Weston Geophysical
CORPORATION

May 9, 1980

Howard Levin
U.S. NRC
Washington, D.C. 20555

Dear Mr. Levin,

At your request, enclosed is a listing of the observed intensities at Northeastern U.S. localities for the December 20 and 24, 1940 Ossippee N.H. earthquakes. Discussion of the results of regression analyses performed on this data set are also included in this transmittal.

A listing of observed intensities for the Cornwall-Massena earthquake of September 5, 1944 will be forwarded in the near future pending inclusion of felt reports observed at Canadian sites.

Data cards of the Ossippee felt reports and the results of regression analysis were sent to TERA Corporation.

Sincerely,

George C. Klimkiewicz

George C. Klimkiewicz

GCK:eag
Enclosure

8301110055 821210
PDR FOIA
GALL082-399 PDR

The Ossippee, N.H. earthquakes of December 20, 24, 1940 are cataloged as having maximum Modified Mercalli Intensities of VII. Due to the occurrence of 2 similar size shocks within the narrow time frame of four days, the public's response to U.S. Coast and Geodetic Survey and the Northeast Seismological Association questionnaires, contained descriptions of the combined effects of both events. Ground Motion effects were not easily associated with any one of the shocks. For this reason, all acquired felt reports, for both events were combined into one data base. The U.S. Department of Commerce publication, "United States Earthquakes, 1940," contains a listing of the intensities interpreted for numerous localities throughout the Northeast, excluding Canada, for the pair of earthquakes. The intensity information in this listing was compared to the original questionnaire descriptions of the felt reports. No reevaluation of intensity, from those listed in USE, 1940, was deemed necessary.

The data base for the attenuation study, includes the information in the USE, 1940 list. Coordinates of the localities for the individual felt reports were determined, and distances computed from the cataloged epicenter coordinate. Regression analysis on intensity as the dependent variable, was performed to determine the attenuation model in the standard form:

$$I_{site} = C_0 + C_1\Delta + C_2 \log_{10}\Delta$$

Two models were computed due to the manner in which the lower intensity data were presented. The localities reporting intensities lower than IV were grouped into an undifferentiated category of MMI=I-III. These localities were assumed to have an average intensity of II in one regression analysis and were assumed to have the maximum intensity of III in the other regression analysis.

REGRESSION MODELS FOR ATTENUATION

PROCEDURE 1 MMI I-III → MMI II

The data points and the regression model for this procedure are shown in Figure 1.

Regression Results:

Intercept:	C_0	0.7441293 E+01
Regression Coefficients:	C_1	-0.4010946 E-02
	C_2	-0.1534778 E+01

Standard Deviation of Regression Coefficients:	C_1	0.6140185 E-03
	C_2	0.3330754 E+00
Multiple Correlation Coefficient:		0.7483367 E+00
Standard Error of Estimate:		0.1213055 E+01

Final Model:

$$I_{site} = 7.441 - .0040\Delta - 1.535 \log_{10}\Delta \qquad \text{Eq. 1}$$

Plotted against the above model in Figure 1 is the model developed on the basis of distance being the dependent variable for a given intensity decrement (Eq. 3). This model is clearly conservative, when used to estimate the site intensity at a given distance, since the curve envelopes most of the observed felt reports in the range of distance of interest, namely distances less than 100-150 km.

PROCEDURE 2 MI I-III → MMI III

The data points and regression model for this procedure are shown in Figure 2.

Regression Results:

Intercept	C_0	0.6693773 E+01
Regression Coefficients:	C_1	-0.4888499 E-02
	C_2	-0.9829572 E+00
Standard Deviation of Regression Coefficients:	C_1	0.5589552 E-02
	C_2	0.3032062 E+00
Multiple Correlation Coefficient:		0.7736629 E+00

Standard Error
of Estimate: 0.1104272 E+01

Final Model:

$$I_{site} = 6.694 - .0049\Delta - 0.983 \log_{10}\Delta$$

COMPARISON OF MODELS

Figure 3 compares the results of regression procedures 1 and 2 using Intensity as the dependent variable, with the results determined by using distance as the dependent parameter.

Models 1 and 2 are preferred for determining the best estimate of Intensity at a given distance for the Ossippee N.H. earthquakes.

