50, 52 & 100

(57FR 47802)

(30)

ASSOCIATION OF ENGINEERING GEOLOGISTS

"Serving Professionals in Engineering, Environmental and Groundwater Geology"

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President

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March 5, 1993

Secretary
U.S. Nuclear Regulatory Commission
Washington, D.C. 20555

Attention: Docketing and Service Branch

Re: Comments by the Association of Engineering Geologists
Nuclear Regulatory Commission
10 CFR Parts 50, 52, and 100
RIN 3150-AD93
Reactor Site Criteria, Proposed Rule Making

Ladies and Gentlemen:

Transmitted with this letter are comments by the Association of Engineering Geologists (AEG) in response to the above referenced Nuclear Regulatory Commission proposed rule making regarding reactor site criteria. In addition to a paper copy of the comments, an electronic copy of the comments and a copy of a paper on earthquake probability referred to in the comments are included. On behalf of the AEG, I appreciate the opportunity to review and comment on the proposed rules. The issue of siting and design of nuclear reactors, and other important and critical facilities, is extremely important. It is essential that appropriate groups of professionals have opportunities to evaluate and respond to proposed regulations governing such facilities.

I trust that the AEG's comments will be helpful to the NRC staff in completing their important task of protecting the health, safety and welfare of the public through regulations governing nuclear reactor siting and design. Please feel free to contact me for additional discussion or questions.

Sincerely

Jeffrey R. Keaton

Jeffrey R. Keaton

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cc: AEG Legislative and Regulatory Affairs Committee
AEG Engineering Geology Standards Committee
AEG Seismic Safety Committee

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Comments by the
Association of Engineering Geologists
March 5, 1993

INTRODUCTION

The Association of Engineering Geologists (AEG) is pleased to have an opportunity to participate in the rule making regarding nuclear reactor site criteria (10 CFR Parts 50, 52, and 100) as indicated in the Federal Register of Tuesday, October 20, 1992 (vol. 57, no. 203, p. 47802-47821). The comments presented below constitute the official position of the AEG, and have been prepared jointly by AEG's Engineering Geology Standards Committee and Seismic Safety Committee. The AEG is a society of approximately 3,000 professional engineering geologists.

The AEG understands that one of the objectives of the Nuclear Regulatory Commission (NRC) in the proposed rule making is to provide a stable regulatory basis for seismic and geologic siting of future nuclear power plants. A second objective is to create a flexible structure to permit the NRC to consider new technical understandings. One of the key issues in the proposed amendment to reactor site criteria is the requirement of the use of probabilistic evaluations as well as deterministic evaluations to define the Safe Shutdown Earthquake Ground Motion (SSE) for a reactor site. Furthermore, the proposed amendment and draft regulatory guide DG-1015 "Identification and Characterization of Seismic Sources, Deterministic Source Earthquakes, and Ground Motion" would mention specifically seismic hazard evaluations conducted by the Electric Power Research Institute (EPRI) and Lawrence-Livermore National Laboratory (LLNL). Both the EPRI and the LLNL studies were probabilistic evaluations in which "expert opinions" were manipulated statistically as though each expert's opinion were equally accurate, and the best representation was the statistical average or median of the opinions of the experts.

The AEG is very concerned about the use of probabilistic evaluations, particularly those based on expert opinion, to develop the SSE for a reactor site. The AEG believes that requiring the use of probabilistic evaluations and specifically mentioning the EPRI and LLNL studies as examples of acceptable procedures are contrary to NRC's objectives of creating a flexible structure and a stable regulatory basis. Requiring the use of probabilistic evaluations is less flexible than acknowledging that such evaluations may be useful but allowing the SSE to be developed by the deterministic procedures described in Appendix A to 10 CFR 100. The expert opinion element in the EPRI and LLNL probabilistic evaluations would tend to create a decidedly unstable licensing environment by providing a framework for pitting the applicant's "experts" against the "experts" produced by those opposed to the reactor.

The AEG is generally in favor of applying probabilistic procedures to quantifying natural processes for risk assessment. Clearly, if all hazardous processes to which a site may be exposed are to be considered collectively, they must be expressed in similar quantitative terms. At the present time, however, probabilistic evaluations can be misused. For example, it would be a relatively straightforward process to calculate the risk of a volcanic eruption in Houston, Texas, based on the surface area of the earth and the rate of recorded volcanic eruptions during historic time. The resulting calculation, of course, would be meaningless because of the current tectonic setting of Houston. This example is easy to visualize. Other misuses of probabilistic evaluations are more obscure. AEG believes that probabilistic evaluations of natural processes other than floods have not been widely accepted, and until such evaluations have been accepted, requiring their use where public health and safety are at risk is unwise and should not be done.

The evaluations required by Appendix A to 10 CFR 100 are considered to be "deterministic"; however, they are inherently probabilistic by virtue of the definition of a capable fault. Consequently, a fault that has not created deformation at or near the land surface once in the past 35,000 years or more than once in the past 500,000 years is considered "safe" in terms of defining the SSE for a site. Such a definition not only is probabilistic in nature, it is an explicit statement of acceptable risk. [The AEG notes that the proposed Appendix B to 10 CFR 100, paragraph III(1), uses 50,000 years in lieu of 35,000 years in the definition of a capable tectonic source.] Furthermore, relationships among earthquake magnitude, length of surface fault rupture, and surface displacement for historical earthquakes are statistical regressions, as are estimates of ground motion based on strong motion records generated by historical earthquakes with known magnitude and distance from the recording site. The scatter in the data upon which estimates of earthquake magnitude or ground motion at a site are based is expressed statistically as the standard error of regression. The NRC staff indicates that probabilistic methods can provide an explicit expression for the overall uncertainty in the ground motion estimates [paragraph V(B)(3)]. The statistical uncertainty based on data collected from historical earthquakes can be expressed in the context of the existing Appendix A to 10 CFR 100, without modification. The AEG believes that placing statistical uncertainty on expert opinions has little sound basis at the present time, and believes that public health and safety is best served by a deterministic evaluation of the seismotectonic regime based on detailed geologic and seismic examination which in turn are based on the multiple working hypothesis. A probabilistic seismic risk assessment is based on an earthquake recurrence relationship that expresses the number of earthquakes equal to or exceeding a range of magnitudes up to the Maximum Earthquake. The rate of occurrence of earthquakes exceeding a given magnitude (the Gutenberg-Richter b-value) and the Maximum Earthquake may be estimated from historical earthquake records or by an expert's opinion of what the values "ought to be".

Although probabilistic seismic hazard assessment methodologies have matured during the past 20 years, the AEG believes they are still as yet inappropriate for defining the SSE at a site. On the other hand, probabilistic assessments appear to be well suited for defining the Operating Basis Earthquake (OBE) ground motion, or assisting an applicant in making a choice of an OBE equal to one-third or less of the SSE, as would be included in the proposed Appendix B to 10 CFR 100. The AEG believes that the issue of acceptable risk must be addressed in its broader context before the results of probabilistic seismic risk evaluations for defining the SSE at a reactor site have a chance of being widely accepted by public decision-makers on behalf of the general public.

Following are specific comments of the AEG on the "Proposed Rule Making."

V. **Major Changes**

B. Seismic and Earthquake Engineering Criteria

NRC: *1. Separate Siting from Design*

Comment: The AEG believes this is an appropriate action.

NRC: *2. Remove Detailed Guidance from the Regulation*

Comment: The AEG believes this is an appropriate action. However, detailed guidance relating to probabilistic assessment procedures acceptable to the NRC would be contained in a Draft Regulatory Guide DG-1015. For all practical purposes, the AEG

believes the Regulatory Guide will be interpreted as part of the regulation. Therefore, the Regulatory Guide must be worded carefully to preserve the desired flexibility in applying basic principles to new situations and the use of new data sets of dramatically higher strong ground motion and evolving methods of analyses in the licensing process.

NRC: *3. Use of Both Deterministic and Probabilistic Evaluations*

NRC: *The proposed regulation would require the use of both probabilistic and deterministic evaluations.*

Comment: The AEG strongly recommends that only the deterministic evaluations be required for determining the SSE. Probabilistic evaluations may be useful, but they should not be required.

NRC: *Using this deterministic approach, an applicant develops a single set of earthquake sources, develops for each source a postulated earthquake to be used as the source of ground motion that can affect the site, locates the postulated earthquake according to prescribed rules, and then calculates ground motions at the site. Although this approach has worked reasonably well for the past two decades, in the sense that SSEs for plants sited with this approach are judged to be suitably conservative, the approach has not explicitly recognized uncertainty in geoscience parameter. Because so little is known about earthquake phenomena (especially in the eastern United States), there have always been differences of opinion among the experts as to how the prescribed process in Appendix A is to be carried out. Experts often delineate very different estimates of the largest earthquakes to be considered and different ground-motion models.*

Over the past decade, analysis methods for encompassing these differences have been developed and used. These "probabilistic" methods have been designed to allow explicit incorporation of different models for zonation, earthquake size, ground motion, and other parameters. The advantage of using these probabilistic methods is their ability to not only incorporate different models and different data sets, but also to weight them using judgments as to the validity of the different models and data sets, and thereby to provide an explicit expression for *the overall uncertainty in the ground motion estimates and a means of assessing sensitivity to various input parameters.*

Comment: The uncertainty in this context was analyzed in detail by Dr. Ellis L. Krinitzsky as part of the 1992 Richard H. Jahns Distinguished Lecture in Engineering Geology. Attached (enclosure 1) is the text of Part One of the Lecture which is the part that deals with uncertainty. Dr. Krinitzsky demonstrates that the procedure for analyzing uncertainty is logically defective and produces worthless results. The AEG requests that the requirement for evaluating uncertainty in this context be eliminated from the proposed changes.

NRC: *The staff proposes to use both the deterministic (currently being used) and the probabilistic evaluations together and compare the results of each to provide insights unavailable if either method were used alone.*

Comment: The AEG strongly advises that the requirement for using the probabilistic method be eliminated. A major problem is that the Gutenberg-Richter earthquake magnitude and recurrence relation (the b-line), which is the heart of seismic probability theory, is defective for predicting large earthquakes. The SSE is defined by the maximum credible earthquake regardless of the probability of its occurrence beyond the implicit definition of capable tectonic sources. A great body of work has come into being during the last 15 years that establishes the deficiency with recurrence relationships for predicting large earthquakes.

NRC: *Using both probabilistic and deterministic evaluations to complement each other should lead to a more stable and predictable licensing process than in the past.*

Comment: The AEG believes that probabilistic evaluations based on expert opinion introduce unsubstantiated and erroneous results. Furthermore, relying on a statistical expression of expert opinions promotes instability by encouraging those opposed to a project to hire a group of experts who have opinions that are more conservative than the applicant's experts.

NRC: *In order to implement this approach the NRC has proposed a requirement that the annual probability of exceeding the Safe Shutdown Earthquake Ground Motion at a site be lower than the median annual probability of exceedance computed for the current population of the operating plants. This requirement assures that the design levels at new sites will be comparable to those at many existing sites, particularly more recently licensed sites. This criterion is also used to identify significant seismic sources, in terms of magnitude and distance, affecting the estimates of ground motions at a site.*

Comment: The philosophy of the SSE and the concept of the median annual probability of exceedance are incompatible. Until an agreed upon level of acceptable risk is defined, explicit estimates of the probability of exceedance imply that more is known about temporal variability of earthquakes than is actually known. Such implications, hidden by very small annual probability estimates, may appear to the public to be an attempt to establish acceptable risk without a commensurate probabilistic assessment of the consequences of an earthquake exceeding the SSE.

Furthermore, selecting the median annual probability of exceedance indicates that opinions from each expert are given equal weight. Therefore, conservative opinions and non-conservative opinions offset each other. The AEG believes that relying on expert opinion to identify significant seismic sources is not in keeping with the underlying goal of the reactor siting criteria -- protecting public health and safety.

NRC: *4. Safe Shutdown Earthquake*

Comment: The AEG believes this may be an appropriate action, but defers to the structural earthquake engineering community for specific comment.

NRC: *5. Value of the Operating Basis Earthquake Ground Motion (OBE) and Required OBE Analyses*

NRC: *An applicant may determine that at one-third of the SSE level, the probability of exceeding the OBE vibratory ground motion is too high, and the cost associated with plant shutdown for* *inspections and testing of equipment and structures prior to restarting the plant is unacceptable.*

Comment: The AEG believes the application of probabilistic seismic risk evaluations tied to a consequence that may or may not be judged acceptable is an appropriate use of probabilistic procedures. In this use, it is the consequences that are judged acceptable or unacceptable by the applicant who also is responsible for conducting the probabilistic analysis.

NRC: *6. Required Plant Shutdown*

Comment: The AEG has no comment on this major change.

NRC: *7. Clarify Interpretations*

Comment: The AEG has no comment on this major change.

XI. Questions

B. Seismic and Earthquake Engineering Criteria

NRC: *There is a general consensus within the NRC staff that the revised seismic and geological siting criteria should allow consideration for a probabilistic hazard analysis.*

Comment: The wording "allow consideration for" is quite different from "require". The AEG believes that the SSE should be determined on the basis of detailed geologic and seismologic investigations, not on expert opinions per se. It must be recognized that geologic and seismologic investigations provide the basis for interpretations (opinions) that are used to determine the SSE. The emphasis must remain on the fundamental geologic and seismologic evidence, rather than on the reputations of the various experts.

NRC: *1. In making use of both deterministic and probabilistic evaluations, how should they be combined or weighted, that is, should one dominate over the other? (The NRC staff feels strongly that deterministic investigations and their use in the development and evaluation of* *the Safe Shutdown Earthquake Ground Motion should remain an important aspect of the siting regulations for nuclear power plants for the foreseeable future. The NRC staff also feels that probabilistic seismic hazard assessment methodologies have reached a level of maturity to warrant a specific role in siting regulations.)*

Comment: **For reasons previously stated, the AEG strongly advises that no probability procedure be required for determining the SSE. The AEG believes that probabilistic procedures may be appropriate for estimating the OBE or the consequences to structures, systems, or components (such as piping) given the occurrences of the SSE.**

NRC: *2. In making use of the probabilistic and deterministic evaluations as proposed in Draft Regulatory Guide DG-1015, is the proposed procedures [sic] in appendix C to DG-1015, adequate to determine controlling earthquakes from the probabilistic analysis?*

Comment: **The AEG has two basic comments regarding Appendix C to Draft Regulatory Guide DG-1015. In general, however, it is the AEG's position that the SSE should be defined on the basis of thorough knowledge of the geologic and tectonic conditions of the region in which a reactor may be located. The SSE must represent the most conservative reasonable earthquake regardless of its probability of occurrence. With these comments in mind, the AEG believes Appendix C to DG-1015 is not appropriate for defining the SSE, although it may be appropriate for estimating the OBE.**

The primary objective of Appendix C to DG-1015 appears to be a demonstration that the procedure will result in SSE ground motion estimates comparable with design bases of currently operating power plants. This objective is based on the concept that the SSE ground motion for currently operating power plants represents acceptable risk. While this level of risk may be considered appropriate by the NRC staff, it is not clear to the AEG that it has been widely accepted by public decision-makers on behalf of the general public. This is particularly concerning because the level of risk represents the median value of the opinions of a group of experts. What makes the expert whose opinion is least conservative as reliable as the expert whose opinion is most conservative?

Section C.2.2 of Appendix C to DG-1015 notes that less uncertainty exists in the western U.S. regarding the significant contributions to the seismic hazard, and the controlling earthquakes generally can be defined deterministically. Therefore, a probabilistic data base for the western U.S. is not available. The AEG believes that the research emphasis should be on improving the understanding of the geology and tectonics of the eastern U.S. rather than trying to quantify the uncertainty of the current understanding.

NRC: *3. In determining the controlling earthquakes, should be [sic] median values of the seismic hazard analysis, as described in appendix C to Draft Regulatory Guide DG-1015, be used to the exclusion of other statistical measures, such as, mean or 85th percentile?*

Comment: The philosophy of the SSE and the concept of an annual probability of exceedance are incompatible. After all, the SSE is the Maximum Earthquake; it has no practical annual probability of being exceeded. Use of probabilistic evaluations should be restricted to estimating the OBE or consequences to structures, systems, or components given the occurrence of the SSE. Relying on the median (or the mean or the 85th percentile) value would constitute an explicit acceptance of risk. The AEG believes the concept of risk should be translated to an analysis of consequence; then the acceptable level of risk becomes a public policy issue, not a geologic or seismologic issue.

NRC: *4. The proposed Appendix B to 10 CFR part 100 has included in Paragraph V(c) a criterion that states: "The annual probability of exceeding the Safe Shut down Earthquake Ground Motion is considered acceptably low if it is less than the median annual probability computed from the current [EFFECTIVE DATE OF THE FINAL RULE] population of nuclear power plants." This is a relative criterion without any specific numerical value of the annual probability of exceedance because of the current status of the probabilistic seismic hazard analysis. However, this requirement assures that the design levels at new sites will be comparable to those at many existing sites, particularly more recently licensed sites. Method dependent annual probabilities or target levels (e.g., 1E-4 for LLNL or 3E-5 for EPRI) are identified in the proposed regulatory guide. Sensitivity studies addressing the effects of different target probabilities are discussed in the Bernreuter to Murph, letter report. Comments are solicited as to: (a) whether the above criterion, as stated, needs to be included in the regulation? and, (b) if not, should it be included in the regulation in a different form (e.g., a* *specific numerical value, a level other than the median annual probability computed for the current plants)?*

Comment: **The AEG believes that probabilistic methods are inappropriate for determining the SSE, and that the concept of the SSE and an exceedance probability are incompatible beyond the exceedance probability explicit in the definition of a capable tectonic source. It is the position of the AEG that specifying probability values or target levels implies that more is known about the temporal variability of earthquake processes than is actually known. The AEG recommends that no probability values for the SSE be included in the regulation in any form. Probabilistic evaluations for selection of the OBE may be appropriate for reasons described earlier in this response.**

PART 100 - REACTOR SITE CRITERIA

Appendix B to Part 100--Criteria for the Seismic and Geologic Siting of Nuclear Power Plants On or After [Effective Date of the Final Rule]

II. Scope

NRC: *Both deterministic and probabilistic evaluations must be conducted to determine site suitability and seismic design requirements for the site.*

Comment: **The AEG recommends that the requirement for probabilistic evaluations be eliminated for reasons described in earlier parts of this response.**

IV. Required Investigations

NRC: *The geological, seismological and engineering characteristics of a site and its environs must be investigated in sufficient scope and detail to permit an adequate evaluation of the proposed site, to provide sufficient information to support both probabilistic and deterministic evaluations required by these criteria, and to permit adequate engineering solutions to actual or potential geologic and seismic effects at the proposed site.*

Comment: The AEG recommends that the requirement for probabilistic evaluations be eliminated in determining the SSE for reasons described in earlier parts of this response.

