Rec'd w/ltr dtd 8/1/92

EARTHQUAKE PROBABILITY IN ENGINEERING

The Third Richard H. Jahns Distinguished Lecture

in Engineering Geology

by

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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 who were engaged were leaders in providing such information. They 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. The experts were not asked to make independent studies. The experts were to give their opinions based on the existing information and whatever else they had. 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 were also asked to give interpreted slip rates and maximum credible earthquakes. Published fault maps were provided to the experts for the states of California, Nevada, and Arizona, plus a tectonic map of the United States. The experts were questioned for their opinions concerning individual faults. There was no field work. The experts 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 the opinions diverged for the corresponding maximum credible earthquakes by 0.5 to 0.75 of an earthquake magnitude unit. However, 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 immediately 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 with only maps to go by 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, they should have done airphoto studies, they should have walked the faults, dug trenches, studied the displacements, gotten 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. 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 geologist and he has to be a geologist who is experienced in dealing with the field evidences of earthquakes. A seismologist or a geophysicist has neither 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 to his credit. On the face of it, I would say at least a third of Eguchi's experts did not have the expertise suitable for giving 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 and their reasons are not given.

Another issue in the depth values is the estimate of 40 km. The 40 km does not accord 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.

Decisions involving the gathering and preparation of data on microearthquakes belong to the seismologists. However, the microearthquakes are clues for interpreting fault activity and the thickness of the brittle crust. These interpretations can be made about equally well by persons in any of the disciplines involved in this study.

Comment

The above expert opinions on faults show:

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

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

(3) one or more experts were incompetent.

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

The Vallecitos Dispute: 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 working for 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 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). I recommend it to you. Get it. Read it. Meehan is a born raconteur. He lures his reader. He trashes his opponents. He has an engaging sense of humor. He is a great polemicist -- in the tradition of Voltaire. His book is a classic. In it he chronicles his view of the battle that took place between his experts, who initially accepted a landslide which posed no hazard to the reactor, and those opposing experts who believed that an earthquake-generating fault existed at the reactor site.

Meehan depicts the USGS and the NRC as staffed by various unsavory characters who deserve to be done away with, along with those organizations. So, the argument became polarized and acrimonious. There is a side, that of the USGS and NRC, whose account is yet to be written. In Table 3, I have tried to lay out this dispute with its pros and cons.

ESA, meaning Meehan, fought heroically against any change in the landslide interpretation. Even when ESA found thrust movements in the trenches that they dug across the extension of Herd's Verona fault, and when ESA found what they called "thrust-like splays" from the Verona fault adjacent to the reactor, Meehan did not alter his views. ESA argued that the splays were not a "major structure," but they did not define what major was. Eventually, Meehan 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 happening. 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. Meehan continued to support the probabilistic interpretation.

Finally,the whole dispute was made moot by Meehan himself. He came into the hearing room and 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 damaging it significantly. 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 dried up.

A nagging question, heard many times, is 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 has been generated so far.

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, 18 returns 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 earthquakes, 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, three experts gave values of 0.03, 0.04, and 0.05 G respectively, for this site. The reason is easy to find. 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 be incompetent; it is not a rare quality. The troubling question is how many other expert opinions in the above tables are of this level?

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 6.6. There are as yet no motions to be had for large 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.

The question to ask at this point is what is 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 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, In Press) 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 epicentral distances. Thus, to compare these curves is like comparing oranges and apples. 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 that comes from a lessened conservatism in the 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, what the pros and cons are in every case, what his engineering analysis requires, and finally what he wants or will accept. He will have to know as much as the experts -- or more. And it is not difficult. He can do it. 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 proceed to assign earthquake ground motions. Experts should then be engaged for peer reviews. The engineer or earth scientist needs to pay close attention to the opinions of reviewers. He needs to judge the opinions carefully, use what is good, and have the knowledge and character to throw out what is bad.

Experts on Engineering Judgments for an Earth Dam: 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. These 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, readings from field instrumentation of the embankment for six years, etc. 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. The experts were not told the values that were observed to have occurred 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, compare these results with the estimates for settlement of a clay layer in the embankment as seen 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 0.66 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 expressed fairly, the uncertainty range should contain the actual value. In this study it did so for only two estimates seen in Figure 3 and none in Figure 4. Thus, a combining of the probability estimates into a single probability value can be seen to 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. Their assumption was not borne out by their two exercises since the results contradicted their 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 model was used in addition to a correct one, statistical manipulation is not a reliable way to adjust away the erroneous value. If the correct model 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, there was 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.

(4) Where differences in modelling are discounted, there is reasonable agreement between LLNL and EPRI.

Since the LLNL and EPRI studies are so basically similar, I will examine in detail only the LLNL study.

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.

There is no way that all of these theories can apply everywhere since the extentional and the compressional postulations contradict each other. Also, 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 on the face of it, must be handled with care because it can mean that large earthquakes will happen almost everywhere and that is not what we observe 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 mapped.

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 are products of past tectonism. The seismic zone is best defined by what we know of its earthquakes.

The United States has the disadvantage of a short seismic history. It is as short as 100 years in parts of the Prairies and is only about 350 years at its longest 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 resembles New Madrid. The region around Xian is the scene of infrequently occurring major earthquakes in an intraplate. 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, much as is the case for the New Madrid events that occurred in the alluvial valley of the Mississippi River. 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: 1556, 1303, 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 stable 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 structure, such as a fault zone or a pluton, and activity should be limited to that structural association. Such a source is a seismic hotspot. 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 may provide structural information to corroborate the boundaries of hotspots.

(2) One earthquake can adjust a boundary to 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 (In Press), 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, 1987 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, 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 infrequent 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 the stability of the source area.

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.)

Expert 5. Seismicity is the only source of information.

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

(2) What are the principal bases for forming the zones?

Expert 1. The Gutenberg-Richter b-line (the b-line is described in Part II 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) What features influenced the zones?

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) How were the above features used for zones?

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

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

(5) Do the zones represent your state of knowledge adequately?

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

(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. The purpose of eliciting was 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) What sort of data is available and adequate for zoning?

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

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 a funny farm. 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.

So LLNL 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 saw could be 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 meretricious 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 ill-informed project managers and incompetent experts.

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 II 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. This process 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 uncertainty.

Uncertainty

In logic, there are in principle no external evaluations for subjective judgments. Nonetheless, in practice subject 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 and 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. Uncritical reliance on opinions flies in the face of good science and good engineering.

Not least is another problem which is the value of the opinions. All of the previously discussed studies by Okrent, Eguchi, Krinitzsky, Hynes, 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, no shmucks? Yet, these and all sorts of other unsavory 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 guides on quality of experts and 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, only seven percent off, while an extreme line was 54 percent off. Combining a large number of best guesses was obviously safer than depending on any one of them.