FIGURE 1

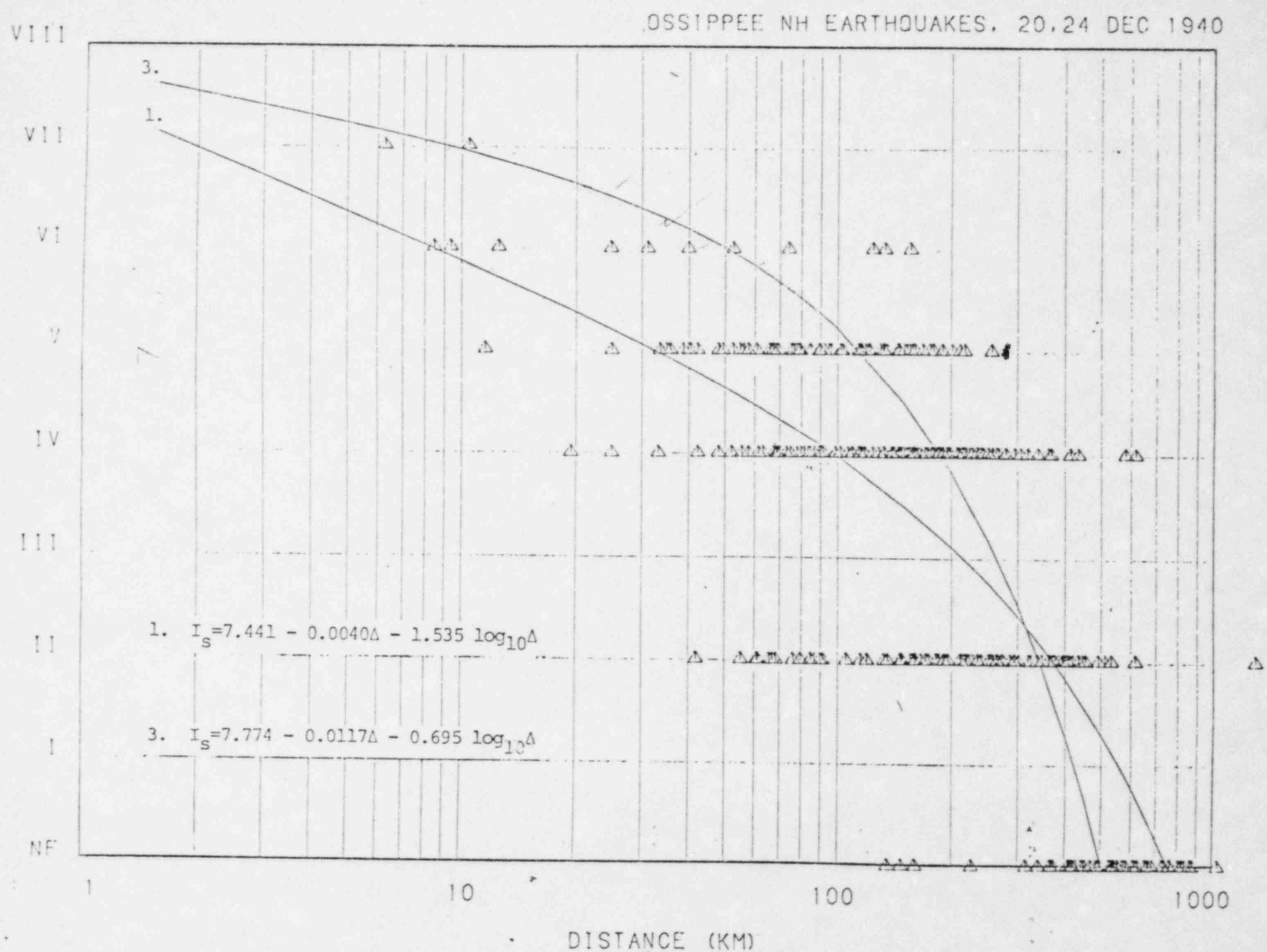

OSSIPPEE NH EARTHQUAKES. 20,24 DEC 1940
VIII
VII
VI
V
IV
III
II
I
NF
3.
1.
1. $I_s=7.441 - 0.0040\Delta - 1.535 \log_{10}\Delta$
3. $I_s=7.774 - 0.0117\Delta - 0.695 \log_{10}\Delta$
1
10
100
1000
DISTANCE (KM)
Weston Geophysical