V. Seismic and Geologic Design Bases

(a) Determination of Deterministic Source Earthquakes.

NRC: *The uncertainty in determining the deterministic source earthquakes must be accounted for in the probabilistic analysis.*

Comment: The AEG recommends that the requirement for determining uncertainty and accounting for it in a probabilistic analysis be eliminated in determining the SSE for reasons described in earlier parts of this response.

(b) Determination of the Ground Motion at the Site.

NRC: *The ground motion at the site must be estimated from all earthquakes, including the deterministic source earthquake associated with each source, which could potentially affect the site using both probabilistic and deterministic approaches. ... Appropriate models, including local site conditions, must be used to account for uncertainty in estimating the ground motion for the site.*

Comment: The AEG recommends that the requirement for using probabilistic approaches and accounting for uncertainty with appropriate models be eliminated in determining the SSE for reasons described in earlier parts of this response.

(c) Determination of Safe Shutdown Earthquake Ground Motion.

NRC: *Deterministic and probabilistic seismic hazard evaluations must be used to assess the adequacy of the Safe Shutdown Earthquake Ground Motion.*

Comment: The AEG recommends that the requirement for probabilistic evaluations be eliminated in determining the SSE for reasons described in earlier parts of this response.

EARTHQUAKE PROBABILITY IN ENGINEERING

The Third Richard H. Jahns Distinguished Lecture

in Engineering Geology

by

Ellis L. Krinitzsky

Geotechnical Laboratory

Waterways Experiment Station

Corps of Engineers

Vicksburg, Mississippi, U.S.A.

INTRODUCTION TO

THE THIRD RICHARD H. JAHNS DISTINGUISHED

LECTURE IN ENGINEERING GEOLOGY

By

George A. Kiersch

Professor Emeritus, Geological Sciences

Cornell University, Ithaca, NY 14850

U.S.A.

The Richard H. Jahns Distinguished Lecture in Engineering Geology is sponsored jointly by the Geological Society of America's Engineering Geology Division and the Association of Engineering Geologists. It is both a memorial to Professor Dick Jahns for his distinguished contributions to Engineering Geology theory and practice and a distinctive honor to the recipient, Dr. Ellis Krinitzsky, in recognition of his scientific stature among geoscientists and acceptance by practitioners worldwide. This award, with its joint sponsorship by the two geologically-oriented professional societies for engineering geologists in America, was envisioned as the highest recognition of distinguished professional attainment in the discipline. By acceptance, the awardee agrees to present a special-subject lecture at selected American Universities that contribute to enhancing the stature of Engineering Geology, as does Dr. Krinitzsky's on "Earthquake Probability in Engineering." Ellis L. Krinitzsky is the recipient of this honor for the year 1991 and is the third outstanding scientist/practitioner to be selected.

Dr. Krinitzsky has had an exceptionally outstanding professional career in engineering geology as a government scientist/servant, teacher and counselor to several decades of aspiring geologists, and consultant/

practitioner worldwide providing guidance for engineered works. He is a Senior Research Scientist, in Geoscience, at the Waterways Experiment Station, U.S. Army Corps of Engineers, in Vicksburg, Mississippi and holds degrees from Virginia Polytechnic Institute, University of North Carolina, and Louisiana State University where he received his doctorate. His career involvements have included site exploration, alluvial sedimentation, riverbank stabilization, construction of roads and airfields using laterites, radiography in soils testing, foundations for dams, geological-seismological evaluation of earthquake hazards, and the specifying of earthquake ground motions at engineering sites. Dr. Krinitzsky is the principal advisor to the Corps of Engineers and the Department of the Army on geological-seismological assessments for major engineering projects. He has published more that 50 technical papers and produced 3 books. His activities include membership and service as an officer and committee member in nine professional societies. He holds adjunct professorships at Texas A&M University and Mississippi State University for whom he teaches graduate courses in Engineering Geology and Engineering Seismology, respe tively. He is also Editor-in-Chief of the International Journal of Engineering Geology.

Besides being named the Richard H. Jahns Distinguished Lecturer in 1991, other awards received by Dr. Krinitzsky include: the Research and Development Achievement Award, and the Decoration for Meritorious Civilian Service, from the Department of the Army; the Meritorious Service Award of the Geological Society of America; and the Best Paper Award of the Association of Engineering Geologists.

His principal interest is in earthquake studies. He has made seismic safety evaluations for dozens of major dams and other critical structures and published studies for the performance of such assessments. During related field investigations he has reconnoitered the sites of major earthquakes worldwide, that include ones in China, Australia, Argentina, Mexico, Italy, Bulgaria, Costa Rica, Guatemala, San Salvador, and other countries. Thus, he comes unusually qualified and field-experienced for his Jahns Lecture topic of Earthquake Probability in Engineering.

Earthquake probability, better known as Probabilistic Seismic Hazard Analysis, was a full grown concept by the late 1960s. The method subsequently gained wide acceptance with strong advocates. That has continued to the present.

Dr. Krinitzsky, as clearly evident in his lecture, is less than an admirer of the probabilistic analysis for estimation of earthquake ground motions. He has long felt the method is an expensive approach and provides unsatisfactory data for the design practitioner. His lecture develops in detail the reasons for this evaluation and why he does not feel that probability is wholly suitable for design purposes today. He has provided a scan of ideas and observations that come together as a powerful critique of seismic probability. Surprisingly, Dr. Krinitzsky's critique is unusual. Throughout the 25 years that the probability method has been widely used and accepted, there has not been a single technical paper offered that gives an encompassing criticism and re-evaluation of the method. Dr. Krinitzsky's lecture and publication is a "first" and an innovative new look at one of today's widely-held scientific beliefs. After study, the reader may feel a personal urge to re-examine the probabilistic approach for estimating earthquake ground motions.

Predictably, criticism of the probability approach will be regarded as controversial. Throughout his lectures, Dr. Krinitzsky quickly realized that he angered many in his audiences. Personally he will tell you -- those were the very scientists he wanted to reach and impact, and they are the ones that must be willing to re-think the approach and reconsider the merits of the deterministic approach for earthquake ground motion assessment.

I believe you will find Dr. Krinitzsky's lecture to be an important scientific contribution that is both thought-provoking and stimulating. Moreover, I am sure he invites the readers to examine his views with care and then form their own opinions.

George A. Kiersch

PART ONE

THE USE AND MISUSE OF EXPERT OPINION

Introduction

We tend generally to assume that several heads are better than one and that experts are more knowledgeable than ordinary practitioners. It follows that engaging a group of experts should be the best way to master a problem. This avenue has been examined and the results are instructive. They may also be unexpected, especially if you have not had experience in dealing with large numbers of experts.

Experts on Probabilistic Earthquake Ground Motions:

The Okrent Study

Okrent (1975) engaged seven experts to give probabilistic estimations of earthquake ground motions at eleven nuclear power plant sites. Locations were broadly distributed over the United States, taking in a variety of geological and seismological environments. The experts were given the description of local geology and seismology provided in the Safety Analysis Report for each nuclear power plant, thus they all were provided with the same basic information. They were not asked to make independent studies. They provided probabilistic motions at recurrence rates of 10^{-4}/year and 10^{-6}/year.

Table 1 gives a comparison of the ranges in values that Okrent obtained. Note that ten of eleven sites have accelerations that vary by factors of 8 to 10. Factors of 2 to 4 predominate for durations, but one factor is 10, and one site ranges from "few" to 30 seconds. Cycles per second have the greatest variances, mostly from 1/3 to 10 or 15.

Comment

Imagine trying to generate accelerograms for engineering analyses by using these parameters. Is it possible that critical structures such as nuclear power plants may have been designed and built from expert judgments that made no more sense than these? David Okrent was onto something very disturbing.

Experts on Faults: The Eguchi Study

Eguchi and others (1979) performed a similar opinion survey, this time concentrating on geological information. They engaged 14 experts to assign dimensions for mapped earthquake-generating faults. The experts also were asked to interpret slip rates and maximum credible earthquakes. Published fault maps were provided for the states of California, Nevada, and Arizona, plus a tectonic map of the United States. The experts were asked their opinions of individual faults. There was no field work. They were allowed to decide if they were knowledgeable about the respective faults. They could choose to give an opinion or decline.

Eguchi does not tell us what his experts were expert in. I tried to do that in a limited sense by tabulating their disciplines:

Geology: 8 persons

Geophysics: 1

Seismology: 4

Theoretical mechanics and geology: 1

Table 2 contains a selection of the ranges in their expert opinions on faults in California and Nevada. Opinions on fault lengths for sections of the San Andreas fault were pretty much in agreement but for the corresponding maximum credible earthquakes there was a divergence of 0.5 to 0.75 of an earthquake magnitude unit. And, the differences were more pronounced for slip rate and fault depth. Factors were as much as 4 for each. Also, when the faults were less well known than the San Andreas, the opinions on lengths were in much greater disarray. Table 2 shows there were ranges for fault lengths up to a factor of 6. The corresponding maximum credible earthquakes varied by one to 2-1/2 magnitude units. Slip rate variances ranged up to a factor of 15.

Interpreting faults from maps only, seems to me to be an invitation to disaster. To have done it properly, the experts in the Eguchi study should have flown over the faults, made airphoto studies, walked the faults, dug trenches, studied the displacements, obtained seismic profiles, performed age dating to determine recurrent movements, and done whatever else that might be relevant. They needed first-hand knowledge of the field evidence. They did not have it and the study shows the lack. Note that where there is a well-known fault, the San Andreas, the expert opinions on lengths of segments are not far apart, but the estimates for slip rate and for depth of fault are again widely disparate. Though the Eguchi study has very little to enlighten us about faults, it has some important things to teach us about experts.

To deal competently with earthquake-generating faults, the expert needs to be a mature geologist who is experienced in dealing with the field evidences of earthquakes. A seismologist or a geophysicist may not have this background nor this skill. The expert in theoretical mechanics and geology might have been excellently qualified, but only if he also had the requisite geological field experience. On the face of it, I would say at least a third of Eguchi's experts did not have the expertise needed to give expert opinions on fault lengths.

The 10 and 12 to 40 km depths given by the experts for the San Andreas fault are of special interest. More than a decade before Eguchi began his study, the U.S. Geological Survey (USGS) had embarked on an ambitious program of monitoring microearthquakes along the major faults in California. The USGS is very good at keeping the profession informed of its activities. An important early paper by Eaton and others (1970) reported that microearthquakes along the San Andreas occurred to a depth of 15 km. Maximum activity was between one and about 13 km. Later observations extended the depth to about 20 km, and that value is cited in the Eguchi report (p. 45). The experts who gave Eguchi the 10 to 12 km depths were evidently acquainted with the field evidence in the USGS studies, but they introduced an element of individual interpretation into their estimates. For a maximum earthquake which at that time would have been expected to break the ground surface and to have been initiated by displacements within the underlying ductile zone, thereby rupturing the entire brittle layer, dimensions of at least 15 km would have been appropriate. Thus, the dimensions given by the experts are their personal estimates but their reasons are not given.

Another issue in the depth values is the estimate of 40 km. The 40 km does not agree with the cited evidence. The explanation is inescapable: at least one so-called expert had no idea of what was common knowledge for a decade.

Comment

The above expert opinions on faults show:

(1) experts were engaged to answer questions that were not in their areas of expertise, and

(2) answers were given that are personal interpretations in which experts modified the observed information.

I think the Eguchi study teaches us that offhand opinions of a group of experts on faults is not a satisfactory substitute for one good data collection and field study by a competent geologist.

The Vallecitos Controversy: Polarized Opinions

In 1977, the Nuclear Regulatory Commission (NRC) shut down General Electric's test reactor at the GE Vallecitos Nuclear Center near Pleasanton, California. The shutdown was ordered when Darrell Herd, a geologist with the U.S. Geological Survey, mapped a fault about 200 ft from the site of the reactor. It was a low-angle thrust fault and was interpreted to be associated with the Verona fault, a known feature in the area.

Earth Science Associates (ESA), a contractor to GE, examined Herd's evidence and concluded that there was no fault and that the low-angle shear resulted from a landslide. The fault and landslide for the same feature are shown schematically in Figure 1. Later, ESA found "shears" in a trench on the other side of the reactor. These dipped underneath the reactor.

The events that were unfolded at Vallecitos were described in a delightfully well-written and easy-to-read book by Richard Meehan, The Atom and the Fault (1984). Meehan is a born raconteur and has an engaging sense of humor. His book chronicles his view of the battle that took place between opposing experts. One group accepted a landslide which posed no hazard to the reactor while the other believed an earthquake-generating fault existed at the reactor site.

Meehan depicts the USGS and the NRC as staffed by unsavory characters whose opinions were flawed and unacceptable. Consequently, the argument became polarized and acrimonious. Table 3 documents this controversy with its pros and cons. Rice and others (1979) discuss the many problems associated with these views. Other commentaries on the controversy are presented by Hund (1986), Kiersch and James (1991), and Bonilla (1991).

ESA and Meehan fought heroically against any change in the landslide interpretation even though ESA found thrust movements in trenches dug across the extension of Herd's Verona fault. Later ESA located what they called "thrust-like splays" from the Verona fault adjacent to the reactor, but ESA did not alter their views. They argued the splays were not a "major structure," but did not define what "major" meant. Eventually, ESA retreated to the extent of saying that both the landslide and the fault were indeterminate. During the legal proceedings, the General Electric Company allowed that the Verona fault could exist and could offset the reactor by one meter. However, Meehan then turned to probability theory and said that the fault would have a one-in-one-million chance of rupturing. In his book, Meehan does not mention that Slemmons (1979) showed that probabilistic reasoning had no validity at the site for several very cogent reasons (see Table 3): a lack of dates of earth movements, an unknown geometry of displacement; alternatives in interpretations, and no evidence of a needed random, or Poissonian distribution of earthquakes. Regardless, Meehan continued to support the probabilistic interpretation.

Finally,the whole controversy was made moot by Meehan himself. He testified to the review board that no fault movement could break the 5-ft-thick concrete slab on which the reactor, about the size of a garbage can, was placed. This principle was learned a decade earlier, during the Managua, Nicaragua, earthquake of 1972. A fault moved beneath the Banco Central building without causing any significant damage. The Banco Central had 45-cm-thick concrete walls and floor in its basement. For discussions of this experience see Wyllie and others (1977) or Niccum and others (1977).

Permission was granted to operate the reactor, but the controversy had dragged on for five years and GE's market for the reactor's products had been destroyed and a profitable business could not be revived.

The never answered question, heard many times since: why did so many high-powered experts, working energetically on this problem, take five years to arrive at this simple, no-cost solution?

Comment

At Vallecitos the expert opinions were polarized and remained so through the five years of acrimonious disputes and is so today, more than a decade later. To speak of this case with the principal players uncovers wounds that have never healed. The Vallecitos dispute was rife with all of the hang-ups that plague group decision making:

(1) the influence of strong personalities on both sides,

(2) promotion of decisions prior to examining the problem in all of its dimensions,

(3) anchoring of views so that changes are resisted,

(4) biases with covert judgments that are never adequately explained, and

(5) group pressures for conformity.

The dispute merits the attention of a psychologist. It is a clear case of what Leon Festinger (1962) called cognitive dissonance. Festinger believed that once a person makes a decision and commits himself to a course of action, his psychological behavior alters powerfully. The person consciously turns away from being objective. His partialities and biases are strengthened and so is his resistance to accepting alternative views. The Vallecitos controversy is a case book for Festinger's views.

Industry Practices in Specifying Earthquake Ground Motions:

The Krinitzsky Survey

Krinitzsky (1980) collected examples of the methods by which earthquake ground motions were assigned for engineering sites by practitioners in government, academia, and industry. The documentation is not published but has been deposited without analysis in the Library of the Waterways Experiment Station. The compilation was made jointly by the Bureau of Reclamation and the Corps of Engineers with the objective of helping to produce a manual. No manual was generated.

Krinitzsky postulated seven hypothetical sites. Motions for them were requested. There were responses from 14 consulting firms, five private consultants, and five government agencies. Of these 24 reports, 18 were suitable for making comparisons.

Table 4 shows site characteristics and the ranges in peak horizontal ground motions on soil given by the 18 respondents. These ranges for motions are far greater than those obtained by Okrent. The largest dispersion in values is for acceleration, between 0.05 and 2.0 g for a floating earthquake in eastern United States. The least spread for acceleration, comparing all sites, is 0.35 to 2.0 g at a reservoir. Other components of motion have even more variances: velocities from 1.0 to 300.0 cm/sec, displacements from 0.05 to 190.0 cm, and durations of 8 to 60 sec, all for eastern United States earthquakes.

Table 4 has a question that asks for motions at a site 150 km from the New Madrid source and an earthquake of m_b = 7.5. The experts responded with an acceleration range of 0.03 to 0.5 g. The site actually is Sardis Dam in northwest Mississippi. During the 1811-1812 New Madrid ~ rthquakes, the

Sardis area experienced a Modified Mercalli intensity of VIII. The MM VIII is established by contemporary observations in the region such as were reported by Street and Nuttli (1984), and by interpreted isoseismal maps, such as those by Stearns and Wilson (1972). MM VIII is hardly represented by 0.03 g.

The threshold of feeling anything at all during an earthquake is about 0.05 g. So 0.03 g would in fact be a microtremor and fully off the Modified Mercalli intensity scale. Nonetheless, for this site, three experts gave values of 0.03, 0.04, and 0.05 g respectively. The reason is because the experts used attenuations from western United States without realizing that attenuations differ between western and eastern United States by a factor of about ten.

Let's face it: experts can make very bad mistakes. The user of expert opinion must be able to examine opinions critically and confront them, prune them, or discard them as necessary. A good principal investigator does this as a routine part of his work!

Dealing with Earthquake Ground Motions

Table 1 from Okrent (1975) and Table 4 from Krinitzsky (1980) show motions assigned by experts in which values vary by an order of magnitude and more. Can we explain these dispersions? Can we bring those motions under control?

