RONDOUT ASSOCIATES, INCORPORATED

PO Box 224, Stone Ridge, New York 12484

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Prof. David Okrent
Chairman, Extreme External Phenomena Sub-Committee
Advisory Committee on Reactor Safeguards
U.S. Nuclear Regulatory Commission
Washington, D.C. 20555

Dear Prof. Okrent:

Pursuant to the request of the ACRS, I have evaluated the Indian Point Probability Risk Study. I have confined my attention specifically to sections 7.2, 7.2.1, 7.2.2, 7.2.2.1, 7.2.2.2, 7.2.2.3; that is, those portions dealing with the probabilistic evaluation of seismic hazard.

Several general comments are appropriate:

1. The studies, by Dames and Moore and Woodward-Clyde Consultants are 'state of the art' for seismic risk evaluation.

2. The conclusions in the report may or may not be valid. This is based 1) on the large number of assumptions in both studies and 2) on the validity of primary data bases.

3. The reports, as submitted, should not be used as valid risk assessments in an absolute sense for two reasons.

A. The admittedly incomplete data base of a few hundred years is open to many alternative exterpretations that result in large uncertainties even for the events of relatively large probability.

B. For the small probabilities of annual exceedance discussed here, 10^{-3}-10^{-9}, the uncertainties cannot be evaluated with the available data base.

These studies might be useful for studying the relative risk at two different sites if the same analyses were performed; e.g. a comparison of the Dames and Moore results for the Zion and Indian Point sites.

4. The amalgamation of the results of the two studies and additional assignments of relative weights to each of the annual exceedance curves as in Figure 7.2-4 leads the reader to assume that a complete range of possible hazard has been considered. In fact, the amalgamation of these two evaluations with somewhat similar assumptions do not address the questions of some seismologists who feel that 'more conservative' (I know your distaste for those words) assumptions may be more appropriate.

I have attached several pages of comments on specific aspects of these studies. I hope that these comments will be useful. I hope to work with Dr. Nuttli in the near future to assemble a more comprehensive commentary on the validity and use of probabilistic risk assessments, such as these, in the siting of critical facilities.

Sincerely yours,

RONDOUT ASSOCIATES, INCORPORATED

Paul W. Pomeroy

Paul W. Pomeroy
ACRS Consultant

PWP:gla

Attachments

Comments on the Indian Point Probabilistic Risk Analyses

Section 7.9.1 Dames and Moore Study

The methodology in this study is essentially equivalent to that of the Zion study so that many of the comments pertinent to Zion are also pertinent here.

Page 2--basic assumptions of the seismic hazard model.

1. Zones of potential future earthquakes are delineated by seismicity and tectonic evidence. While this statement is almost an act of faith among the seismological community, there is certainly evidence that previous seismicity alone does not delineate future active zones and, while tectonic evidence of ancient faulting is abundant, its relationship to present day seismicity is far from clear. If this assumption were not made, what are the implications for probabilistic seismic risk analyses?

2. The relative frequency of earthquake magnitudes in seismogenic zones can be represented by a truncated exponential distribution. What is the evidence that a truncated exponential is appropriate? What are the implications of a non-truncated exponential distribution?

Pages 3 and 4--seismogenic zones.

The report discusses two sets of possible seismogenic zones, the "Northeast tectonic zones" and "Piedmont and Piedmont Cape Ann zones. In the latter case "subjective" probabilities of .8 and .2 are assigned. What if these subjective probabilities are completely wrong? What is the effect on the analyses?

On page 4, the report states "other seismogenic zones might be hypothesized which would indicate larger (or smaller) seismic hazard at the site but it is felt (!) that no such zones can be justified on a geologic basis, given the present understanding of tectonic processes in the northeastern US." The zones identified are either 1) not based on geology or 2) if they are, what relationship does that have to uniform hazard areas?

Page 5--seismicity parameters.

Conversions of intensity to magnitude were made using the formation $m_b=0.5\ (I_o+3.5)$ "which is considered (!) appropriate for the northeastern US." The WCC study does not accept the "considered" opinion nor would many other seismologists. Parenthetically, in both of these studies, there is a great tendency to use the Nuttli results for the central US where they conform to a preexisting idea and discard them as inapplicable where they do not. Further "activity rates were calculated for occurrences of earthquakes with $m_b \geq 4.0$. This decision was based on the observation that earthquakes of smaller magnitude rarely cause structural damage,..." What does that have to do with determination of activity rates?

Page 6--Richter b-value.

