NUCLEAR REGULATORY COMMISSION

ORIGINAL

ATOMIC SAFETY AND LICENSING BOARD

In the Matter of:	:	
	:	
SOUTH CAROLINA ELECTRIC & GAS COMPANY	:	DOCKET NO. 50-395-OL
	:	
Summer Nuclear Station, Unit 1	:	

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400 Virginia Ave., S.W. Washington, D. C. 20024

Telephone: (202) 554-2345

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UNITED STATES OF AMERICA

BEFORE THE

NUCLEAR REGULATORY COMMISSION

In the Matter of:	)	
	)	
SOUTH CAROLINA ELECTRIC & GAS COMPANY	)	Docket No. 50-395-OL
	)	
Summer Nuclear Station, Unit 1	)	

Room 239
University of South Carolina
Law School
Columbia, South Carolina
Saturday, January 16, 1982

PURSUANT TO ADJOURNMENT, the above-entitled matter came on for further hearing, at 9:00 a.m.

BEFORE:

Board Members:

HERBERT GROSSMAN, Esq., Chairman
Administrative Judge
Atomic Safety and Licensing Board Panel
U. S. Nuclear Regulatory Commission
Washington, D. C. 20555

GUSTAVE A. LINENBERGER
Administrative Judge
Atomic Safety and Licensing Board Panel
U. S. Nuclear Regulatory Commission
Washington, D. C. 20555

FRANK HOOPER
Administrative Judge
Atomic Safety and Licensing Board Panel
U. S. Nuclear Regulatory Commission
Washington, D. C. 20555

APPEARANCES (Continued)

For the NRC Staff:

STEVEN GOLDBERG, Esq. & MITZI YOUNG, Esq.
U. S. Nuclear Regulatory Commission
Washington, D. C. 20555

For the Applicant - South Carolina Electric & Gas Company:

JOSEPH B. KNOTTS, Jr., Esq. & JEB SANFORD, Esq.
Debevoise & Liberman
1200 Seventeenth Street
Washington, D. C. 20036

and

RANDOLPH R. MAHAN, Esq.
South Carolina Electric & Gas Company
P.O. Box 764
Columbia, South Carolina 29218

For the State of South Carolina:

RICHARD P. WILSON, Esq.
Assistant Attorney General
State of South Carolina
P.O. Box 11549
Columbia, South Carolina 29211

and

DR. SAMUEL L. FINKLEA, III, PhD.
South Carolina Department of Health and
Environmental Control
2600 Bull Street
Columbia, South Carolina 29201

For the Intervenors:

BRETT ALLEN BURSEY
Route 1
Little Mountain, South Carolina

I N D E X

WITNESSES:	DIRECT	CROSS	REDIRECT	RECROSS
Panel consisting of:				
Pao-Tsin Kuo)				
James Knight)				
Leon Reiter)				
Phyllis Sobel)		5874		
Robert Jackson)				
Carl Newton)				
Andrew Murphy)				
Panel consisting of:				
Geoffrey Martin)				
John Blume)	5966			
Robin McGuire)				
Sheldon Alexander)				

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PROCEEDINGS

JUDGE GROSSMAN: The 22nd day of hearing is now in session.

We left off yesterday concluding with the Board witnesses, who came on at the conclusion of the presentation of the Staff, before the Staff witnesses had been cross-examined.

If Mr. Goldberg has anything further in the way of presentation, we'd like to hear it now; if not, we'll proceed with Mr. Knotts' cross-examination. Mr. Goldberg?

MR. GOLDBERG: I think Dr. Newton wanted to review a statement he made when he gets a copy of the transcript. He might have a clarification. But I think at this point we are ready to proceed with Mr. Knotts' examination.

JUDGE GROSSMAN: Dr. Newton, are you ready with the clarification now?

DR. NEWTON: I need to see the transcript.

JUDGE GROSSMAN: I see, you haven't seen it yet. Fine, we'll proceed with Mr. Knotts.

CROSS-EXAMINATION

MR. KNOTTS: Dr. Murphy, I understood you to say in your summary basically that you hadn't changed your view since the last session of the hearings, when you were present back in June or July, I guess, of last year.

DR. MURPHY: That's correct, I made no major alterations.

MR. KNOTTS: Could we extend that back a little further,

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into the first part of 1981, and ask you to address the same question? Do you think your position has changed since the first part of 1981? I'm thinking specifically about the views that you expressed to the Advisory Committee on Reactor Safeguards.

DR. MURPHY: I'm not certain when I made the final formulation that went into the recommendation that came out in the June hearings, but I believe my thinking was on that same order at that time.

MR. KNOTTS: I'm not suggesting an inconsistency, sir, I just want to get it clear.

DR. MURPHY: I don't know that there is or is not an inconsistency.

MR. KNOTTS: Well, let me ask you whether you recall having given your views to the ACRS?

DR. MURPHY: Oh, yes, very definitely.

MR. KNOTTS: And do you recall addressing toward the end of the ACRS meeting the question of margins, engineering margins, and the question of depths?

DR. MURPHY: No, I don't remember what I said at that time.

MR. KNOTTS: Let me show you a page of the transcript so we're operating on the same basis.

Dr. Murphy may want to take a couple of minutes to look at the context of his statement.

(Mr. Knotts hands document to Dr. Murphy.)

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MR. KNOTTS: I think we should give Dr. Murphy a few moments to look at it.

(A discussion was held off the record.)

(The hearing is at ease while Dr. Murphy reviews the document.)

MR. KNOTTS: In responding to a question by Dr. Kaku, Dr. Murphy, I understood you to say in effect that it was a very reasonable assumption that earthquakes would have to be a sufficient depth so that the larger earthquakes, like 4.5 magnitude and above, would not be a particular problem. Have I put that properly? I don't want to put words in your mouth.

DR. MURPHY: I think that's what I was meaning to say, yes.

MR. KNOTTS: Did you have a chance to look at the part where you talked about engineering margins that you deferred to others?

DR. MURPHY: That's correct. I still do.

MR. KNOTTS: Would it be fair to say that in your estimation of magnitude the distance parameter is more important than the stress drop parameter, that the magnitude you estimate is more sensitive to distance than it is to the differences in stress drop between your calculations and some other calculations?

DR. MURPHY: I'm not certain I understand the question.

MR. KNOTTS: How much difference in magnitude does it imply to go from 1 kilometer to 3 kilometers source dimension,

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as opposed to going from about 25 bars to about 60 bars in stress drop?

DR. MURPHY: I believe it's a difference of 3 in magnitude. It's my recollection if you double the rupture line you go up by .6 magnitude units; if you double stress drop, you go up by .2 magnitude units.

MR. KNOTTS: Thank you. That's exactly it.

JUDGE GROSSMAN: I was wondering how you were going to phrase it, Mr. Knotts, and get two different units in for the comparison; but I think the witness took you off the hook on that.

MR. KNOTTS: For which I am grateful.

Can you indicate to us, Dr. Murphy, other instances where the volume defined by microearthquakes clusters and subsequently had a main shock with a fault dimension approximately the size of the cluster?

DR. MURPHY: I'm not certain I can characterize a specific case where that has occurred. I guess a basic seismological tool where the microearthquakes or earthquakes have been used to define presence and the size and strength and depth and so forth of the structures, rather than go directly from the size of microearthquakes, indicate a rupture zone that's going to rupture in a future earthquake. That may be where there's some misinterpretation of what I'm saying.

What I have done is take the size of the seismicity patterns to indicate the size of the structure. Not necessarily

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to indicate that structure has a through-going fault and will rupture in the future, but to add a piece of my mind more convincing evidence about what the scale dimensions are involved in the area of the Monticello reservoir.

MR. KNOTTS: Thank you for the clarification, Dr. Murphy.

You have suggested depths, as I understood you, for your large magnitude earthquakes on the order of 5 kilometers, perhaps a little shallower, but not as shallow as whatever it was you said yesterday.

DR. MURPHY: I am not trying to be specific as to where I'm recommending this earthquake to place, but just indicating that I felt it was again reasonable, without really trying to define what reasonable is, that the earthquake is not going to occur in the upper 2 kilometers; and indicating the possibility that it might occur above 5 kilometers, on the idea, the suggestion, that in some sense the induced seismicity we're seeing at Monticello and at other reservoirs is in some sense different than a normal tectonic earthquake. And I'm not saying that the earthquake itself is different, but possibly the triggering mechanism and the stresses that may have been induced by the reservoir may make it possible to have an earthquake at a shallower depth than normal.

MR. KNOTTS: You would agree with me, I take it, that the earthquakes which have occurred and been observed at Monticello have occurred in the upper 2 kilometers?

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DR. MURPHY: I would agree that some of the earthquakes have occurred in the upper 2 kilometers.

MR. KNOTTS: Would you say nearly all?

DR. MURPHY: I understand that somewhere around 80 to 90 percent is what the estimates located in hypocentral locations indicate. As I understand it, the error limits are at least as large as 1 kilometer on those. And I think that might be verified by looking in one of the series of bulletins that have been provided by Dr. Talwani to the utility.

MR. KNOTTS: You would be guided by the bulletins that Dr. Talwani provided, then?

DR. MURPHY: I would be guided by that, and also guided by my own knowledge of the limitations associated with the small networks.

MR. KNOTTS: Let me ask you to assume--it's somewhat at variance with what you've just said, so I'm going to ask you to assume.

DR. MURPHY: Fine.

MR. KNOTTS: That something close to 98 percent of all of the activity at Monticello has occurred in the upper 2 kilometers. And on that assumption--well, I'm getting ahead of myself. I'm going to ask you to assume that in a moment, but first let me ask you a different question.

The seismicity patterns that you've told us about, how deep are they?

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DR. MURPHY: It's my recollection that the locations are listed as being principally in the upper 2 kilometers, as you suggested.

MR. KNOTTS: So, assuming that 98 percent of the activity has been observed in the upper 2 kilometers, on what basis would one be able to reach conclusions about the possible size and volumes of earthquake sources at about 5 kilometers from seismicity in the area of 2 kilometers?

DR. MURPHY: On what basis? I think that the thrust of the Applicant's argument with the scale dimensions is not necessarily limited to the upper 2 kilometers, although most, if not all, of the data would be pertinent to the upper kilometer. And I am going on the data base that's available, and that base includes the seismicity patterns which are, we're assuming, in the upper 2 kilometers. And this is indicating a scale dimension.

Going on that scale dimension--and it's an assumption--

MR. KNOTTS: Okay.

DR. MURPHY: We can assume that the scale dimension is appropriate below those.

MR. KNOTTS: That's the assumption?

DR. MURPHY: Right, that you can learn something from the upper 2 kilometers or upper kilometer about the deeper structures.

MR. KNOTTS: I think that concluses my questions for the Staff. Thank you.

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JUDGE GROSSMAN: It's time for the Board questioning now. I'd like to first state that I find the testimony heard at this session much more reassuring than what we heard previously during the summer. Although we find, perhaps, some elements of the presentations a little dubious, overall the presentations are reassuring; and one matter that is extremely reassuring to me personally is the testimony to the effect of no observations of damage to engineering structures for magnitude 5 or less events.

And it would seem to me that if that's all there was to the story that we have made much ado about nothing. But it does appear as though there are some problems, nevertheless, that could be associated with a magnitude 5 event occurring at the Summer site that should be considered in more or less significance as to some of them, and I'd like to hear the Staff's comments on those matters, as to whether they have been fully considered and whether there is any cause for concern or whether we shouldn't even consider these things at all.

Let me just indicate my thinking along these lines, and that is that very few, if any, of the data with regard to magnitude 5 events have really been observed in the very near field or near source field; or if they have been observed, they haven't been tied to any particular magnitude. So, it is very difficult to simply rely upon lack of observations in the past.

That secondly, most observations with regard to earthquakes have occurred out West, where most earthquakes have occurred,

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and that notwithstanding Dr. Nuttli's presentation about differences in subjective evaluations between the East and the West, that there may be physically different attentuation rates in the East from the West, and that therefore the effects of an earthquake may be greater in the East than in the West.

That the indication at the Summer site is that the seismicity has been confined to what would be usually considered near source or near field, and that that would almost rule out somewhere in the high 90's percent of the observations we've had in the past with regard to magnitude 5 or less events as to the effect on structures.

And that further, the seismicity has been shallower at Summer than has normally been the case for even magnitudes of 2.5 to 3, and so that suggests a significant difference between what might occur at Summer for a 4 or 4.5 or 5 magnitude event than what has been observed with regard to those magnitude events somewhere else, where the depths might be considerably greater.