For background, consider how dramatically peak motions for earthquakes have changed during recent years. Table 5 shows the growth that occurred in accelerations from the 1920s to the 1970s, from 0.1 g to 1.25 g. Questions concerning the validity of the 1.25 g recorded at Pacoima Dam have since been quieted by a half dozen additional records of one g and greater as shown in

Table 6. But notice that in Table 6 the values are for moderate earthquakes, those with magnitudes of 5.4 to 6.8. There are as yet no instrumental or other motions to be had for larger earthquakes close to their sources. Is there a saturation limit for their peak motions by which they will be no higher than what we see now? How high can an acceleration at a fault be? The experts have to interpret these values. In Table 4, the 3 g at the San Andreas fault is clearly such an interpretation and it may turn out to be the best of the lot. Iio and Yoshioka (1992) interpreted a horizontal acceleration of 2.0 g and a frequency of about 1 Hz from a boulder displacement measured following the Nagano Prefecture earthquake of 1984.

The question to ask at this point is how to consider the frequency content of the peak motions? There can be spectral components of motion of very high frequency, such as 10 to 25 Hz, that are high accelerations but have little energy, with the result that they commonly produce no significant effect in a dynamic analysis of structures such as dams when they are introduced in an accelerogram or in corresponding response spectra.

Should the expert contribute a very high acceleration with no practical meaning or should he give an acceleration for the spectral content that he knows to be meaningful? The problem is that the high acceleration may come to be used in analyses that are not spectral dependent and the engineering seismologist will try to avoid that eventuality in order not to contribute to unforeseen possibilities for mistakes. I see, in the values that have been given in our tables, ones that are theoretical and others that are practical. However, there is also a broad variety of meanings within what is called practical.

Table 7A presents the types of earthquake ground motions (from Krinitzsky and others, 1992) that are suitable for use in various categories of pseudostatic analyses. Table 7B shows motions that are appropriate for dynamic analyses. Not only are experts likely to specify the motions that they think the customer should have, as indicated in these tables, but they also may be speaking from limited experience within one or another of the categories of analysis. Their motions may be unwittingly parochial. Additionally, there is within the above categories another adjustment which is not described in the tables and which provides what are called <u>effective</u> motions.

Effective motions can be lower than peak motions where there are either non-repetitive spectra, high frequency spectra, or configurations in the site and structure that may mitigate the effects of ground motions. Such situations include:

(1) the size of loaded area compared to patterns of wave incidence,

(2) depth of embedment of structure,

(3) damping characteristics, and

(4) stiffness of structure and formation.

These factors, and possibly others, are being researched but there are no established procedures for evaluating them. Nonetheless, effective motions have been introduced into engineering analyses of earthquake effects quite extensively. Krinitzsky (1989) gives examples from the Trans-Alaska Pipeline; the Van Norman Reservoirs, CA; Diablo Canyon Nuclear Power Plant, CA; and San Onofre Nuclear Power Plant, CA. Reductions in peak motions varied from 25 percent at the Trans-Alaska pipeline and the Van Norman Reservoirs, to 40 percent at the Diablo Canyon and San Onofre Nuclear Power Plants. The specifying of effective motions is basically an engineering decision and there are pressures or desires to include them as a practical matter in assignments of earthquake ground motions.

The above observations assume that the experts have been working at assigning earthquake ground motions and have their own preferred selections of data, which some of them do. However, there are experts who only contribute motions that they take from published sources and, in doing so, they introduce other possible vagaries. Figure 2 shows a comparison of currently used magnitude and distance curves by various authors for accelerations on rock for M = 7.5. Joyner and Boore (1981), Campbell (1981), and Seed and Idriss (1983) are lower than Krinitzsky and others (1988) and at close-in distances from the source they are appreciably lower. Why the differences? The reasons for these differences are in the respective selection and handling of the basic data.

The Krinitzsky curves are for focal distances; the other curves are for distances on the ground surface. Thus, the curves compare unlike data. Joyner and Boore (1981) excluded data from abutments of dams, such as the Pacoima record with its 1.25 g. They assumed that 1.25 g represented a topographic effect and was not what would have been a free field value had such been recorded. When 1.25 g was obtained in 9 February 1971, there was a rush to repudiate the record. Campbell (1981) did not use it and Seed and Idriss (1983) revised it down to 0.80 g. Joyner and Boore (1981) also adjusted the distance from source to site, making it the nearest distance to a projection of the causative fault onto the ground surface. They also assumed that distances where instruments were operational but not triggered were the limits of an earthquake. No triggered values beyond that limit were used for that earthquake though they might have been available. They also tried to resist any preferential selection of high amplitude records by noting the smallest distance for such a record and excluding all other such records of the same amplitude at equal or greater distances.

I believe that wave propagation comes first from a fault at depth and rupture propagation, with focusing of waves, then comes into play so that the source to site distance is not a fixed quantity but is a dynamically changing one.

The Joyner and Boore (1981) values are moderately lower than those of Krinitzsky and others (1988) but reflects a lessened conservatism in Joyner and Boore's handling of their data.

Campbell (1981) took the shortest distances to surface projections of fault planes. He excluded soft soil deposits and he excluded the Pacoima record. He also assumed that the same accelerations are produced by all magnitudes of earthquakes near a source. At 0 to 10 km from the surface trace of a fault, his motions are very similar to each other for magnitudes that range from 6.5 to 8.0. Campbell's (1981) conception does not allow for the focusing of waves. For the above reasons, his lesser values are derived from a lessening in conservatism that does not appear to be warranted.

Seed and Idriss (1983) reduced the Pacoima record from 1.25 g to 0.80 g. Close to a source, their peak motions for M = 6.0 to M = 8.5 are nearly unchanged. The effect is to provide near-source values that can be unconservative.

Thus, even the simplest use of published strong motion curves involves selections that can result in great differences in ground motions.

Comment

Despite the enormous variations that occur in earthquake ground motions, the differences between interpretative models can be identified, the reasons for these differences can be understood, and some order in the selection process can be achieved. However, a project engineer has to know what is available, the pros and cons in every case, what his engineering analysis requires, and finally what he wants or will accept. He will have to understand the meaning of data as the experts do. This is not too difficult. I have described the essentials by which he can do it.

My contention is that the best way for a project manager to operate is to have someone, either an engineer or an earth scientist, who will learn the intricacies, learn to use geological and seismological evaluations, and either proceed to assign earthquake ground motions himself or have it done by a specialist. Experts should then be engaged for outside reviews. The engineer or earth scientist needs to pay close attention to the opinions of reviewers. He and the project manager need to judge the opinions carefully, have the knowledge and character to throw out what is bad, and decide what to accept accordingly.

Experts on Engineering Judgments for an Earth Embankment: the Hynes and Vanmarcke Study

Hynes and Vanmarcke (1975) studied variances in expert judgments by obtaining responses from seven experts to questions on settlement in an earth embankment and on failure from additions to the height of the embankment. The experts were given laboratory and field data for the embankment that included Atterberg limits, water contents, vertical and horizontal consolidation strain at a constant rate, unconfined compression, triaxial tests, field vane tests, piezometer data, slope indicator data, Standard Penetration tests, grain size distributions, dry densities, drained strength, and readings from field instrumentation of the embankment for six years. Additionally, undisturbed samples of the foundation clay were available. The experts had every element of data that reasonably could be expected for making calculated determinations but were not given the observed values for settlement and height-induced failure.

The interpretations produced by the seven experts are shown in Figure 3 for settlement of a clay layer in the embankment and Figure 4 for failure from added height. The experts provided a best estimate and their "confidence" was obtained by having them provide ranges of ± 10 percent, 25 percent, or 50 percent of their degrees of certainty from their best values.

The experts used a variety of methods to obtain their results and the methods represented different degrees of sophistication and originality, according to Hynes and Vanmarcke.

Figure 4 shows that the best estimates for added height to failure differ by a factor of 3. None of them are closer than 5 ft from the actual value. The average of the best estimates is 15.8 ft which is about 3 ft from the observed value of 18.7 ft. The average minimum-to-maximum range is 9.1 ft. The results of the exercise show that statistical merging of the estimates produces only a slightly better estimate than do the best of the individual predictions. The average does represent an improvement.

However, when these results are compared with the estimates for settlement of a clay layer in the embankment as given in Figure 3, the latter variances have a factor of 7. Yet, two of the estimates are practically at the observed level. The average of the estimated settlement values is 2.75 in. compared to the observed value of 0.6r in. Averaging the estimates in this case does not result in an improved estimate and devalues two of the estimates that were accurate.

The steps of the interquartile range, at 25 and 75 percent, helped to plot a range of uncertainty that could be interpolated into a probability. If the technical assumptions were valid and the expert's assumption of uncertainty were express d fairly, the uncertainty range should contain the actual value. In this study it did so for only two estimates given in Figure 3 and none in Figure 4. Clearly, a combining of the probability estimates into a single probability value can be misleading.

Comment

The purpose of the Hynes-Vanmarcke study was to examine how disagreements among experts could be dealt with in civil engineering evaluations. Their initial assumption was that statistics and probability theory could supplement the engineer's judgment and be a useful part of the decision-making process. This assumption was not borne out by their two exercises since the results contradicted the assumptions.

The question is can statistical manipulation be applied usefully to subjective engineering judgments? Not by the evidence of this study. We saw that probability values based on the experts' confidence levels could have no validity since they touched the actual values in only two instances out of 14.

It stands to reason that, if an erroneous approximation was used in addition to a correct one, statistical manipulation is not a reliable way to adjust away the erroneous value. If a correct approximation was never used, statistical juggling cannot be depended on to make up for its absence. The answer is clearly that, when subjective judgments are based on a variety of inferences or differing models and the resulting judgments vary, statistical manipulation for decision making is a treacherous route to follow.

Probabilistic Seismic Hazard Analyses for Nuclear Power Plants in Eastern United States: Studies by the Lawrence Livermore National Laboratory and the Electric Power Research Institute

The Lawrence Livermore National Laboratory and the Electric Power Research Institute each conducted a series of extensive studies of earthquake hazards in eastern United States. Eastern United States for both studies was east of the Rocky Mountain Front. Both studies were based on multiple expert opinions and probabilistic interpretations.

EPRI (1986 to 1989) engaged 50 experts for this work, separating them into six teams. Each team was intended to have an interdisciplinary association of geologists, seismologists, and geophysicists.

LLNL (Bernreuter and others, 1989) engaged 19 experts who they separated into two teams called Panels. The Panels were as follows:

(1) Zonation and Seismicity Panel

Number of members: 14

Specializations: 2 geologists
12 seismologists and geophysicists

Mission: Principally to divide eastern United States into source zones for earthquakes.

(2) Ground Motion Panel

Number of members: 7 (2 from the Zonation and Seismicity Panel)

Specializations: 7 seismologists

Mission: Make use of data and models for development and specification of earthquake ground motions at the sites of nuclear power plants.

Except for the two floating members, the two LLNL panels did not interact. The experts in both panels were furnished with existing geological and seismological information. No independent investigations were called for or undertaken in the zoning effort. The experts did introduce information and new techniques for the seismological evaluation of ground motions. Within each panel the members had limited group interaction with feedback and there was an elicitation process.

Both the LLNL and the EPRI studies generated probabilistic earthquake ground motions for nuclear power plant sites in eastern United States.

Differences in methodologies between LLNL and EPRI were explored in detail by Bernreuter and others (1987). They noted that:

(1) LLNL used an earthquake database that began at magnitude 3.75, EPRI at 5.0.

(2) The models for attenuation of ground motions from source to site were different.

(3) LLNL accounted for site conditions; EPRI did not.

Since the LLNL and EPRI studies are basically similar, only the LLNL study will be examined in detail.

Background on Seismic Source Zones in Eastern United States

Before considering the seismic source zones developed by the LLNL's Zonation and Seismicity Panel, let us consider seismic zoning in general for eastern United States so that we can establish a point-of-view from which to make comparisons.

In eastern United States, earthquakes are generally assumed to result from one or more of the following possible causes:

(1) Focusing of regional compressive stresses along lithologic or other rock boundaries and release of these stresses by movement through reactivation of ancient faults.

(2) Possible small-scale introduction of magma at depth with an accompanying buildup of stresses.

(3) Focusing and release of regional stresses along ancient rifts which remain as zones of crustal weakness.

(4) Slow, very broad regional compression causing reactivation of ancient thrust faults in the region.

(5) Extensional movement along a sagging coastline with activation of normal faults that bound major grabens.

Each of these theories can be interpreted as meaning that a major earthquake can happen at a location where no historic earthquake has occurred. That idea, though reasonable, must be handled with care; if valid it means that large earthquakes can happen almost everywhere and that is not what occurs in the world.

We consider a seismic source zone to be an inclusive area over which an earthquake of a given maximum size can occur anywhere. That earthquake is a floating earthquake. A seismic zone is supplemental to, and can include, the causative faults that have been identified as sources of earthquakes. The purpose of zones is to avoid surprises, particularly earthquake generating faults that have not been located to date.

The seismic zone represents present-day tectonism which is seen in the occurrences of earthquakes. Seismic zones need not relate in extent to geological basins or other structural or physiographic provinces since those features were generated by past tectonism. The seismic zone is best defined by the occurrence of known earthquakes.

The United States has the disadvantage of a short seismic history, as short as 100 years in parts of the Prairies and the longest about 350 years in New England. However, we can obtain analogous situations in other parts of the world where the records are many times greater. A case can be made that the largest earthquakes are likely to be restricted to relatively small and stable source areas.

Xian in central China is the scene of infrequently occurring major earthquakes. The Great Shenshi earthquake of 1556, M = 8 with 830,000 deaths, took place in the Wei Ho plain with no remaining evidence of the fault in the alluvial valley. Figure 5 shows the locations and dates of major earthquakes near Xian. The historic record in this region is about 3,500 years. There were three M = 8 earthquakes: 1303, 1556, and 1695. Note that these, and lesser earthquakes associated with the large events, are closely restricted to a narrow, sinuous belt only about 20 km wide, while the adjacent areas are abruptly less seismic. These relationships should be the basis for defining a seismic zone in the area.

An even more striking example of the restriction of large earthquakes to a small and persistent source, or a hotspot, is seen in Figure 6 for a portion of Italy east of Naples. There is a zone barely 5 km across, situated south of the Ofanto River, that has a Mercalli-Cancani-Sieberg (MCS) Intensity of XI, rated "catastrophic." The zone was established by Iaccarino (1973A) on the basis of earthquakes between 1500 and 1972. Iaccarino (1973B) counted 2,130 earthquakes between years 1 and 1972. Of these he interpreted 60 as MCS Intensity X, considered "ruinous," with 20 more that were greater than X, or "catastrophic." The latter occur sporadically along the mountain spine of the country, well away from the coasts, and are in the form of very small zones, or hotspots. Significantly, the Campania-Basilicata earthquake of 1980, M = 6.8, occurred in the zone near the Ofanto River, precisely where Iaccarino indicated his highest level of susceptibility.

On the basis of observations similar to those above, seismic zones can be determined by the patterns of earthquakes and the maximum sizes can be guided by the sizes of observed and inferred earthquakes.

Criteria for shaping seismic zones are:

(1) Zones that have great activity should be as small as possible. They are likely to be caused by a definite geologic structure, such as a fault zone or a pluton, and activity should be limited to that structural association. Such a source is a <u>seismic hotspot</u>. A seismic hotspot requires locally large historic earthquakes, frequent to continuous microearthquakes and a well defined area. Maps of residual values for magnetometer and Bouguer gravity surveys and from seismic reflection or refraction profiles may provide structural information to corroborate the boundaries of hotspots.

(2) One earthquake can adjust a boundary of a seismic zone but cannot create a zone.

(3) The maximum felt earthquake is equal to or less than the maximum earthquake assigned to the zone.

(4) The maximum zone earthquake is a floating earthquake, one that can be moved anywhere in that zone.

(5) Assignment of the maximum zone earthquake is judgmental.

Figure 7, from Krinitzsky and others (1992), shows seismic zones with Modified Mercalli intensity values for floating earthquakes. These zones are for the eastern United States. The most seismically active areas are very concentrated zones, or hotspots; notably Charleston, South Carolina; Giles County, Virginia; Cape Ann, Massachusetts; and New Madrid, Missouri. Following are the key determinants for these hotspots:

(1) Charleston. Microearthquakes were found by Tarr (1977) to be concentrated in an oblong zone with a maximum dimension of 40 km. The zone is outside of Charleston and coincides with the epicentral area of the Charleston earthquake of August 31, 1886 of MM Intensity X. The zone has been further identified by White and Long (1989). Work was done by Obermeier and others (1989) and by Amick and others (1990) on paleoseismic evidences of soil liquefaction from earlier earthquakes. The Atlantic coastal plain was extensively reconnoitered. The conclusion was that pre-1886 craters are concentrated near Charleston in the same zone as the 1886 event and that this condition prevailed throughout Holocene time (the previous 10,000 years.)

(2) Giles County. Bollinger (1981) reported a concentration of microearthquakes from which he postulated a source zone about 35 km in length. The seismicity is in the same source area as the May 31, 1897 earthquake that was ranked as MM Intensity VIII.

(3) Cape Ann. An earthquake occurred offshore on November 18, 1755 with an MM Intensity of VIII. Because of its offshore location this area has not been studied in detail but there is no evidence to require extending the source area.

(4) New Madrid. For New Madrid, the site of four enormous earthquakes felt over all of eastern United States in 1811 and 1812, there has been an abundance of information (see Gori and Hays, 1984) that locates intense and continuing microseismicity in a 150 km-long

zone. The zone coincides with the source area of the 1811-1812 events. There is no basis for extending this zone.

(5) Terre Haute. Figure 7 shows MM VIII source zone at Terre Haute. This is not a hotspot but it is a zone that is based on historic seismicity as are other such zones in Figure 7. Coincidentally, recent paleoseismic field studies by Obermeier and others (1991) for this area have indicated the presence of widespread liquefaction features resulting from a large but infrequently occurring earthquake. Obermeier's work may prove to have an important effect on estimating the maximum credible earthquake for this zone. However, the zone was already known and the paleoseismic discoveries confirm a persistent activity.

The interpreted seismic zones in Figure 7 are presented as a point-of-view from which we can consider the seismic zones in the LLNL study.

Seismic Source Zones from the LLNL Zonation and Seismicity Panel

Figure 8 shows the individual zoning of seismic sources in eastern United States that was done by 11 of the experts in the LLNL Zonation and Seismicity Panel. In the lower right corner of Figure 8 there is shown for comparison the locations of the principal seismic hotspots of Figure 7. Observe that these hotspots were dealt with by the LLNL experts as follows:

(1) Charleston, South Carolina:

Experts 2, 3, 5, 7, 10, 12, and 13 restrict a Charleston earthquake to a small area at Charleston. Experts 1, 4, 6, and 11, a third of the experts, place the Charleston event as a floating earthquake that will move over much larger areas.

