United States Department of the Interior
GEOLOGICAL SURVEY
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IN REPLY REFER TO:

Branch of Engineering Geology and Tectonics
Mail Stop 966

April 25, 1983

50-395

Dr. Robert E. Jackson, Chief
Geosciences Branch
Division of Engineering
Office of Nuclear Reactor Regulation
Nuclear Regulatory Commission
Washington, D.C. 20555

Dear Bob:

Enclosed are three letter reports, one from W. B. Joyner and two from Noel Bycroft, concerning the Virgil C. Summer Nuclear Station Unit 1.

Please let me know if you have any questions regarding these reports.

Sincerely,

Ted

S. T. Algermissen

Enclosures

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United States Department of the Interior

GEOLOGICAL SURVEY

OFFICE OF EARTHQUAKES, VOLCANOES, AND ENGINEERING
Branch of Engineering Seismology and Geology
345 Middlefield Road, MS 77
Menlo Park, CA 94025

April 8, 1983

Dr. Jerry L. King
U. S. Nuclear Regulatory Commission
Mail Stop P-514
Washington, D. C. 20555

Dear Jerry:

In preparation for our scheduled meeting with the Licensee later this month, I am setting down my preliminary comments on the report "Seismic Confirmatory Program, Virgil C. Summer Nuclear Station Unit 1", including the Addendum to Appendix B, dated March 1983.

Noel Bycroft will comment on the theory used in interpreting the pad tests, and I will leave that subject largely to him. I would like to suggest, however, that since the modal approach, which was used to get the final answer, is not completely rigorous, it would be of great interest to see a comparison between the observed displacement amplitude and phase from the low-force-level test (e.g., Figure 4 of Appendix A) and the predictions of the model obtained by the modal analysis. That should be a relatively simple thing to do.

With respect to the explosion tests as a means of determining foundation effects, I don't believe the submissions demonstrate the applicability of the explosion tests to earthquakes. They show that the explosion seismograms are dominated by what they call S-wave and higher mode surface waves. They claim to show that the spectral ratios are similar whether computed for a P-wave window, an S-wave window (including the surface waves), or for the whole record. They show that fundamental mode surface waves do not contribute to the spectral ratios. All of this, however, does not demonstrate that the relative response of foundation and free-field sites is the same for earthquakes as for explosions. If we accept the description of the dominant portion of the seismograms as a combination of S-waves and higher mode surface waves, we have to presume that the relative excitation of the various components of this combination is sensitive to the depth of the source, and further, that the relative response of foundation and free-field sites is sensitive to the relative excitation of the components. Looked at from a different point of view, it may be more realistic to consider the dominant portion of the seismogram to be a combination of scattered S-waves, scattered not only in the vicinity of the recording site, but also in the vicinity of the source and in the vicinity of any surface reflection points between the

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source and the recording site. From this point of view, one would expect the character of the motion, and in particular, the relative response at foundation and free-field sites to be sensitive to the depth of the source. Either way, the source depth may be a significant variable. The average depth of the explosions must be significantly less than the average depth of the earthquakes, and all of the explosion depths may in fact be less than all of the earthquake depths. Two shots were at depths comparable to the computed hypocentral depth of the October 1979 earthquake, but no data from those shots is shown and, as I interpret Table V.C.1, no data from those shots was used in computing horizontal spectral ratios. In any case, the uncertainty of the computed hypocentral depth forces us to acknowledge that the real depth could be substantially greater than that for all of the explosions. This leaves me with substantial doubt concerning the applicability of the explosion tests for determining relative response in an earthquake. (My conern on this point is increased by comparing Figure IV.C.1 with Figures IV.C.3 and IV.C.6. The comparison is difficult to make because of the scale change, but the records of the shots seem to have higher coda amplitudes at free-field sites than the record of the earthquake, and the particle-motion diagrams show a different pattern for the earthquake compared to the explosions.)