FIGURE 2

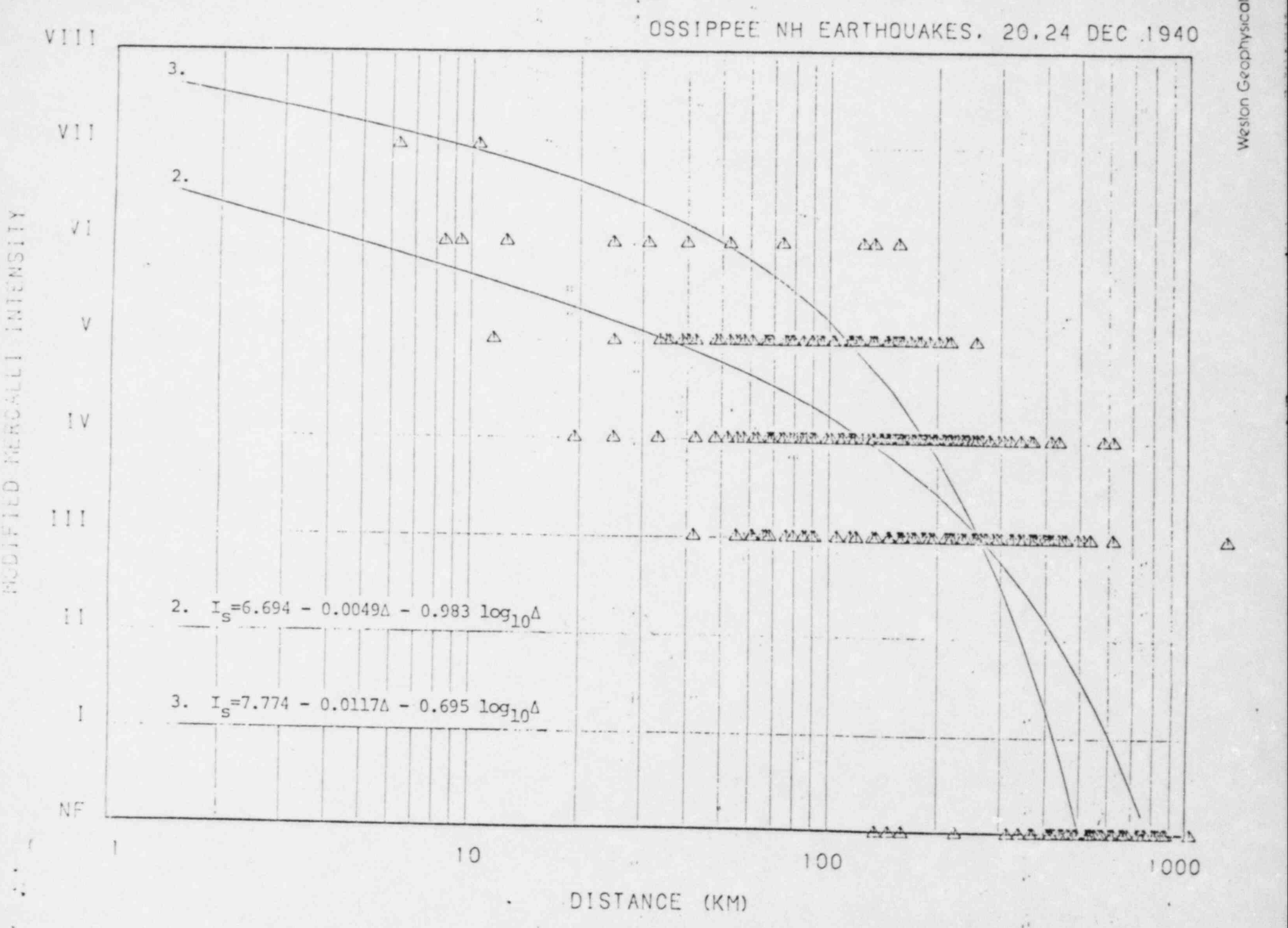
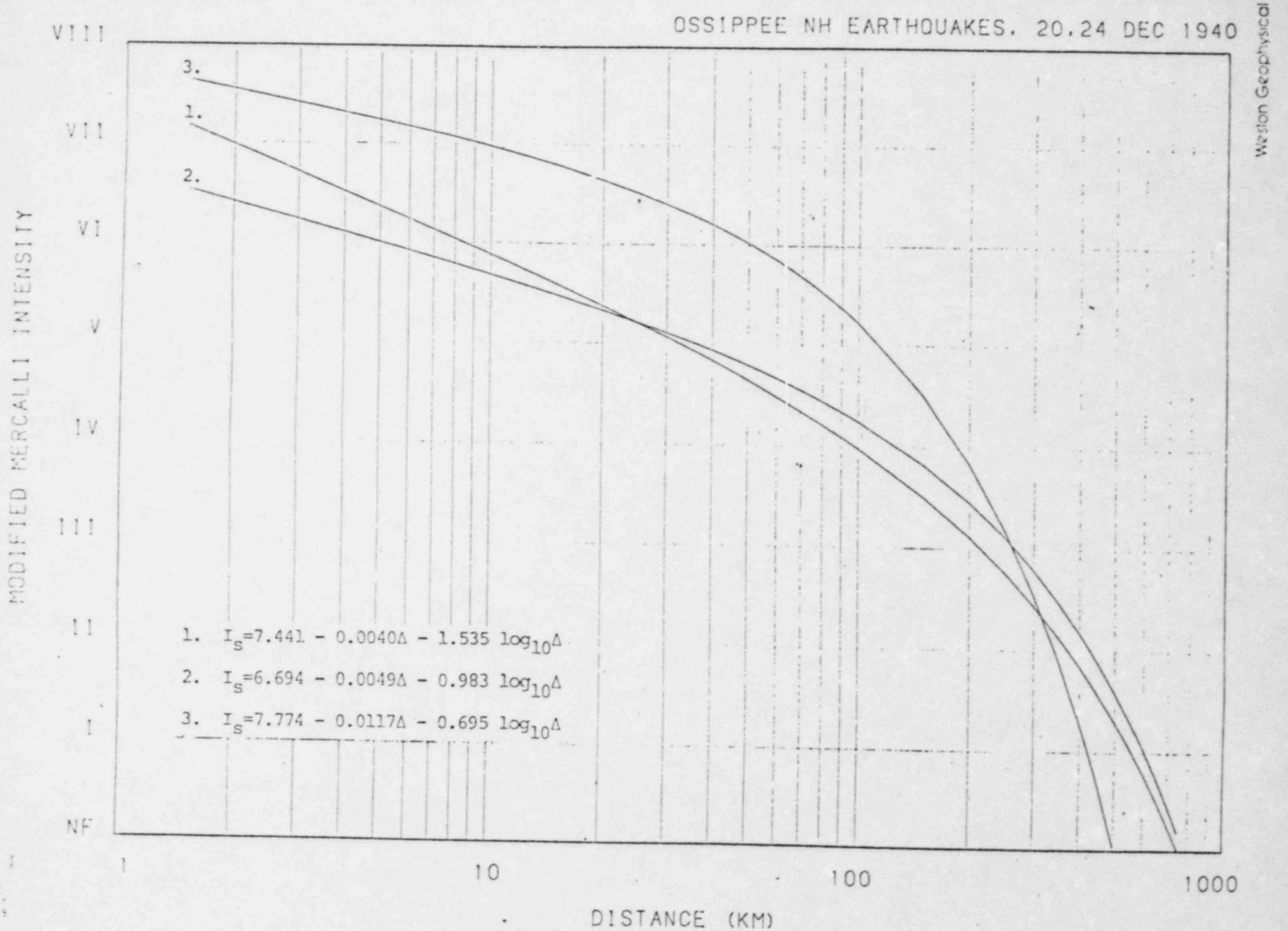

OSSIPPEE NH EARTHQUAKES, 20.24 DEC 1940
Weston Geophysical
MODIFIED MERCALLI INTENSITY
VIII
VII
VI
V
IV
III
II
I
NF
3.
2.
2. Is=6.694 - 0.0049Δ - 0.983 log10Δ
3. Is=7.774 - 0.0117Δ - 0.695 log10Δ
1
10
100
1000
DISTANCE (KM)