(2) Giles County, Virginia:

None of the experts treated Giles County as a discrete source. (It is the site of the third largest historic earthquake on the eastern seaboard.)

(3) Cape Ann, Massachusetts:

Also not a discrete source. (Site of the second largest historic earthquake on the eastern seaboard.)

(4) New Madrid, Missouri:

All of the experts give relatively restricted source areas for New Madrid, however, the sizes and shapes of the source areas vary significantly.

The zoning exercise was followed by an elicitation to identify uncertainties. The experts were asked to give each of their zones a rating based on their percentage degree of certainty. Only zones with high certainties were to remain. When areas with lesser certainties were removed, boundaries were changed to redefine the remaining zones. New zones produced this way are shown in Figure 9. The results are startling. Larger and more inclusive zones came to dominate and some of them have boundaries that are unnerving:

(1) Expert 5 begins New Madrid in the St. Lawrence valley and carries it without interruption into the Gulf of Mexico. Port Sulphur, Louisiana, is shown to have the same seismic potential as New Madrid, Missouri.

(2) Hotspots along the eastern seaboard disappeared completely.

(3) Expert 6 went from a complicated pattern of zones to a single super zone that covers all of eastern United States. It is One-Size-Fits-All.

The LLNL report documents questions and answers that accompanied the elicitation process for the zones, so we can examine the results in somewhat more depth. Of the 11 experts who provided seismic zonations, 7 gave responses to questions, but not to all questions. Following is a synopsis gleaned from 57 typewritten pages of testimony:

(1) <u>What sort of data is available and adequate for zoning</u>?

Expert 1. Paleoseismicity at New Madrid and Charleston is good.

Expert 5. Seismicity is the <u>only</u> source of information.

Experts 10, 12. Geological and geophysical data determine the zones.

(2) <u>What are the principal bases for forming the zones</u>?

Expert 1. The Gutenberg-Richter b-line (the b-line is described in Part Two of this review) and geological structure of the basement rocks.

Experts 5, 7, 10, 11, 12. The broad geology and the geological structure.

Expert 6. Seismicity. (This is the author of the single zone that covers all of eastern United States.)

(3) <u>What features influenced the zones</u>?

Experts 1, 3, 5, 7, 10, 12. Patterns from geological and geophysical data. (Expert 7 commented that the zones are too broad for site-specific calculations.)

(4) <u>How were the above features used for zones</u>?

Expert 1. Geology and the b-line were the principal determinants.

Experts 6, 10, 11. Seismicity was the determinant.

(5) <u>Do the zones represent your state of knowledge adequately</u>?

Experts 1, 3, 6, 11, 12. Yes.

Expert 10. No.

Expert 7. Not sure.

Comment on Seismic Zones

In the LLNL study, seismic source zones were created overwhelmingly on the basis of the geographic extent of broad geological structures. These structures were the ones that are seen on geological maps of continental dimensions and indirectly from geophysical maps that also reflect these major geological features. Seismicity was reported as an important determinant, but the seismicity was broadly extrapolated onto the above geological evidence.

Significantly, this heavy reliance on the patterns of geological structures of continental scope did not come from geologists. There were only two geologists among these experts, the rest were seismologists, not the best people to understand all of the nuances and meanings to be found in the geological evidence. Had there been more geologists among the experts, I believe large scale geological features, resulting from powerful but long vanished orogenies, would have been played down in favor of small scale and more specific local structural anomalies that key directly to seismic events and to evidence from recent paleoseismicity.

None of the experts in the LLNL study followed the principles that I gave for forming the zones in Figure 7. If truth can be guaranteed by a strong wind of elitist populism, then the LLNL approach is right. But, look again at the extraordinary disparities between zones within Figures 8 and 9.

The LLNL project managers accept the zones of Experts 2, 3, 4, 7, 11, and 12 on Figure 8 and the elicited zones of Figure 9. Successful elicitation should have diminished the differences between the subjective opinions and should have brought about a convergence of views. Yet, the opposite happened.

The resulting zonations were more disparate than they had been in the beginning. I think it is easy to see what went wrong.

In essence, all the LLNL investigators did in their elicitation was to ask the experts what is the percentage degree of certainty for this or that zone? That is asking the expert to add another subjective judgment to what is already a subjective judgment. It is not a dependable way to get worthwhile information. Let me take it to a *reductio ad absurdum*. Imagine that an investigator is at an asylum for the mentally deranged. He interviews a person:

> Question: Madame, what is your percentage degree of certainty that you are Marie-Antoinette?
>
> Answer: One hundred percent, you idiot!

The investigator writes on his clipboard:

> Confidence: 100%.
>
> Changes: None.

LLNL also elicited "self weights" from their experts. The experts were asked to rate themselves as follows:

(1) Your level of expertise relative to the other panel members.

(2) Your level of expertise relative to the scientific community at large.

(3) Your level of expertise relative to an "absolute level" of overall knowledge.

The ratings were used to establish "weights," based on a relative weighted averaging process, for adjusting the experts' subjective opinions.

LLNL then proceeded to use the results of their percentage-of-certainty elicitations for shaping their zones and for subsequent calculations. Remembering the Hynes and Vanmarcke study, LLNL used subjective judgments in a manner that we consider extremely treacherous.

Consider again the extreme case, the second elicited zonation by Expert 6 in Figure 9, the One-Size-Fits-All zone, the identical seismic potential to be found in every part of eastern United States: Land's End, Louisiana must gird itself for the same size of earthquake as New Madrid, Charleston, Giles County, and Cape Ann. And what did the LLNL project managers do with such a patently puerile expert opinion? LLNL used it. I believe they will tell you that they were meritorious in doing so, because it gave their conclusions a measure for uncertainty. I do not see the nonsense by Expert 6 as a means to measure uncertainty. It is purely and simply a sordid and disastrous failure of judgment and I think it should have been regarded by LLNL in no other way. But how many of the other zones have comparable failures of judgment? Compare the zones given by the various experts with the seismic sources in Figure 7. There are, I think, a great many judgments by the so-called experts that would have benefitted from a rigorous reevaluation and a therapeutic pruning.

I suggest at this point that we have a desperate need to protect our hard-won professional expertise in the study of evidence from depredations by project managers who would substitute uncritically accumulated opinions.

Earthquake Ground Motions from the Ground Motion Panel

Seven models were developed for assigning earthquake ground motions and attenuating them from the source zones to the nuclear power plant sites. The models were as follows:

(1) Boore-Atkinson. Based on physical assumption of the source spectrum and vibration theory, for rock.

(2) Toro-McGuire. Same as Boore-Atkinson but with different values.

(3) Another version of the above, with different parameters.

(4) Trifunac. Empirical correlation of peak acceleration versus epicentral intensity and Gupta-Nuttli attenuation of intensity, for rock, deep soil, and intermediate.

(5) Nuttli. Model based on corner frequency and seismic moment, for soil.

(6) Nuttli. Same as above, with different values.

(7) Veneziano. Empirical relationships of intensity and strong motion data, for rock or soil.

Additionally, methods were developed for assessing motions for soil versus rock at the sites and for expressing motions as spectral compositions for seismic excitations at the sites.

Eastern United States was divided into four regions, northeast, southeast, northcentral, and southcentral. Each expert was asked to select anonymously:

(1) a best model for each region,

(2) as many as six other models in which the expert had less confidence, and

(3) assign degrees of belief to show exactly how less confident the expert was in each of the latter selections.

Calculating the Seismic Hazard

Returning to the source zones, the seismic potential in each zone was determined from the Gutenberg-Richter relation between magnitude of earthquakes and frequency of occurrence. (Merits and shortcomings of the Gutenberg-Richter equation are discussed extensively in Part Two of this review.) The relationship produces a straight line on semilog paper. The curve can be projected to interpret the larger and less frequent earthquakes that may not yet have occurred. The curve is open ended so that limiting maximum sizes of earthquakes must be interpreted. Ground motions and attenuations from the Ground Motion Panel were applied to these source earthquakes and the calculated ground motions through time at the nuclear power plant sites were developed.

To obtain the above curves, every expert opinion for every seismic source and every model for ground motion were calculated individually. Typically there were 2,750 such curves calculated for each site, 50 simulations per ground motion expert x 5 ground motion experts x 11 seismic zone experts. The multiplicity of curves were then combined into curves for mean values and standard deviations for each site. The process for combining these data is termed a Monte Carlo simulation. Figure 10 shows these values for acceleration in the combined curves produced by LLNL and EPRI for the Vogtle Nuclear Power Plant site in Georgia (see Berneuter and others, 1987). Note the open ended extensions of the curves and the enormous dispersion in the values between the 15 and 85 percentiles. The spreads in the LLNL and EPRI curves each are one to two orders of magnitude. And there is an order of magnitude difference between LLNL and EPRI. Other curves were developed to show spectral compositions at the median, 15, and 85 percentiles for 1,000 and 10,000 year periods. LLNL labels the spread between the 15 and 85 percentiles as an essential element of information that gives a measure for <u>uncertainty</u>.

Uncertainty

In logic, there are in principle no external evaluations for subjective judgments. Nonetheless, in practice subjective judgments, or opinions, are widely used in decision making. They also contribute the contingent theoretical assumptions from which all of our scientific progress is achieved. And there are criteria that can be applied to judge opinions, though they must be used with reservations. Following are three taken from Seaver (1978):

(1) Subjective decisions should be responsive to evidence.

(2) The opinions should occur with a frequency resembling the probability. Events for which the probability is 0.75 should occur about 75 percent of the time, and about 50 percent of the values should fall below the median of the probability density and conform to the interquartile ranges.

(3) Opinions should be extreme in their range. For individual judgments, probabilities assigned to events expected to occur should be near 1.0, while non-occurring events should be near 0.0. Continuous assessments should have a high density at the true value and a density of 0.0 elsewhere.

The experience of Hynes and Vanmarcke showed that the requirements of (2) and (3) could be fulfilled but the resulting conclusions can be wrong when (1) is not fulfilled entirely. In forecasting the times at which seismic events occur, (1) is never fulfilled unless the earthquakes occur. In the LLNL study, it appears that an attempt was made to shore up the deficiency in (1) by the strong emphasis that was made to obtain the maximum breadth called for in (3). This was done by engaging a large number of experts and getting shades of their opinions, representing (2), through eliciting various levels of their degrees of confidence. The range of uncertainty thus obtained was significantly enlarged over that which the best estimates alone would have produced. However, this enlarged assessment of uncertainty falls afoul of a different problem in logic.

The uncertainty of a statement is simply the degree of its logical weakness or lack of informative content. With increasing content, uncertainty decreases. To state it differently, increases in informative content produce increasing certainty.

When everything is known for an engineering decision, our knowledge is said to be <u>deterministic</u> and there is no <u>uncertainty</u>. Though inductive logic <u>always</u> contains uncertainty, enough can be known to have full knowledge of a forecast hazard and a preventive design. For an engineering decision at a critical project, there need be no more than a maximum earthquake attenuated from a source to a site, done on conservative and defensible principles.

The rationality of science lies in its critical approach, and good engineering involves the effective use of evidence. <u>Un</u>critical reliance on opinions flies in the face of good science and good engineering.

Not least is another problem: The <u>value</u> of the opinions. All of the previously discussed studies by Okrent, Eguchi, Krinitzsky, Hynes and Vanmarcke, and LLNL, reveal the presence of experts, in large numbers, whose opinions are unsatisfactory for one reason or another. Uncertainty, obtained from them, has doubtful meaning. How then should experts be valued for certainty?

Evaluating the Experts

The advocates of decisions by multiple experts have a copious literature on the art of judging the quality of experts. Two very notable guides are Meyer and Booker (1990) and Bonano and others (1990). Both are well organized, clearly written, and informative. They review a great body of diligent if unadventurous research. They represent the best of the writings in this genre.

Do you want to engage an expert? Bonano et al. tells you what to do. Check the expert for

(1) Education

(2) Publications

(3) Research grants

(4) Professional societies

(5) Professional activities

Get peer judgments to assess his

(1) Communication skills

(2) Interpersonal skills

(3) Flexibility of thought

(4) Command of topics

(5) Ability to simplify

and so on.

The failing is that the authors of this and other guides gingerly avoid applying unpleasant cautions. They choose to inform their readers of platitudinous goodnesses and not to be concerned about encountering ogres. The marble has no fissures, the tapestry has no missing threads, the crystal is without bubbles, none of the experts are muttonheads, and there is no need to probe for these deficiencies so as not to be fooled. Do you expect to never encounter fee-hungry knaves? No panjandrums, no time servers, no dodderers in their dotage? Yet, these and all sorts of other characters can pass inspections, especially when their most serious deficiencies are submerged in tepid douches of banality.

This activity in dealing with experts created a new type of expert, the expert in the managing of experts. And it contributed to creating a new peril: management experts who have no knowledge of what they are managing, who can give no worthwhile direction, and who are not equipped to know when they are dealing with mountebanks.

Do you want to believe in Edens that have no snakes? Then the current crop of engineering design recommendations based solely on expert opinions were written expressly for you.

Why Engage Multiple Experts?

Bernreuter and others (1986) give the following reasons for creating the LLNL methodology:

> Because of the short historical record, low rate of earthquake occurrence and a general lack of agreement as to the causes of earthquakes in the eastern United States (EUS) both the physical data alone and/or mathematical models are inadequate for describing the seismic hazard throughout that region. Therefore, it is a common practice to supplement the data with professional judgment and opinions when attempting to estimate the future seismic hazard in the EUS. Because of the limited historical record and the use of subjective judgments it can be expected that diverse opinions and large uncertainties will surround seismicity and ground motion descriptions. Therefore, any estimation of future seismic hazard in the EUS must deal with this uncertainty and diversity of opinions.
>
> Recognizing these facts, the U.S. Nuclear Regulatory Commission (NRC) funded the Lawrence Livermore National Laboratory

(LLNL) to develop a seismic hazard assessment methodology which deals with the diverse opinions and uncertainties and to implement the methodology....

A priori assumptions were made that:

(1) a large variety of subjective opinions provides the best information that can be obtained: and

(2) gathering subjective opinions is the only valid route to follow.

Those assumptions were contradicted by what we saw in the studies of expert opinions that we reviewed; yet, in decision analysis there is material that can be cited in favor of the assumptions and, I suspect, may have misled the management experts. What I am speaking of are rather simple exercises that involve answers to questions for which very little depth of analysis is called for.

Researchers in the 1920s asked subjects to estimate lengths of lines, weights of objects, ages of people, or provide other simple judgments. The individual answers might vary greatly but the averages were close to the real values. An example is a paper by Gordon (1924) reporting the results of using 200 university students to judge weights. Mean attainment as individuals was 0.41 but together the attainment was 0.94. The group was distinctly superior to the individuals and equal to the best individuals. It is easy to perform exercises of this sort yourself and you will very likely obtain corroborative results.

I asked 23 colleagues to draw a two-inch line. They gave me lines that varied from 0.92 to 2.65 inches. The average was 1.86 inches. Combining a large number of best guesses is obviously safer than depending on any one.

The effect of group size on group error was examined by Dalkey (1969) in the famous Delphi studies. Dalkey used almanac-type questions. Example: How many telephones are there in Uganda? The questions had single answers. There was virtually no depth of analysis, but much speculation.

Dalkey took the group error as the absolute value of the natural logarithm of the group median divided by the true answer. The relation between error and group size is seen in Figure 11. The gains with increasing group size has a marked regularity and in a group of 15 persons an accuracy is achieved that is enormously better than what a few individuals are capable of and does not increase appreciably with further increases in size of the group.

What happens in exercises of the above sort is that a bell-shaped curve is formed. Constructing its median is a compensatory integration mechanism that provides a tradeoff among the disparate evaluations. A smooth shape to the bell suggests a coherent and balanced process.

In statistics, Dalkey's observations can be seen in Fisher's null hypothesis in which the regularity of a bell shape determines the validity of a procedure. Fisher held that a statistical hypothesis should be rejected by any experimental evidence which, based on the hypothesis, is relatively unlikely, the unlikelihood being determinable when it is a significant deviation from the bell. For a demonstration of Fisher's approach, see Howson and Urbach (1989).

Fisher's null analysis can be applied to more complex relationships, those in which both $\underline{x}$s and $\underline{y}$s are values assumed by random variables. This process falls under the aegis of correlation analysis. A conditional density called the bivariate normal distribution is determined (see Miller and Freund, 1985) to which Fisher applies a $\underline{Z}$ transformation and a solution that again provides a bell curve when the two probabilities form a symmetrical density. However, a satisfactory correlation does not prove a causal relationship between the two random variables. It is likely that we could discover a high positive correlation between the sale of mouthwash in the United States and the incidence of crime. Both relate to size of population. But banning the sale of mouthwash would not eliminate crime. In the Hynes and Vanmarcke study, an aspect of this problem of meaning can be seen visually in Figures 3 and 4. The combined expert opinions would have passed Fisher's null analysis for a bivariate distribution, yet the resulting median value would have no useful meaning. Statistical analysis alone cannot tell us when ideas are meaningless.

The idea that feedback and elicitation can focus expert opinions received its major impetus from work in the Delphi studies. The objective was to make group judgments less disparate and more meaningful. Figure 12 shows results from work by Dalkey and Helmer (1963). Controlled feedback done individually with no group interaction, and done on an iterative basis, brought the initial disparities down remarkably. A correction was made in the last step that factored in the experts' estimates of effective disruption from less than total destruction. A fourth convergence was obtained.

Experiments of this sort helped to establish elicitation and its objective of obtaining convergencies of opinions. It further justified the use of multiple expert opinions.

We should look at the questions asked in these exercises. Besides almanac questions, they asked questions for which there were no credible answers. Figure 12 shows the results of a query on how many bombs are needed to level a metropolis. Numbers of this sort are never more than speculative. Who knows all the factors, the weather, availability of planes, determination, military resistance, logistics of supply, structural resistance, and goodness knows what else? These are questions that not only require no depth of analysis, but for which there are no essential answers.

With this approach we are intruding into another territory namely a political one. Some managers, especially in the political arena, have a perceived need to use experts in abundance in order to have persons to blame, other than themselves, should the results be disastrous.