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 <u>x</u>s and <u>y</u>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 <u>Z</u> 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 pizza in the United States and the incidence of crime. But banning the sale of pizza would not eliminate crime. In the Hynes and Vanmarcke study, an aspect of this problem of meaning is seen visually in Figure 3. The expert opinions would have passed Fisher's null analysis for a bivariate distribution, yet the median value for the group was wrong. Statistical analysis alone may not tell us when a group is wrong.

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, resistance, logistics of supply, and goodness knows what else?

A sampling of other sorts of questions that the procedures were developed to elucidate were:

(1) Should the United States build a moon vehicle?

(2) Should the vehicle be tethered?

(3) Should power be applied to all four wheels?

(4) Should a camera be mounted or held?

(5) If mounted, should the camera be mounted above the driver?

(6) Should the vehicle be recovered?

These are questions that not only require no depth of analysis, but for which there are no essential answers. Some of the questions could have been answered by a kindergarten class with about as much validity as that of the experts or better yet they could have been answered by the same industrious drudges who ask these sorts of questions. However 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. I find it very difficult to accept that someone 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 and see the result taken as the very best that can be obtained for engineering design.

We noted that the LLNL Zonation and Seismicity Panel produced seismic source zones, seen in Figure 8, that reflected their opinions, but that mostly bore no relation to fundamental geologic and seismic evidence. The results of elicitation shown in Figure 9 produced zones that were greatly more disparate than those produced initially. Instead of the convergence that we saw in Figure 12, which was the expected benefit of elicitation, there was a greatly pronounced divergence. That divergence was a reflection of problems with the expert opinions. I think it should have caused the management experts to smell a rat.

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

No study has been made that

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

(2) allows him to gather and digest evidence,

(3) allows him to form a conclusion,

(4) has his work and his conclusion checked for mistakes and reviewed by other professionals,

(5) allows him to correct obvious errors and decide to accept or reject judgmental advice, and

(6) present his 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 conclusion against a conclusion averaged from the massaged, off-the-cuff opinions from a herd of experts.

A confrontation of this sort, done enough times to be statistically valid would tell us something about the usefulness of multiple expert opinions for deciding complex issues. But I don't think it needs to be done. All we need to do is consider again the experiences summarized in this review, those of Okrent, Eguchi, Krinitzsky, those experienced in the Vallecitos dispute, by Hynes, and LLNL. In no way do they discern any advantage in relying on multiple expert opinions. At their best, see Hynes and Vanmarcke, those opinions are shown to be treacherous, and there is no way to tell that they are treacherous without having the correct answer. At their 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 creative work along the lines of developing evidence in eastern United States. They gave their experts existing information, and they produced some additional seismic attenuation models for analyses. The work was mostly getting opinions and in the 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 opinion a miasmatic waste.

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 sources to the plants within 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 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 one hundred dollars and two million dollars. They are also between a method with defensible results and a method that, for many reasons enumerated here, should not be trusted.

Assumptions in the LLNL Method

It may seem from the wide-ranging acceptance of opinions that are the basis for the LLNL-EPRI studies that they were not constrained by prior assumptions. That would not be true. They used very binding assumptions. Following are several of the most critical. They are from Bernreuter and others (1989):

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

(2) 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.

(3) The occurrence of earthquakes is assumed to be independent between zones.

(4) The expected number of earthquakes of magnitude m or greater occurring within a zone can be described by the magnitude-recurrence relation.

I disagree with all of these assumptions. My reasons for disagreeing will be discussed in Part Two of this review.

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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 Vallecitas 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.
		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.
	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.	

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 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 based 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;	7.6	6.5	1.17
	Transmitter Hill	--	6.5	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

Table 7-A. Earthquake Ground Motions for Use in Pseudostatic Analyses

	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 anaylses do not apply. Use dynamic analyses.	1. Use 1/2 $(A_{max})_{BASE}$ 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% nonexceedance.	1. Peak hor motions (mean)* from (a) MM intensity (b) Mag-distance attenuation (c) Probability ~50-yr, 90% nonexceedance. 2. Use 1/2 $(A_{max})_{BASE}$ 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% 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 $(A_{max})_{BASE}$ 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% nonexceedance.	1. Peak hor motions (mean + S.D.)* from (a) MM intensity (b) Mag-distance attenuation (c) Probability ~250-yr, 90% nonexceedance. 2. Use 1/2 $(A_{max})_{BASE}$ 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% nonexceedance.
C. Underground cavity.	Use dynamic analyses.	1. Attenuate appropriate peak hor motions at ground surface to depth of cavity.		

* Adjust if necessary for site condition; shallow plate boundary, deep subduction zone, or intraplate area; near field or far field; effective motions when near an earthquake source.

Note: A_{max} is the peak value in a time history. It may be obtained as a parameter from the indicated curves or from the probabilistic interpretation.

Table 7-B. Earthquake Ground Motions for Use in Dynamic Analyses

	Foundation Liquefaction	Earth Embankments and Stability of Slopes	Earth Pressures	Concrete and/or Steel Frame Structure
Critical facility in an area of moderate to strong seismicity (Peak hor accel ≥ 0.15 G). 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 response 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.
Obtain Operating Basis Earthquake (OBE).	1. Peak hor motions (mean + S.D.)* 2. Peak motions from probability ~50-yr, 90% exceedance + S.D. 3. Generate time histories.	1. Peak hor motions (mean + S.D.)* 2. Peak motions from probability ~50-yr, 90% nonexceedance + S.D. 3. Generate time histories.	1. Peak hor motions (mean + S.D.)* 2. Peak motions from probability ~50-yr, 90% nonexceedance + S.D. 3. Generate time histories.	1. Peak hor motions (mean + S.D.)* 2. Peak motions from probability ~50-yr, 90% nonexceedance + S.D. 3. Generate time histories and/or obtain response spectra. 4. Check response at the natural frequency of the structure.
Underground cavity.	1. Attenuate appropriate peak hor motions at ground surface to depth of cavity. Underground accelerogram records may provide guidance for subsurface spectral content.			

* Obtain peak hor motions from (a) MM intensity or (b) magnitude-distance attenuation charts. Adjust for site condition; shallow plate boundary, deep subduction zone, or intraplate area; near field or far field; effective motions when near an earthquake source.