And, of course, the implication here is that if those shallower events occurred at lower magnitude that there is a possibility that we can't just draw a line and say "Everything below 3 is up here at 2 kilometers or less, and everything above that drops down dramatically three more kilometers and then takes place at a lower depth."

This is a very broad area, but I do want to have Dr. Reiter's comments on what I've just said, with the input being

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that even though we shouldn't have an overwhelming concern with regard to lower than 5 magnitude events that we do have some concern in how you evaluate that, Dr. Reiter, with regard to these particular matters.

DR. REITER: I don't think I can address all of them, just several of them.

There's no doubt in my mind that we can come up with a configuration that would result in exceedingly high ground motion at Summer. And we cannot actually rule out that configuration. I don't care what anybody says, we can always come up with one higher.

And the thrust of the Staff in this case and in other cases is to look at the evidence to make a reasonable and to make a conservative judgment.

In terms of the maximum magnitude, the Staff feels that there is a preponderance of lines of evidence indicating that the upper magnitude that we would get in this region would be about a magnitude of 3.

We feel that, taking Dr. Nuttli's estimates, what we've seen in the Eastern U.S. as to what would be the largest that you could get in the upper 2 kilometers, and using the depth that you get anywhere in the Eastern U.S. assuming a magnitude 4, we think that's the range we should apply of sensitivity.

In coming up with our estimates, we applied that sensitivity, and by using the extremely conservative envelope of

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the existing motion, we feel assured that that motion from this magnitude 4, if it were to occur, would be no greater than the envelope that we've assigned.

Now, it's possible, and we cannot say with absolute certainty that you might get a 5. In all honesty, Judge Grossman, I cannot say with actual certainty we'll not get a magnitude of 8 next to the Summer Plant. And that has to be very clear. But we are making what I think in a very difficult situation, difficult in terms of coming to mutually satisfying solutions to all members of the community, the seismologic community--I think we're making a reasonable and conservative estimate.

With respect to a magnitude 5, if it would occur, based on all the experience that we have, this event would occur at the normal tectonic depths, based on our experience of large triggered events in regions such as the Piedmont, where large events have been questionably or not questionably associated with reservoir-induced earthquakes. And that type of event, magnitude 5, if it were to occur, would occur at depths such as the ground motion estimates we have given either by the SSE or by the 4.5 RIS or by the envelope for the ground motion we have indicated--would be covered by those three sets of ground motion parameters.

With respect to attenuation, maybe I misunderstood you, but the importance of Dr. Nuttli's work, and pointing out that he is the one who has really laid this all out, is primarily with respect to large distance. The difference in the attenuation that

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we see are at distances usually greater than 50 or 100 kilometers. In other words, for some reason the earth's crust or the earth's structure in the Eastern United States transmits waves much better than in the Western United States. These factors only become significant after great distances. In other words, if we were to have an earthquake the size of the 1906 earthquake that occured in New Madrid, Missouri-- and we felt that to a much larger extent.

As we get nearby--Dr. Nuttli has pointed out, and I think quite correctly, that in the Western United States very small earthquakes or much smaller earthquakes have been known to rupture the earth's surface and are close to the earth's surface, and yet generally the ground motion for the small earthquakes we see or the ground motion we'd see would generally--for the earthquakes that are close to the surface had to be greater in the Western United States than the Eastern United States.

Now, we have not taken the simple relationships predicted by Dr. Nuttli in making our estimates for the shallow earthquakes. Remember in the plots that Dr. Nuttli had for normal depth earthquakes his estimate came out to something like .065 g for the normal depth 4.5. And if you made such a calculation for a magnitude 3 for a "normal depth," then you might get a lower estimate. We have not done that because of the specific case of Summer seismicity.

So, the important thing, I think, to point out is that

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attenuation is primarily a function of the far field, and in that case East would be larger than the West; and if a magnitude 5 were to occur, we think it would occur at normal depths, which are already encompassed by the ground motion descriptions suggested by the Staff.

JUDGE GROSSMAN: Do I understand correctly--I'll give you a chance, Dr. Jackson. Do I understand correctly that with regard to your estimates of depth for normal tectonic events or what you characterize as normal tectonic events, in contrast to what you characterize as RIS events, is based on empirical data solely and not on physical parameters with regard to how deep an earthquake must occur in order to be of a certain magnitude?

DR. REIDER: We have classified--there are several. We have classifed RIS into two parts. One part which we think is most directly related to the reservoir loading and the migrating water in the reservoir, and that we call the shallow seismicity. However, we realize there are a class of events that are questionably related to reservoirs. An example would be the Clark Hill event, which occurred near--it's a magnitude 4.3 at the Clark Hill Reservoir.

We have looked at those type of events, and on that basis we arrived at the magnitude 4.5 RIS event. But because of the questionable nature of the relationship to reservoir-induced seismicity and because of the characteristics of that event, the felt area, the low damage, and the comparison of that with other

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normal tectonic events, that event we believe occurred at normal tectonic depth.

Now, aside from that, there are the other normal tectonic events which nobody postulates being related to reservoir-induced activity.

JUDGE GROSSMAN: Dr. Jackson, could you first answer my last question, and then if you want to add something to the prior discussion.

DR. JACKSON: I think the simple answer to your question is our judgments are based more on the empirical observations than on parameter determinations, but they're a combination of both and I don't know that they can be sorted out into one exclusive lump or the other as to what we put the greatest amount of weight on for judgment. I think all the items have been considered. And I'm sure Dr. Reiter could comment on that further, or Dr. Sobel.

I wanted to mention a few other items. One, lest the newspaper this evening read that the NRC Staff has concluded that a magnitude 8 will occur near the site, I think that Dr. Reiter, knowing him well, indicated that it's not a seismologist's job to rule out anything happening anywhere. And I think that that was the intent, and I wanted to make sure the record was clear on that.

Two other items which are more pertinent to the point is that for the reservoir-induced events we've taken a mean plus

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one sigma of the envelope of the available data from the spectra for the two events that have occurred in the reservoir. And under normal practice, and again a hypothetical case, if we had 50 reservoir events of magnitude 3 that had been recorded, each one of those would have a slightly different ground motion to it, and therefore slightly different response spectra. We would then take those 50, combine them and take the mean plus one sigma distribution and use that with some confidence that the ground motion description from those events would be reasonably contained within that at most frequency ranges.

And so in this case where we have only two events, and I think you probably saw it yesterday, we are taking the envelope of it, so you do gain some different perspective, and doing that over our normal policy and practice.

I think something that tends to be overlooked -- and being a geologist and not a seismologist, I look somewhat more simplistically at these problems -- is that I think everyone concerned here, although seismology and geology are spoken to, although they cannot rule out the fact that large magnitudes won't occur shallow, I think the intuition of all geologists and seismologists is indeed that you have to go deeper to get larger magnitudes it's not impossible to get them at shallow depths, and in doing this if you do get it shallow and the earthquakes become smaller there is a frequency shift from a lower frequency to a higher frequency with the magnitude size, so you're actually changing the shape of the spectrum, and I think Dr. Trifunac, this was the main point that he had brought out, so you have a continuum here, and I think many of the discussions that have gone on in this hearing and many others I've been in, it's a perception that we have a binary or step function system, and the idea is we have a continuum with reservoir-induced seismicity we may be closer to a binary system in that shallow events are disconnected from the deeper type of events.

So my point was I don't think we've tried to draw a

line, I don't think you're inferring that, but I don't think we've tried to draw a line of embarkation if that's the proper word of separation between one layer and the other in a finite sort of way, it's a general continuum.

JUDGE GROSSMAN: Well, let me also clarify that when I used 5 magnitude it wasn't in the context of the maximum estimates used by the staff or the applicant or Dr. Murphy, it was in the context of the figure used for damage observed, and so I wasn't inferring any acceptance of any particular figure, and that wasn't the thrust of what my question was.

Now --

DR. SOBEL: I would like to add to the question you just asked. The magnitude versus depth estimates that we've made and the ground motion models. the ground motion that we've used to define the shallow reservoir-induced events and the deeper events are based largely on empirical observations.

JUDGE GROSSMAN: That was my understanding and, nevertheless, since there is an element of physical parameters to a shallow event, I wanted to ask what effect the new estimate of the October '79 event, being only 70 meters deep, has on the concept that larger magnitude events must generally occur at a greater depth.

Okay. Dr. Reiter.

DR. REITER: I think it emphasizes the point that Dr. Zoback was making in that this is the same size earthquake

we've had before, about magnitude 2.8, and one of the results of Dr. Zoback's estimate as pointed out by Dr. Fletcher was that the in situ stress measurements indicate that the -- it appears to be a region at the top, the upper few hundred meters which has for measurements the most amount of quote stress available for having the fracture process.

JUDGE GROSSMAN: By the way, is that a commonly agreed on figure now, 2.8, or are there -- Before that October '79 event I heard some discussion that indicated that the estimates were put up at about 3.

DR. REITER: I'm talking about local magnitude. The 3 is a moment magnitude. The staff in arriving at its estimates has used -- if both were consistent has used both, if they were not inconsistent has used local magnitude, the reason being that a lot of the empirical judgments were based on local magnitude estimates.

JUDGE GROSSMAN: Well, let's assume that the 3 was the moment magnitude. Is that the same as what the moment magnitude was for the prior event, or is that an increase?

DR. REITER: I think we could check the table the applicant has to look at it. In the context of the kinds of estimates that we're looking at here, if we're dependent upon whether something is 2.8 or 3.0 in our calculations then I think we're really in a very sad state. It's really -- Otherwise, if that's going to be the point we are really not

dependent upon that. That's what I'm trying to emphasize.

JUDGE GROSSMAN: Okay. But nevertheless let's get back to the main question as to whether a 70-meter depth implies that perhaps we can't consider very strongly physical parameters to limiting a magnitude for an event at a shallow depth, and I would also like to hear from Dr. Murphy on some of these things that we just discussed.

DR. REITER: Did you want to hear from him first, or I can --

JUDGE GROSSMAN: Dr. Murphy.

DR. MURPHY: Just a quick comment on the 70-meter depth.

As I understand the calculations from Dr. Fletcher and Dr. Joyner that they estimated a rupture diameter on the order of 200 meters for that event, and as I understand it there was no signs of surface rupture associated with that event, so that means we've got to push it down or shrink one of the two, so that the strict use of a rupture dimension as calculated by either, principally say by the Brune model would have to be taken with a bit of salt, and the depth estimates also have to be taken that way.

At that shallow a depth there has to be -- there is some constraint by the fact that there has not been any visible surface rupture, so those parameters do control things, but they are not the limiting controls at this stage.

JUDGE GROSSMAN: Let me ask you this, then. Let's

assume that you can have -- that the 70-meter estimate was correct, and so was the estimates of magnitude both local and moment magnitude, and it was -- they were respectively 2.8 and somewhere around 3.0. What does that imply with regard to the shallowest depth that you could estimate a 4.5 magnitude event? Could you give me a ballpark figure, Dr. Murphy?

DR. MURPHY: I think -- let me use a 5.0 because I remember the numbers that we've juggled around, and if we constrain the experiment in a hypothetical case to exclude surface rupture, a 5.0 nominally has to occur some depth deeper than 1.6 kilometers, so we're talking say at least 1.7 kilometers.

I think from the table -- not the table, the figure that Dr. Nuttli showed that the insistence on surface rupture at least is not the controlling parameter, something else is controlling the depth of the earthquake, of the events, the five events he showed on that table.

None of those with the -- Let's see, let me back up just a second. There are two events on that table from Monticello, there was one from Jocassee, and those indicated that surface rupture was not the controlling parameter, something besides surface rupture was controlling the depth at which that earthquake has occurred.

Thers is in my mind at this stage no information that precludes surface rupture in the east, it is an empirical

observation that the earthquakes are occurring at depths greater than allow surface rupture, so this constraint on rupture diameter or rupture radius is not the controlling parameter, there is something else that is controlling the depths of the earthquakes.

JUDGE GROSSMAN: But now let's assume that surface rupture is a controlling parameter with regard to depth and that it doesn't generally occur in the east.

My understanding of your physical model is that you assume a circular area more or less, and therefore you have your constraint as to how shallow you can place a certain magnitude event, but in light of what happened in October of 1979 doesn't it appear as though that constraint really should be insignificant, that that parameter should be insignificant, that the depth can be considerably shallower than might be accounted for in a circular model?

Do you understand my question? Perhaps it's inelegantly phrased.

DR. MURPHY: I think I understand your question.

In my own mind, given a special set of circumstances such as caused or brought on by reservoir-induced seismicity, I don't understand why we have to be restricted to not having surface rupture. The observation is we don't have surface rupture as far as feild evidence shows at this time.