I am also concerned about the use of a zero-phase-shift filter to represent the transfer function between foundation and free-field sites. The effect of this is difficult to assess and could be quite significant. Relative phase controls the extent to which an increase in modulus represents an increase in peak amplitude and the extent to which it represents an increase in duration. The effect of an increase in amplitude on damped response spectra may be very different from the effect of an increase in duration, depending on the character of the signal. To help us assess this aspect of their work, I think the Licensee should be asked to show comparisons of typical observed free-field records with synthetic free-field records formed by filtering Auxiliary Building records with the corresponding zero-phase-shift filter.

Another major problem in the interpretation of the explosion tests is the treatment of variablility. The consultants use the mean spectral ratio. I question this choice in spite of the ingenious argument offered by Robin McGuire. As you remember, he argued that, if the mean ratio had turned out to be 1.0, NRC would not have required that the spectrum be raised; so no extra margin over the mean ought to be required in the case of a mean less than 1.0. I disagree. I think that if the mean had (surprisingly) turned out to be 1.0, prudence would have required giving serious consideration to raising the spectrum. From the standpoint of safety, the question is not whether the spectrum is exceeded on the average, but, rather, whether it is exceeded anywhere. If the Envelope Spectrum embodied a large degree of conservatism, it might be argued that the use of the mean spectral ratio was justified, but the envelope of all events recorded in approximately a five-year period cannot be considered conservative without some debatable assumption about the rate at which the earthquake sequence is dying off.

I cannot find a clear explanation of how the "band pass spectral ratios" for the various windows were calculated. I may just have missed it, but, if not, the consultants should supply it. In comparing the band pass ratios shown in Figures VI.C.55 and VI.C.58 with the ratios in Figures VI.C.20 and VI.C.24, it does not seem to me that the comparison is very good, particularly in the vicinity of 20 Hz. Similarly, I don't think the agreement is very good between the ratios shown in Figures VI.C.47 and VI.C.48. The consultants apparently consider disagreements by a factor of two as matters of trivial importance. That being the case, they will presumably not object to raising their final answer by a factor of two.

The problems discussed above are the most important issues I see at this time. In going through the report I noted some minor problems. At the bottom of page 1, it is stated that the results of the pad tests confirm testimony at the 1982 ASLB hearings. I do not believe that is the case. You have access to the transcripts and could check. If the statement is not correct, perhaps it should be deleted; otherwise, the record gives a false impression of consistency. I don't understand the last paragraph on page 8. I expect you do, but, if not, it should be clarified. It is not clear to me that the reasons they give on page 9 (paragraph 1) are adequate to show that the Auxiliary Building motions are representative of motions in the Intermediate Building; an engineer should look at this. On page 10, paragraph 2, I don't understand why motions recorded in the sump would overestimate motions expected on the main floor of the Diesel Generator Building. Either Figures IV.A.7 and IV.A.8 are mislabeled or Figure IV.C.3 is mislabeled. The labeling of Figure V.C.1 is inconsistent. Does it represent data recorded at site F3 or site FR? In Table C.V.1 the term "Code Revision #" should be defined. On page 43 of Appendix B, line 13 up from the bottom, a reference is made to Figure VI.C.7 when Figure VI.C.6. is obviously intended. The discussion in the first paragraph of page 43 of the differences between spectral ratios computed for different time windows is entirely in terms of the vertical component. It would have been more relevant if it have been done for the horizontal components.

If you want to discuss any of these problems before the meting, give me a call.

Best regards,

William B. Joyner
Geophysicist

Copy to:
Ted Algermissen

United States Department of the Interior

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OFFICE OF EARTHQUAKES, VOLCANOES, AND ENGINEERING
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345 Middlefield Road, MS 77
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April 8, 1983

Dr. Jerry L. King
U. S. Nuclear Regulatory Commission
Mail Stop P-514
Washington, D. C. 20555

Dear Jerry:

Bill Joyner and I agreed that he would review the explosion tests, and I would review the forced vibration tests on the pad.