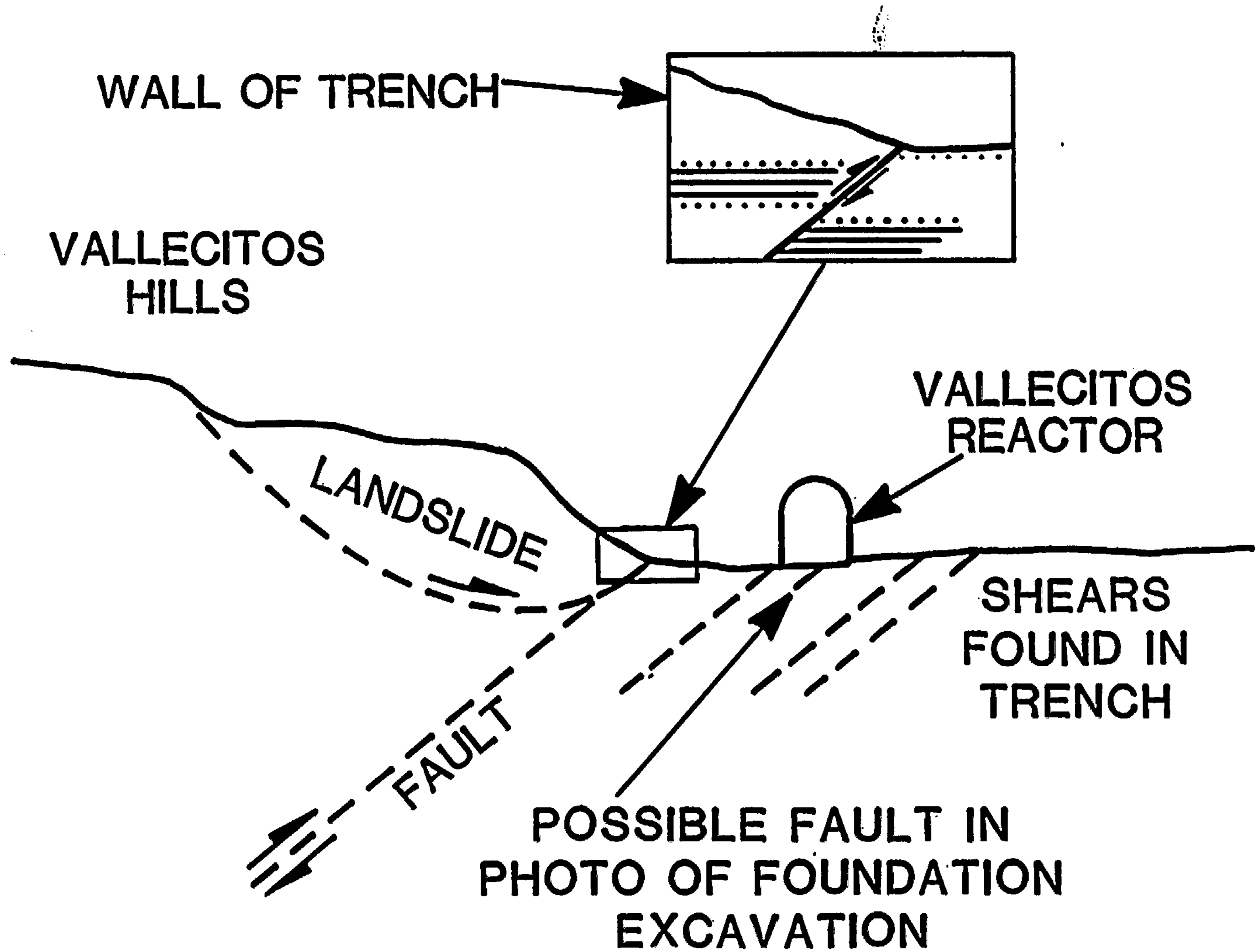


Figure 1. Fault and landslide interpretations at the Vallecitas Test Reactor, near Pleasanton, CA.

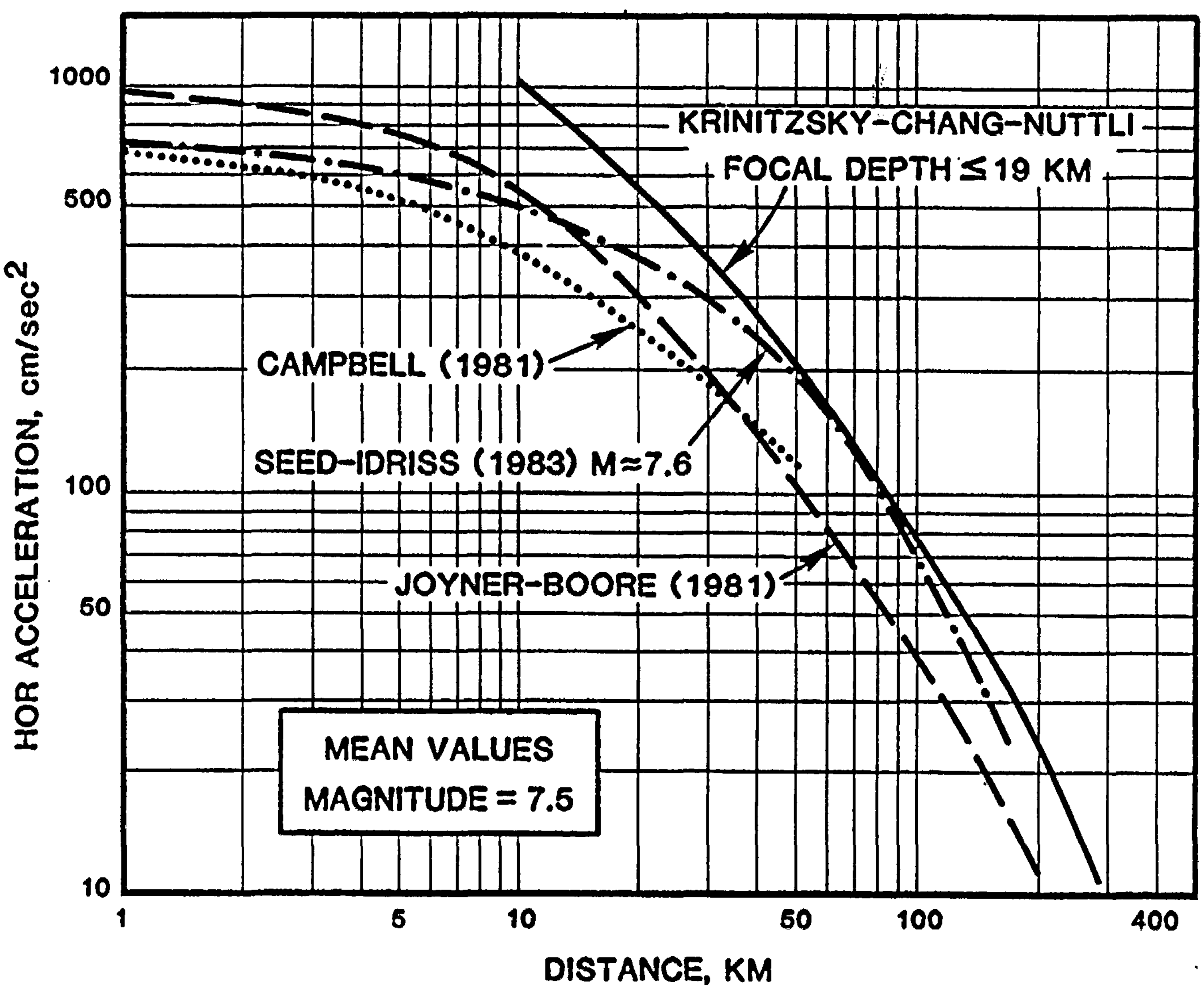


Figure 2. Comparison of curves by various authors for acceleration on rock by distance from earthquake source at M = 7.5.