DR. NEWTON: Can I add something to that?

JUDGE GROSSMAN: Yes.

DR. MURPHY: I see no reason not to have surface rupture also when you're talking about a magnitude 3 or less. You're talking about maybe a millimeter of motion along the fault and the displacement on the fault something on the order of a millimeter. You're not going to find that particularly when the thing is sliding out there and it's not vertical offset and you have earth tides producing motions of a foot in the ground, and so there are all kinds of things going on out there that are by scale a whole lot bigger than the motion of a magnitude 3 fault,and the idea is that it's something that a larger fault with displacements of centimeters or more is what you preclude happening to rupture the surface out there.

JUDGE GROSSMAN: Okay. So in other words you don't accept the rupture, not that there couldn't be a rupture, as a parameter for your physical model, but my question goes further and says "Well, even if you did accept that, could you feel constrained by what I understand to be a circular model, and maybe it isn't, or a semicircular model, whichever it is, which limits your depth dimension to basically what your width dimension is or somewhere in the ball park when you might have a different, an entirely different configuration as far as a rupture plain goes?

Now, maybe I'm way off base on this and that isn't

where your parameters come from, but do you understand my question, Dr. Murphy?

DR. MURPHY: I believe so.

The circle is simply a mathematical convenience. If you want -- Dr. Brune developed the model, possibly he couldn't handle in his computer and his mathematics anything except a simple circular rupture, so the circle in itself is not meant to be a strong constraint, it just is an incication of the size rupture that we're talking about in a particular stress drop.

JUDGE GROSSMAN: But isn't your depth parameter based upon your area consideration and therefore whether it is because of the mathematical model or or just your own evaluation or estimate, isn't it really somewhat based on there being a reasonable relationship between the depth and the width of that fault plain that may not be true as observed from the October 1979 event, Dr. Murphy?

DR. MURPHY: Okay. I think I understand the question.

The model does place some constraint, it's not a strong constraint, on the depth at which the earthquake would occur.

The stronger constraint at this stage in the case of the Monticello reservoir effectively is the observations, that the observations given an error bar on most of the earthquakes, my guess would be more like a half a kilometer, plus or minus half a kilometer or a kilometer.

That places a stronger constraint on where the earthquakes are going to be occurring, and other than that it's a best guess on where the magnitude 5. 5.1, 5.2 earthquake that I've recommended would occur.

JUDGE GROSSMAN: Dr. Sobel, did you have something to add to this whole discussion?

DR. SOBEL: Yes. First the depth of 70 meters can't be considered an absolute figure, some uncertainty is associated with that.

I believe Dr. Talwani said it may be on the order of a hundred meters or so.

Secondly, the observation that we have of approximately a magnitude 3 event at a depth of 70 meters plus or minus a hundred meters isn't inconsistent with Dr. Zoback's suggestion that the higher stress drops and therefore the larger magnitude events should be occurring in the upper few hundred meters below the reservoir.

As stated in our testimony, we believe that the reservoir had a major effect on the shallow events because they're spatially and temporally associated with the reservoir, but the mechanism for generating these earthquakes may not be the same as for generating the larger events.

JUDGE GROSSMAN: Okay. That's what you mean by your physical constraint on the magnitude, and it has nothing -- and you're not at all relying upon then the area of the source

dimension, the area of the rupture surface; is that correct?

DR. SOBEL: That's correct.

DR. REITER: I might point out that for that reason we did not tend to place much emphasis on the kind of arguments Dr. Murphy presented in terms of the dimension of the cluster in stress drop in trying to arrive at maximum magnitude, and I would -- I think your statement about faulting in the east of generally not being observed is not quite as strong as it should be.

We in the NRC, and I'm sure Dr. Jackson who has been involved in many of these fault studies, we have searched high and low at many nuclear power plant sites around, and every time we get wind of anything for any observed surface rupture there is absolutely no evidence of surface rupture anywhere from any earthquake that we know of in the eastern United States in recent time.

Now, the only possibility that there may have been rupture, and I think Dr. Duffey has indicated, is with the 1811-1812 sequence of earthquakes which were of the surface wave magnitude order of approximately magnitude 8 1/2, so although that is quote an empirical argument it is very strong empirical evidence that these earthquakes are occurring in such a manner and at a depth that do not indicate surface rupture.

Dr. Nuttli has done some, recently has done some attempts at scaling, he got some different source parameters

of these earthquakes.

Again I want to point out that the prime difference in earthquakes between the eastern U.S. and particularly wave attenuation will be at the larger distances, and in that case the eastern ground motion from the eastern earthquakes will be larger.

JUDGE GROSSMAN: By the way, I'm not sure that was exactly fair to Dr. Murphy on the source parameters. My recollection is that that model was raised by the applicant and that Dr. Murphy merely adjusted it to what he believed to be the evidence in the area.

Is that correct, Dr. Murphy?

DR. MURPHY: That's correct.

DR. REITER: In that case we reject the arguments of the applicant posed using source dimensions --

THE REPORTER: I'm sorry. Repeat that last phrase, please.

DR. REITER: That would indicate that we also reject rigorous use of the source dimension arguments by either the applicant or Dr. Murphy in arriving at estimates of maximum magnitude.

JUDGE GROSSMAN: Dr. Murphy.

DR. MURPHY: I don't like the use of the word "rigorous." My attempts were not rigorous use of this model, but use of this model to indicate the levels of uncertainty that were involved

in the problem.

You look at the numbers and carry them out, you simply multiply them out, and they may look rigorous, but they are intended as indicators of the uncertainty that's involved in the problem.

JUDGE GROSSMAN: By the way, before I forget, Mr. Knight, let's get that clarification so the record is straight on the category 1 dams in the area that we are discussing.

My recollection is I was asking you about dams that contained the Monticello reservoir, and somehow we got off to an answer that related to the dams holding the heat sink or --

DR. KNIGHT: Surface water pound.

JUDGE GROSSMAN: Okay. Now, with regard to the earthen dams that contain the Monticello reservoir, those I take it definitely are not category 1 dams.

DR. KNIGHT: That is correct.

JUDGE GROSSMAN: Okay. Now, I heard there was a third category of concrete dams that aren't category 1 dams, is that correct, or one dam that was mentioned with regard to the pumping station? Fairfield?

MR. KNOTTS: Judge, those were my words I think. It's the power house, it's not a dam.

JUDGE GROSSMAN: Okay.

MR. KNOTTS: It's the power house at the foot of the earthen dam if you will, speaking loosely.

JUDGE GROSSMAN: Dr. Newton, what relevance did your discussion with regard to total energy release in prior events have to do with anything involved in the hearing? Is that a means of predicting magnitudes of future events?

MR. NEWTON: I'm just trying to indicate how much energy had been available for exciting ground motion out there.

What we have seen so far in terms of the stored strain energy and what has been released by the changes brought about by the reservoir or whatever mechanisms has been very small, and if we increase the amount of strain energy available by a factor of 2 or more then it has very little effect on the ground motion.

JUDGE GROSSMAN: Well, you know, I could view that in two ways. One is that if we haven't had a great release now we can expect one in the future, or if we haven't had a tremendous release now we can expect something on the same order.

My question is, does that have any value with regard to predicting what is going to happen in the future, and if it doesn't why don't we just disregard it? I mean if it has no contribution to what we're doing here, why should we even consider it?

DR. NEWTON: We've had no evidence that there is any considerable strain energy available for larger earthquakes, and that the stress calculations or measurements by Zoback

indicate that that is the case out there. The deviatory stresses are small, and the observations of the amount of energy, strain energy released is in concert, and I would think the two facts together show us that there aren't going to be larger magnitude earthquakes out there, we shouldn't expect them.

JUDGE GROSSMAN: Wouldn't you agree with a statement that if we took the total energy available in the area, even based on Dr. Zoback's calculations, we could have tremendous size earthquakes in the area, and so the observation of energy released in prior events really shouldn't be any limitation on magnitude events? Is that a fair statement?

DR. NEWTON: It depends what kind of a structure you're going to put the stress across. I don't know that I agree with that statement.

JUDGE GROSSMAN: Dr. Jackson.

DR. JACKSON: Well --

DR. NEWTON: Maybe somebody understands it better than I do.

DR. JACKSON: As usual I'll make an attempt.

I think what I interpret happening is that there's a perception of looking at faults if you like beneath this facility where these earthquakes are occurring let's say in the California sense in which the rupture is occurring. I think from all that I have heard about this kind of problem

what you really have is a brittle rock which is fractured between I guess 150 and 200 million years ago which is filled with -- and those faults have long since healed -- it's filled with brittle joints and minor plains, all of which -- there's a multitude, millions of them throughout the zone beneath the plant, these microfractures, and what's happening they're triggering movements on these microfractures, so in effect you wouldn't have the ability to store up strain energy along a single throughgoing fault plane as you might have say on an active fault in California. At least that's the way I perceive the problem.

Now, I think -- I don't mean to put words in Dr. Newton's mouth, but the -- I think he is using the stored energy argument to reflect the rocks giving up their strain over a period of time, and the water present and the loading present from the reservoir, either one or the other, is allowing that strain to come out.

As an example, strain comes out of rocks on a daily continuous basis all over the southeastern United States, and they call it exfoliation, it's a weathering process by which pieces of rock spall off that you see when you're driving along the road, it's relieving its energy usually due to expansion in the veins from the weather.

Not to get off the point, but I think what's happening is the only thing that can be put on that point is that the

energy is coming out on a reasonably slow basis and probably wouldn't have come out in the form of earthquakes if the reservoir hadn't been there, but you're not storing this long huge single throughgoing fault plain such that you can store up vast amounts of energy that may be available in the rock.

JUDGE GROSSMAN: Dr. Murphy, do you have anything to add to this?

DR. MURPHY: The only thing I had has sort of a caveat on Dr. Zoback's work, and I had a chance to skim through part of the paper that was introduced earlier, and like I said, the only caveat I would like to offer is that the measurements that he's taken and used for that paper were within the upper I believe 700 meters in one hole and the upper 900 meters in another hole, so his comments were not maybe appropriate for that portion of the active zone and for the areas in which the holes were drilled, but as noted by Mr. Knotts the data base they're working from is somewhat shallow, and we don't have any way of really extrapolating that to greater depths in the case of bore hole or deviatory stress information, that means from 900 meters down.

JUDGE GROSSMAN: By the way, Dr. Jackson, you observations on those throughgoing faults, that is also based on the two bore holes, isn't it, and you're not making a categorical statement that other than those two bore holes you believe there are no throughgoing faults in the area?

DR. JACKSON: Unfortunately I haven't examined the reactor excavations or other excavations at this site like I have at many others, but talking to others in the branch, Mr. McMullen and Dr. Alterman, the Watteree Creek fault which was discussed at length previously is the only reasonable candidate at all, and I understand that is not quite reasonable, but the excavation, the faults were mapped, they were mapped in detail and pretty much ruled out.

Now, from what I understand from talking to others that there are no throughgoing faults in this area after a detailed investigation.

JUDGE GROSSMAN: Dr. Sobel, I believe in your presentation you refer to using the RIS spectra and scaling it up or adjusting it to a higher magnitude, and I don't believe that you allowed for shifting the model to indicate higher amplitudes at lower frequencies.

Am I incorrect in that?

DR. SOBEL: I may need a clarification. Are you referring to my statement that we consider using the applicant's model for a stress drop of 50 bars on hypocentral depths of 5 to 16 kilometers?

JUDGE GROSSMAN: No, that wasn't -- Maybe it wasn't your presentation, I don't recall whose it was.

DR. SOLEL: We didn't do any scaling of spectra in our study.

DR. REITER: Let me -- I think the staff's position is really based upon observations and where at all possible to avoid controversial scaling techniques as prime estimates of ground motion.

With respect to the magnitude of shallow seismicity we found felt our best estimate, the predominant lines of evidence were indicating that a magnitude 3.0 would be the maximum event that we would get in the shallow zone.

We felt that because of the work of Dr. Nuttli that if we look anywhere in the eastern United States we might get magnitude 4 to 2.3, it would be appropriate for us to look at the sensitivity of our assumptions with respect to larger magnitudes.

We started out, and the first thing we do, we started out by taking the most conservative characterization of the ground motion, recorded ground motion that we could, and that was the envelope, and as Dr. Jackson pointed out this is not the standard staff procedure.

Then the staff went on to look and see if we could get an estimate of what the ground motion might be from a larger earthquake at a distance which -- at 2.3 kilometers. Again, 2.3 kilometers is based on Dr. Nuttli's estimate for all over the eastern United States and what the minimum depth would be.