It is for these rather shady and mostly inconsequential purposes that the Delphi studies of group opinions were originally developed. Along the way, the methodologies experienced a transference and grew from answering questions that required no depth of analysis and had no great consequences, to answering very complex questions that are crucial to engineering and life safety. Totally lost was the basic question of what the substance of expert opinions really is -- mature judgment supported by facts and substantial related experience. I find it very difficult to accept that someone lacking facts or experience needs only to look inside himself, form an opinion that expresses his on-the-spot, prejudiced inclination and then have his opinion averaged with others of the same sort and see the result taken as the very best that can be obtained for engineering design.

The LLNL Zonation and Seismicity Panel produced seismic source zones, given in Figure 8, that reflect their opinions, and largely bear no relation to the factual geologic and seismic evidence. Consequently, the elicitation shown in Figure 9 produced zones that were greatly more disparate than those produced initially. Instead of convergence, as expected, there was a greatly pronounced divergence. That was a reflection of problems with the expert opinions. Capable management experts should have quickly realized the error of judgment involved.

Research into subjective group estimations has never grappled successfully with opinions that are based on genuinely complex information, such as the inputs discussed in our section on earthquake ground motions and for the multiplicity of usages in engineering analyses presented in Tables 7A and 7B.

No study has been undertaken that

(1) follows a single person, a principal investigator, who is not necessarily an expert,

(2) allows the person to gather and digest evidence,

(3) allows the person to form conclusions,

(4) has the work and its conclusions checked for mistakes with a review by other professionals,

(5) allows the person to correct obvious errors and an option to accept or reject judgmental advice, and

(6) present conclusions.

In other words, allows a working professional to do what is done normally in every respectable engineering firm. And

(7) then pits this principal investigator's conclusions against conclusions averaged from the massaged opinions of multiple experts.

A confrontation of this sort, repeated enough times, becomes statistically valid and tells us something about the usefulness of multiple expert opinions for deciding complex issues. But is it necessary? All that is needed are the experiences summarized herein, those of Okrent, Eguchi, Krinitzsky, the Vallecitos dispute, Hynes and Vanmarcke, and LLNL. In no way do they discern any advantage in relying on multiple expert opinions. At best, see Hynes and Vanmarcke, those opinions are shown to be treacherous, but there is no way to tell this without having the correct answer. At worst, see Figure 9, they contain elements that verge on idiocy.

Cost of the LLNL Study

I was informed through the sponsors in the U.S. Nuclear Regulatory Commission that the cost of the LLNL study from 1982 to 1989 was 1.2 million dollars. Allowing for inflation, the present-day cost would be at least two million dollars.

LLNL did very little original work along the lines of developing evidence in eastern United States. They provided their experts with existing information, and they produced some additional seismic attenuation models for analyses. The work was mostly compiling opinions followed by an extraordinarily elaborate massaging and processing that they gave the opinions. The results, a typical example of which is shown in Figure 10, are in my judgment unsatisfactory and misleading.

How else might earthquake ground motions be assigned to all of the 69 nuclear power stations in eastern United States without doing independent investigations? Let me suggest the following:

(1) Take the seismic source zones shown in Figure 7.

(2) Locate the nuclear power plants.

(3) Get the distances from the seismic source zones to the plants within a radius of 150 or 200 miles.

(4) Attenuate the source intensities using curves by Chandra (1979) to get site intensities.

(5) Assign equivalent ground motions for the site intensities. Values are available from relationships published by Krinitzsky and Chang (1988).

Those earthquake ground motions would be reasonable ballpark values. The method is deterministic; it lacks the probabilistic time dependence of the

LLNL motions. For a nuclear power plant, where the consequences of failure are intolerable, the design must consider a maximum credible earthquake which the deterministic method supplies in a defensible form.

To do the above exercise, the steps could be set up so that a technician might perform the study in about half a day. The cost would be about a couple of hundred dollars.

Myron Tribus (1969) cites the following comments on practical needs in engineering written by A. M. Wellington in 1887:

> It would be well if engineering were less generally thought of, and even defined, as the art of constructing. In a certain important sense it is rather the art of not constructing; or to define it rudely but not ineptly, it is the art of doing that well with one dollar, which any bungler can do with two after a fashion.

The costs between deterministic ground motions based on doing no independent site studies and the probabilistic motions based comparably on opinions are not between one dollar and two dollars, they are between two hundred dollars and two million dollars. They are also between a method with a database that can give defensible results and a method that, for many reasons enumerated here, is suspect and should not be trusted.

The LLNL expenditures are by no means ended. The Nuclear Regulatory Commission announced as Policy Issue SECY-92-122 on April 8, 1992 that an additional 2.3 to 2.8 million dollars will be allocated to resolve the differences between the LLNL and EPRI studies.

The National Research Council's Panel on Seismic Hazard Analysis

In 1984, the National Research Council, which functions under the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine, established a Panel on Seismic Hazard Analysis. Ten members were appointed for their "special competencies," along with ten Liaison Members. The latter had no voting rights.

The mandate to the Panel was as follows:

> The Panel on Seismic Hazard Analysis is to assess the capabilities, limitations, and future trends of probabilistic seismic hazard analysis (PSHA) in the context of alternatives.
>
> The objective of SHA is to quantify for engineering design and public policy purposes the probability, p, that at a particular site a certain specified level of ground motion will be exceeded in the next n years, where p may be on the order of 10^{-1} to 10^{-5} and n may be on the order of 1 to 100 years or, in the case of nuclear waste disposal, on the order of thousands of years. A secondary objective is to define more or less quantitatively the uncertainty in that probability estimate.
>
> Many engineering decision makers and several public agencies use, or are evaluating for future use, formal risk analyses. When seismic hazards are involved in these analyses, quantitative probability and uncertainty statements are requisite input. The panel should evaluate current seismic hazard analysis theory and application with respect to (1) its consistency with the wider, general use of quantitative risk analysis in science, technology, and public policy, (2) its technical merits in terms of applied probability and statistics, and (3) its relationship to the earth sciences and earthquake engineering. On one

hand, scientists have argued that the field knows too little to make such quantitative statements and that, therefore, PSHA may "abuse" their science. On the other hand, given that some decisions must be taken, seismologists and other scientists have often been asked to take large responsibilities with respect to engineering decisions and public policies regarding seismic hazards, even when they may be lacking the information regarding costs, impacts, and alternatives that are crucial to the problems; PSHA has been presented as a way to transmit unequivocally to the responsible parties what earth scientists are uniquely qualified to evaluate: seismic probabilities and their current uncertainties.

The Panel on Seismic Hazard Analysis is to report to the Committee on Seismology in approximately two years for a presentation of its assessments and its recommendations.

Uncertainty, as cited in the mandate, is of course the statistical massaging of subjective opinions obtained from multiple experts.

The mandate is an odd one. It assumes that the probabilistic method is the only acceptable method and this decision was settled a priori, a false assumption. The major construction and regulatory agencies of the United States government had not, as a matter of policy, accepted the exclusive use of the probabilistic approach for critical structures. These organizations included the U.S. Army Corps of Engineers, the Bureau of Reclamation, and the Nuclear Regulatory Commission. At this time, 1992, these three organizations still have not accepted probabilistic analyses as policy. The Department of Energy, on the other hand, appears to be going the probabilistic route for specifying the seismic safety for permanent storage of hazardous nuclear wastes. There still are crucial decisions to be made at high policy levels regarding what methods are to be preferred for assuring seismic safety at critical facilities.

The mandate to the Panel of the National Research Council was in my opinion a deliberate attempt to circumvent these contentious issues and to be a fait accompli for the probabilistic method. In the parlance of the National Academies, the Panel was a "controlled committee." Its report, see Panel on Seismic Hazard Analysis (1988), predictably describes the LLNL-EPRI studies as models for proper seismic evaluations. Deterministic analysis is passed over as an outdated method and is treated simplistically and inadequately.

The Panel's recommendations are:

(1) Use simple probabilistic analyses for non-critical structures.

(2) Use sophisticated probabilistic analyses that include uncertainties to assess critical structures.

(3) Use probability to reexamine the deterministic values by which older projects were designed.

These panel recommendations, appearing as they do under the auspices of the National Academies exert an enormous pressure on governmental agencies and the engineering profession.

Desires and beliefs lie behind man's creations. I believe the veiled spirit that drove this Panel can be seen best when we conjure it into being by metaphor:

We on this Tribunal were assembled by the highest authorities to render a final and unequivocal verdict on the obvious truth that the world is guided by a multiplicity of gods. Having formulated our most profound beliefs, we come before you as exceptionally qualified by our knowledge and our sincerity. We will provide a final solution that will be binding on all persons regardless of other and now obsolete beliefs. Be assured that our judgment will be in accordance with our very best understanding. Thus, those who are affected by our decision need not be concerned that the members of this Tribunal happen to be devoted supplicants of all the gods on Olympus.

If you think that is far-fetched, consider the following:

(1) Two of the members of the NRC Panel worked on the LLNL study of probabilistic seismic hazards at nuclear power plants in eastern United States.

(2) Four others on the Panel worked on the EPRI study for eastern United States and were developers of the EPRI methods.

(3) Another was employed by EPRI on consultations involving seismic probability.

(4) Yet another published papers that dealt with probability theory.

(5) Of ten persons on the Panel convened to evaluate probabilistic methods, eight had prior commitments to probabilistic analyses.

(6) There remained two persons who were not fully committed to the probabilistic process. However, they were not well qualified to make either a case for the deterministic method or to criticize effectively the shortcomings in the probabilistic process.

With a Panel like that, the exclusive recommendations for the probabilistic methods were certain before the Panel ever met. Yet I would not deny that the Panel acted from the very loftiest motives.

Probability theory is founded on the belief that every future event is uncertain, thus it is probable to some degree. That assumption is beyond dispute. The question to ask is what makes the probability necessary? We can admit to uncertainties and still take a positivist approach. We do it all the time in engineering. We select design levels with the assurance that what we select covers all reasonable possibilities and fulfills the need for safety in a structure. (Part Three of this review will explore this subject.) Ironically, probability theorists do the same. From their calculations of endless variations through time, they must always at some point snap their minds shut and take a value for design. That value is as deterministic as any other. My quarrel with them is that their attempt to quantify what they call uncertainties and to project earthquakes through time is done in ways that are logically defective. (The problems with time-related projection of seismic events is the subject of Part Two of this review.)

Probability theorists become shackled by their logic. First, they exaggerate its importance. Then they proceed to do things that are illogical and erroneous, not to mention costly, under the false claim that what they do is necessary.

Comment

A slanted mandate established and instructed the NRC Panel on Seismic Hazard Analysis. The unbalanced mandate contributed to an imbalance in the appointments to the Panel and consequently in the deliberations. Inevitably, the Panel's recommendations were biased.

Constraints on the Experts from Within the Seismic Probability Methodology

It may seem from the wide-ranging acceptance of opinions that the LLNL-EPRI studies were not constrained by prior assumptions. That is not true. There were very binding constraints that resulted from probability theory. Following are some of the most critical. They are from Bernreuter and others (1989) and they describe requirements in the LLNL studies.

(1) <u>For each zone, it is assumed that earthquakes could occur randomly over time and uniformly at random within the zone.</u>

(2) <u>All earthquakes are assumed to be point sources, thus the fact that earthquakes are created by the rupture of tectonic faults of finite length is neglected.</u>

(3) <u>The occurrence of earthquakes is assumed to be independent between zones.</u>

(4) <u>The expected number of earthquakes of magnitude m or greater occurring within a zone can be described by the magnitude-recur-</u>

rence relation.

Effects on the Experts of Constraints from Seismic Probability Theory

The experts had no opportunity to critize either the theory or the method by which seismic probability was evaluated. The experts might be wrong-headed or very wise in these matters; the method did not care. The experts were engaged to provide input only.

Since the experts were never asked to judge the method, it may be argued that a purpose of the exercise was to co-opt these experts.

It should be no surprise that constraints built-in from assumptions in the probability method affected the experts' opinions in unintended ways.

Experts are not usually muttonheads or knaves; they are more often the brightest and the best. LLNL and EPRI engaged almost all of the best and the brightest of the earthquake hazards fraternity. Nonetheless, as sensible people, they are capable of performing with a protective bias. Some of the choices can be explained by this kind of thinking. Some experts could very logically decide to give as conservative an interpretation as possible and, pleading lack of knowledge, they might not see any reason to restrict large earthquakes to the vicinities of persistent seismicity as I suggested they should.

The expert who assigned an identical seismic potential for the whole of eastern United States (Figure 9) did not understand what the seismic zones were meant to represent. However, his zone could make sense if he had in mind that in some short period of time, 40 to 60 or 80 years, which is the life of a nuclear power plant, the maximum earthquakes would be greatly smaller than the larger of the historic earthquakes and consequently might be uniformly distributed. However, the methodology required an endless projection of earthquakes through time. Also the methodology required that the earthquake ground motions for the zone be assigned by someone else who should not be expected to be thinking along the same lines. It may be that the expert did not know the implications of his choice. The methodology was supposed to take the process to a conclusion and this reliance on the "rest of the method" introduces a major problem. It allows experts to furnish information that lead to conclusions the experts might themselves never have permitted if the conclusions were known to them up front.

Experts who are unquestionably the best and the brightest can be confronted by another problem in the methodology which involves the nature and applicability of their expertise. The methodology is structured so that experts may be obliged to give replies for which they are not expert. The experts could not choose the questions that they felt competent to answer. The methodology demanded any and all opinions in order to show a breadth for uncertainty. Uncertainty and ignorance became confused.

Does the anonymity used by LLNL and EPRI make the expert opinions braver, more penetrating, more fruitful? There is no reason to believe that it does. Anonymity can be a cover for haste, for shoddiness, and especially for thoughts that are weakly held and irresponsible. We ought to know what an expert is willing to sign his name to. Anonymity only worsens the intrinsic defects noted above.

Errors in the Assumptions of Seismic Probability Theory

The assumptions given by Bernreuter and others (1989) and quoted in the above section are basic to probabilistic seismic hazard evaluations. There are severe problems in these assumptions that become apparent on examining the mechanics of faulting and the accompanying behavior of earthquakes. These deficiencies will be examined in Part Two of this review.

Conclusion

Probabilistic seismic hazard analysis, when based on multiple expert opinions, is intrinsically unreliable and excessively costly. The method is not suitable for developing design applications in engineering.

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Table 1

Ranges in Peak Earthquake Ground Motions by Seven Experts for 10^{-4}/year Earthquakes at Nuclear Power Plant Sites from Okrent (1975)

Site	Acceleration g	Duration sec	Cycles/sec
Brunswick (North Carolina)	0.15 - 1.0	5-20	1/3-10
Cooper (Nebraska)	0.1 - 1.0	3-20	1/3-10
Davis Besse (Ohio)	0.1 - 1.0	5-20	1/3-10
Diablo Canyon (South California)	0.5 - 1.1	15-17	2-8
Grand Gulf (Mississippi)	0.15 - 0.5	15-20	1-3
Pilgrim (Massachusetts)	0.1 - 1.0	5-30	1/3-15
Rancho Seco (North California)	0.15 - 1.0	16-30	1-15
River Bend (Louisiana)	0.1 - 0.5	5-50	1/6-10
Summer (South Carolina)	0.1 - 1.0	10-20	1/3-15
Summit (Delaware)	0.18 - 1.0	10-30	1/3-15
Trojan (Oregon)	0.2 - 1.0	"few"-30	1/4-10

Table 2

Ranges in Expert Opinions for Fault Lengths, Earthquake Magnitudes, Slip Rates, and Fault Depths from Eguchi and others (1979)

Fault	Number of Experts	Fault Length km	Maximum Credible Earthquake M	Slip Rate cm/yr	Fault Depth km
California:					
Death Valley	7	30-109	6.6 - 7.8	0.001 - 0.05	--
No Name (#150,151)	4	184-260	6.5 - 7.5	--	--
Oakridge	3	39-54	4.5 - 7.5	--	--
Ozena	2	36-106	5.5 - 7.3	--	--
Palos Verdes	4	11-76	5.5 - 7.0	0.05 - 0.1	--
Raymond	4	14-21	4.0 - 6.8	0.0013	12-20
San Andreas, Northern Section	5	409-459	7.7 - 8.3	3.0 - 5.0	12-40
San Andreas, Central Section	5	289-293	8.0 - 8.5	2.0 - 4.0	12-40
San Andreas, Southern Section	5	183-200	7.5 - 8.25	1.0 - 4.0	10-40
San Gabriel	3	78-108	5.0 - 7.5	--	--
Sierra Madre (East)	3	16-55	6.5 - 7.5	0.001 - 0.8	12-20
Nevada:					
Dixie Valley	3	85-130	6.8 - 8.0	0.1 - 1.5	--
Fairview Peak	3	40-80	6.8 - 7.5	0.1 - 1.5	--
Pleasant Valley	3	40-70	7.6 - 7.75	0.1 - 1.5	--
Pyramid Lake	2	17-90	6.0 - 7.5	0.15 - 1.0	--

Table 3

The Vallecitos Controversy According to Meehan in "The Atom and the Fault," 1984, and in Other Related Documents

From Meehan (1984): Allegations Concerning an Active Fault at the Reactor	From Meehan (1984): Adversarial Positions	From Other Sources (See References): Other Positions
1977: Herd (USGS): Mapped the "Verona Fault 200 ft from reactor. NRC: Ordered reactor shut down. Brabb (USGS): Endorsed fault interpretation. Stepp, Jackson (NRC): Endorsed fault interpretation.	1977: Harding (ESA): Trenches and boreholes find low angle shear. Interpreted an ancient landslide. Jahns (C): Endorsed landslide interpretation.	1977: Herd: (USGS): The fault is based on alluvial stratigraphy, scarps w/truncated gravels and a line of springs and seeps. Noted a recent history of small, felt earthquakes.
1978: Slemmons (C): Endorsed fault interpretation. Fault may displace 3 m below reactor.	1978: Harding (ESA): Two miles of trenches, plus seismic reflection and refraction, and soil age dating: Shears were found on both sides of reactor and extend under the reactor. 3-ft displacements interpreted every 17,000 years. Cause of movement, landslide or fault, is indeterminant.	1978: ESA: The Verona fault interpretation is an error, but there are several shears and a possible low angle thrust fault along base of the hillfront to the northeast of the reactor.
1979: NRC: Established design-basis fault displacement under reactor at 1 m. Brabb (USGS): 1 m is not enough. Jackson (NRC): Probability interpretation is not reliable.	1979: GE: Photos of foundation excavation at reactor suggest possibility of faults. Meehan (ESA): Probability calculation at reactor shows remote recurrence of 1/1,000,000 per year. Jahns (C): Verona fault is very doubtful but cannot be ruled out.	1979: ESA: A trench along the Verona fault found "a large, steeply dipping strike-slip fault with minor or near surface thrust-like splays." But it is not a major tectonic structure. 1979: Davis (CDMG): Three ft of surface displacement at the reactor site is conservative for either a landslide or fault interpretation. Slemmons (C): The probability analysis is not valid because there are (1) no accurate dates, (2) only one measured individual displacement, (3) the number of paleosols are not known, (4) cumulative displacements can imply shorter recurrences and greater risks, (5) the geometry of associated movements is not known, and (6) a Poisson distribution may not be appropriate. Page (USGS): Fault mechanism is correct. Some faulting occurred intermittently until few thousand years B.P. and may occur again.
	1981: GE: Accepted the fault interpretation affecting the site. Meehan (ESA): Fault movement would not break a 5-ft-thick concrete slab under a reactor that is the size of a garbage can.	1980: Herd and Brabb (USGS): Fault traces found near the reactor displace the modern soil profile, show multiple movements during Pleistocene, and dip beneath the reactor structure. The structure sits on a fault zone.