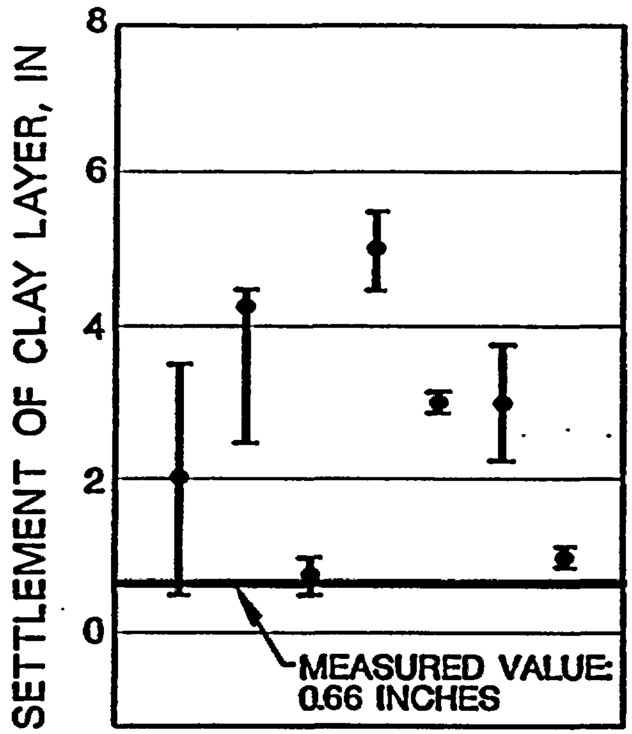


Figure 3. Expert opinions on settlement of a clay layer in an earth embankment: best estimate and maximum-minimum range, in inches. From Hynes and Vanmarcke (1975).

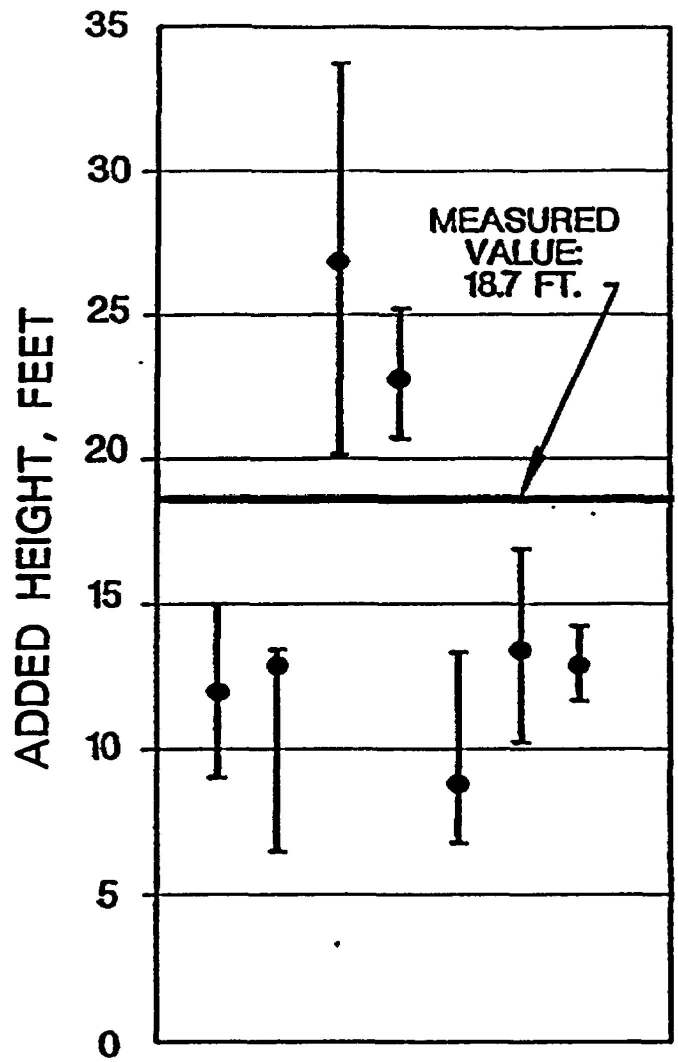


Figure 4. Expert opinions on added height to failure in an earth embankment: best estimate and maximum-minimum range in feet. From Hynes and Vanmarcke (1975).