We scaled -- And then at that point we said okay, here's our ground motion estimate how sensitive is it to

larger earthquakes, and then we took the peak acceleration and using the factors that Dr. Joyner used and taking into account the scaling factor for distance, we came to the conclusion that the peak acceleration would be equal to or less than that which is already observed.

Now, Dr. Joyner then made an estime for velocity from magnitude 4.5, and I think I indicated during testimony based on his estimate of what the response velocity would be for magnitude 4.5, and then correcting for distance that we think the 4.5 would occur, again we came out to something which is equal to or less than the prospective we've proposed, so the scaling was used only in the context of sensitivity, and the parameters that we used in scaling were those provided by Dr. Joyner or that we got from the original record.

The scaling functions we used are those empirical observations which scaling has been formed when we went over rock for distance, and the scaling magnitude was that supplied by Dr. Joyner, and I think Dr. Nuttli indicated he used a similar type of scaling.

I want to -- It's very important to emphasize that our prime estimates in working that we're starting out from are not based on any elaborate scaling procedures, and indeed although we really think that the applicant has made a great use of the Hanks & McGuire model, the thrust of the staff's review has been when at all possible to test that model against the

available data at Mammonth Lakes and peak accelerations which we thought were applicable.

Clpw

JUDGE GROSSMAN: Dr. Sobel, did you have something to add to that?

DR. SOBEL: No.

JUDGE GROSSMAN: I believe a statement was made at the beginning of the presentation that contrasted the RIS event to normal tectonic events for Monticello. Was that your presentation, Dr. Sobel?

DR. SOBEL: Yes.

JUDGE GROSSMAN: Are you then ruling out the possibility that the RIS events were normal tectonic events for Monticello that may have been occurring much sooner than possible -- than would ordinarily have occurred, because they were triggered by the reservoir but that is the normal tectonic pattern for that area?

DR. SOBEL: There's no visible difference in an earthquake be it reservoir induced or normal tectonic event. We do believe that these events might have occurred eventually but that they were triggered to occur sooner by the presence of the reservoir.

JUDGE GROSSMAN: So there really wasn't the distinction that I thought you might have had in mind as to a difference in kind.

DR. SOBEL: No. We agree then with Dr. Trifunac's statement on that same subject.

JUDGE LINENBERGER: Let me pursue that just a moment. I

C2pw

though I heard Dr. Trifunac agree with Dr. Luco that between them there was a consensus that so-called background seismic events should be of more concern that mechanistically similar events caused by the Monticello reservoir. I know Dr. Luco felt very explicitly that was the case, I thought I heard Dr. Trifunac agree with him. Now are you saying something different here or do you agree with that?

DR. SOBEL: We agree that -- we considered both reservoir induced and tectonic events and Dr. Trifunac, I'm a little unclear as a result of his total testimony, but I believe in the end he agreed with us that it's in the higher frequencies that these small shallow reservoir induced events would exceed the design spectra.

JUDGE LINENBERGER: Oh, but that's a separable consideration because you know somebody could pull a design spectrum out of the air, so the question of exceedance I want to leave aside here. It's a question, in my mind, it's a question of which is the more worrisome event and to be more worrisome, this to me is a matter of energy releases and design spectra drawn by fallible people but energy releases Mother Nature hands us, so --

DR. SOBEL: I believe that perhaps it would be best to describe this in terms of frequency constant. I think at the lower and intermediate frequencies, the tectonic event would be the controlling one in terms of design and that the smaller

C3pw

nearby reservoir induced events would be more important in terms of design for very high frequencies.

JUDGE LINENBERGER: Okay, let's leave it there --

DR. JACKSON: If I could insert just a correction because I think the record would be muddled in this area a couple of lines ago. In agreeing with Dr. Trifunac, I recall vaguely a question being asked of him is there a difference between earthquakes, so-called reservoir induced earthquakes and normal earthquakes, and saying we agreed with him we're saying, you know, physically there is no difference between the two. I think we went beyond that in the discussion.

JUDGE GROSSMAN: Dr. Reiter?

DR. REITER: Judge Linenberger, I think that with Dr. Trifunac who arrived at the conclusion of significance by his probability calculation, that what he called background seismicity was of more importance than reservoir-induced seismicity. I think Dr. Luco did not enter into the problem of seismicity and which was more important.

JUDGE LINENBERGER: Well Luco did stress the importance of background seismic events right at the very beginning of his testimony, but I see what your position is so let's leave it at that.

JUDGE GROSSMAN: I see a sharp delineation here again creeping into the discussion between RIS events and tectonic events now in the context of the frequencies. Now am I incorrect

C4pw

in believing that as you go from lower magnitude events to higher magnitude events, there would be a shifting of the spectra towards the lower frequencies and that's all you're talking about when you're comparing RIS to tectonic but there isn't just a sharp cutoff that says RIS events have an influence on high frequencies and tectonic events on the lower and intermediate frequencies, but that you just happened to be going from lower to higher magnitude events and therefore there is a shift towards the lower -- toward the intermediate and lower frequencies. Is that correct, Dr. Sobel?

DR. SOBEL: That's correct.

MR. KNOTTS: Judge, will we be getting to a convenient breaking point sometime soon?

JUDGE GROSSMAN: We will be because we have to get to the post office. Let's go off the record for a second.

(Discussion off the record.)

JUDGE GROSSMAN: We'll take a ten minute recess.

(A short recess was taken.)

DR. SOBEL: I'd like to make a statement on the subject we were discussing at the end of the last session.

You were talking about shifts in spectra.

JUDGE GROSSMAN: Yes.

DR. SOBEL: Smaller nearby events would tend to have relatively high frequency energy and larger magnitude events which occur at greater distance, tend to occur at greater distances

C5pw

would have lower frequency energy.

JUDGE GROSSMAN: Right. And my question was directed towards whether you were just having an arbitrary cutoff between RIS events and tectonic events or whether you recognize that what I believe to be the case, as you go more gradually from a lower magnitude event to a higher magnitude event, you have a gradual shift in the energy content from the higher to the lower frequencies and it isn't that there's an arbitrary line between RIS events and tectonic events.

DR. SOBEL: We agree.

JUDGE GROSSMAN: Fine. Now getting back to the physical limits on shallowness for events, my recollection is that we did have some shifting of distance or depth within the RIS events in which you put your magnitude 4 event down to a 2 kilometer or hypocentral distance. I guess it must have been a hypocentral distance. Now does the fact that the October, 1979 event occurred at 70 meters depth indicate that you might be able to consider or should consider a shorter hypocentral distance -- I said depth, I'm sorry, hypocentral distance than 2 kilometers for a 4 magnitude event? Dr. Sobel?

DR. SOBEL: As I explained before, we believe that the higher stress drop events and therefore the larger magnitude events tend to be occurring in the upper 200 meters and we have considered that if a larger event were to occur, it would tend to occur deeper and the ground motion from that event would be

C6pw

enveloped by the estimates we've made for the envelope of the existing ground motions and the estimated RIS spectra.

JUDGE GROSSMAN: But doesn't the observation of the October, 1979 event bring into question your assumption that the higher magnitude events, within the range we're discussing for RIS events occur in the lower depths rather than the same depths as all the events that have been occurring at the Summer site?

DR. SOBEL: I don't believe it does because we've considered that there is a zone near the surface and in that zone and any depth a maximum magnitude limit would be about 3.

JUDGE GROSSMAN: Dr. Reiter?

DR. REITER: A simple answer to your question, Judge Grossman, is absolutely not.

There is no evidence of recurrence of incidents that would indicate that the estimates of 2.3 kilometers for a magnitude 4 event should occur at shallower depths, it's based on separate lines of reasoning. The occurrence, the location and magnitude of a 2.8 event is consistent with past occurrences of an event of this size or a similar size consistent with the stress measurements that have been observed.

The use and it is the best estimate of the staff that magnitudes 3, about that size, is the appropriate size; however, we realize based on Dr. Nuttli's work that there may exist in the eastern United States other large events at shallow depths. Now we've used Dr. Nuttli's work which estimates the minimum depth

C7pw

for magnitude 4 anywhere in the eastern United States and assumes that would occur at Summer and that's the sensitivity test we ran with respect to our estimate for the magnitude 3. I might add that our estimate for magnitude 3 as I indicated before was an enveloping, very conservative estimate so there are several lines of reasoning going on here. There is not one -- somehow I sense your feeling for one simple explained physical model which would tie everything together, and there isn't. There are various lines of reasoning, there are physical intuitive reasons which I think we've laid out in as explicit a manner as we can in our testimony and what we're saying here. There's a great deal of empirical observations. I might add that most of the information that we have about eastern United States is based on empirical information, the kinds of work that Dr. Nuttli and his colleagues at St. Louis University have done.

JUDGE GROSSMAN: I don't want to belabor the point and perhaps I have already done that but my understanding was that Dr. Nuttli's work related more to your jumping from the RIS event at the 2 kilometer and less level, speaking of depth, to the 5 to 16 kilometer level for the normal tectonic event and I was referring to a very small portion of your projections which is from the range 3 magnitude to 4 magnitude with respect to your change in hypocentral distance from .8 kilometers to 2 kilometers and asking you specifically with regard to that whether there was a firm foundation for that change in light of the October, 1979

event which to my understanding was the maximum event that had occurred or at least equal to a maximum event that had occurred and seemed to be at approximately the shallowest depth of any event that was over 2 magnitude.

Now with that in mind, perhaps someone else can answer also. Dr. Murphy, do you have any thought on that?

DR. MURPHY: I think I have indicated that I have some doubts or skepticism about the rest of the staff's position that magnitude 3 is the largest event that can occur in the upper 2 kilometers. In effect, to use the old word again, I was unconvinced about that. I had only seen that argument some ten days ago and have not really gone back and looked at anything in the literature or done anything of that sort other than to look at Dr. Nuttli's curve to say whether I agreed or disagreed. I just had strong -- maybe I'm a skeptic but I don't accept that at this stage but that's the state of my comment.

JUDGE GROSSMAN: And the qualifications you've already put on your testimony, I will accept your answer in that context that you really haven't prepared that thoroughly on this matter and that this is really an offhand observation on that and that you're definitely not speaking for the staff position on that, but I would like to hear Dr. Jackson's comments.

DR. JACKSON: I think this is an area which I would forego to Dr. Reiter and Dr. Sobel.

JUDGE GROSSMAN: Okay, Dr. Reiter, do you have anything

C9pw

further?

DR. REITER: Just to reiterate what I said before, there is absolutely nothing in the occurrence of the October, 1979 event that would indicate that our estimate of magnitude 3 as a maximum magnitude is incorrect or inappropriate. We think it is entirely consistent with the past record, entirely consistent with the stress observation; however, to be conservative and we realize that it's very difficult at this point to put a maximum magnitude, we have gone out to the rest of the eastern United States and looked at the maximum based on what we consider the prime interpreter of eastern United States seismicity in the world. Based upon his interpretation, we are going out and we are transplanting the largest magnitude four at the depths that have been observed anywhere in the eastern United States based upon his observation and assuming that will occur on the site, and we conducted a sensitivity study to an envelope of the magnitude 3 event. That's what we've done and I really don't know what else we can do.

DR. JACKSON: Could I ask -- I'm not sure that we clearly understand your question. Maybe Dr. Reiter does but I may not. Does your question go to the point stated simply have you considered the magnitude 4 at 70 meters?

JUDGE GROSSMAN: Actually what I had in mind was one kilometer which is considerably greater than 70 meters and I was wondering how you jump from 70 meters at a moment magnitude of 3

C10pw

and a loca magnitude of 2.8 to 2 kilometers for a 4, and it appeared to me that perhaps you didn't consider 70 meters for the 2.8 because it's a recent estimate and that that is a considerable gap within the shallow depths we're talking about.

DR. REITER: Again, Judge Grossman, please excuse me, you may be misinterpeting it. The distance to the instrument was more like .8 kilometers, the depth was 70 meters, so the distance to the instrument, hypocentral distance, was more like 800 meters, approximately.

JUDGE GROSSMAN: Okay.

DR. REITER: We used, in testing the sensitivity of our assumption, we used that record, that highest record or high peak acceleration. Dr. Joyner revised estimates of the velocity to see what would happen to a magnitude 4. The distance we chose for magnitude 4 was based upon the experience or the evaluation of what had happened in historical times in eastern seismicity. We did not make an estimate for magnitude 4 at .8 kilometers or magnitude 5 or any other magnitude. Our best estimate is magnitude 3 but because of the work Dr. Nuttli has done for the Applicant, we felt it appropriate to look at a larger size at the minimum depth that has occurred anywhere in the eastern United States.