CDMG: California Division of Mines and Geology
C: Consultant
ESA: Earth Sciences Associates (Meehan's Company)
GE: General Electric Co.
NRC: Nuclear Regulatory Commission
USGS: U.S. Geological Survey

1981: Three-man Atomic Safety and Licensing Board reviewed the contentions.
1982: License for GE to operate the reactor approved.
1983: Appellate Board affirmed first Board's decision. NRC gave final approval six years after the shut down.

Table 4

Ranges in Peak Horizontal Ground Motions on Soil by 18 Experts*

from Krinitzsky (1980)

Location of Site	Acceleration ~~g~~	Velocity cm/sec	Displacement cm	Duration sec
San Andreas fault, M_s = 8.3	0.35-3.0	46-550	40-30	20-90
5 km from San Andreas fault, M_s = 8.3	0.35-3.0	46-550	20-300	20-90
50 km from San Andreas fault, M_s = 8.3	0.18-0.4	20-100	10-40	20-50
150 km from New Madrid source, M_s = 7.5	0.03-0.5	5-100	1-50	2-120
Floating earthquake, Eastern U.S., M_s = 6.5	0.05-2.0	1-300	0.05-190	8-60
Floating earthquake, Western U.S., M_s = 6.5	0.15-2.0	10-300	4-190	10-30
Reservoir-induced earthquake, M_s = 6.5	0.35-2.0	40-300	20-190	10-30

* 11 consulting firms, 4 individual consultants, 3 government agencies.

Table 5

Growth of Values for Peak Horizontal Accelerations Through Time

Year	Events	Peak Horizontal Acceleration G
1920s	Lateral loads for buildings in San Francisco	0.10
1927	California Uniform Building Code, for pseudo-static analysis on rock	0.10*
	(Late 1930s First strong motion accelerographs)	
1940	El Centro, California, earthquake; M - 7.1, soil	0.33
1967	Parkfield, California, earthquake; M - 5.6, soil	0.50
1971	San Fernando, California, earthquake; M - 6.5, rock	1.25

* ≈ 1/2 A_{max} applied at base of structure.

Table 6

Peak Horizontal Accelerations ≥ 1.0 G

Year	Earthquake	Distance to Fault km	Magnitude M	Horizontal Acceleration g
1971	San Fernando, Pacoima Dam	4	6.6	1.25
1983	Coalinga, Anticline Ridge; Transmitter Hill	7.6 --	6.5 6.5	1.17 0.96
1984	Morgan Hill, Coyote Dam	At site?	6.1	1.29
1985	Nahanni, Site 1	At site	6.6	1.25
1987	Palm Springs, Devers Substation	At site	6.0	0.97
1987	Cerro Prieto	At site?	5.4	1.45
1992	1984, Western Nagano Prefecture (Motions interpreted.)	Near	6.8*	2.0

$^{*}M_{JMA}$

Table 3a

Earthquake Ground Motions for Use in Pseudostatic Analysis. From Krinitzsky and Others (~~In Press~~ 1992)

	Foundation Liquefaction	Earth Embankments and Stability of Slopes	Earth Pressures	Concrete and/or Steel Frame Structures
A. Non-critical facility in any zone of seismic activity, and/or critical facility in an area of low seismicity (peak hor accel <0.15g)	Pseudostatic analyses do not apply. Use dynamic analyses.	1. Use 1/2 $(A_{max})_{mean}$ at base for sliding block. 2. A_{max} is obtained from peak hor motion (mean)* from (a) MM intensity (b) Mag-distance attenuation (c) Probability ~50-yr, 90 percent nonexceedance.	1. Peak hor motions (mean)* from (a) MM intensity (b) Mag-distance attenuation (c) Probability ~50-yr, 90 percent nonexceedance. 2. Use 1/2 $(A_{max})_{mean}$ for backfill.	1. Seismic-zone coefficients/factors in building codes. 2. For generating ratio of A_{max} to A of structure or element, A_{max} is obtained from peak hor motions (mean)* from (a) MM intensity (b) Mag-distance attenuation (c) Probability ~50-yr, 90 percent nonexceedance.
B. Critical facility in an area of moderate to strong seismicity (peak hor accel ≥0.15g ≤0.40g).	Use dynamic analyses.	1. Use 1/2 $(A_{max})_{mean}$ for sliding block. 2. A_{max} from peak hor motions (mean + S.D.)* from (a) MM intensity (b) Mag-distance (c) Probability ~250-yr, 90 percent nonexceedance.	1. Peak hor motions (mean + S.D.)* from (a) MM intensity (b) Mag-distance attenuation (c) Probability ~250-yr, 90 percent nonexceedance. 2. Use 1/2 $(A_{max})_{mean}$ for backfill.	1. Seismic-zone coefficients/factors in building codes. 2. A_{max} from peak hor motions (mean + S.D.)* from (a) MM intensity (b) Mag-distance attenuation (c) Probability ~250-yr, 90 percent nonexceedance.

* Adjust for site condition, near field or far field.

Note: A_{max} is the peak value in a time history.

7

Table 6b

1992

Earthquake Ground Motions for Use in Dynamic Analysis. From Krinitzsky and Others ~~(In Press)~~

	Foundation Liquefaction	Earth Embankments and Stability of Slopes	Earth Pressures	Concrete and/or Steel Frame Structures
A. Critical facility in an area of moderate to strong seismicity (Peak hor accel ≥0.15g) 1. Obtain Maximum Credible Earthquake (MCE).	1. Peak hor motions (mean + S.D.)* 2. Generate time histories.	1. Peak hor motions (mean + S.D.)* 2. Generate time histories.	1. Peak hor motions (mean + S.D.)* 2. Generate time histories.	1. Peak hor motions (mean + S.D.)* 2. Generate time histories. 3. Obtain responses spectra for above time histories. 4. Alternatively, go directly to response spectra, entering with the above peak motions. 5. Check response at the natural frequency of the structure.
2. Obtain Operating Basis Earthquake (OBE).	1. Peak hor motions (mean + S.D.)* 2. Peak motions from probability ~50-yr, 90 percent exceedance +S.D. 3. Generate time histories	1. Peak hor motions (mean + S.D.)* 2. Peak motions from probability ~50-yr, 90 percent non-exceedance at S.D. 3. Generate time histories.	1. Peak hor motions (mean + S.D.)* 2. Peak motions from probability ~50-yr, 90 percent non-exceedance + S.D. 3. Generate time histories.	1. Peak hor motions (mean + S.D.)* 2. Peak motions from probability ~50-yr, 90 percent nonexceedance + S.D. 3. Generate time histories and/or obtain response spectra. 4. Check response at the natural frequency of the structure.

* Obtain peak hor motions from (a) MM intensity or (b) magnitude-distance attenuation charts. Adjust for site condition and near field or far field.

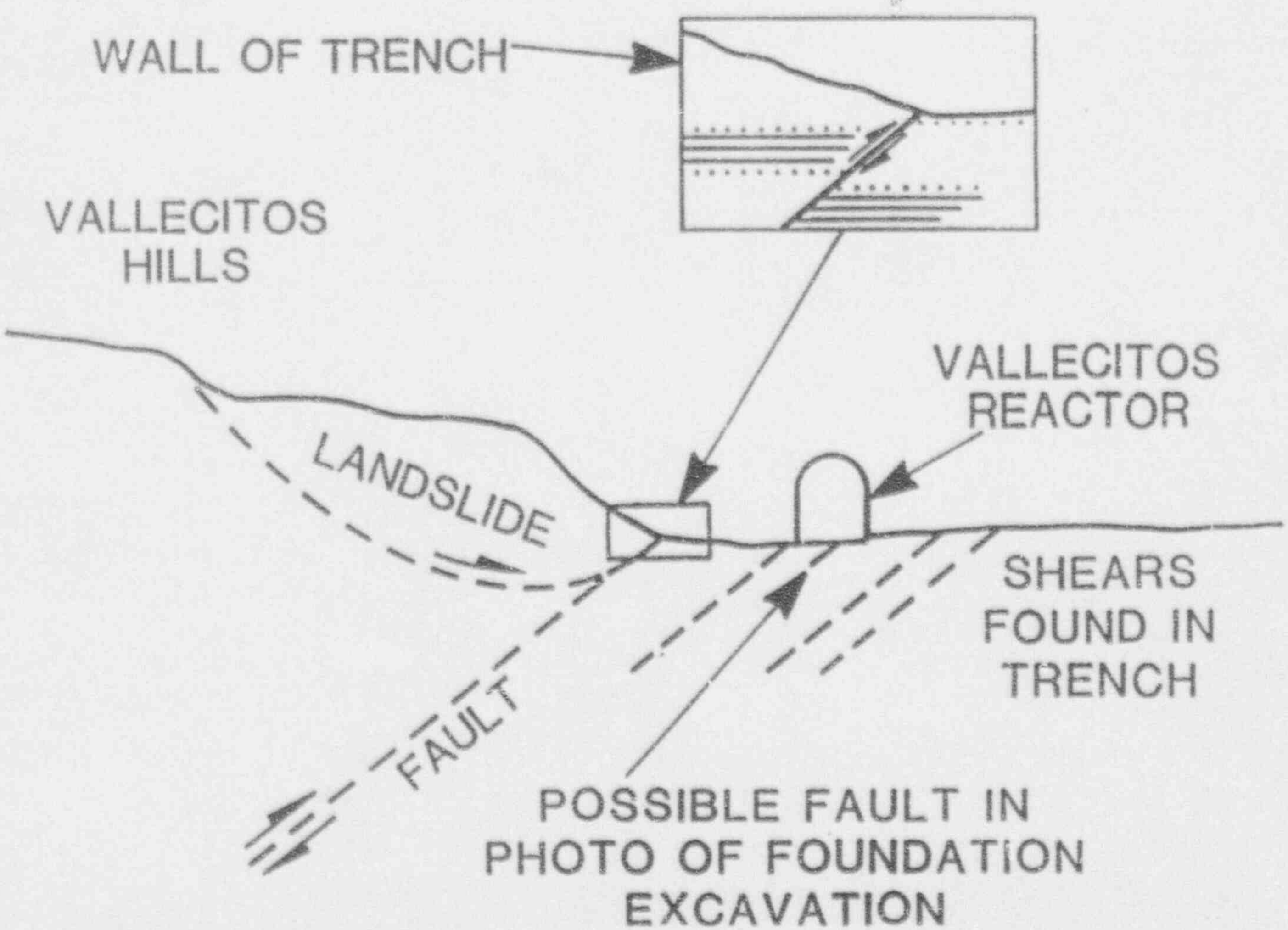


Figure 1. Fault and landslide interpretations at the Vallecitos Test Reactor, near Pleasanton, CA.

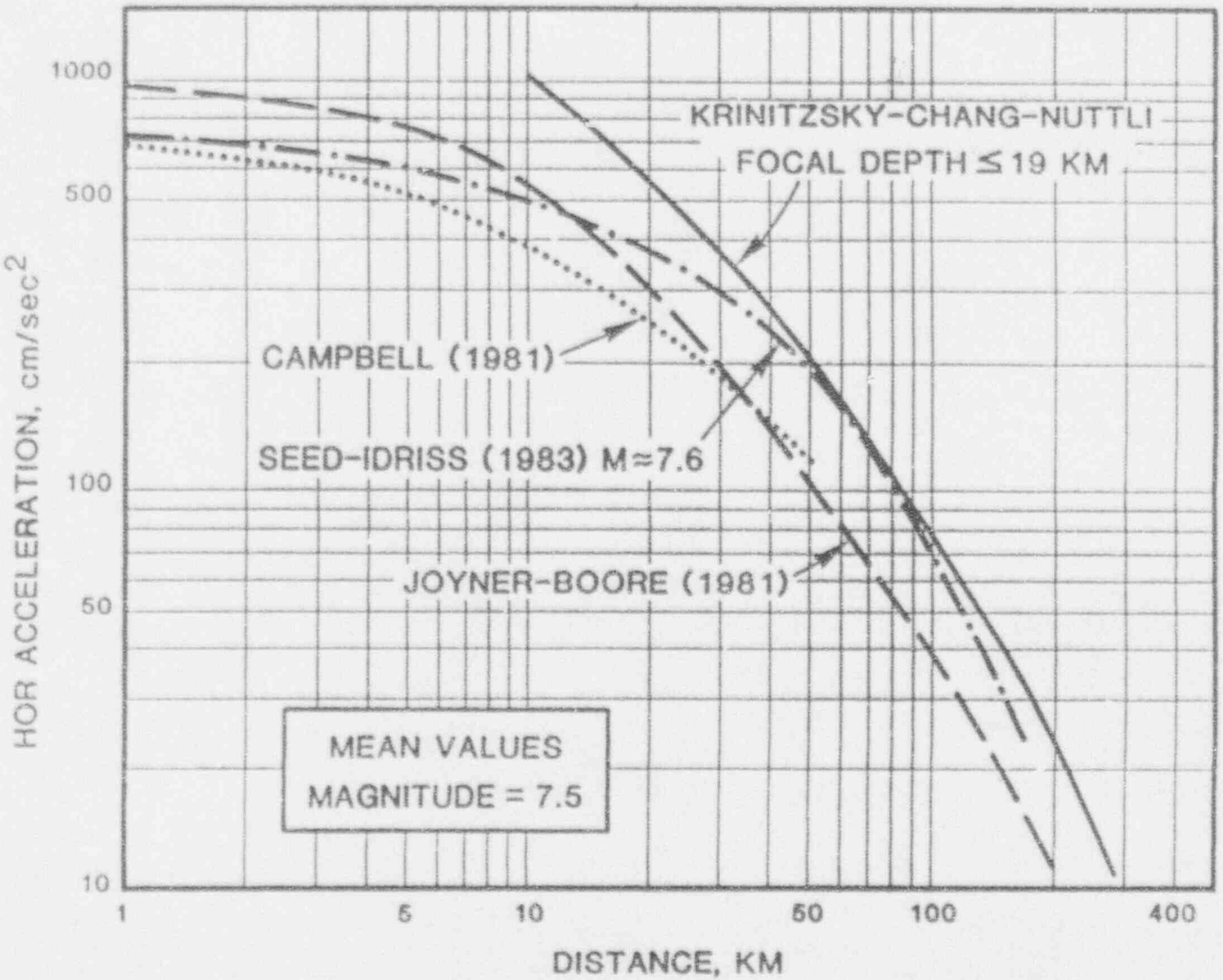


Figure 2. Comparison of curves by various authors for acceleration on rock by distance from earthquake source at M = 7.5.

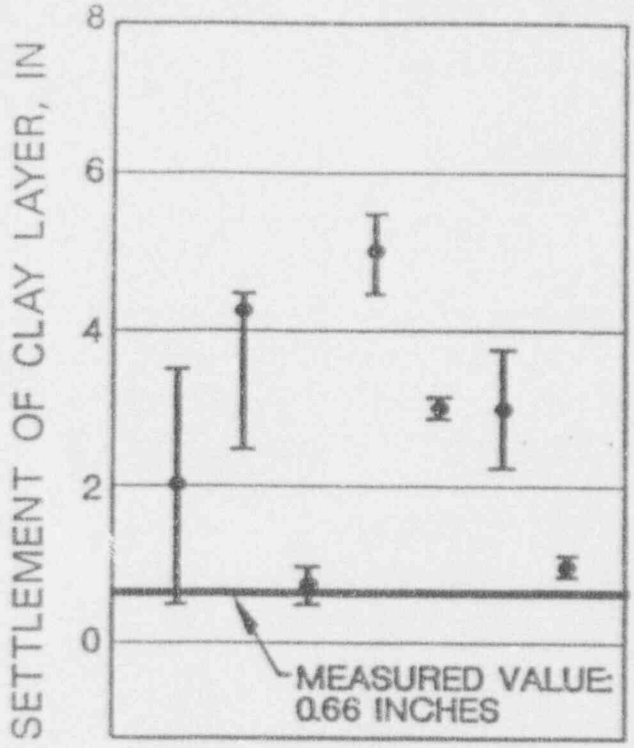


Figure 3. Expert opinions on settlement of a clay layer in an earth embankment: best estimate and maximum-minimum range, in inches. From Hynes and Vanmarcke (1975).

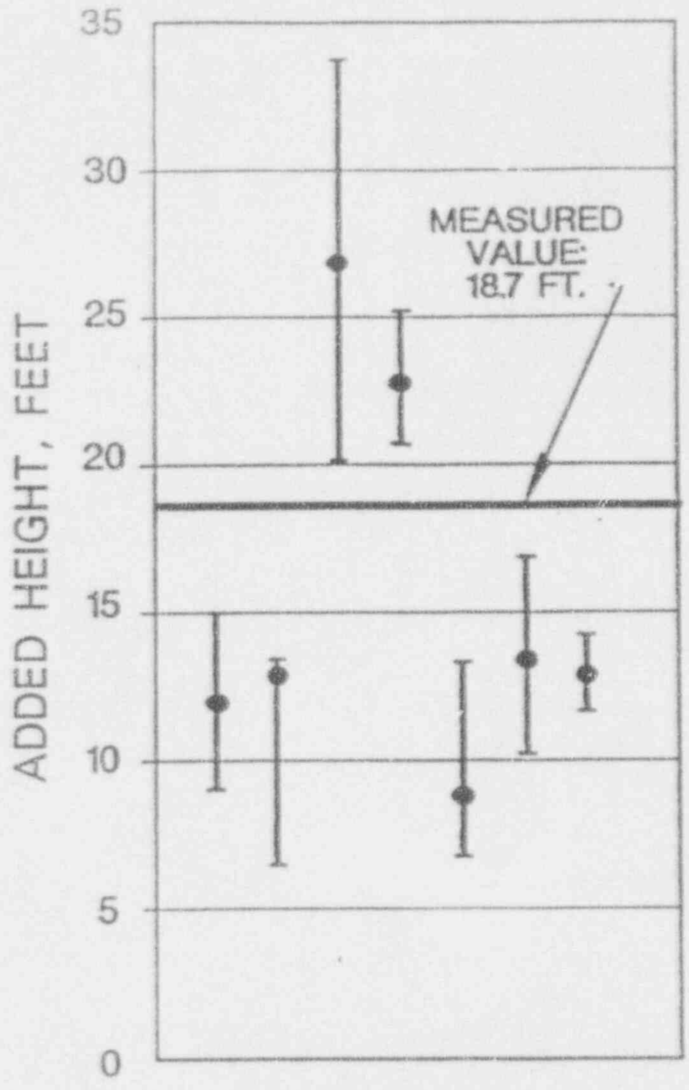


Figure 4. Expert opinions on added height to failure in an earth embankment: best estimate and maximum-minimum range in feet. From Hynes and Vanmarcke (1975).