FIGURE 3



OSSIPPEE NH EARTHQUAKES. 20.24 DEC 1940
Weston Geophysical
MODIFIED MERCALLI INTENSITY
VIII
VII
VI
V
IV
III
II
I
NF
1
10
100
1000
DISTANCE (KM)
3.
1.
2.
1. $I_s = 7.441 - 0.0040\Delta - 1.535 \log_{10}\Delta$
2. $I_s = 6.694 - 0.0049\Delta - 0.983 \log_{10}\Delta$
3. $I_s = 7.774 - 0.0117\Delta - 0.695 \log_{10}\Delta$

DATA FORMAT

Ossippee Earthquakes, December 20, 24   Felt Reports

Card 1

Col.	Parameter	Format
1-8	Epicenter Latitude	F8.4
9-16	Epicenter Longitude	F8.4
17-20	blank	4x
21-80	Earthquake Name, Date	6A10

Cards 2→N

Col.	Parameter	Format
1-2	Modified Mercalli Int.	I2
3	blank	
4-5	Degree part of Latitude(N) of Felt Report	F2.0
6-9	Minute Part of Latitude	F4.1
10	blank	1x
11-12	Degree part of Longitude(W) of Felt Report	F2.0
13-16	Minute Part of Longitude	F4.1
17-19	blank	3x
20-59	Locality of Felt Report	4A10

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July 8, 1980

Lawrence Livermore National Laboratory
Attn: D. L. Bernreuter, L-90
P. O. Box 808
Livermore, CA 94550

Dear Don:

We are providing herein our revised results for the scaling between response spectra of different damping values. As in the August 1979 report, we have used our improved data base for regression on the decimal damping factor for a fixed site intensity and log (distance). We have excluded the undamped spectral ordinates from the regression because of their statistical instability. As the attached table indicates, there is very little difference between the previous results and these new results.

The application of this scaling law to the SEP results requires an assumption whose validity we have not evaluated. The required assumption is that the source zone loads that make up the spectral ordinates are proportioned the same, independent of damping factor. While this seems reasonable, the process by which we combine loads is sufficiently complex that we cannot be absolutely certain of the necessary independence.

Very truly yours,

Larry

Lawrence H. Wight
Vice President

LHW/hlj
Attachment

7/15/80 -

xc: Dr. Leon Reiter
Mr. Howard Levin

TERA CORPORATION
2150 SHATTUCK AVENUE BERKELEY, CALIFORNIA 94704 415·845·5200

SUMMARY OF REGRESSION RESULTS

$$\log(GM) = C_I + C_2 I_s + C_3\Delta + C_4 \log(r)$$

I_s - MMI

r - kilometers

Δ - decimal damping

GM	Units	Frequency Hz	C_3 Aug. 1979	C_3 Current
PSA	cm/s^2	25.00	*	*
PSA	cm/s^2	20.00	*	*
PSA	cm/s^2	15.30	-0.337	-0.290
PSA	cm/s^2	12.50	-0.639	-0.600
PSA	cm/s^2	10.00	-0.954	-0.904
PSA	cm/s^2	7.70	-1.310	-1.270
PSA	cm/s^2	5.00	-1.680	-1.700
PSA	cm/s^2	3.30	-1.900	-1.990
PSA	cm/s^2	2.50	-1.920	-1.950
PSA	cm/s^2	1.33	-1.750	-1.810
PSA	cm/s^2	1.00	-1.900	-1.960
PSA	cm/s^2	0.50	-1.520	-1.600

* statistically insignificant coefficient

April 4, 1980

Lawrence Livermore Laboratory
Attn: Mr. D.L. Bernreuter, L-90
P.O. Box 808
Livermore, California 94550

Dear Don:

We have received and reviewed the letters written by the Attenuation Panel Members and are herein providing a summary of their views and our recommendations for a final attenuation model for use in the SEP. Our recommendations are, of course, built primarily upon our intuition and experience, but are reinforced by ideas advanced from all participants in the attenuation panel and the summary letters provided by the panel members. For your convenience, we have consolidated the panel member views on Attachment A.

Our recommendations are in two parts; First, we indicate how the presently available SEP results could best be applied, given our current perspective of attenuation. Second, we provide recommendations for further analysis that will, we believe

(1) Confirm the short term recommendations, and

(2) result in a significant advance in understanding of the EUS attenuation.

Our short term recommendation for interpretation of the current SEP results is to use our latest regression results [of the form log (GM)=F(M,I_S)] with the Ossippee intensity attenuation relation. We strongly recommend that this be used for the CUS sites, on the basis of the good, and often slightly conservative comparison between this model and Nuttli's theoretical model for CUS. Application of the same model to NEUS sites is probably more conservative, since there is some evidence that the absorption coefficient is slightly greater there. It would be very interesting to see what Nuttli's theoretical model would yield for values of the absorption coefficient appropriate to NEUS.

In terms of longer range recommendations, we propose the following:

(1) We strongly favor an empirical approach that builds upon the relation log (GM)=F(M,I_S). We acknowledge, however, certain statistical biases that result from this formulation. Specifically, the limited range of site intensity values in the CIT data base result in substantial extrapolations beyond the data in application of the model - and furthermore, the use of an intermediate, imperfectly correlated, variable always introduces a bias.

These two problems may be interrelated and motivates us to propose the following. Start with two WUS models; first a WUS ground motion model of the form log (GM)=F(M_L,r) and second a WUS intensity attenuation model of the form I_S=F (M_L,r). Elimination of r between these two equations results in a model of the form log (GM)=F(M_L,I_S) which should not suffer from some of the statistical biases that result from direct estimation.

(2) The next step would be to incorporate your analyses with Nuttli's to develop a model relating M_L to m_b.