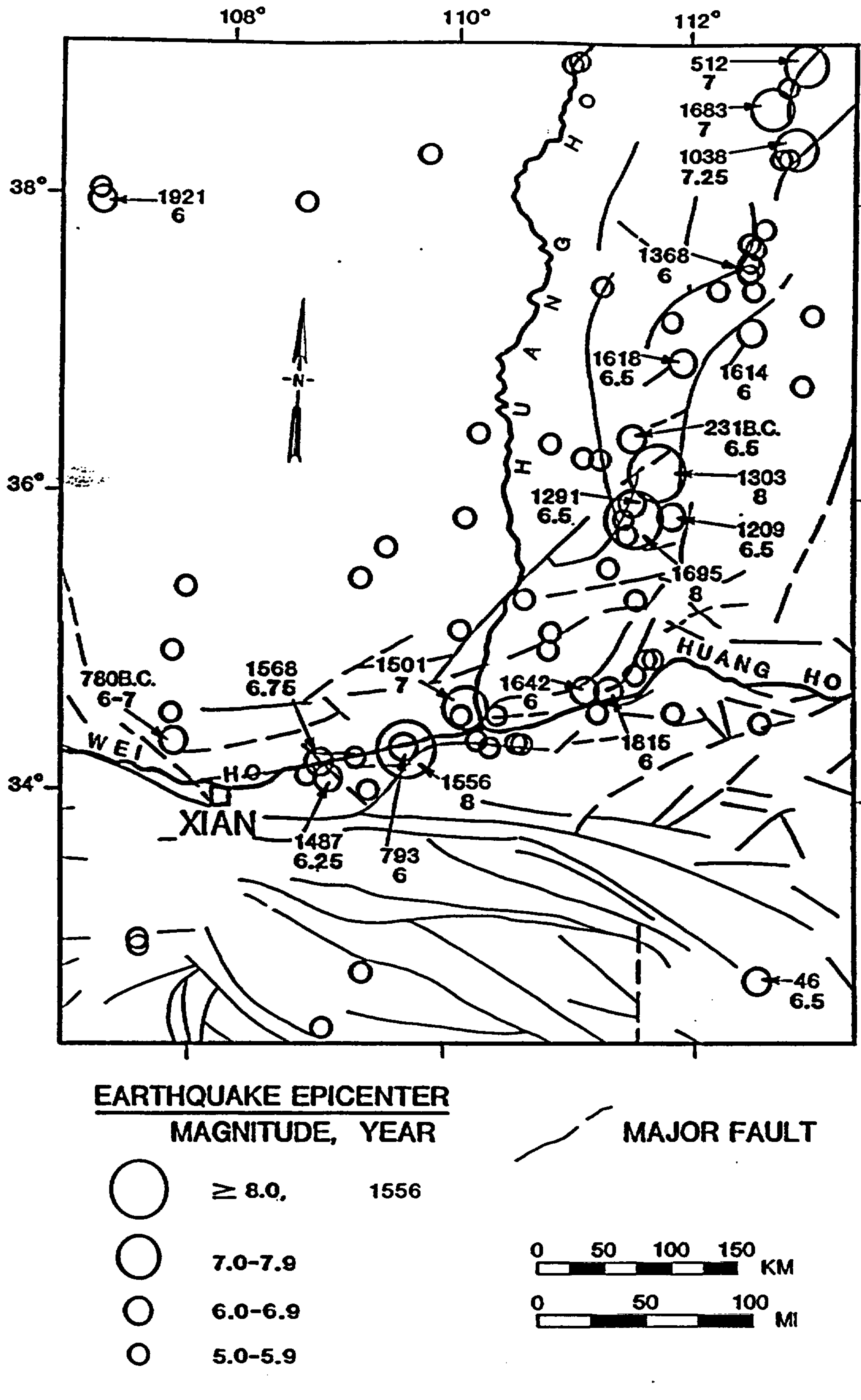
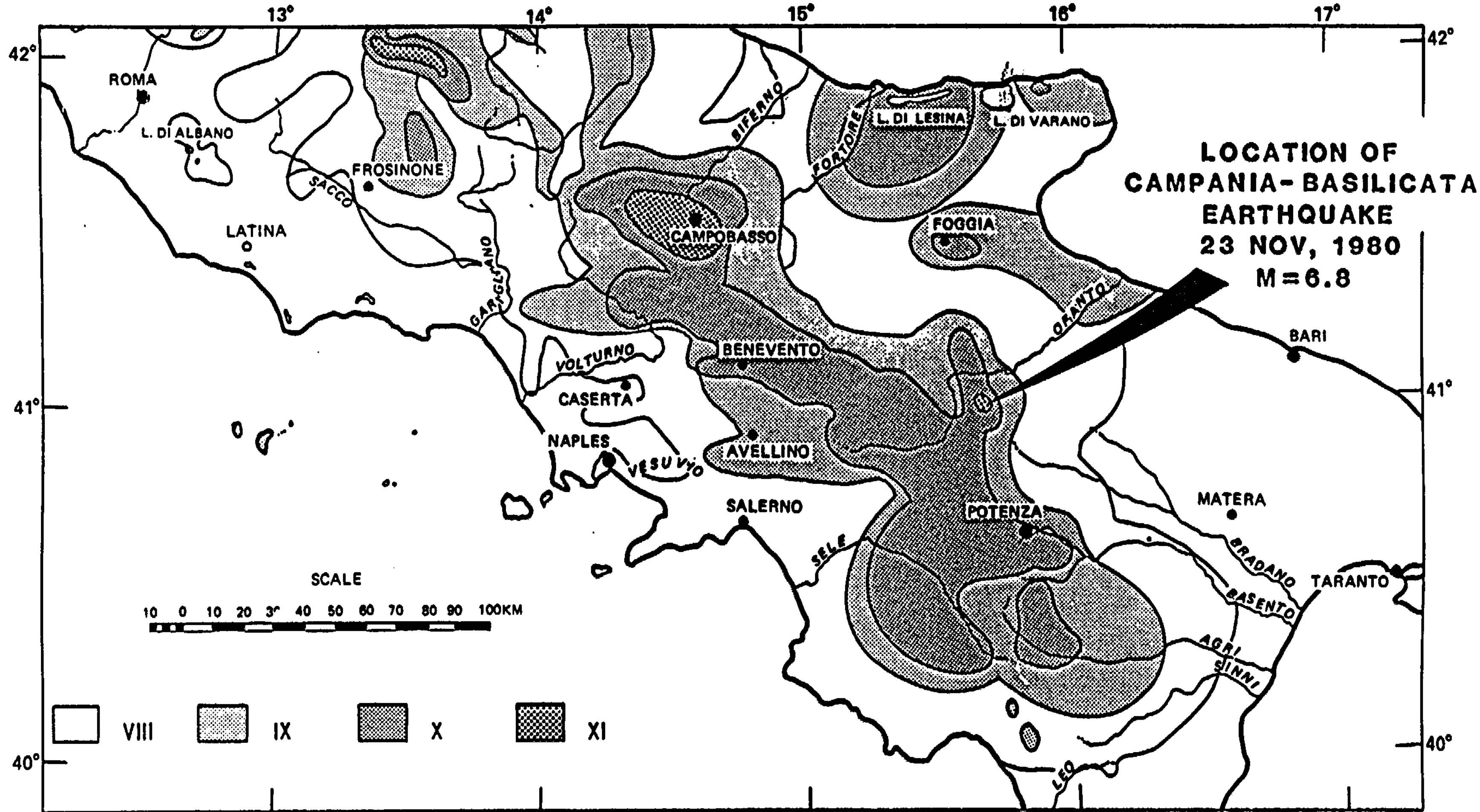


Figure 5. Major earthquakes near Xian, People's Republic of China, where the historic record is about 3500 years. Note concentration of large earthquakes, M = 7 and 8, to a relatively narrow zone. From State Seismological Bureau (1979).



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.