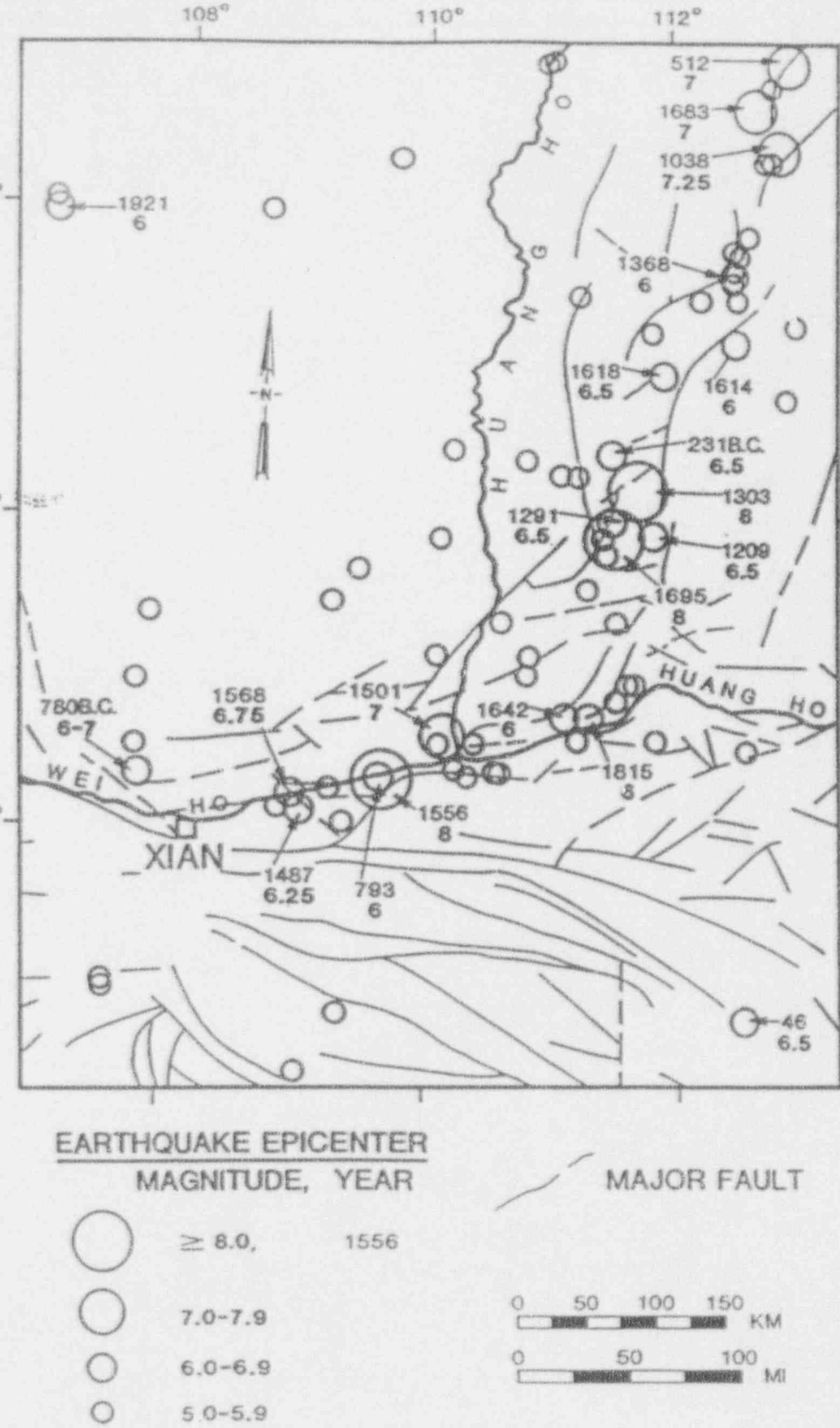
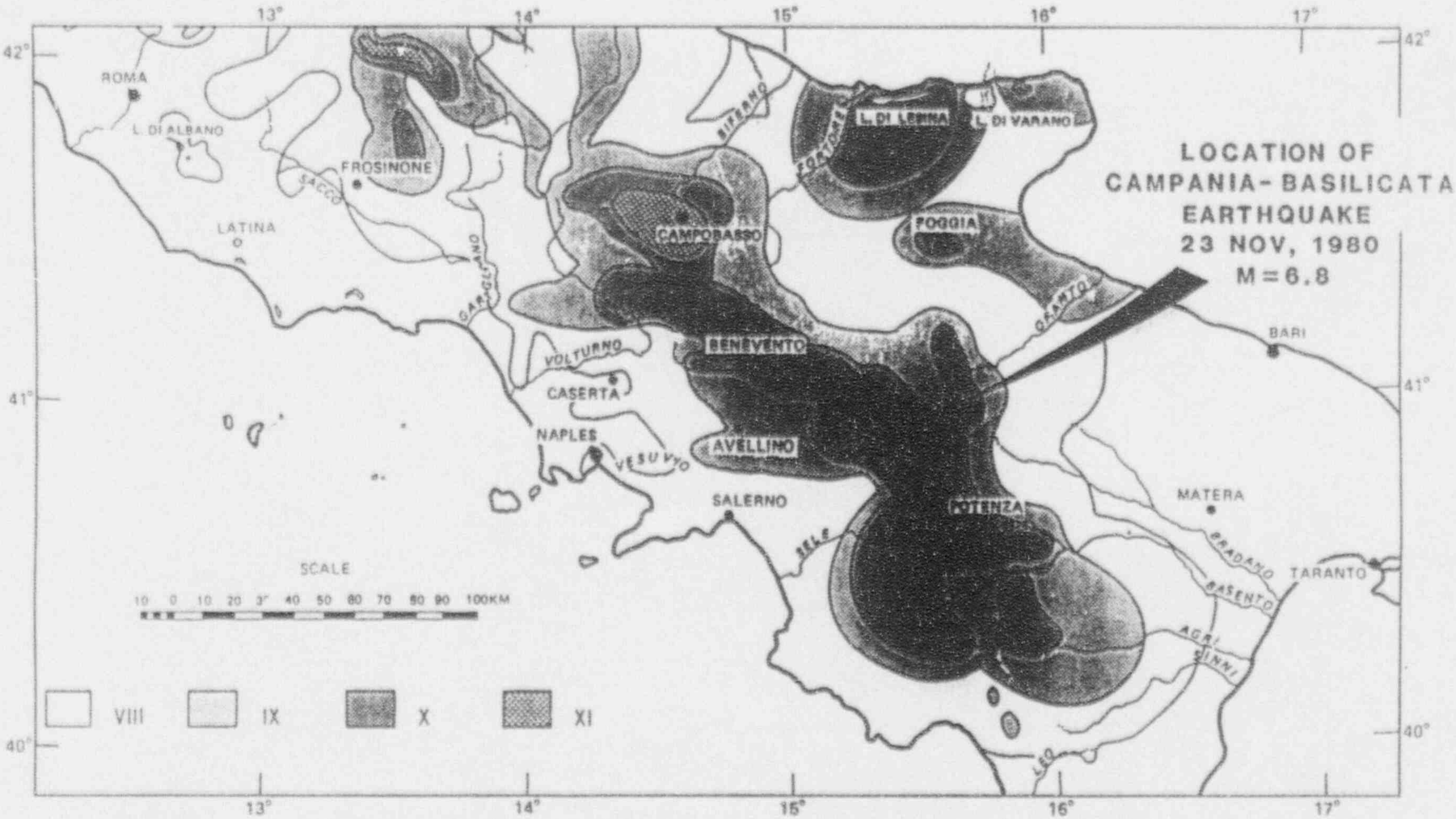


Figure 5. Major earthquakes near Xian, People's Republic of China, where the historic record is about 3500 years. Note concentration of large earthquakes, M = 7 and 8, to a relatively narrow zone. From State Seismological Bureau (1979).



CARTA SISMICA D'ITALIA
1500 - 1972

Figure 6. Detail of Seismic Zone Map of Italy by Iaccarino (1973A) based on seismic history from 1500 to 1972. Note that the 1980 earthquake occurred in a greatly restricted zone that was previously interpreted to have a potential MCS Intensity XI.

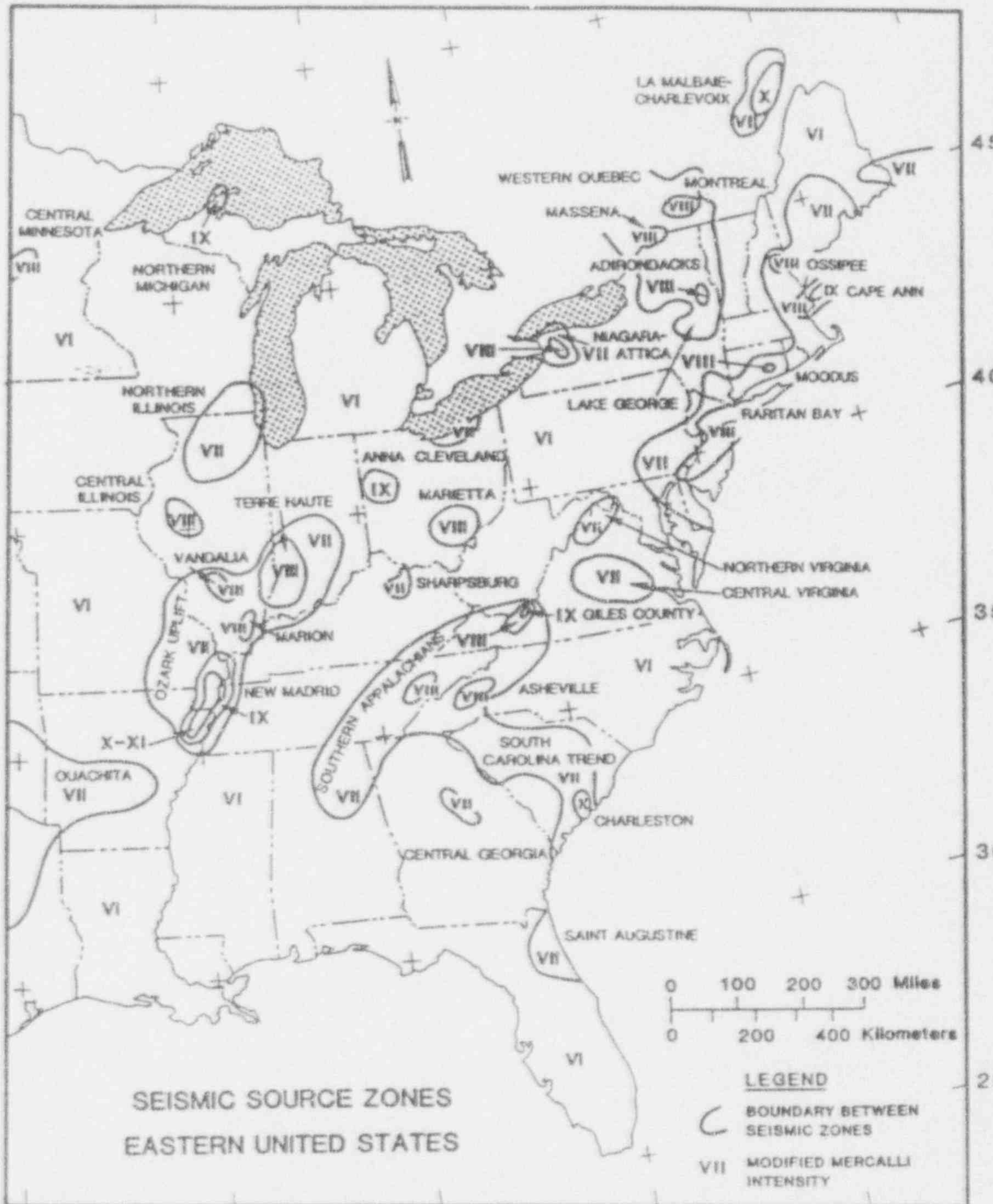


Figure 7. Zones of seismic source areas in eastern United States. From Krinitzsky and others (In Press).

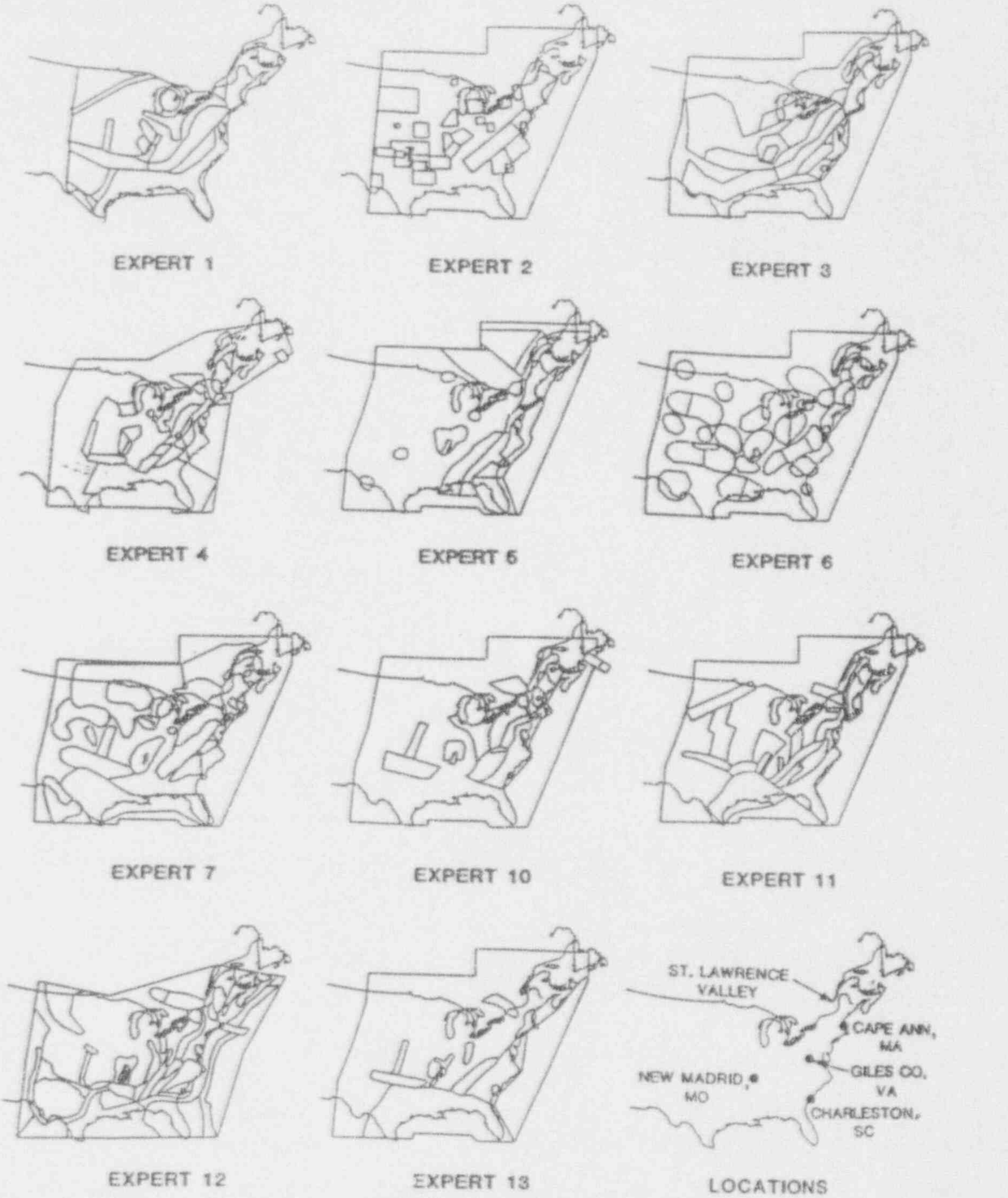


Figure 8. Seismic source zones in eastern United States by 11 experts. From Bernreuter and others (1989).

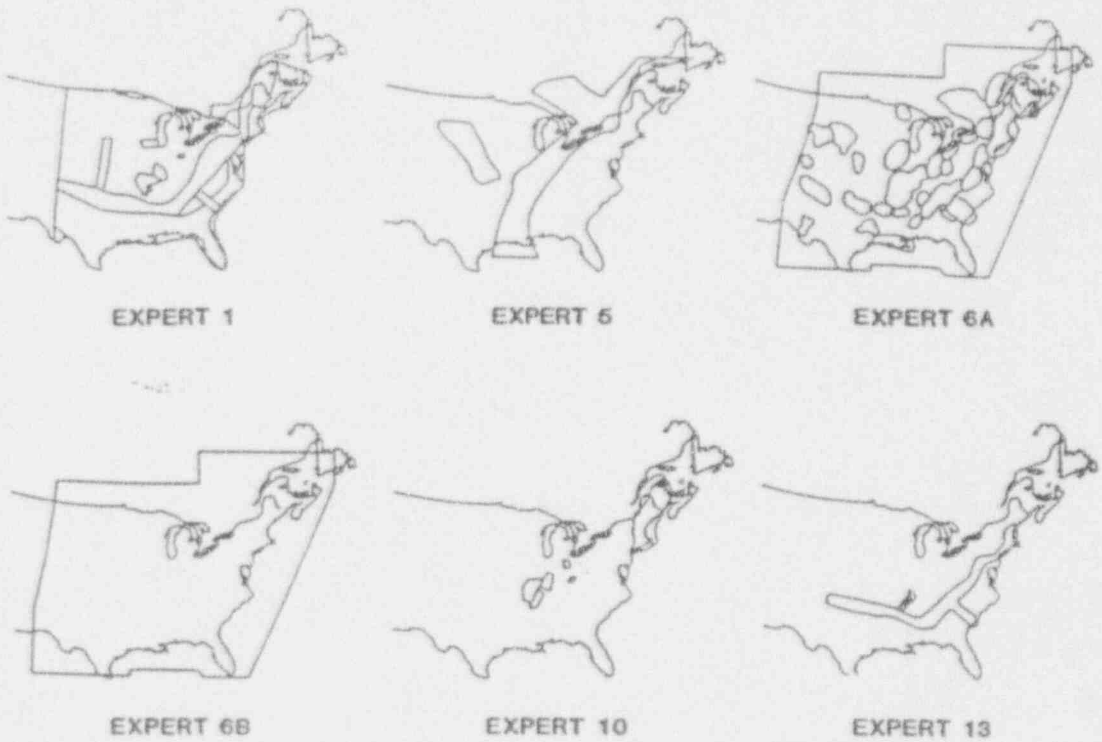


Figure 9. Six alternative seismic source zones in eastern United States by five experts. From Bernreuter and others (1989).

