seismic risk and how the geological and seismological input are translated into seismic design (Knight 2-7). Mr. Knight next discussed the meaning and use of the "g" value and of Reg. Guide 1.60 (Knight, 7-8). He discussed the relationship between vibratory ground motion and the maximum acceleration readings on seismographs (Knight 8). He also discussed the spectral shape of Reg. Guide 1.60, and the "frequency range of interest" of nuclear power plants. The parties were directed by the Commission to discuss these matters. See 12 NRC at 298. Finally, Mr. Knight discussed the engineering conservatisms already included in the present seismic design of Seabrook. (Knight 9-15). In part, this testimony was prompted by questions by the Commission to the Staff at the public meeting held May 29, 1980. In that regard it should be noted that the two issues which the Commission requested the parties to address at that public meeting were essentially the same issues encompassed in the Commission's remand order; viz, 1; "the Staff's methodology for establishing the design ground acceleration associated with a Safe Shutdown Earthquake" and; 2, "Dr. Chinnery's methodology for calculating recurrence times of larger than historical earthquakes in a tectonic province."[27] In that

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26/ See 6 NRC at 106, 111-13;

27/ See letter from Leonard Bickwit, Jr. to each party (April 29, 1980).

public meeting, the Commission, apparently in considering both the question of the validity of Dr. Chinnery's methodology as well as the Staff's methodology for correlating vibratory ground motion in connection with NECNP's petition, introduced, within those two questions, the subject of margin of error, or the sensitivity of reactor design to "g" values anchoring response spectra. There were a number of questions asked of the Staff, by the Commission, regarding that matter, the most closely related question to the testimony in question was: "How sensitive is the reactor design to these numbers as you go up the scale, assuming once you get up there you really are going to have to make some major modifications?"[28/] The final pages of Mr. Knight's testimony address the question of margin of error and sensitivity of the reactor to the Staff approved seismic design based upon an examination (including a field examination) of the Seabrook plant. Indeed, the Staff in its October 17, 1980 filing outlining the nature and scope of its testimony expressly stated that it would present expert structural engineering testimony on the present seismic design of Seabrook. Moreover, both Dr. Chinnery and Dr. Trifunac testified at the hearing that a decision such as seismic design must by necessity embody and include structural engineering judgments (Chinnery, Tr. 113-114; Trifunac, Tr. 764). Thus, the safety factors built into the structure are an essential part of the assessment of seismic risk. This conclusion is wholly supported by Appendix A which not only defines the Safe Shutdown Earthquake in

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28/ "Public Meeting, Oral Presentation In Seabrook Seismic Issue" p. 52 (May 29, 1980). See also Tr. 45-52).

terms which expressly include structural engineering,[23/] but which also requires structural engineering input into the seismic design process. Thus, Appendix A provides, in pertinent part:

> "The geologic, seismic and engineering characteristics of a site and its environs shall be investigated in sufficient scope and detail to provide reasonable assurance that they are sufficiently well understood to permit an adequate evaluation of the proposed site, and to provide sufficient information to support the determinations required by these criteria and to permit adequate engineering solutions to actual or potential geologic and seismic effects at the proposed site. The size of the region to be investigated and the type of data pertinent to the investigations shall be determined by the nature of the region surrounding the proposed site. The investigations shall be carried out by a review of the pertinent literature and field investigations and shall include the steps outlined in paragraphs (a) through (c) of this section." 10 C.F.R. Part 100, Appendix A § IV.

29/ "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. These structures, systems, and components are those necessary to assure:

(1) The integrity of the reactor coolant pressure boundary.

(2) The capability to shut down the reactor and maintain it in a safe shutdown condition, or

(3) The capability to prevent or mitigate the consequences of accidents which could result in potential offsite exposures comparable to the guideline exposures of this part." 10 C.F.R. Part 100, App. A, § III(c) (emphasis added).

With respect to Mr. Knight's testimony, it should also be pointed out that NECNP voluntarily assumed a risk in relying exclusively on its motion to strike, rather than attempting to counter such testimony, by rebuttal testimony. The Staff notes in this regard that NECNP did file counter-affidavits to Mr. Knight's affidavit in connection with NECNP's "Motion To Suspend Construction" filed October 19, 1980.[30/]

The final motion to strike was filed with respect to the second question and answer of Dr. Reiter's rebuttal testimony (Tr. 488). The sole argument advanced by NECNP was apparently that the testimony discussed the probabilities of risk "over an area as opposed to probability over time" (Tr. 488). One of the essential points made in Dr. Reiter's rebuttal testimony in criticising Dr. Chinnery's estimates of seismic risk in the Boston-New Hampshire zone (see Chinnery 16) is that the risk may differ by several orders of magnitude depending upon the size of the province selected for study (Reiter Rebuttal 1-2). Thus, the rebuttal testimony, as is inherent in any such testimony, critiques the calculations of seismic risk that Dr. Chinnery used on the basis of both the size of the province and the question of whether the risk computation was for the province or specifically for the site. NECNP's position is apprarently that such criticisms are beyond the scope of the proceeding, but that Dr. Chinnery's seismic risk calculations are within the scope of the proceeding. The Staff's position is that Dr. Reiter's testimony is proper rebuttal, and that the motion to strike question and