(3) We feel it is important to subject the EUS intensity data to additional analysis and interpretation. Multiple regression analysis of the data set represents one option, while application of Gupta-Nuttli to CUS with development of a similar model for NEUS represents a second option. We slightly favor the latter approach.

(4) Combination of these models results in log (GM)=F(m_b,r). We advocate verification of this model by comparison with Nuttli's peak acceleration model. We feel this comparison should be made only over the distance range 25-200 kilometers. Major discrepancies would be reconciled, but miner differences would be accepted. We envision this comparison would be for both CUS (Nuttli's existing model) and NEUS (which would be developed).

(5) We recommend, on the basis of an anticipated favorable comparison with Nuttli's model, that the final models be incorporated into the hazard analysis with a natural lognormal value for dispersion of 0.65. The distribution should be truncated at some limiting acceleration, such as 2g, but for practical purposes the same results could be obtained by truncating at three sigma. Finally, on the basis of scale saturation arguments, the acceleration model should have a limit corresponding to m_b 6.7.

We hope these ideas and views will help you in presenting an overall recommendation to the NRC and we are prepared to elaborate on these points with you at any convenient time.

Sincerely,

Larry

Lawrence H. Wight
Vice President

LHW/plh
Attachment
cc: Dr. Leon Reiter

Attachment: letter to
D.L. Bernreuter,
Dated April 4, 1980

Summary of Attenuation Panel Member Views

Ground Motion Model Building

Nuttli proposed that models be developed differently depending on the distance range. In the near field (<25 km) he proposed that any empirical approach would be adequate while in the intermediate field (25-200 km) he favored his theoretical approach or an empirical approach that started with the relation log (GM) = $F(M, I_S)$. In the far field, he recommended his theoretical approach. Trifunac was consistent with this in his overall recommendation in that he proposed seeking the functional forms that have the most theoretical basis and then selecting among them on the basis of an anslysis of residuals in the regression analysis. Donovan pointed out the biases in going through intermediate parameters while McGuire discussed these biases in much more detail. McGuire therefore recommended use of Nuttli's theoretical results.

Intensity Attenuation

McGuire recommended using the actual intensity data as opposed to basing results on isoseismals. He also pointed out the significance of the focal depth on the intensity distribution. He further acknowledged the possible significance of a non-linear scaling between epicentral intensities but observed that there is likely insufficient data to support such a model. Finally, McGuire proposed that the epicentral intensity report for given earthquakes be carefully evaluated, perhaps using a method such as developed by Chandra. Donovan also emphasized the possibility of non-linear scaling and further observed that since site intensity data were often anomolous due to site characteristics that the data should be carefully reviewed.

Data Base Issues

Trifunac advocated the use of all the CIT data with disregard for the "triggering" problem ("... not sure it makes all that much difference..."). He suggested that European data not be used and he proposed that separate models for both horizontal and vertical components ought to be developed. McGuire, on the other hand, recommended that the data contaminated by a late trigger be excluded from analysis, as should records with anomolously high accelerations in the far field.

Distribution of Spectral Accelerations

Trifunac suggested that the issue be treated with statistical tests, seeking the distribution with greatest statistical validity. McGuire cautioned against premature abandonment of the lognormal distribution until proper theoretical basis was available. McGuire, furthermore, observed that there was no basis for truncation of the lognormal distribution at any level.

Magnitude Scales

Donovan strongly recommended use of the M_L scale in EUS. Trifunac indicated that the scale (magnitude, intensity, ...) should be selected on the basis of quantity and quality of available data. Nuttli recommended use of the m_b scale but acknowledged several deficiencies in this (and indirectly, in any other) specification. An important point made by Nuttli, was that m_b values over 6.7 should not be considered in a hazard analysis. McGuire acknowledged the difficulty of the problem and observed that there were likely substantial differences in source characteristics between WUS and EUS.

Regression Weighting

McGuire felt that weighting was important in order that the San Fernando earthquake would not bias the results. Trifunac also advocated a weighting scheme, but cautioned against the scheme being too exotic.

Near Field Models

Nuttli advanced the idea that near field shaking could be empirically modeled in a variety of ways, that the approaches have equal validity and that the results should not be greatly different. Trifunac advocated a particular model - a direct correlation between shaking and intensity.

Dr. Reuter

(8)

TERA

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May 30, 1980

Lawrence Livermore Laboratory
P.O. Box 808
Attn: D. L. Bernreuter, L-90
Livermore, California 94550

Dear Mr. Bernreuter:

Following up on our discussions with you and the NRC on May 15, 1980, on the Site-Specific Spectrum Program (SSSP), we are writing this letter to summarize our views on the Program and the usability of available results.

As we have indicated previously, there are three points that we hope you and the NRC will consider in developing an SEP seismic design recommendation:

1) Consistent with the philosophy and goals of the SSSP, we strongly recommend that the hazard results be integrated into a risk assessment which will formally account for facility age, population, radionuclide inventory and dispersibility, and structural resistance. Much of the power of the SSSP approach is lost when a uniform hazard is selected for each facility, independent of the aforementioned risk parameters.

2) Whether the results are applied uniformly to each facility or used as input to a risk assessment, we note that the hazard is expressed as instrumental free-field ground motion, and not design ground motion. Provision should be made in the recommendation for an explicit accounting of the design implications. For example, we have separately developed case studies and statistical data to show that the design input could be as little as one-half the ground level free-field shaking.

3) We feel that there is unnecessary conservatism in the limited results that have been developed under the SSSP thus far. As an illustration, we present results below that show that the 1,000 year loads can be conservatively represented as 5,000-10,000 year loads. Any assessment of the available results should take this into account.