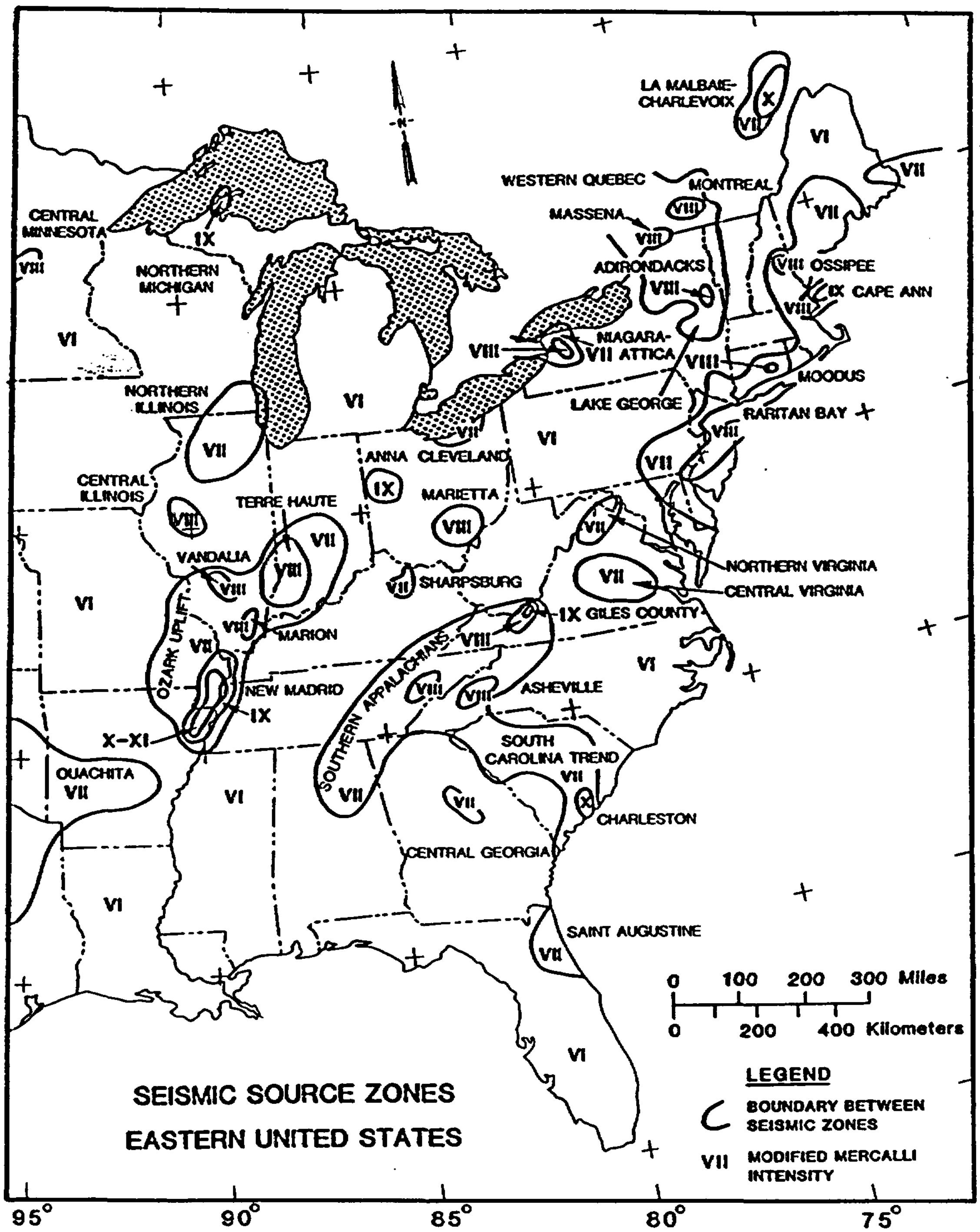


Figure 7. Zones of seismic source areas in eastern United States. From Krinitzsky and others (In Press).

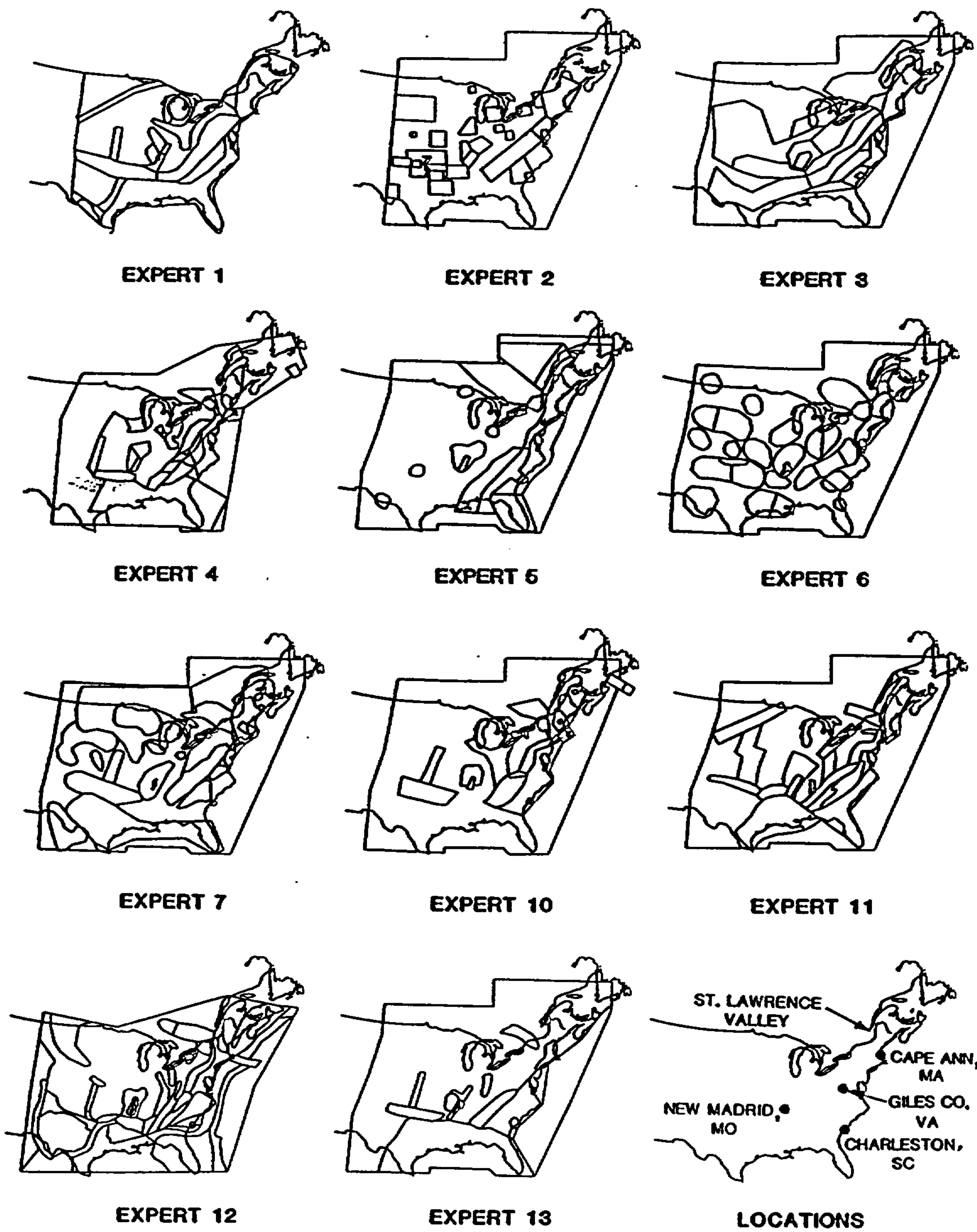


Figure 8. Seismic source zones in eastern United States by 11 experts. From Bernreuter and others (1989).