Usually, in the relationship $\log_{10} n\ (m_b)=a - bm_b$, n is the cumulative number of events with magnitude $\geq m_b$. The uncertainty in b values was modeled by changing the mean value of .90 by $\pm$ 15 percent but no uncertainty in b values was investigated for the Piedmont-Cape Ann zone because it was assigned a low subjective probability. That subjective probability could be completely wrong! Similar comments apply to the use of a single $m_b$max for that zone.

Pages 6 and 7--maximum magnitudes.

What is the basis for the assumption that there is an $m_b$max for any of the postulated zones? What is the rationale for the distribution function in $m_b$max?

Page 8--estimation of seismic ground motion.

Modifications to the Nuttli theory involve 1) the use of Q of 900 rather than 1500 and 2) the use of an adjustment factor of .9 to correct for a randomly oriented direction. No justification for the second correction is given. For the first, Nuttli is cited as to its appropriateness.

More importantly, the factor 1.37 is used at all magnitudes to convert from sustained to peak acceleration. Figure 5 indicates that using this value rather than the actual values determined significantly reduces the acceleration.

The distribution of acceleration about the mean is modeled by a log normal distribution about the mean with a standard deviation of .6 corresponding to a factor of 1.8 in uncertainty. Is this reasonable?

No consideration is given to the presence of the Ramapo Fault in this report.

Section 7.9.2 Woodward-Clyde Study

In this study, WCC attacks the same problem. They propose "preferred choices" and, to test sensitivity to those choices, they provide a series of additional calculations in which each preferred choice but one is used and, for that one, a series of other choices are substituted which are representative of alternative opinions. This is done wherever reasonable (!) alternative choices are available.

Page 4--identification of seismic sources.

This report states "It is judged appropriate to limit the extent of the study area to same (sic.) finite distance (e.g. 200 km)." On what basis and by whom? This eliminates many of the larger events which may be random in occurrence.

Page 6, last paragraph--It is not clear that any of the zones have the characteristics cited namely 1) uniform seismicity and 2) similar contemporary tectonic environments and geologic structures. Particularly, why not consider the Piedmont-Cape Ann province?

Page 7, paragraph 1--It is not clear how limiting the background area to 200 kms from the site facilitates the comparisons with tectonic provinces proposed by others.

Page 7, paragraphs 2-4--The relationship cited are N versus m_b but the plots of Figure 5 are N versus I. Are the slope values consistent with the .9 $\pm$ 15 percent used in the Dames and Moore study?

Page 8--upper bound.

The choice of an upper bound is taken to be a composite value with 80 percent likelihood of Intensity VII and 20 percent likelihood of Intensity VIII even though WCC considers VII to be the upper bound. Using the reverse probabilities significantly change the result although, as the report states, the change is smaller than we might expect, particularly at low valyes of acceleration.

On page 9, WCC states, in paragraph 2, "There are no reasonable tectonic boundaries, conditions, or tectonic arguments which indicate that an earthquake of Intensity VIII should be anticipated near the Indian Point site." That is an opinion with which many seismologists disagree.

On page 10, the report states that "Accelerations of .6g, however, are not associated by observation with events of Intensity VII or VIII." Is is not clear what kind of accelerations the report is talking about here.

Page 10--intensity-attenuation.

Cornell and Mertz equations are chosen for the preferred model. Those curves result in a more rapid attenuation of intensity with distance than that accepted by many seismologists for the region.

Page 11, paragraph 2--The uncertainty in the acceleration is modeled, as in the Dames and Moore study, by a log normal distribution with a standard deviation of 0.6 corresponding to a factor of 1.8. Is it reasonable?

Page 12--intensity ground motion relationships.

The report indicates use of Trifunac and Brady relationships although they do not indicate whether means or means + one standard deviation are used.

Page 12--intensity-magnitude-ground motion.

Although the report indicates that it does not use conversion to magnitudes, two pages of text are devoted to a discussion of the most acceptable relationships among three; namely, Nuttli, Aggarwal, and Sykes and WCC. For a given intensity say VIII, the first two relationships give m_b values of approximately 5.8 while the WCC relationship gives 5.2. Assumptions abound in all of these relationships and Figure 6d indicates the nature of the resultant discrepancies.

Page 14--discussion of sensitivity to input parameters.

Again the composite choice-80 percent of VII and 20 percent of VIII. A choice of VIII shows higher values.

Page 18 onward--upper bound for sustained acceleration.

The argument on page 20 relating to the association of large accelerations with surface faulting is not correct and the WCC probabilities on page 21 are not documented.

Throughout this report, the Ramapo Fault is ignored. The report would be more credible if the Ramapo as a source had been included.