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P.O. Box 224, Stone Ridge, New York 12484

March 9, 1981

Dr. David Okrent, Chairman
Summer Plant Subcommittee
Advisory Committee on Reactor Safeguards
U.S. Nuclear Regulatory Commission
1717 H St. 10th Floor
Washington, D.C. 20555



Attn: Mr. Paul Boehnert, Staff Engineer

Dear Dr. Okrent:

Pursuant to your request at the recent meeting of the subcommittee in Columbia, South Carolina, I am writing to express my views relative to

- a. Reservoir Induced Seismicity (RIS) and
- b. Seismic Design of the Summer Plant

1. Reservoir Induced Seismicity

In my opinion, the current state of knowledge on RIS is so limited that the maximum magnitude earthquake should be selected as roughly the largest earthquake that has occurred in the tectonic province or approximately magnitude $M_L=5$. As Dr. Simpson pointed out in his letter to the subcommittee, the Summer plant is located in a region which has been characterized as a region of thrust faulting. In such a region, increased seismicity (and the occurrence of the largest earthquakes) can be anticipated following large decreases in water level. The induced seismicity associated with loading (or increases in the water level) is related to small scale adjustments in stress and, in general, may not be related in any way to the maximum possible earthquakes.

I would like to expand briefly on my comments during the meeting.

A. Considerable emphasis was placed on the interpretation of the cumulative number vs. magnitude statistics used by the applicant (Dr. McGuire) to determine return periods. Two points should be made:

i. As pointed out above, the use of these data obtained during reservoir filling may not relate to the question of the maximum possible induced earthquake.

ii. If, in spite of that, they are used, the subcommittee should realize that a distribution of the type shown (that is, tapering downward at the large magnitudes) is commonly obtained during relatively short microearthquake surveys indicating that the time period of the survey was not long enough to record the higher magnitude events that would 'smooth' out the distribution. Thus the use of truncated distributions etc. is not justified. The LASL approach is probably the most reasonable to follow if these statistics are considered at all.

B. The applicant has produced a model for the processes involved based on extensive geologic and geophysical data. The arguments put forth are not unreasonable in terms of a typical earthquake that might be expected. However, I was unable to elicit any degree of uncertainty from the applicant and, in view of the limitations in our knowledge of RIS cited above, those uncertainties must be considered. The degree of uncertainty is indicated, to my mind, by the fact that the analyses and models of the Staff (and its advisors, Drs. Newton and Murphy) are equally valid and thus we see competent, responsible scientists with a divergence of views that are finally expressed as M_L varying from 4.0 to 5.3 for the maximum earthquake. At present, our knowledge is limited in regard to:

i. The distribution of stress below 1 km although the depths of the events extend at least to 3 kms (and there is considerable uncertainty in those depths).

ii. The validity of using any of the techniques described by the applicant or the Staff (and its advisors).

iii. The influence of the Wateree Creek Fault if it extends into the plant or reservoir area. This is particularly important since it would provide a failure surface much greater than 1 km in extent adding increased weight to Dr. Murphy's arguments. The ACRS should press for maximum speed in resolving the extent of this fault. Given what is not known and given the uncertainty, an earthquake of $M_L=5$ should be considered possible during the 30 years. The probability of an induced earthquake larger than that occurring during the 30 year useful life of the plant is, in my opinion, very small (i.e. $< .1\%$)

2. The Seismic Design of the Summer Plant. Aside from the RIS question, the seismic design of the plant is, in my opinion, adequate given our current state of knowledge. The critical earthquake with regard to the design of this plant and indeed, all critical facilities in the Southeast or East is the Charleston 1886 event of intensity X. It is my understanding that the last time the ACRS reviewed this question, there was general agreement that there were unique geological and geophysical features in the Charleston area such that the earthquake was considered to be possible only in that area. Indeed, this reflected the essentially unanimous opinion in the scientific community at that time. However, so little was known about what the exact conditions were that the ACRS recommended and the NRC, through Dr. Harbour's research group, implemented an extensive research program in the Charleston area. This research program has been carried out for a significant number of years by the U.S. Geological Survey.

The results of the USGS work in the area and other work (notably COCORP) have not led to the isolation of the unique causative structures or features in the Charleston area. In fact, a number of important hypotheses have been developed in the seismological, geophysical and geological committees which suggest that the earthquake could occur elsewhere. Moreover, a number of respected scientists now at least consider that there is a possibility that the event could occur elsewhere. Thus the research program has led to greater uncertainty. During the next few years, it may no longer be possible for the NRC (and its advisor, the USGS) to say "We are not convinced by hypothesis X and therefore, Charleston is unique". The NRC may rather be obligated to establish beyond a reasonable doubt that Charleston is unique. Each of the arguments offered by the Staff (and the USGS) can be debated and, given that uncertainty, an argument could be made that Charleston is not unique.

Dr. David Okrent

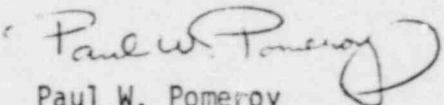
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It is my strong recommendation that the ACRS consider requesting the NRC to enhance its research program in the Charleston area including broadening the program to include non USGS participants.

I realize that you would have liked more definitive calculations. I do not believe that more precision is justified here based on the available data and theory. I hope that this is helpful to the subcommittee. If I can be of further help, please let me know.

Sincerely yours,


Paul W. Pomeroy

PWP:gla