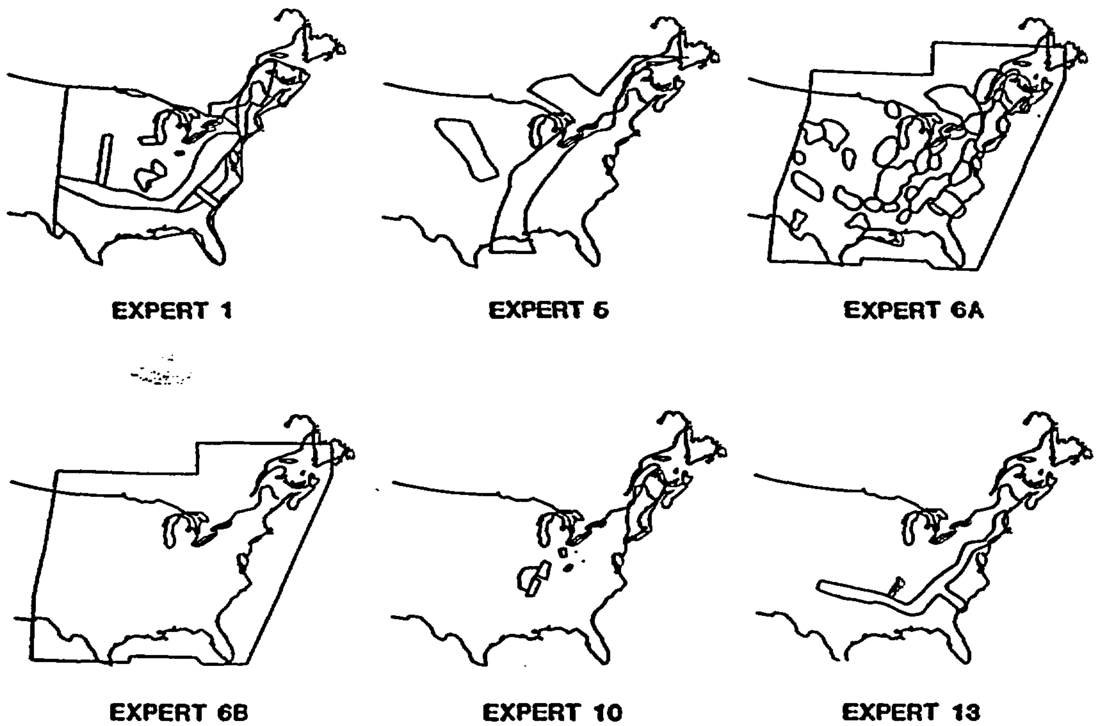


Figure 9. Six alternative seismic source zones in eastern United States by five experts. From Bernreuter and others (1989).

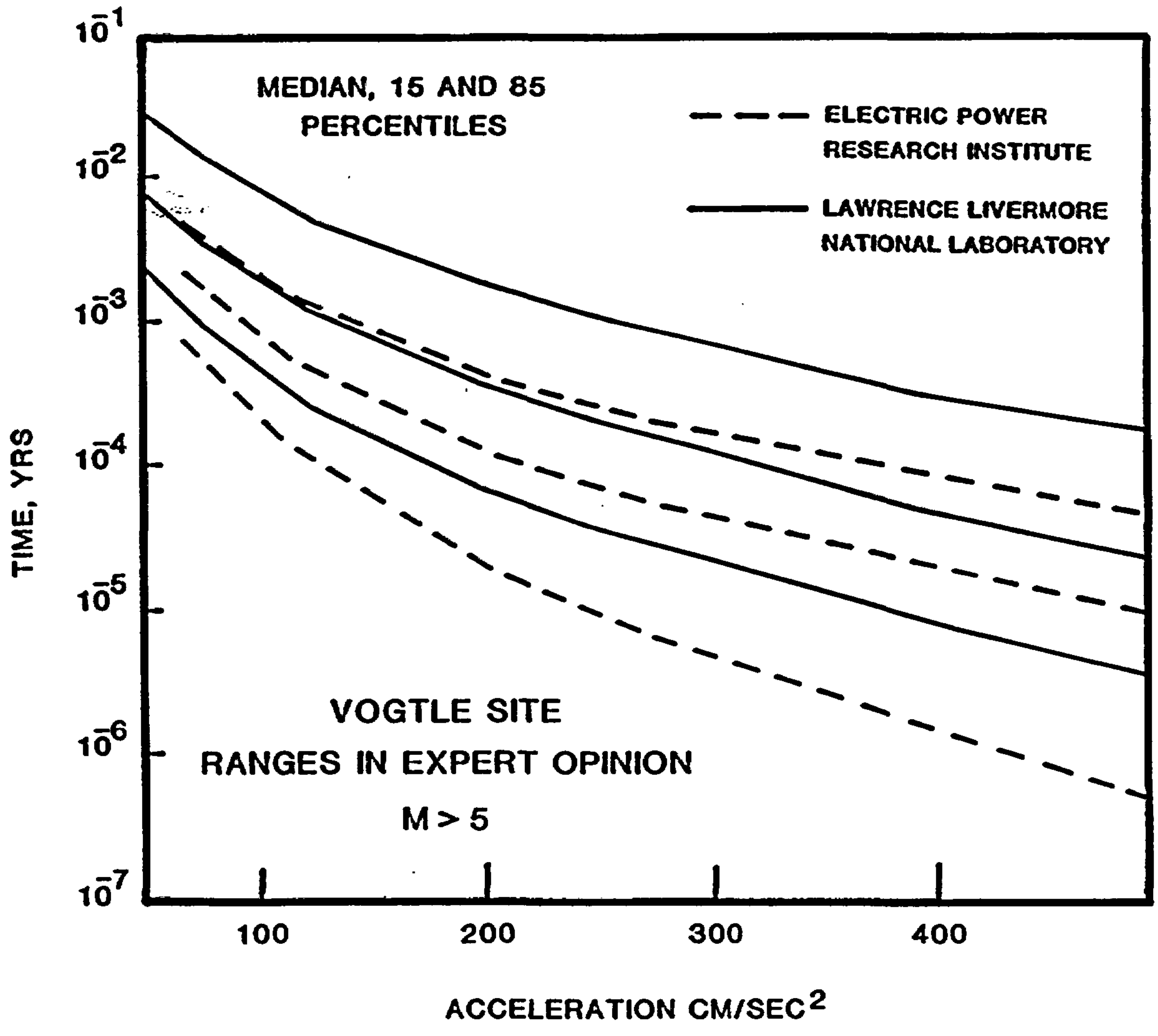


Figure 10. Ranges of calculated acceleration-through-time curves generated by Lawrence Livermore National Laboratory and Electric Power Research Institute for the Vobtle Nuclear Power Plant Site, Georgia. From Bernreuter and others (1987).