Summarily, if a decision has to be made at this time with the limited alternatives available to NRC, the LLL/TERA results form a conservative specification of the free-field ground motion. While complete quantification of *this conservatism probably cannot be established on the scientific level, our preliminary judgment of the level of conservatism is provided below. We believe that it is important for NRC decision-makers to have an understanding of the level of conservatism in order to reach a balanced decision. Regarding the direct use of the LLL/TERA results, we believe that provision should be allowed for explicit accounting for the differences between free-field and design ground motion and that an accounting be made of important risk parameters.*

The first two points are self-explanatory, and therefore, the balance of this letter will address the conservatism issue.

We respect the fact that the specification of appropriate level of conservatism is the NRC's responsibility, and feel a strong need for you to understand our assessment of the conservatism in the SSSP results. We hope that this will allow a more balanced decision by the NRC to be reached on the overall level of conservatism required.

In order to provide a framework for our comments below, we will briefly review the goals of the SSSP and the relationship of those goals to the quality of the available results.

As we have often observed, the goal of the SSSP has changed with time. Originally (July 1978), the SSSP was founded as a $50,000 research program directed at improving the EUS seismic data base, modifying Dr. Mortgat's seismic hazard computer code and illustrating the approach by assessing the seismic hazard at the Dresden SEP site. The program was subsequently (December 1978) expanded to include eight other EUS SEP sites. The goal of this expanded project was to perform extensive sensitivity studies on the seismic hazard (see Attachment A, Task 7) to determine the most important parameters of the problem and to, therefore, establish a basis for possibly directing the licensees to undertake specific studies that could result in licensee-proposed site-specific spectra (see Attachment B).

The program changed emphasis again in the Spring of 1979 when we began to incorporate the subjective input from the ten expert seismologists. Consistent with the framework of any expert opinion analysis, we attempted to minimize the role of TERA's opinions and, instead, replace those opinions with either an expert's opinion or a consensus. For example, although we performed a complete set of analyses for all the sites in May 1979 using a value of acceleration dispersion of 0.7, we performed the next set of analyses using a value of 0.9 based on consensus input to us. While we did not personally support this value, we nevertheless incorporated this conservative value into the analysis since the scope of the project at the time was to provide extensive sensitivity results.

Indeed, we initiated a more formal sensitivity study in November 1979 (see Attachment C) with a scope consistent with the earlier effort; that is, to "... determine quantitatively which parameters or component models are driving the results, particularly for the northeastern sites." The proposal went on to say that"... we feel that this is a most important effort that should be undertaken as soon as possible in order to provide a partial explanation for the unusually large accelerations reported in the August 24, 1979 report."

Thus, we note that the manner in which the August results were developed and the manner in which the sensitivity studies were scoped were both very exploratory and, in a limited sense, academic. Neither the formal work scope nor our informal conversations were directed at construction of our "preferred" case, although it was clearly our mutual expectation that such a case would be constructed by September 1980. In the remainder of this letter, we attempt a quantification of the conservatism in the currently available results.

The probabilistic calculation of extreme hazards in any analysis is always a blend of the available data and judgment. We herein provide our judgment at this time for the northeastern sites, based on a model that contains a more reasonable degree of conservatism. We fully expect that between now and September 1980, we will have a technical basis for removing certain additional conservatisms.

We report, in Table 1, the peak (instrumental) ground acceleration (PGA) for Expert 3 (who is typically very near the synthesis) using our judgment as to more reasonable, yet conservative input.

Attenuation - Functional Form

At this time, we favor the use of the Nuttli theoretical attenuation model for prediction of PGA at distances greater than 25 kilometers, based on:

(1) our own judgment
(2) the judgment of our attenuation panel
(3) the current confusion regarding the Ossippee data.

We feel that the Gupta-Nuttli attenuation model, as modified by us, is an adequate and conservative representation of Nuttli's theoretical model, when applied to the northeastern sites. We expect that the conservatism will be demonstrated by incorporating into Nuttli's model the actual absorption coefficient applicable to the Northeast. This was, in fact, one of our recommended efforts for the next phase of the SSSP, as summarized in our April 4, 1980 letter.

Attenuation - Dispersion

We recommend the use of a natural logarithm dispersion on PGA of 0.6, based on:

(1) recommendations from our attenuation panel
(2) our judgment and experience.

We feel strongly that even this value is high, particularly in the near-field, and we expect that certain studies currently underway will demonstrate this conservatism.

We acknowledge that a formal statistical combination of errors in the development of the attenuation model results in a higher value of dispersion*, but we believe that this is a by-product of using intensity as an intermediate parameter. We believe that the close (but conservative) agreement with Nuttli's model justifies using a substantially lower value. Furthermore, this is in keeping with the original spirit of the program of separating the random and systematic errors.

The value of 0.6 is not hastily chosen by us, but instead is representative of our judgment on dispersion for over three years, as expressed in various final reports. For example, in December 1978, we reported on a seismic hazard analysis at the Battelle Memorial Institute facilities in Columbus, Ohio, that had been performed four months previously (just as the SSSP was starting). This analysis, which used a value of dispersion of 0.6, was reviewed and approved by various NRC staff members and Dr. Newmark.

As another point of comparison, in July 1978, we reported upon our seismic hazard analysis for the Vallecitos Nuclear Center. This analysis used a western United States attenuation model with a value of dispersion equal to 0.45, based, in part, on the San Fernando data. Again, this analysis was subjected to careful review by the NRC staff and Dr. Newmark. Thus, it should be evident that we are including in our judgment additional uncertainty in our eastern United States attenuation modelling.