JUDGE GROSSMAN: By the way, just to set the record straight, I'm aware of the fact that the depth is not the hypocentral distance. I would assume that that was not a significant --

C11pw

that would not make a significant change in the way I stated the question, whether we're talking about a 2 kilometer depth or 2 kilometer hypocentral distance. We have a matter of a few hundred meters in there. One clarification that I would like, Dr. Newton, was your statement that there was motion contamination in the October, 1979 event, in the recording of the October, 1979 event. What were you referring to?

DR. NEWTON: I was referring to the fact that there are surface waves in that. To take the record as pure shear waves to get a stress drop or RMS acceleration to get stress drop, you've got more energy in there. The ARMS is going to be high because you've got the contamination, it's not a pure shear wave. So if you're going to compare that to the situation in California where you might have separation of the shear wave from the surface waves, you're going to get results that are over-estimating the stress drop. And so that I would prefer to use the record that came from an event that was deeper, that would have not the same shear wave excitation as the October '79 event.

JUDGE GROSSMAN: Dr. Reiter?

DR. REITER: At this point I'd like to bring up a point. Dr. Newton's observation about the existence of surface waves in the magnitude 2.8 earthquake is consistent with the interpretation by Dr. Luco and Dr. Trifunac that it would be Rayleigh waves, the type of surface waves, which are predominant in the blast at 40 meters. Remember the distance there was 70

C12pw

meters and they were arguing, they were indicating that the reduction factors applied may be more appropriate for shallow type events which are the predominance of surface waves, and if that is the case then this type of reduction factors they talk about which was observed in the blast test may indeed be applicable to the kind of shallow events when have occurred in the Monticello area, which Dr. Newton observed also became surface waves.

JUDGE GROSSMAN: I take it the staff has not made any formal presentation on this?

DR. REITER: No. But while there was a great deal of discussion of what -- the significance of Rayleigh waves and surface waves, we have not reached a final conclusion. I thought it would be important to point out to the Board that there were different types of sources here and that indeed if there is a reduction due to surface waves, they would also be present in shallow earthquakes.

JUDGE GROSSMAN: I take it this is basically in line with your position of de-emphasizing the stress drop calculations rather than suggesting that we adjust them downward and therefore come to a different conclusion.

DR. REITER: I'm not sure what that -- the question I was talking about was with relation to the comments by Dr. Trifunac and Luco with respect to blast results.

JUDGE GROSSMAN: Yes, but you projected those comments to the stress drop --

C13pw

DR. REITER: No, I did not.

DR. NEWTON: I made some mention about stress drop being over-estimated because of the contamination.

JUDGE GROSSMAN: Right, but you're not suggesting that we adjust the stress drops downward somewhat or take that into consideration, you were just attempting to de-emphasize our reliance upon those stress drop calculations, is that correct?

DR. NEWTON: I think that is essentially correct.

JUDGE GROSSMAN: If there's nothing that disturbs anyone further on this, then we can go on.

DR. MURPHY: I'll just make one point that we brought out with the questioning with Mr. Knotts and that is the emphasis that the stress drop does get, that the factor of 2 change in stress drop amounts to a .2 change in the magnitude levels.

JUDGE GROSSMAN: And the point being that even if we were to take into account what Dr. Newton said now is an adjustment, it would be an insignificant -- it might in all likelihood be an insignificant adjustment in any event.

DR. MURPHY: It might be depending on how much of a factor it is. If it's simply a factor of 2 contamination that's a .2 change, if it's a factor of 4 you pick up another .2 change. So there's some question there as to the significance.

JUDGE GROSSMAN: And if it's a factor of 1.2 then we can basically disregard it?

DR. MURPHY: That's correct.

C14pw

JUDGE GROSSMAN: Dr. Newton, did you have a comment?

DR. NEWTON: Yes, I don't think we're concerned about the magnitude here, I think we're concerned about peak accelerations. If you scale stress drop by a factor of 2, according to the mode. you have to scale acceleration by a factor of 2. The question of a .2 magnitude doesn't come in.

JUDGE GROSSMAN: Yes, but my understanding was you weren't even talking about factoring the stress drops by whatever adjustment but changing your input into the formula at which you arrive at the stress drop and so I'm not going to pursue it any further. I would assume unless someone brings in some hard calculations that this is not a factor that we would consider at all, and I'll just leave it at that.

MR. GOLDBERG: I believe Dr. Newton has something he wanted to add. Obviously, you know, we've heard direct testimony given orally as well as pre-filed and I take it that this examination is in that same nature now. I don't know if Dr. Newton feels that his testimony is satisfactory as it stands or he wants to add to it.

JUDGE GROSSMAN: You have the transcript and you have reviewed --

MR. GOLDBERG: No, I'm saying on this exchange.

JUDGE GROSSMAN: Oh, on the point.

DR. NEWTON: My prefiled testimony I think stands clear there that I don't agree to use the October '79 event as a means

C15pw

of getting stress drop to get acceleration. I think that their RIS seismicity accelerations are exaggerated and that if there's some exceedance in the stress drop calculations that says that the accelerations for the plant for high frequency motions are too high, the estimates.

JUDGE GROSSMAN: I had forgotten about your prefiled testimony on that point, I don't even recall it too well now, thank you for that clarification.

Have you had a chance, Dr. Newton to review your testimony yesterday to clarify a point that you thought might need clarification?

DR. NEWTON: Yes, I have, on page 5833 of transcript from yesterday, it has the line that we could expect to have -- well in essence expect to have a magnitude 4.5 event. I shouldn't have used the word "expect" and I want to stand on my prefiled testimony. I think it's a possible event but not a likely event. My prefiled testimony shows likely seismicity of magnitude 3.

JUDGE LINENBERGER: We can return to the question that was raised yesterday, Mr. Knight's answer to which was interrupted for reason of scheduling of other witnesses. So perhaps -- well do you recall the question or do you need to have me review it?

MR. KNIGHT: I certainly recall the general nature and the general subject. I am reluctant to, lest I get off on a

C16pw

tangent, I'm reluctant to try and --

JUDGE LINENBERGER: All right, let's try and prevent tangents and let me see if I can tighten up the question just a little bit.

Are you familiar with Dr. Sobel's figure 1?

MR. KNIGHT: Yes.

JUDGE LINENBERGER: Which shows on a log plot of response velocity versus periods and seconds that the envelope of Monticello event exhibits an exceedance, velocity exceedance at periods less than about a tenth of a second. Energy is proportional to the square of the velocity under certain conditions and therefore I see at periods shorter than a tenth of a percent something that I interpret as meaning that there may be more energy available to cause disruption to the plant than the plant might have been designed to accomodate. I fold that into the consideration that for reasons which I don't understand it is generally assumed that the operating basis earthquake is the only -- and "only" is my word -- about a factor of 2 less in magnitude than the -- do I mean magnitude or ground acceleration? I think I mean ground acceleration, a factor of 2 less than ground acceleration than the safe shutdown earthquake. The operating basis earthquake and I'll put words that are maybe wrong here for sake of emphasis, the OBE is a business as usual kind of affair. We may take a quick look and go right back on line as soon as possible. The SSE is a considerably more serious affair, at least

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if one derives a degree of seriousness from the way the Regs treat an SSE.

So with that observation and the Figure 1 observation and the staff's testimony, I come down to the point that I don't understand the basis for confidence with this kind of exceedance, that components and equipment that are more susceptible to these low periods than structures, are really going to do their job properly.

That in a nutshell is the question I want you to address.

MR. KNIGHT: Let me, if I may, try to address the question in about three parts. The first I think very significant thing to keep in mind is that a demonstration of the exceedance of the design spectra does not in and of itself demonstrate that the equipment in the plant will see an exceedance of its qualification level.

JUDGE LINENBERGER: That comes out of the testimony and I'm asking for the basis of confidence in that.

MR. KNIGHT: And it is several fold. Just through the process of analysis and qualification, it is rare actually that a piece of equipment or a system is in fact stressed, to use stress in the broad sense of the word, if it's a relay or electronic component it will have been tested but it's rare that the equipment will be stressed in that broad sense, up to its capacity or up to a limit at a ground motion that just equals the design spectra.

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JUDGE LINENBERGER: All right, sir, now that's the game. There is a statement, is that an opinion or have you got some data to back it up.

DR. KNIGHT: No, sir. I think if you will--my statement comes from both a great deal of experience in looking at the results of designs for nuclear plants over the years and explicitly looking at the results of the design calculations for the Virgil Summer Plant. I think we need to go even beyond that point in this discussion.

JUDGE LINENBERGER: Well, before you go too far beyond that point, if I took literally what you said so far, I would see no basis for asking the applicant to do any sort of re-qualification program which indeed this testimony says that the staff is going to insist that he does. Now, how do I put those two into a consistent pattern?

DR. KNIGHT: It 's the difference between what is most likely and what is certain. The purpose of the re-qualification program would be to explicitly, item by item, certify that this is in fact the case, that either the design of the piece of equipment as it actually exists--and I should not use the word design--the configuration of the piece of equipment as it actually exists is such that it has capacity in excess of that required for the original motion.

JUDGE LINENBERGER: That reflects some doubt about an earlier statement then, that it is rare that a piece of

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equipment or an instrument ever runs into an overstress.

DR. KNIGHT: Uncertainty only in a sense that unless one in fact goes back and digs into the record or redoes the calculations, you cannot say that, lets say, in the engineering sense, that I can certify that this is the case. It is only that slice--what I would characterize that last measure that we see is the primary purpose of a confirmatory program. Of course, it does have another aspect, that if there is something with an exquisite sensitivity for some reason, we also ferret that out.

JUDGE LINENBERGER: Let me ask about that confirmatory program. I believe the testimony states that the ACRS has indicated and that the staff agrees that this confirmatory program should be completed but it need not hold up the initiation of commercial operation of the plant. Is that a correct characterization?

DR. KNIGHT: Yes, sir, it is.

JUDGE LINENBERGER: I don't want to lay a hypothetical on you but either the confirmatory program has a purpose or it doesn't. If it has a purpose, then that to me implies there is a possibility of it leading to some undesirable results and if the plant is working on confirmatory programs has led to some undersirable results, then there is an awful messy uncomfortable situation that somebody has to face up to and so I wanted to get into that just a little bit.

I can see dollar signs going thrugh Mr. Nichols head on money spent on this kind of program. I can see him saying to himself, well, if we find out something the staff isn't going to like, where is that going to put me because I am already in operation; he doesn't know whether he is going to get a license or not, and we don't either, but lets assume for the sake of this discussion that he is going to get one.

Now, the first point I want to get at is, ACRS said this and the staff said they agree with it; did the staff agree with it because ACRS said it or did the staff take a completely--make a completely independent assessment to see if they agreed with ACRS?

It just so happens that I don't think in many areas ACRS is always on the wave length of the Creator...

(Laughter.)

And so I am wondering to what extent the staff did their own thinking here and analysis and came to this decision that the plant could start operating before they know all the answers about the environmental qualification of equipment?

DR. KNIGHT: At the risk of sounding somewhat bold, I would say that the staff came to this conclusion prior to the ACRS people and in fact went into that meeting with that recommendation.

JUDGE LINENBERGER: Okay. Fine. Now, do you have any comments to make about what the staff position might be

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if one year into the commercial operation, things don't look so good about environmental qualification of equipment, what are your options? You can shut the plant down, you can't unbuilt it, but what are your options?

DR. KNIGHT: I think in looking at the options, it is well to come back to what I believe were developed as a consensus here among experts, that a likelihood of problems arising independent of major civil structures that it would require very extensive reconstruction work or whatever, it is somewhere to extremely low to non-existent, and once you get over--I consider that to be a major hurdle.

Without question, if the staff felt that there was the likelihood that you had to go in to do major structural work it would not recommend that that plant operate.

The second is the major reactor components, massive items such as reactor vessels, steam generators, and pumps and loops that for some reason we would think that--and, of course, at a time which the radiation levels are the highest--if for some reason we felt that some step was necessary there, once again, not trying to make a representation saying we would not support operation of the plant.

It is our very firm belief that we are at a level of equipment, to use the broad term, that is essentially auxiliary to the primary system in the generation of the plant be it whether something should turn up, either instrumentation

or additional piping is required in the system, or increase the size of a snubber, these are all typical things that might occur. These are really no different than the maintenance procedures that are going to take place during the lifetime of the plant. Things are going to wear out and they are going to have to be replaced and the level of activity which would be contemplated would be well within the level of normal plant maintenance and retrofit.