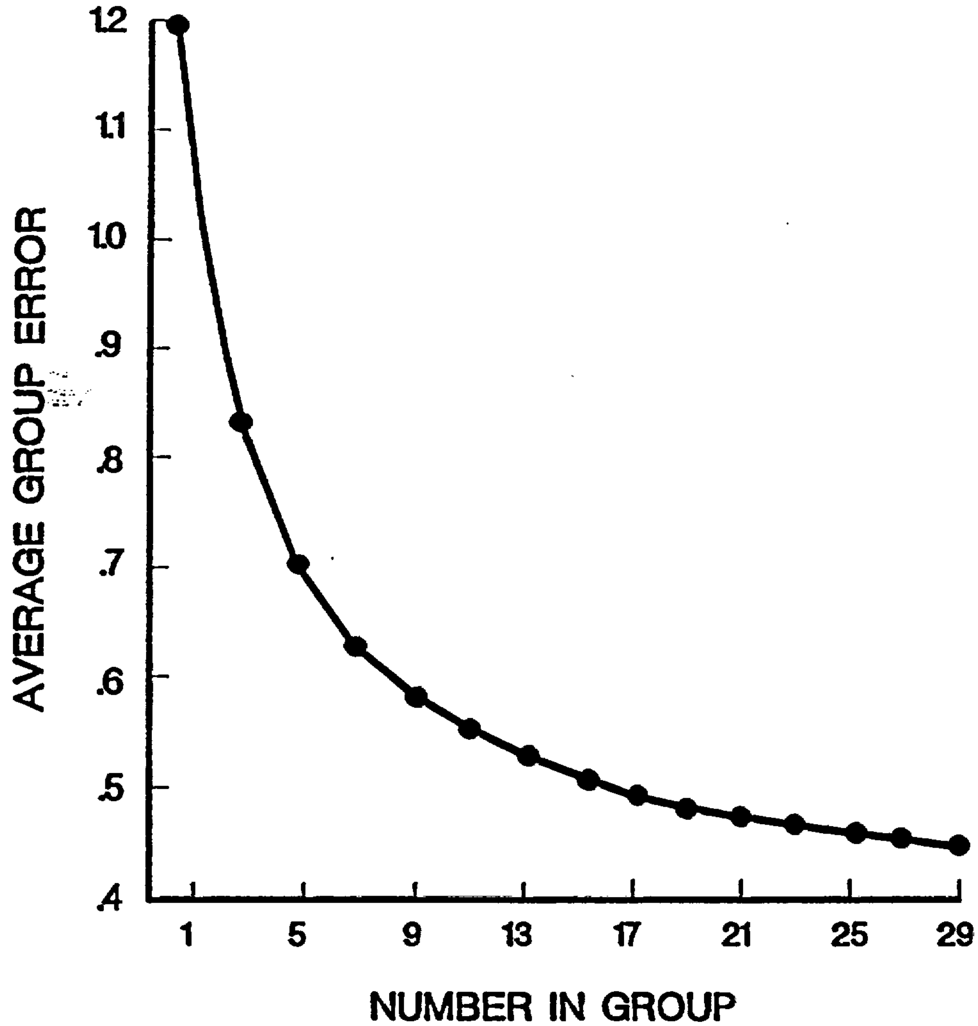


Figure 11. Relation of group size to group error in the Delphi study. From Dalkey (1969).

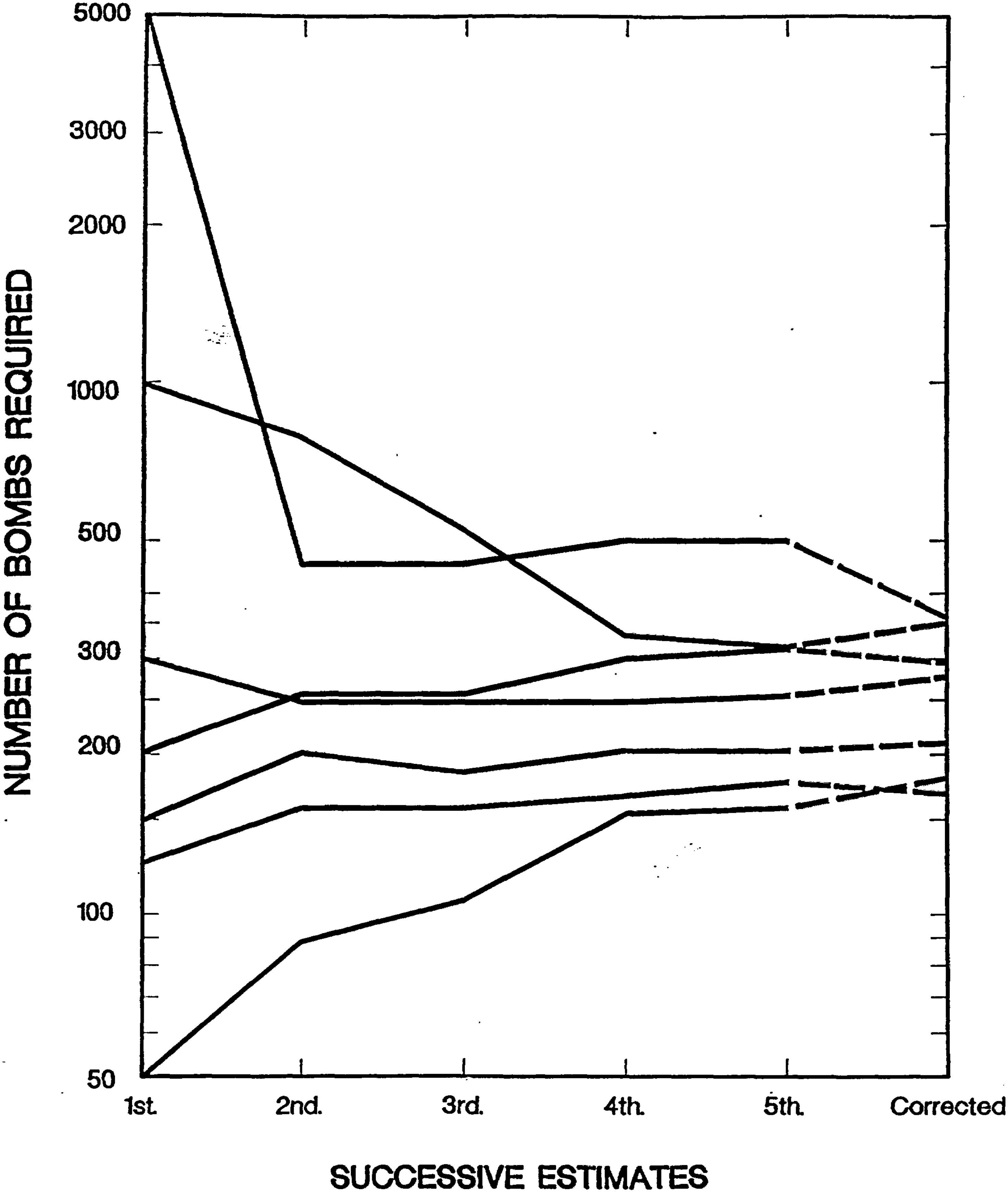


Figure 12. Elicitation with feedback and convergence of opinions by experts.
From Dalkey and Helmer (1963)