I have found no analytical errors in either the stiffness or modal approach. The stiffness method which was correctly elected to be the fundamental method of analysis is a rigorous method and involves no assumptions, except that of linearity. However, it is obvious from the results that something is amiss. This is shown by the negative damping obtained, by the large value of the transfer functions, by the fact that the resonance of the hut appears in the value of the stiffness functions K_{11}, K_{12}, K_{21}, and especially in K_{22}, by the strange behavior of K_{12} and K_{21}, and by the fact that the real part of K_{θ} should be approximately a constant value. It is hard to suggest why this is all so. Suggestions about the phases being difficult to determine are a possibility. Certainly, it is difficult to separate out the needed information from complex data where all the modes are being excited at the same time, unless the measurements are very clean and the equations well conditioned. Possibly, it would be better experimentally if the hut was removed and treated separately and a pure torque obtained from two out of phase vibrators, symmetrically placed, be applied. Similarly, a translation motion with minimum rotation could be obtained by exciting the pad through its center of gravity by a horizontal rod connected to a vibrator

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placed on the adjacent ground. This would be experimentally more difficult, but may produce more tractable measurements.

The essential assumption in the modal approach is that classic normal modes exist. This is not strictly the case in this application because the compliance functions are frequency dependent. This frequency dependence is shown in Figures 4 and 5 of the enclosed paper for the case of translation and rotation of a circular plate on the surface of an elastic halfspace. If we assume a shear wave velocity of 600 ft/sec then for an effective r_0 value of 2.3 ft., the value of the non-dimensional frequency factor a_0 is about 1.2 for the resonant frequency of, say, 50 Hz. For this value of a_0 it is to be noticed that the values of f_{1_H} and f_{1_R} are not too different from their values at a_0 = o and consequently, the modal approach is a reasonable approximation. C. B. Crouse, in a private communication, has pointed out that this is an embedded foundation (18" embedment) and that Dominguez (referenced in the Ertex Appendix A) has shown that this degree of embedment substantially changes the compliance functions from those of a foundation on the surface. I have not yet seen this reference. Presumably, Dominguez assumes that the sides of the foundation can take tension as well as compression. I assume that compaction takes place at the sides and so the effective embedment is somewhat less than 9" and that the plate on the surface is a reasonable approximation. The results obtained from the modal approach appear entirely feasible. We have programmed Eq. 58 of the enclosed paper in order to make an analytical assessment of the problem and initially obtained values of the free-field similar to those of Appendix A. The effect of the hut was not included on the assumption that we are most interested in the region of the second resonance. However, at the last minute of writing this note, we suspect our computer program and are currently checking it out. We used the following values of the parameters:

Shear wave velocity = 600 ft/sec

$b_1 = 3.0$

$b_2 = 1.0$

$\lambda_1 = 0.45$

$\lambda_2 = 1.2$

The value of R discussed in the enclosed paper for these parameters has not yet been calculated. Figure 8 in the enclosed paper shows R for values of the parameters somewhat different from the above values but roughly in the same region. Remembering that $a_0 = 1.2$ corresponds to about 50 Hz a comparison between Figures 8 and 9 of the enclosed paper and Figures 4 and 13 of Appendix A shows that they fall in the same ballpark for the second resonance. The relevant larger value of $b_1 = 3.0$ will increase the value of R shown in Figure 8.

Bill Joyner notes that as the modal approach is not completely rigorous, it would be useful to make a comparison between the observed displacement amplitude and phase such as shown in Figure 4 of Appendix A and the predictions of the model obtained by the modal approach.

Conclusions

1. The results of the stiffness method are invalid.
2. The results of the modal method appear entirely acceptable.

Sincerely,

G N Bycroft

G. N. Bycroft
Physical Scientist

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T. Algermissen