JUDGE LINENBERGER: All right, sir, so be it. Now, I should like to--did I cut you off before you had finished?

DR. KNIGHT: No.

JUDGE GROSSMAN: I thought you had indicated that you had three areas and you were--

DR. KNIGHT: Oh, well, it seems my visual or mental image of the question was different than it was in reality, so--

JUDGE GROSSMAN: That's fine.

JUDGE LINENBERGER: I am looking for a quote and I cannot--I thought had cited it in my notes and I can't find it. Lets change the subject for just a moment while I find what I am looking for.

I want to get into seismic considerations for just a moment. This has been discussed before, it is very explicitly stated at the bottom of page 19 of this prefiled testimony where it is said in the last sentence, "However, where we have

no manifestation of a through going fault, the diameters of clusters should not be used to estimate the area available for rupture."

That statement conceptually bothers me. Let me say why it bothers me. I have the impression and this may be wrong that generally speaking very few, if any, eastern seismic events of any significance have occurred on a previous historically identified fault. That may be a wrong impression. Lets let it hang for just a moment and whoever wants to answer my question can come back to it if I am wrong, but when I think about a cluster of events, I can think of it as defining a zone or region of physical conditions whereby stresses are beginning to make themselves felt and that says to me that that ought to be an excellent way to anticipate the occurrence of a future fault because here is a cluster of events that says something is happening there and sure there is no fault but the people in Washington state will tell you that no significant seismic event in Washington state ever occurred on previously historically observed fault, so what is wrong with looking at a cluster of little events as, even though you don't have any evidence of a through-going fault as potentially an area, or an area of potential weakness that can develop into a fault--and I don't care who answers me here.

DR. REITER: Well, first, with regard to some of the initial statements about known through-going faults, there

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are areas, particularly one area in the eastern United States where seismic patterns and observed seismicity, linear configuration to seismicity, and work, reflection work indicates the existence of a fault and that is an imaginary, Dr. Nuttli I am sure can comment very strongly. We have in this situation here a blob of little earthquakes, several blobs of earthquakes. The nature of what we think the mechanisms for causing reservoir induced seismicity, namely either the load or migration of fluid, is consistent with the idea of that blob and these earthquakes occurring on little graphs.

If, on the other hand, we had a clear--a rupture that we knew was there, we knew was active; if on the other hand, these little earthquakes occurred along the longest plane, then I think we would have to re-assess that statement, and then if we would make, in that situation, where you have to be careful and make a careful determination as to what would be the appropriate dimension of that cluster to use in estimating what the size of the earthquake is.

Lacking all those, we just don't think it is appropriate to do that kind of study in this particular case.

DR. JACKSON: I might like to add a comment or two. I think the generalization that earthquakes in the eastern United States are not identified with faulting. It is really too much of a general statement. I think the key word is recognized. We don't for the most part as a geological

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geoscience community look that carefully, so I think the key word is recognize. I think all of the more significant earthquakes in the eastern United States can be related in a general way. I know that we could probably get into a long debate about that, it can be related in a general way to some structure.

JUDGE LINENBERGER: Excuse me, that existed prior to the event?

DR. JACKSON: This is prior to the event. Take as an example, to bring it back to--it is the earthquake that occurred in eastern Kentucky. The first indications are, well, that is just a random earthquake that occurred in an area where we wouldn't have known--this was the July--what is the context of time?--1980 event, and, but when you really begin to look hard at the seismicity patterns and the geology structure at depths beneath that, you can see something there.

One other point, I think this comment has been made in previous proceedings that, you know, all faults, all new faults have to be generated some time, and all old faults have to be generated some time, and it is a common comment made by geologists.

The problem is, I think, here you don't have that kind of formation taking place. The faults question I think Judge Grossman raised a couple of days ago, was the fact that were these formed--or maybe you did--were these faults formed

at the surface and based on their nature and character, they were more than likely formed when this rock was buried tens of kilometers beneath the ground surface where you see it now, and is exposed by erosion, so you really don't have those zones of potential weakness there. Joints and cracks which you can't rule out that they won't join eventually and become a through-going fault but current tectonics, I think the judgment is that that is not the case.

Another minor point is that in talking about these earthquakes as being unique here, I think one of the problems is we have instrumentation, and we are seeing these earthquakes, and these earthquakes fall in the level of background seismicity and they are probably occurring everywhere and there is a favorite saying around that the instruments attract earthquakes and I think that's the case. There is a constant level of background seismicity that is occurring throughout the eastern U. S. probably at the level here, although the reservoir has accelerated that to about this magnitude, that we just don't see most of the time.

JUDGE LINENBERGER: Okay.

DR. MURPHY: One comment on blobs. There are two parts to the argument that I presenting in using the 3 kilometers. One, that there are these clusters and that nominally there is three of them at this stage, the applicant at various stages has indicated as many as six, just breaking them off in different

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groups, but some of these groups have remained as--at least three of them have remained as independent clusters I think over the time period the seismicity has occured, but the dimensions of these are on the order of three kilometers, maybe a little bit more, maybe a little bit less for some of them, and during some of these swarms or clusters of activity, that these blobs have acted as blobs, as blobs of these dimensions and indeed there may be through-going faults in the area and may not. It may be possible that at some future time, Dr. Talwani takes this data, puts a PhD student to work on it for two years and finds some indication of through-going fault. That is in part speculation at this point. Part of it is that these blobs that have been sitting out there since January of '84, just sort of popping off and they've been popping off in groups that have scale dimensions that are larger than 1 kilometer.

JUDGE GROSSMAN: I believe you misspoke. You meant January of 1978.

DR. MURPHY: Sorry. I do that sometimes.

JUDGE LINENBERGER: Thank you, Dr. Murphy.

Dr. Sobel, at page 15 of the prefiled testimony, the middle of the first full paragraph, there is a statement about staff conclusion having been, in fact it says, "Our conclusion was" that activity, seismic activity continued to warrant careful attention because there is no reason to assume

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that the largest event induced by the Monticello reservoir hasn't already occurred. That statement is in the past tense, has that conviction or conclusion changes or is it still the position of the staff that there is no reason to belive that the largest event has yet occurred?

DR. SOBEL: I believe that for the reasons stated in the sentences following that, that we still feel that the activity warrants careful attention, that is why we have asked that the monitoring be continued.

JUDGE LINENBERGER: Okay. I thought I heard Dr. Newton earlier express his opinion that he wouldn't expect to see any larger events occur in the future than have occurred in the past, and I just am wondering if there is a diversity of opinion or if I heard something improperly here and I am not trying to--I am not really trying to cause trouble per se, I just want to understand whether there is a difference of opinion.

DR. SOBEL: I think the staff is in agreement that we considered Dr. Newton about earthquakes and their shallow zone seismicity to be true. We consider it unlikely that any events greater than magnitude 3 would occur, but we are less certain of the mechanism that would induce a larger event and since these events have been known to occur up to 10 years after a reservoir filling, we want to continue the seismic monitoring.

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JUDGE LINENBERGER: Anything ten years after reservoir filling brings up the question of after shock and some of the discussions we have had about what is one and whether it should be included or not and I came away with a bit of confusion here because it seems to me that if one is looking to make an assessment or prediction of probability of a random event in excess of a certain magnitude, there may be a reason not for looking at known after shocks from a historical point of view in order to make a futuristic projection. On the other hand, if one is looking at certain--I don't know wave propagation phenomena or ground behavior phenomena or whatever, there may be excellent reason for wanting to study after shock, so I've got to my mind make a categorical statement about including or not including after shocks is too sweeping and must be asked, what is it you are trying to get at ultimately before you decide to include or exclude it, is that--am I right about that or--

DR. REITER: I think you are absolutely right, and the context of discussion of after shocks was whether to include them in models which assumed that earthquakes were occurring random with respect to time and place and in my judgment, it seems that it is better to eliminate them, and there is controversy about this, and I wanted to point out that different people have different approaches to it, but with respect to study of aftershock, you are absolutely right. In fact, that

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is how we very often with our instruments we miss the large shocks and teams go in and study after shocks and we have learned a tremendous about the earth and the earthquake source from the after shock.

JUDGE LINENBERGER: I think I have detected perhaps more than an undercurrent passing in various directions here with respect to deterministic versus probabilistic analyses, and I don't fully appreciate why there should be an undercurrent or even a current, if you will. It seems to me both approaches have to start with historical inputs and each approach has its merits depending on what you are trying to get at. Now it is true that anybody can diddle with history anyway they want to, lets leave out biases that go into how you pick historical events, that sort of thing, but just in terms of methodology, would one of you just very briefly summarize why is there a controversy over whether one approaches things deterministically versus probabilistically? You people decide who comments.

DR. JACKSON: We probably all will have something to say.

JUDGE LINENBERGER: Well, this may take longer than the first one.

DR. JACKSON: I will try to be brief then. In the context of this proceeding, I don't think it is a significant element except for Dr. Trifunac's testimony and weight that

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the applicant would -- in the context of the ACRS questions that were raised and we have not given great weight. I think the undercurrent is, well, an undercurrent, it is a problem of how you deal with determining ground motion or other parameters in the geosciences area. Probabilistic method is one way of doing it. I think if we had--there is a perception that the use of probabilistic methods is a cure-all and that there is, one way of doing it and one way of selecting the parameters that go into it and therefore, every expert who takes and says what is the probability of the occurrence of a ground motion at point X will come up with the same thing, even using the same inputs, some of us have a different way of calculating it and great disagreement on what those input parameters are, so what happens is, those numbers--and I am talking about it from a licensing perspective, and the problems we have--we really encourage probabilistic studies, and we use them because they help greatly in putting other judgments that you have to make in perspective and the deterministic judgments that we must make.

On the other hand, the probabilistic studies can have the fact of only being used in decision making, especially as you get removed from the person who did the study, the individual who did the study's views.

For instance, it is a factor, you know, one in a thousand is thrown about as certainly an acceptable level for

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SSE, or one in ten thousand, and none of the caveats or the methodology that was used to get to that number is carried along with it, so our concern from a licensing point of view in the geosciences area is abuse of that system.

If there is a willingness to take the uncertainty with it, Dr. Reiter mentioned yesterday order of magnitude probability calculations for an SSE for a given facility and Dr. Okrent has made many observations of this.

Until we can agree, I think that the undercurrent that you are talking about is a perceived, that everyone would love to embrace it and use it for certain decisions.

On the other hand, they don't see the depth of the decision on the parameters that go into it. For instance, the other day, we were talking about attenuation parameters and I mentioned it yesterday in answer to Judge Grossman.

In the systematic evaluation program study, the attenuation parameter became so difficult that a special panel had to be convened to try to come up with a consensus judgment as to what should have been used for that, so I think seismic zonation, what zone an expert has. In that particular study, I believe 10 experts were solicited and each one of those ten drew a separate seismic zonatin map which was, everyone of them, different than the other, and some closer than others as to the earthquake zonation would be, so I think that is all, goes into this question of probabilistic aspects.

JUDGE GROSSMAN: Let me state right now that I agree Dr. Trifunac did bring probabilistic considerations into his testimony, but that he did not bring it into the case. We did not tell him any more than what was in the record at our meeting in July and I didn't particularly want to get into that kind of area but that was his judgment as to what he was going to do and so I don't think that had changed the parameters of the case at all, and so I don't really think it is profitable to go much further in this area.

DR. REITER: Judge Grossman, I might add that three of us in this jury box last year sat in a court room in New Hampshire and spent the week with the Appeal Board on Seabrook discussing this very issue and Dr. Chinnery's article in particular and Dr. Trifunac was a Board witness and that ruling has not come out yet, so to the extent that the discussion of all these issues are in that transcript.

JUDGE LINENBERGER: Okay, I found what I was looking for and I am sure it is a knit pick, but at page 12, the last sentence in the paragraph that concludes at the top of the page, in essense says in summary, a lack of conservatism in one or more areas of seismic design of a facility is not necessarily critical--not necessarily critical in its ability to perform its safety function.

The other side of that coin is--it could be very significant. Now I know I am quibbling with the words here.

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Is that a poor choice of words, was that a poor choice of words or is it expressing an area of ignorance or are you just saying it is not necessarily critical that you would not be conservative and yet an awful lot of lip service is paid to design conservatism these days, so is the emphasis on design conservatism overplaced or this statement slightly out of context?