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30/ See "Affidavit of Gregory C. Minor" appended to "NECNP Reply To Licensee And Applicant [sic] Arguments On Its Motion To Suspend Construction" (December 4, 1980).

answer 2 of Dr. Reiter's rebuttal testimony should be denied. Once Dr. Chinnery feels that his methodology permits him by linear extrapolation to predict, numerically, seismic risk, the Staff submits that those numbers are subject to evaluation and criticism as was done in Dr. Reiter's rebuttal testimony.

3. Applicable Legal Standards

The question of whether NECNP's motion to strike portions of the Staff's expert testimony should be granted or denied inherently involves the question of whether such testimony is admissible expert testimony within the scope of the remanded seismic issues. At the outset, we should start, it is suggested, with two observations. The first, which is case specific, is the very first sentence of the Commission's remand Order:

> The New England Coalition on Nuclear Pollution (NECNP) has petitioned the Commission to review certain aspects of the Atomic Safety and Licensing Appeal Board's decisions regarding the seismic design of the Seabrook nuclear power plant. 12 NRC at 96. (emphasis added).

That sentence serves as a reminder, the Staff submits, that this proceeding has been framed in the specific context of seismic safety issues regarding the construction of nuclear power plants. This is not an academic controversy as to the acceptability (or not) of methodologies.

The second observation, which is, in essence a legal precept, and that goes to the heart of the motions to strike testimony, is that even prior to the Administrative Procedure Act, administrative agencies, by a series of Supreme Court decisions, have been freed "from the compulsion of technical rules [of evidence] so that the mere admission of matter

which would be deemed incompetent in federal [court] proceedings would not invalidate the administrative order." Consolidated Edison v. NLRB, 305 U.S. 147, 229-230; 59 S. Ct. 206; 83 L. Ed. 12 (1938).[31/] Essentially this view has been codified in the Administrative Procedure Act, which provides, in pertinent part:

> . . . Any oral or documentary evidence may be received, but the agency as a matter of policy shall provide for the exclusion of irrelevant, immaterial, or unduly repetitious evidence. 5 U.S.C. § 556(d).[32/]

Although there are relatively few decisions explaining the reasons underlying this principle, one older, but still viable precedent is Willapoint Oysters, Inc. v. Ewing, 174 F.2d 676 (9th Cir. 1949), cert. den. 338 U.S. 860, reh. den. 339 U.S. 945. In that decision, the ninth circuit Court of Appeals explained, in part, the basis for this principle. The Court noted that agencies should exclude irrelevant, immaterial, or unduly repetitious evidence as a matter of efficiency and good practice, but it noted that in a federal administrative proceeding (here the Department of Agriculture), "there is no lay jury to be protected from improper influence" of such evidence. 174 F.2d 676, 690 n. 19. The Court concluded:

31/ Accord, Opp Cotton Mills v. Administrator, 312 U.S. 126, 155 (1941); Tagg Brothers & Moorehead v. U.S. 280 U.S. 420, 442 (1920); ICC v. Louisville & Nashville Railroad Co., 227 U.S. 88, 93 (1913); Swift & Company v. U.S., 308 F.2d 849, 852 (7th Cir. 1962).

32/ The counterpart in the Commission's rules of Practice is 10 C.F.R. § 2.743(c).

> Thus the receipt of such evidence, over objection, and even if "immaterial, irrelevant, etc. is not such a grievous error as to require reversal. The receipt of irrelevant, immaterial and hearsay evidence is no cause for reversal of an administrative order though the validity of the order can never rest upon conjecture, guess or chance. 174 F.2d at 690.

This pronouncement in Willapoint Oysters is consistent with the view of the leading commentator, Kenneth Culp Davis, who states:

> Irrespective of what agencies' regulations provide, reviewing courts sometimes push agencies toward admitting nearly all relevant evidence. The basic attitude of many courts differs drasticaly from the basic attitude behind the Federal Rules of Evidence; many courts favor admission of all evidence whose admissibility is doubtful. Kenneth Culp Davis, "Administrative Law of the Seventies," § 14.07 p. 96 (1978 Supp.) (emphasis added).