In terms of additional characterization of the distribution associated with this dispersion, we assume a lognormal distribution truncated at three standard deviations. We feel that this, too, is conservative, and trust that future work will support a different, less conservative distribution.

In addition to truncating at three standard deviations, we have, for purposes of this calculation, truncated all site intensities less than MMI V from the analysis. Although this does not greatly influence the results for the northeastern sites, we feel that it is an important technical point to incorporate. The basis for this is contained in the unabridged definition of the Modified Mercalli Scale (Attachment D). This clearly shows that slight earthquake damage begins at MMI VI, and even then, only to poorly constructed buildings. Furthermore, any building designed in accordance with nominal UBC standards would experience only negligible damage at MMI VII-VIII. We feel that incorporating accelerations for site intensities less than V can produce misleading representations of the actual earthquake hazard. This logic is identical to the arguments that earthquakes of low magnitude should not be included in a hazard analysis.

---

* Cornell, C.A., Baron, H., and Shakal, A.F., (1977) MIT Report No. R77-34; also in Western Geophysical Corporation reports to TVA.

Background

We report, in Table I, the results for "no background." We judge that the conservatisms in the analysis are more than sufficient to accommodate alternative models for the background.

Summarily, we note that the results presented in Table I are substantially lower than those presented in the LLL/TERA reports. The 1,000-year loads in Table I are reduced 50-60 percent from the results in the reports or, put another way, the 1,000-year loads appear to be at least 5,000-10,000-year loads. We further believe that, as the return period increases, there is marginally increasing conservatism since loads at such long return periods as 10,000 years principally result from a very conservative modelling of the tails on the acceleration distribution.

Finally, we note that an explicit accounting of the facility age, assuming a 40-year facility life and allowing a uniform probability of exceedance during the remaining life, even further reduces the loads. For example, Millstone changes from 112 to 94 cm/s^2 and Yankee Rowe changes from 127 to 92 cm/s^2, for the 1,000 year return period.

We sincerely hope that our quantification of conservatism in the SSSP and certain elements of the risk assessment can be incorporated into decision-making.

Very truly yours

L. H. Wight

Lawrence H. Wight
Vice President

cc: Dr. Leon Reiter
Mr. Howard Levin

Attachments

TABLE I

PGA (cm/s^2) LOADS FOR TERA 5/20/80
PREFERRED INPUT

	SITE				
RETURN PERIOD	Oyster Creek	Yankee Rowe	Ginna	Conn. Yankee	Millstone
200	35	64	27	59	53
1,000	91	127	81	119	112
4,000	145	193	129	182	172

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JANACEK,R.F. Commonwealth Edison Co.
RECIP.NAME RECIPIENT AFFILIATION
O'CONNOR,P. Systematic Evaluation Program Branch

SUBJECT: Comments on encl Fugro,Inc review of LLL/Tera rept re seismic hazard analysis.Variance in expert opinion suggests that program output is inaccurate.Questionable areas must be resolved in order for methodology to be useful.

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Commonwealth Edison
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Address Reply to: Post Office Box 767
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November 29, 1979

Paul O'Connor, Project Manager
Operating Reactors-SEP Branch
U.S. Nuclear Regulatory Commission
Washington, D.C. 20555

Subject: Comments on the LLL/TERA Reports
NRC Docket 50-10/237

Dear Mr. O'Connor:

Commonwealth Edison requested Fugro to review the LLL/TERA reports and to provide their comments. Attached are Fugro's general comments on the LLL/TERA reports. In addition to the Fugro comments, Commonwealth Edison's general comments are provided in the following paragraphs.

In the type of study performed by LLL/TERA, the usefulness of the results is directly related to the consistancy of the expert opinions. This survey is attempting to determine the state of knowledge on the expected maximum seismic event to occur at a given site. If the expert opinion varies significantly the reasons for the variation must be understood before the opinions can be used. In the LLL/TERA report there is a wide variance in the opinions generated by the experts polled by LLL. Due to the wide variance in the opinions the output of the LLL/TERA program appears to be wrong. As a result, the output is unusable. We believe the three basic reasons for the wide variance in the opinions are: 1) It appears not all the experts properly understood the questions in the survey, 2) the experts were not allowed sufficient time to provide required responses and, 3) in some cases, the experts were commenting on tectonic areas they were not intimately familiar with as to it's seismologic history.

Commonwealth Edison believes for the methology developed by LLL/TERA to be useful, LLL/TERA must go back and examine the causes of the variances in the expert opinion and resolve those areas they find questionable. They should then obtain expert opinion about a particular tectonic area from the expert most familiar with the area. In addition, a feedback mechanism should be established to make certain the experts understand the questions and that they have sufficient time to provide the required answers.

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Please address any questions you may have concerning the matter to this office.

One (1) signed original and thirty-nine (39) copies of this transmittal have been provided for review.

Robert F. Janecek

R.F. Janecek
Nuclear Licensing Administrator
Boiling Water Reactor

CRITIQUE OF LLL/TERA REPORTS

ON

SEISMIC HAZARD ANALYSIS

Prepared for:

NUCLEAR SERVICES CORPORATION
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October 26, 1979

INTRODUCTION

At the request of Commonwealth Edison and Nuclear Services Corporation, Fugro has reviewed the following three draft reports prepared by Lawrence Livermore Laboratory (LLL) and Tera Corporation on Seismic Hazard Analysis:

- "Seismic Hazard Analysis: A Methodology for the Eastern United States," Tera Corporation, August 23, 1979.
- "Seismic Hazard Analysis: Solicitation of Expert Opinion," Tera Corporation, August 23, 1979.
- "Seismic Hazard Analysis: Site Specific Response Sectra Results," by D. L. Bernreuter (LLL), C. P. Mortgat (Tera) and L. H. Wight (Tera), August 23, 1979.