End take D

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DR. KNIGHT: In the context of that statement--well, let me turn that around. I would say that our intent, if you will, is that that statement be looked at in the context that there are a number of individual steps, a relatively large number of individual steps between the definition of the ground motion at the seismological level and, let's say the size of the structural member that the designer finally settles on; and that, just to pick a number, out of eight steps, each of which has some margin in it, if you will, some conservatism, if one of those out of our lack of knowledge or any other reasons should happen, not intentionally, but should happen to be unconservative, that that one step will not color, if you will, the ultimate outcome. You would still end up with an acceptable end product.

JUDGE LINENBERGER: So, you're saying the intent of the statement was that the lack of conservatism is not necessarily critical, but there are so many elements of conservatism that if one of them goes bad you'd still have plenty left, if one of them goes sour?

DR. KNIGHT: That's correct.

JUDGE LINENBERGER: Okay. I didn't quite read that into the statement.

DR. KNIGHT: I don't wish to be gratuitous, but there is a down side to that and I would mention it from time to time. One of the goals I see as both engineers and scientists and regulators is to continually review the process that we in the

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industry have come uponto see if we can limit as is prudent the pileup of--and the word's gotten overused, as you say--conservatism to margins that do occur, because in some instances we end up literally with the tail wagging the dog, particularly in the seismic area.

JUDGE GROSSMAN: I think this is probably a good time to break for lunch, and we'll make it a short one, until 1:20, about 45 minutes, or is that not time enough? I think some people want to get out of town. Okay, 1:20.

(Whereupon, a recess was taken for lunch, the hearing to resume at 1:20 p.m.)

JUDGE GROSSMAN: Back on the record.

Judge Linenberger is questioning, and I'm not sure that he has concluded. He has, and now it's up to Judge Hooper.

JUDGE HOOPER: I guess I only have one basic quarrel with the Staff, and I guess this really goes to sort of a basic inconsistency in how they choose their data.

I see that they are using magnitude 3 within 2 kilometers, and in doing this you take the small magnitudes from Professor Talwani's data on this site. Yet when you want to do something else, you reach out and say "Well, it's okay for us to pick up the Clark Hill Reservoir and use it when we need it for setting the maximum magnitude," and it seems to me that if you're going to do this on some occasions and not on other occasions, my question to you is why don't you take as a basic set of data

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all the earthquakes in this whole province that we have here, where we have three reservoirs, all very much alike, all in the same kind of materials? And it seems to me this is a very logical array of data. But one time you say "Well, I'm going to use this" and "Now I'm going to use this," and so this is sort of a basic inconsistency.

For example, you say 2 kilometers and magnitude 3. On the other hand, you can go down the way here and find a lake where you can get a 3.7 with something between 2 and 3. You pick up all sorts of depths within this array of data.

And I haven't heard the Staff or the Applicant really telling me in a very convincing way why we should pick on this reservoir and exclude these other reservoirs. I've heard some data that "Well, maybe this soil is a little bit different."

But if you go back to the situation, you say "Well, Clark Hill had a 4.3," but if you applied some of your arguments you would say "The stresses were building up there for maybe 25 years and finally were released."

So, my basic problem is in the way of consistency. It seems to me that there is a very logical array of data of all of these events in reservoirs in this homogenous area which would be a probablistic basis, a very legitimate set of data.

Now, I ask you--I'd like to ask the panel: Why haven't you considered this as some sort of probablistic way, both as far as depth, distance, magnitude and et cetera?

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DR. SOBEL: I'm sure the others will want to comment, but I'll try to give you my understanding of how we've used the data from the various reservoirs in the Piedmont in our ground motion estimates.

First, in terms of ground motion data, the only ground motion data we have at a reservoir from an accelerometer is at this reservoir, and so we've chosen to envelope all of the spectra that have been calculated from recordings at this reservoir.

Secondly, the maximum intensity that's been associated with any of the reservoirs in the Piedmont is an intensity 6, and any ground motion expected from that would be less than the SSE intensity, which is intensity 7.

JUDGE HOOPER: Excuse me. Why do we list it? 3.7 is equivalent to 6, is that what you're saying now?

DR. SOBEL: Approximately a magnitude 4 would be the equivalent of an epicentral intensity 6 event.

JUDGE HOOPER: All right.

JUDGE GROSSMAN: That intensity, that's not the Clark Hill intensity, is it?

DR. SOBEL: Well, there are several reservoirs in the Piedmont that may have been associated with intensity 6 events. This is based totally on felt effects.

JUDGE GROSSMAN: My question only went to whether you were excluding Clark Hill from your resonant maximum intensity 6.

DR. SOBEL: That would be one of the reservoirs that

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would be included in the list of reservoirs in the Piedmont that have some intensity event associated with it.

Thirdly, in terms of that, the maximum magnitude associated with any of these reservoir, the maximum magnitude we believe would be roughly a magnitude of 4 associated with one of the historical intensity 6 events.

JUDGE HOOPER: Well, that's all fine, but then I'm saying why did you come back and say "Well, we're going to have magnitude 3 with an upper limit on depth of 2 kilometers"? That's my question. Maybe Mr. Reiter?

MR. REITER: No.

JUDGE HOOPER: This is the basic point of inconsistency. In other words, it goes back to the Staff has in the past set aside what they call geological provinces and said "Well, we're going to set up a certain array of events here and things that are happening in this area." Well, now, here's an area, and how do you exclude it as being the basis for setting up some sort of a system for characterizing reservoir-induced earthquakes? You have a legitimate area, set of earthquakes, and I fail to see why you are inconsistent and not using this whole array.

MR. REITER: Judge Hooper, it's our impression, and I think it's correct, that we are entirely consistent; and I'd like to explain how. But that doesn't mean that every estimate was derived in exactly the same manner.

For instance, if we would follow the current crowd's

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approach to reservoir-induced seismicity, we would not be sitting here. When we say the maximum intensity of 6, the maximum intensity on the relationship that we use for ground motion is .065 g, as determined by a relation to Dr. Trifunac's rating. By the way, the Clark Hill Reservoir was probably an intensity 5.

So, if we were to do just that alone, then we would not be sitting here, there'd be no problem whatsoever.

JUDGE HOOPER: I'm aware of that. Tell me something new.

DR. REITER: The next question, how have we utilized data. As Dr. Sobel has pointed out, there's only one set of ground motion data, and we've enveloped that. And that is not at 2 kilometers, but it's at .8 kilometers distance.

The second point is that in deriving the maximum magnitude, which is not 3 but approximately 4.5, we went to the regional historical data of Piedmont Reservoir or postulated reservoir-induced event, and the maximum was 4.3 for Clark Hill. We rounded that to 4.5.

Third, in estimating the sensitivity of our assumption of 3, which I'll get at later on, we went to look at Dr. Nuttli's data; and we noticed Dr. Nuttli's curve is based in large part on reservoir-induced events, Monticello and Jocasee. That's how we arrived at a magnitude 4 at the depth that we used, 2.3 kilometers.

And finally the reason -- our best estimate for the shallow zone of magnitude 3 was because of the of the site-specific data that they had available at this reservoir which was not available at any other reservoir.

JUDGE HOOPER: I can appreciate the fact that you had to find some sort of an envelope for your data. On the other hand, it seems to me that when you're talking about your reservoir-induced data, don't you -- didn't you set an upper limit? I heard somewhere this morning, you were talking to the Chairman on an upper limit, magnitude 3, upper limit magnitude 3 and a depth of 2 kilometers.

DR. REITER: That's right.

JUDGE HOOPER: What are those figures for?

DR. REITER: That was based on Monticello, but the distance which we used to estimate the ground motion was the envelope of that recording, essentially determined by an event at .8 kilometers from the recording site.

JUDGE HOOPER: I'm aware of that, but what I'm saying is that why did you select 3 and 3 kilometers when if you have used the data from this whole series of reservoirs you might come up with, with quite a different set of numbers instead of the 3 and 2 kilometers.

DR. REITER: We did use the data. That's how we came up with Dr. Nuttli's estimate of 4.0 and 2.3 kilometers. That could -- The curve is controlled by several events,

several of which include Piedmont-induced (sic) events, namely two events at Monticello and another event at Jocassee.

DR. JACKSON: Judge Hooper, maybe we're misunderstanding you totally, because I think that we have handled these in a consistent manner.

Maybe I could try to phrase your question and see if I understand it properly.

You're saying since you had a magnitude 3.7 at Jocassee or whatever reservoir --

JUDGE HOOPER: At a different depth too, at about 2 1/2.

DR. JACKSON: Okay. And now you're saying this upper -- we're limiting it to 3 at Fairfield Dam area, why is it, isn't it inconsistent based on what we know about Piedmont reservoir; is that your question?

JUDGE HOOPER: No. It's -- well, it's more than that.

You see, here's the thing. Dr. Reiter says well, we're, we scale it up to 4.5, but you've also scaled up the depth, and so that you're not being consistent with the set of data from the Piedmont in this sense, and now furthermore I would like to sort of take a little argument here on another little point.

I don't know whether this is -- comes from being in a different scientific area, but I haven't heard today and I haven't seen anything that would convince me that there are any theoretical reasons for this eastern magnitude limit that Professor Nuttli has in his curves, and I haven't heard Mr.

Reiter, Mr. Reiter hasn't given me any reasons, theoretical reasons for this, it's true it's an empirical data set, I have no quarrel with that, I have no quarrel with using empiricism in things like this, but I sometimes question the -- how overpowering it may be as a, as a choice in the case of making a decision, so that here I come down to the fact that you've determined the, you've used Professor Nuttli's information, you say "Well, we're going to -- this disagrees with Clark Hill, therefore we've got an upper limit," and then you say "Well, since he has said the, what the depth of of these things is we're going to use, we're going to use that number too," you've drawn that out of another box, so, uh, from 6 to 16 is what I'm talking about, so you've taken that out of another box, so that, that's the type of inconsistency that I see in the whole, your whole approach.

JUDGE GROSSMAN: Okay. That was a long question, and I see a number of questions in there, and so I just want to make sure we get answers to all of them.

One question that Dr. Jackson put his finger on was the -- why don't we include the Lake Jocassee 3.7 in the RIS considerations for Summer;

Secondly, why don't we put the 4.3 in the same consideration which may or may not have been reservoir-induced;

Thirdly, why don't we put the tectonic earthquakes at the same shallow geometry for those either postulated as RIS

or pretty clearly assumed to be RIS events.

So there are at least those three questions in there.

Dr. Jackson, you were about to answer at least with regard to the 3.7.

DR. JACKSON: No, I was not. I think Dr. Reiter could answer that better that I could.

JUDGE GROSSMAN: Okay.

DR. REITER: I really think there's a misunderstanding of what we've done, Judge, and I'll try to clarify it and if not maybe we should go over it again because it's fundamental to the staff's approach.

I really think we have been entirely consistent with our approach at doing it except for the point of probability, and that's a separate issue, I don't want to go into that at this point.

What we -- The depth of Jocassee, that event was considered, and it was considered at the depth at which it occurred, and that event was part of the enveloping curve used by Dr. Nuttli in drawing his curve for size events and depth, and we took that and we took the magnitude 4 even for there and used that as the maximum, as the test case for our observations.

We took the data observed at the Jocassee, again we took the envelope of it, we said we think our best estimate of the 3 is based on various lines of data, -- sorry -- the best

estimate of what the maximum magnitude is based on various lines of data, and those are based on the local measurements, local seismicity probably more than we have at any other reservoir in the Piedmont and maybe in the world, and that's how we arrived at estimate three.

We then realized that that may not be sufficient, and then we began to say let's test this assumption to what has happened, to the maximum of assumptions that have happened at other reservoirs in the Piedmont than any other place in the eastern United States.

Then we look at magnitude 4, and aside from that we then said let's worry about an event which occurred at a Piedmont reservoir which may or may not be induced by the reservoir, and that's the magnitude 4.5.

Now, what we've not done is taken those events and extrapolated them arbitrarily to the shallowest possible depth.

JUDGE HOOPER: My quarrel is not with your magnitude for use, I can see these things, but I guess it's the -- it's the inconsistency of when you turn in, when you use one depth and when you use another depth, and that's -- that is -- I guess that's where I -- I can't -- I detect some sense of selecting events here.

I'm not sure -- I understand your basic, what you basically have done, I have really not very much quarrel with it about where you came out. I think it's, it's more the

logical data set that you start out to use and how many of these deviations -- The thing I can't decide is how many of these deviations are really necessary and how many of them were -- some of them were ones that were a matter of convenience, I guess that's really my worry.

DR. JACKSON: Judge Hooper, I think I might -- it's a problem that we have in this science, or as Dr. Blume indicated the other day in this art that we're in that we don't -- we have to use very limited data sets that we do have.