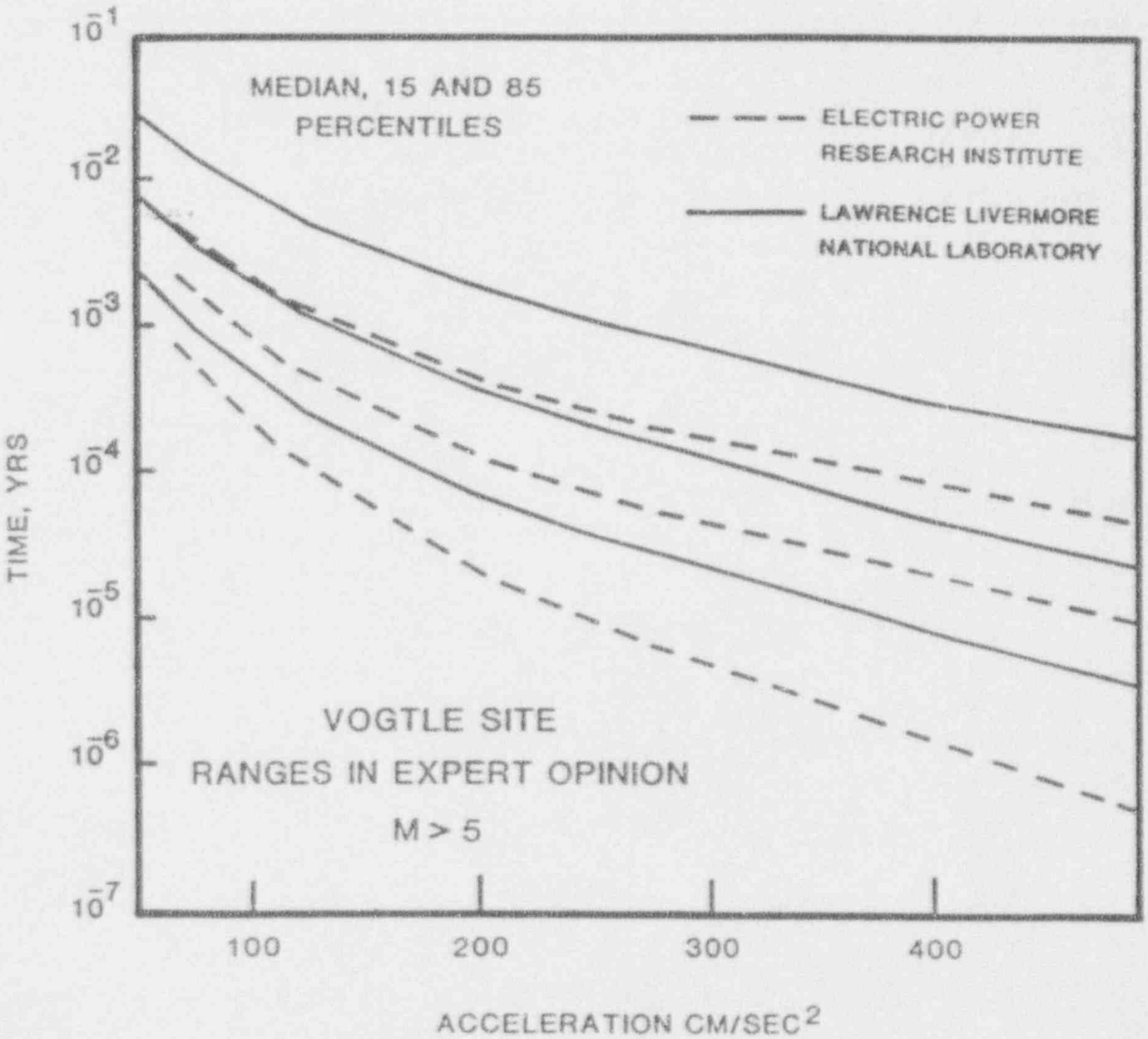


Figure 10. Ranges of calculated acceleration-through-time curves generated by Lawrence Livermore National Laboratory and Electric Power Research Institute for the Vogtle Nuclear Power Plant Site, Georgia. From Bernreuter and others (1987).

Vogtle

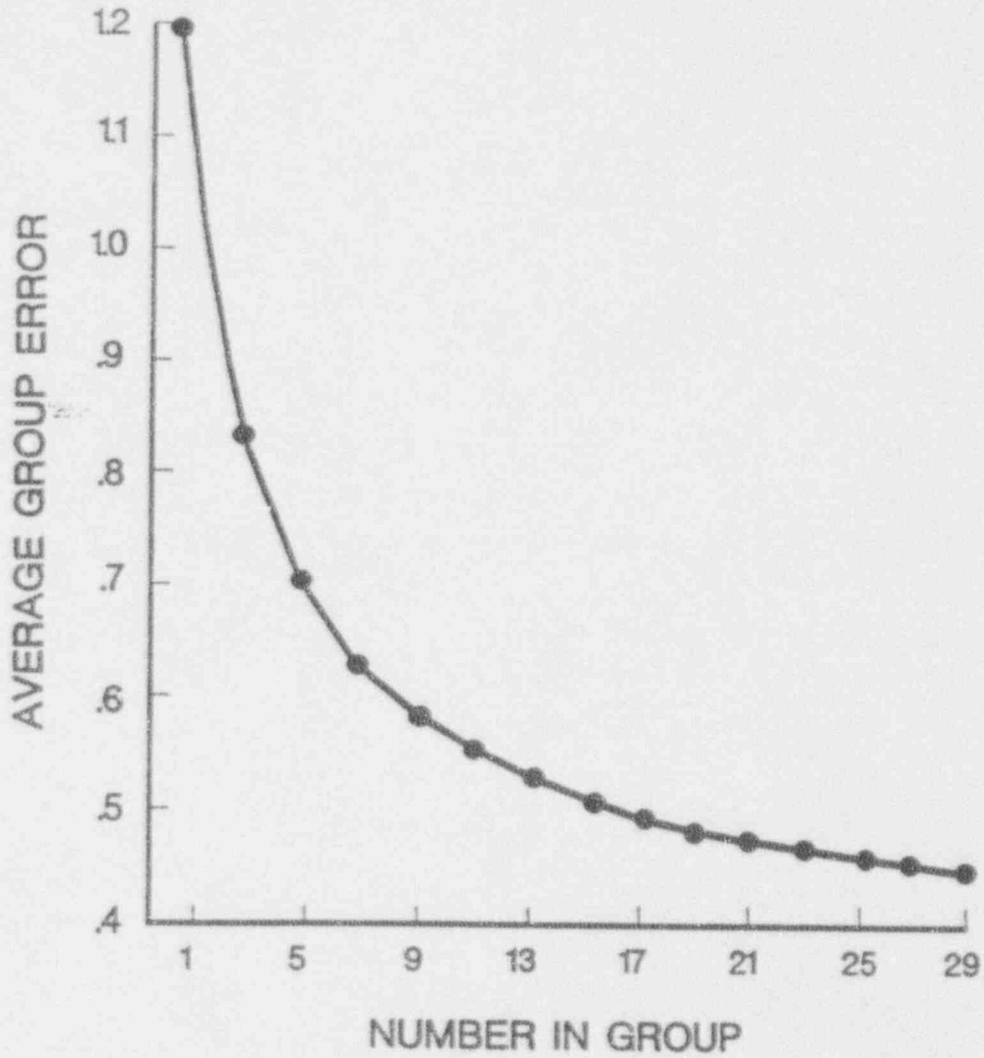


Figure 11. Relation of group size to group error in the Delphi study. From Dalkey (1969).

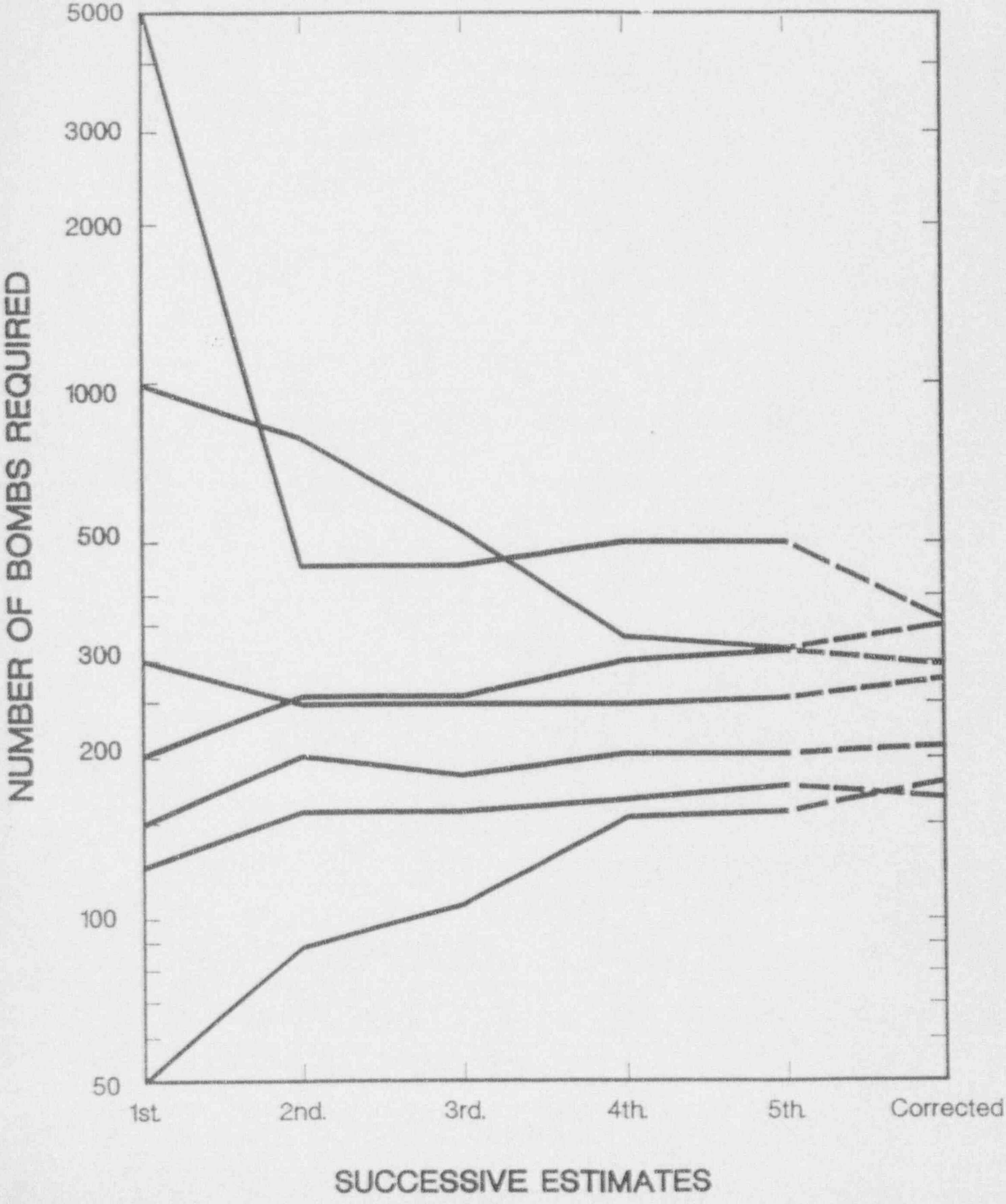


Figure 12. Elicitation with feedback and convergence of opinions by experts.

...ministic Source Earthquakes, and ...nd Motion." The draft guide ...vides general guidance and ...commendations, describes acceptable procedures and provides a list of references that present acceptable methodologies to identify and characterize capable tectonic sources and seismogenic sources.

2. DG–1016, Second Proposed Revision 2 to Regulatory Guide 1.12, "Nuclear Power Plant Instrumentation for Earthquakes." The draft guide describes seismic instrumentation type and location, operability, characteristics, installation, actuation, and maintenance that are acceptable to the NRC staff.

3. DG–1017, "Pre-Earthquake Planning and Immediate Nuclear Power Plant Operator Post-Earthquake Actions." The draft guide provides guidelines that are acceptable to the NRC staff for a timely evaluation of the recorded seismic instrumentation data and to determine whether or not plant shutdown is required.

4. DG–1018, "Restart of a Nuclear Power Plant Shut Down by a Seismic Event." The draft guide provides guidelines that are acceptable to the NRC staff for performing inspections and tests of nuclear power plant equipment and structures prior to restart of a plant that has been shut down because of a seismic event.

5. Draft Standard Review Plan Section 2.5.2, Proposed Revision 3 "Vibratory Ground Motion." The draft describes procedures to assess the ground motion potential of seismic sources at the site and to assess the adequacy of the SSE.

6. Draft Regulatory Guide 4.7, Revision 2, dated December 1991, "General Site Suitability Criteria for Nuclear Power Plants." This guide discusses the major site characteristics related to public health and safety and environmental issues that the NRC staff considers in determining the suitability of sites.

VIII. Future Regulatory Action

Several existing regulatory guides will be revised to incorporate editorial changes or maintain the existing design or analysis philosophy. These guides will be issued to coincide with the publication of the final regulations that would implement this proposed action.

The following regulatory guides will be revised to incorporate editorial changes, for example to reference new paragraphs in appendix B to part 100 or appendix S to part 50. No technical changes will be made in these regulatory guides.

1. 1.57, "Design Limits and Loading Combinations for Metal Primary Reactor Containment System Components."
2. 1.59, "Design Basis Floods for Nuclear Power Plants."
3. 1.60, "Design Response Spectra for Seismic Design of Nuclear Power Plants."
4. 1.83, "Inservice Inspection of Pressurized Water Reactor Steam Generator Tubes."
5. 1.92, "Combining Modal Responses and Spatial Components in Seismic Response Analysis."
6. 1.102, "Flood Protection for Nuclear Power Plants."
7. 1.121, "Bases for Plugging Degraded PWR Steam Generator Tubes."
8. 1.122, "Development of Floor Design Response Spectra for Seismic Design of Floor-Supported Equipment or Components."

The following regulatory guides will be revised to update the design or analysis philosophy, for example, to change OBE to a fraction of the SSE:

1. 1.27, "Ultimate Heat Sink for Nuclear Power Plants."
2. 1.100, "Seismic Qualification of Electric and Mechanical Equipment for Nuclear Power Plants."
3. 1.124, "Ser...ce Limits and Loading Combinations for Class 1 Linear-Type Component Supports."
4. 1.130, "Service Limits and Loading Combinations for Class 1 Plate-and-Shell-Type Component Supports."
5. 1.132, "Site Investigations for Foundations of Nuclear Power Plants."
6. 1.138, "Laboratory Investigations of Soils for Engineering Analysis and Design of Nuclear Power Plants."
7. 1.142, "Safety-Related Concrete Structures for Nuclear Power Plants (Other than Reactor Vessels and Containments)."
8. 1.143, "Design Guidance for Radioactive Waste Management Systems, Structures, and Components Installed in Light-Water-Cooled Nuclear Power Plants."

Minor and conforming changes to other Regulatory Guides and standard review plan sections as a result of proposed changes in the nonseismic criteria are also planned. If substantive changes are made during the revisions, the applicable guides will be issued for public comment as draft guides.

IX. Referenced Documents

An interested person may examine or obtain copies for the documents referenced in this proposed rule as set out below.

Copies of NUREG–0625, NUREG–1150, and NUREG/CR–2239 may be purchased from the Superintendent of Documents, U.S. Government Printing Office, P.O. Box 37802, Washington, DC 20013–7082. Copies are also available from the National Technical Information Service, 5285 Port Royal Road, Springfield, VA 22161. A copy is also available for inspection and copying for a fee in the NRC Public Document Room, 2120 L Street, NW. (Lower Level), Washington, DC.

Copies of issued regulatory guides may be purchased from the Government Printing Office (GPO) at the current GPO price. Information on current GPO prices may be obtained by contacting the Superintendent of Documents, U.S. Government Printing Office, P.O. Box 37082, Washington, DC 20013–2171. Issued guides may also be purchased from the National Technical Information Service on a standing order basis. Details on this service may be obtained by writing NTIS, 5826 Port Royal Road, Springfield, VA 22161.

SECY 79–300, SECY 90–016, and WASH–1400 are available for inspection and copying for a fee at the Commission's Public Document Room, 2120 L Street, NW. (Lower Level), Washington, DC.

X. Submission of Comments in Electronic Format

The comment process will be improved if each comment is identified with the document title, section heading, and paragraph number addressed. Commenters are encouraged to submit, in addition to the original paper copy, a copy of the letter in electronic format on 5.25 or 3.5 inch computer diskette; IBM PC/DOC or MS/DOS format. Data files should be provided in one of the following formats: WordPerfect, IBM Document Content Architecture/Revisable-Form-Text (DCA/RFT), or unformatted ASCII code. The format and version should be identified on the diskette's external label.

XI. Questions

In addition to soliciting comments on all aspects of this rulemaking, the Commission specifically requests comments on the following questions.

A. Reactor Siting Criteria (Nonseismic)

1. Should the Commission grandfather existing reactor sites having an exclusion area distance less than 0.4 miles (640 meters) for the possible placement of additional units, if those sites are found suitable from safety consideration?

2. Should the exclusion area distance be smaller than 0.4 mile (640 meters) for plants having reactor power levels significantly less than 3800 Megawatts (thermal) and should the exclusion area