These reports were submitted to the Nuclear Regulatory Commission (NRC) as part of their Safety Evaluation Program (SEP) of older nuclear power plants.

This document presents a critique of these reports. Because of the large amount of material contained in these reports and the limited amount of time available for review, the comments below are mostly general in nature.

CRITIQUE

General. The three LLL/Tera reports basically present: (1) a probabilistic model for estimating uniform risk spectra[1] at a site, (2) the solicitation of expert opinions and the interpretation and incorporation of these opinions in the probabilistic model,

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[1] Uniform risk spectra are response spectra whose ordinates have the same probability of being exceeded during some time period.

and (3) the results of the application of this model to nine nuclear power plant sites included in the NRC's SEP.

The results for each site consist of a set of uniform risk spectra, each spectrum derived using one expert's opinions, and an overall spectrum representing the synthesis of all expert's opinions. Comparisons are also made between the synthesized spectrum and spectra obtained from more deterministic approaches.

The reports make no recommendations as to which spectra are to be used in the SEP or the corresponding performance criteria of structural components and local soils. However, some discussion is given on the merits and limitations of the use of each type of spectrum. An important criticism of uniform risk spectra that was mentioned is that the spectra essentially represent the contribution of all possible earthquakes in the site region. On the other hand the facility has to resist the shaking from only one earthquake at a time. Therefore, the uniform risk spectra may be overly conservative in this regard, and other types of spectra may be more appropriate for the SEP.

The critiques of each of the three reports are given below.

Solicitation of Expert Opinion. Ten experts in the fields of seismology and tectonics responded to specific questions in these areas in order to provide the necessary inputs to the probabilistic model. Ideally, the experts should be very knowledgeable in all the problem areas and have plenty of time to seriously study the questions and all of the relevant data and respond in an unbiased and objective manner. However, for various reasons, it appears that these ideals were not achieved.

The experts generally were seismologists with some expertise in tectonics. To our knowledge none of the experts has done extensive research on the attenuation of strong ground motion or the response of local soils during earthquake shaking, two important considerations in any site-specific analysis. The limited expertise in the latter area was evidenced by the response to Question 4-20 (p. 11-106), which asked what was the maximum acceleration that various types of soil deposits could sustain. Only three experts responded and their answers indicated a limited knowledge in this area. Some of the experts emphasized their low confidence in their answers. Even within their fields of expertise, the experts are usually most knowledgeable about the seismicity and tectonics in the region in which they are located. In this regard it appeared that the experts concentrated their efforts on the data they were most familiar with and spent little time with other data. Sometimes only two or three experts responded to a particular question or group of questions.

More feedback between the experts and LLL/Tera would have been desireable during the course of the study. The experts did not have a chance to judge the reasonableness of the LLL/Tera interpretations of their responses to the questions, nor did the experts review the methodology or the results of the Tera/LLL probabilistic analyses for each site. The experts may well have modified their responses to some questions if they had understood the way in which those responses were going to be used.

The method of self-ranking of the experts' opinions and the inclusion of the information by LLL/Tera to weight all of the experts' opinions is questionable. The resulting synthesized spectrum from the application of the weighting technique is biased toward experts who have a high degree of confidence in their opinions. A high self-confidence in one's answer does not necessarily mean that that answer is likely to be correct. Conversely, a lower self-confidence ranking does not necessarily reflect a lesser understanding of the problem. Some type of cross-ranking between the experts might have established whose opinions were most respected.

<u>Methodology for the Eastern United States</u>. The major concern with the methodology was the treatment of the uncertainties associated with input parameters of the probabilistic model, and the impact of these uncertainties of the results. A detailed analysis of the uncertainties in order to fully evaluate their impact was not given. However, LLL/Tera did question whether or not the rather large uncertainties in the attenuation relations based on statistical analysis of empirical data and the uncertainties associated with the expert's opinion were accurate representations of reality. Limited sensitivity studies performed on the attenuation uncertainty showed in one particular example that an 80 percent increase in the standard deviation had a similar increase in the spectral level for a given return period. Although

the uncertainty in the final probabilistic distribution for the response spectral ordinates was not estimated, it is important to know if the model is combining the uncertainties of the input parameters in such a way so as to produce a grossly overestimated uncertainty in the final distribution. If the model is doing this, then at long return periods the corresponding uniform risk spectra will be overestimated.

Site Specific Response Spectra Results. The reasonableness of the results could be better evaluated if a more complete documentation were given on the effects the various input parameters had on the results. For example, the relative contributions of the various earthquake magnitudes, in addition to the seismic sources, would have been useful. More analysis of the effects of the uncertainties of the input parameters, through formal statistical methods and sensitivity studies, would also be useful.

A review of the results for the Dresden nuclear power plant was made based on our knowledge of the seismicity and tectonics of that site region. The LLL/Tera results showed that the New Madrid seismic zone contributed anywhere from 35 to 47 percent to the 1000-year peak ground acceleration while the Central Stable Region seismic source contributed 45 to 61 percent. The relative contribution from the New Madrid zone seems unreasonably high simply because the zone is 300 to 500 km from the site (depending on the expert's choice of the appropriate northern boundary) at its closest approach. Expected peak ground accelerations at these distances from an earthquake of epicentral intensity equal to XII (MM) should be less than 0.01g according to the formula given on Table 5-5 (p. 5-21) of the LLL/Tera report, "Site Specific Response Spectra Results." The report offers no concrete explanations or presents any analysis to explain why the New Madrid seismic zone should contribute so heavily to the results and, until this is investigated, the reslts for Dresden should be interpreted with reservations.