Thus, according to Davis, close questions of admissibility should be resolved in favor of admission.33/

One helpful decision as a guide to admissibility of evidence where there are challenges to its relevancy, in administrative proceedings is Rosedale Coal Co. v. Director of U.S. Bureau of Mines, 247 F.2d 299 (4th Cir. 1957). In Rosedale the seventh circuit concluded, on the basis of Opp Cotton Mills v. Administrator, supra, that "the strictness of the rules of evidence which are observed in courts of law may be relaxed somewhat in an administrative proceeding if fundamental fairness is not departed from." 247 F.2d at 305 (emphasis added).

In that context, disposition of the pending motions would be facile. The questions would become, is it "fundamentally fair" to permit

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33/ The Staff does not believe that such "close questions" have been herein presented.

Mr. Knight to address in his testimony the concept of "maximum effective acceleration" as directed by the Commission? 12 NRC at 298. Is it "fundamentally fair" for Mr. Knight to address in prefiled testimony the margin of error inherent in the seismic design of the Seabrook plant, in light of the conservatisms in its design, in light of the Commission's questions to him in this proceeding on this very subject? Is it fundamentally fair for Dr. Reiter in his rebuttal testimony to critique Dr. Chinnery's quantitative estimates of seismic risk in the Boston-New Hampshire zone (the size and shape of which were drawn by Dr. Chinnery) on the ground that both the size of the province and the location of the risk would seriously affect Dr. Chinnery's calculations? Is it fundamentally fair for Dr. Jackson in his prefiled testimony to discuss Dr. Chinnery's methodology in light of its intended use by NECNP, the requirements of Appendix A, and Dr. Chinnery's calculation of seismic risk? On the basis of the foregoing, the Staff submits that NECNP's motions to strike testimony should be denied. There is no jury to be prejudiced. Any objections to the testimony interposed by NECNP that the Appeal Board considers to have validity should go to the weight, and not the admissibility of such testimony.

In taking this position as to the admissibility of expert testimony in administrative proceedings, the Staff does not concede that if strict rules of evidence were to be followed, the expert testimony in question would be inadmissible. In fact, the Staff believes that such testimony would be admissible in the federal courts, in a jury trial. The test for the admissibility of such expert testimony under the "strict rules of evidence" would be whether such testimony would assist the trier of fact

in understanding the evidence or determining a fact in issue. Jenkins v. U.S. 307 F.2d 637, 643 (D.C. Cir. 1962). To put it another way, the test for the admissibility of such expert testimony would be "not whether it was strictly necessary" but "would it be helpful to the jury in reaching a sound result." Lofton v. Agee, 303 F.2d 287, 288 (8th Cir. 1962); see also Goldwater v. Ginzburg, 414 F.2d 324, 343-44, (2d Cir. 1969), cert. den. 397 U.S. 978 (1970), reh. den. 397 U.S. 978 (1970). Utilizing such tests, the Staff submits that the testimony in question would also be admissable. For example, in commenting upon the "factual validity" of Dr. Chinnery's methodology, Dr. Reiter estimated the uncertainty associated with Dr. Chinnery's methodology by comparing it and its results with the results of a general probabilistic methodology in which Dr. Chinnery participated. Dr. Jackson discussed Dr. Chinnery's methodology in the context of its proferred use by NECNP, i.e., that an SSE greater than MMI VIII must be used for the seismic design of Seabrook. The Staff submits that such testimony assists the trier of fact in resolving the issues in this proceeding, and that the testimony would be admissable in a judicial proceeding. For all of the above reasons, the Staff believes that NECNP's motion to strike Staff testimony as previously outlined, should be denied.

V. CONCLUSION

For the reasons set forth above, the Staff believes it has been clearly demonstrated that Dr. Chinnery's particular version of a probabilistic methodology lacks factual validity and such methodology is not presently an acceptable way to proceed for making licensing decisions as to the seismic design of nuclear power plants. The Staff believes,

moreover, that it has been clearly demonstrated that the Staff's methodology for correlating vibratory ground motion with the Safe Shutdown Earthquake is consistent with Appendix A to 10 C F.R. Part 100. Moreover, in the course of addressing these two questions it is uncontradicted that the present seismic design of Seabrook of Modified Mercalli Intensity VIII, .25g, Regulatory Guide 1.60 is acceptable, safe, and conservative. Finally, for the reasons stated, the Appeal Board should deny NECNP's motion to strike portions of the Staff's prefiled testimony.

Respectfully submitted,

RP Lessy

Roy P. Lessy
Deputy Assistant Chief
Hearing Counsel

Dated at Bethesda, Maryland
this 16th day of June, 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 copies of "PROPOSED FINDINGS OF FACT, CONCLUSIONS OF LAW, AND SUPPORTING ARGUMENT OF THE NRC STAFF ON REMANDED SEISMIC ISSUES" in the above-captioned proceeding have been served on the following by deposit in the United States mail, first class, or, as indicated by an asterisk, through deposit in the Nuclear Regulatory Commission's internal mail system, this 16th day of June, 1981:

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