The fact that we have such limited data sets, on the other hand, should give some reassurance that this isn't a phenomenon that has to be worried about as much as say earthquakes in California, so that the limited data set is a reality and we have to make the best judgment on that, and if you -- if you try to -- I think a number of times in this testimony it states that certain things are beyond the state of the art or at the state of the art, and it's my personal opinion in the past year this site and the hearing process, not the administrative process, ACRS included, has advanced the state of the art in reservoir-induced seismicity, we're pushing the state of the art forward in this hearing process.

And I don't know, as a regulator as well as a scientist I think we have to be aware of that, and we're making judgments, and if you start -- We as the staff tend to be a lot more conservative than applicants would like in that we are unwilling

to make those state of the art judgments or give them full credit as you can see here by our safety evaluation report and the testimony.

I think we still have not answered the questions posed by the judge.

The three-seven Jocassee is included, I think that's answered.

One question we haven't answered is the tectonic earthquake and reservoir -- take the reservoir-induced earthquake and assume it occurs at the site. Is that a -- I think that was addressed earlier this morning in terms of availability of stored energy and stress.

JUDGE HOOPER: I don't think I ever said anything about tectonic earthquakes being at the site.

DR. JACKSON: Okay. Judge Grossman.

There was a second point which I missed.

JUDGE HOOPER: I think my only -- my only question would go to the matter of the distance, and depth distance, and I have no quarrel with your magnitudes that you used, I think they are very conservative, but I guess my quarrel is I can see why you had, have had to get a spectra from a particular array of data, but I guess my quarrel is how you ended up with a depth that may or may not be consistent with the -- as a matter of fact, it may be too large, I'm not being an advocate one way or the other, but the -- I'm certainly not advocating anything about

putting a tectonic earthquake here, so I guess my only residual question is one that has to do with another matter here.

It seems to me there is a little bit of disparity between some people in this hearing in regard to the significance of the bore hole data that you have achieved, you found.

I take it that some of your panel have used this as a, paying a great deal of attention to this, but I've heard other members, other people here who haven't. My question is, are these the only two bore holes that have given any stress data from this part of the Piedmont, stress data in the sense that Zoback has used them?

DR. NEWTON: I would say yes, in the Piedmont. Zoback went over to Charleston and had some bore holes over there; otherwise, the Piedmont measurements usually come from over-coring stresses rather than the bore hole stress measurements.

JUDGE HOOPER: My question, then, Dr. Newton, is that how can you have this much faith upon two bore holes in a rather heterogeneous area? What -- how do you have faith that the stress phenomena that you have talked about are representative over any great area from two bore holes?

DR. NEWTON: I think it was pleasing to know that they went to the area where the earthquakes were occurring rather than just measuring some regional stress field, they went to the area where the earthquakes were and put the holes down there and measured the stresses there.

JUDGE HOOPER: But you're taking, you're giving considerable weight to this stress phenomenon, aren't you, as an explanation for quite a few things regarding seismicity of the area, the whole area.

DR. NEWTON: I don't know I'm putting a great deal of weight on it; I give it some value.

JUDGE HOOPER: My question really is how do you know that this is a typical, this is typical of any great area, or do you know this, or don't you know that?

DR. NEWTON: No, it's not typical of any great area, it's typical -- it characterizes where the earthquakes are occurring and the principal stress directions vary from these measurements to other places in the Piedmont and Coastal Plain.

JUDGE HOOPER: Do you detect then it would, that it -- does it exclude stress releases of a greater sort in other areas within Talwani's dome where he has looked, has found this microseismicity?

DR. NEWTON: There are other stress measurements at the site, those made in the foundation or under the foundation, but they're all very shallow and I think these are the best measurements, and I would apply them to the area where the seismicity has occurred.

DR. JACKSON: Judge Hooper, I'm not sure that was your question. I interpreted your question to be can you based on the stress and these two bore holes, do you have confidence that

over this region the stress level is the same.

JUDGE HOOPER: That's exactly right.

DR. JACKSON: I think that we've got two bore holes, and like it or not we've got two bore holes, and I think from the stress measurements made elsewhere in mines and other excavations in the Piedmont, and there are stress measurements throughout the Piedmont, a number of them in reactor excavations, that it's reasonably consistent across the area.

Now, obviously because of the reservoir loading and the water lubrication or core pressure induced by this reservoir it does set up a different situation ability for the rocks to relieve their stress.

JUDGE HOOPER: Then you told me something new which is what I've been wanting to hear. There are some other data which tend to substantiate your two holes in the stress field?

DR. JACKSON: Let me make a comment. There is -- If you'll bear with me one minute I'll bring it back to the relevance.

There is an ACRS member who favors the use of stress measurements at all reactor sites as a basis upon which to make the final decision on the seismological parameters.

Now, the problems with stress measurements make the problems with ground motion look small in comparison.

The problem we've had is stress measurements can be made in a multitude of different ways using different equipment

and different techniques.

Most stress measurements in the Piedmont, for instance, would be shallow, either on the rock surface or a surface that's ground off and measured as Dr. Angler has done up in New England, or shallow drill holes of on the order of 15 to 20 feet and an inference made from them to deeper stress.

I think that these are the only two bore holes that I'm aware of, and I imagine there are many in the oil industry that have such measurements that actually hydrofract, do actually hydrofract testing of a deep bore hole, and I might add somewhat gratuitously that these bore holes were done by the USGS under NRC research funding for the earthquakes hazard reduction program as part of the overall program, but I think there is a consistency, but I don't think you can make from those bore holes comments about regional stress. I think they're reasonably consistent, but I think you're very limited by the data you have.

JUDGE HOOPER: All right. I appreciate your frankness on this matter. I think that clears up a lot in my mind. Thank you.

JUDGE GROSSMAN: By the way, to set the record straight, I wasn't posing any further questions to you, I thought there was some that were implicit in the overall question asked by Judge Hooper, and I tried to phrase it may way, and perhaps I should have stayed out of that.

I have heard references to the highest magnitude earthquake that occurred in the Piedmont province once or twice during the testimony, but we never had any statement as to what that was, and the one that's not associated with reservoir-induced seismicity. What was the highest magnitude earthquake in the Piedmont province?

DR. SOBEL: The largest earthquakes that have been observed in the Piedmont were historic events for which there are no instrumental recordings, and those were of intensity, epicentral intensity 7, and we generally assume that they would correspond to about a magnitude 5.3.

JUDGE GROSSMAN: Okay.

DR. JACKSON: The closest I believe -- I may be wrong, but the closest one to this site would be Union County, South Carolina earthquake, and that's a 7. That's what's been used as the SSE for the sites in this region.

DR. NEWTON: I reported to the ACRS that looking at the magnitudes either based on the surrounding area or recorded by instruments that the magnitude 4.5 plus or minus a half represents the maximum tectonic earthquake from the Chesapeake Bay down to Alabama, the Gulf Coast.

DR. SOBEL: Let me make a minor correction to Dr. Jackson's statement. The Union County, South Carolina earthquake was epicentral intensity of 6-7. There are slightly larger events of intensity 7, however, in the Piedmont. This is

according to EARTHQUAKE HISTORY OF THE UNITED STATES by the National Oceanic and Atmospheric Administration.

JUDGE GROSSMAN: By the way, I don't want to inject anything at the last minute, but we did receive a paper that was served on everyone from the GEOPHYSICAL JOURNAL which had an intensity map for the Charleston earthquake, and just by looking at that map without much investigation it appears that it was an intensity 8 from Charleston around the site. That was a map by Bollinger I guess, an isoseismic intensity map.

Do you have a comment on that, Dr. Reiter?

DR. REITER: Yes. That was not from Charleston. Those are the historic earthquakes.

I think perhaps the question of the applicant to Dr. Chinery's data set was that those earthquakes did not just seem -- some of the intensities of those earthquakes did not seem consistent with U.S. earthquakes, and they were questioning whether Dr. Chinery knew the data set was a modified Mercalli. I think that's one of the reasons perhaps the applicant was getting at.

Those were not aftershocks, those were all earthquakes in the region.

JUDGE GROSSMAN: I thought that was specifically Charleston.

DR. REITER: No.

MR. KNOTTS: No.

DR. REITER: We can get the figure and reference the article by Chinery.

JUDGE GROSSMAN: But it's earthquakes in the south-eastern United States, and the date on it if you want me to be specific about that --

DR. JACKSON: Is it the Figure 2 from the Chinery paper that you're referring to?

END OF F

Glpw

JUDGE GROSSMAN: Well let's --

DR. JACKSON: I want to make sure we're talking about the same thing.

JUDGE GROSSMAN: Okay, fine. First of all, this was a document that was sent to us after our hearings during the summer and served on all the parties. My recollection about the article was that it mentioned a line of site change in Columbia from the Charleston earthquake and there was a map that was in there and I didn't check to see the descriptions of the intensity levels but I assume a change -- a line of site change perhaps calls for an intensity 8 and maybe that was why there was that area with intensity 8.

DR. REITER: I stand corrected. You're probably referring to the Seaber and Auk (?) report.

JUDGE GROSSMAN: I believe so.

DR. REITER: It was their interpretation of the data and they were making interpretation of intensity and you're correct it was their interpretation of intensities such as in Charleston earthquake. I stand corrected, we're talking about three different reports.

JUDGE GROSSMAN: Okay, and I believe it was a map by Bollinger in 1966 -- no, that doesn't sound right, 1976 or perhaps -- but in any event, I assume the staff took into account all these recent speculations or questions as to the intensity felt at or near the Summer site from the Charleston earthquake.

G2pw

DR. REITER: I think that's really correct and Judge, here's the problem that we have with intensity and ground motion. I'm sure Dr. Nuttli could comment on this also. It's inappropriate to use ground motion associated with intensity at near distances to assume you would get the same, particularly peak acceleration as far as distances. We have to be careful about that. I'm not even saying whether it was intensity VIII or intensity VII but one has to be careful in making those kinds of , converting those kinds of estimates to ground motion.

JUDGE GROSSMAN: I understand what you're saying but I don't understand the relevance here because you weren't talking about epicentral intensity, we were talking about intensity at the Summer site from the Charleston earthquake and I was just asking whether you considered those recent conclusions that some people had that there was an intensity VIII at around the Summer site or perhaps I'm off in my geography, from just eyeballing that diagram.

DR. JACKSON: Let us talk for one minute.

(Discussion off the record.)

DR. SOBEL: This matter was reviewed earlier on the OL review and we had considered those observations of intensity near the site from the Charleston earthquake and they were about intensity VII or less, and the gound motion calculations that I reviewed were in the context of a large intensity X event near Charleston in terms of what the ground motion at the site would

G3pw

be from that event and I found that the peak acceleration to be used as the anchor point for the OBE should be .10 or less and the Charleston event is the basis for the OBE design spectrum.

DR. JACKSON: I might add just a brief comment, that intensity has inherent in it side effects and the intensity observations in a regional isoseismer like that and the contouring regional isoseismer would necessarily note those areas in which you had highest intensities which would be the worst soil conditions. Then you draw a contour of those isoseismers. So you have inherent in the isoseismer map that aspect.

JUDGE GROSSMAN: I was really just asking whether you considered the speculation that there might have been intensity VIII around the site and if you did, that's the answer to the question. And I take it you did, Dr. Sobel?

DR. SOBEL: Yes, we did.

JUDGE GROSSMAN: Mr. Goldberg, redirect?

MR. GOLDBERG: No questions.

MR. KNOTTS: No questions, Judge.

JUDGE GROSSMAN: Well thank you very much.

(Panel excused.)

MR. KNOTTS: As soon as the staff panel have had an opportunity to return to their seats, we'll recall Doctors Alexander, Blume, Martin and McGuire, who have been previously sworn.

Whereupon,

G4pw

GEOFFREY MARTIN

JOHN BLUME

ROBIN MCGUIRE

SHELDON ALEXANDER

were recalled as a panel by and on behalf of the Applicant, in rebuttal, and having been previously sworn, was examined and testified as follows:

JUDGE GROSSMAN: Mr. Knotts, your panel has resumed it's place, is that correct?

MR. KNOTTS: That is correct and all of the gentlemen, Doctors Alexander, Blume, Martin and McGuire have been previously sworn.

JUDGE GROSSMAN: Does the reporter have them in order?

THE REPORTER: Yes.

JUDGE GROSSMAN: Fine.

DR. MCGUIRE: Thank you. Dr. Martin will give the first part of our presentation, after which we will invite questions.

DIRECT EXAMINATION

DR. MARTIN: I will address two items in my rebuttal. Really the first item is the matter about staying in business which resulted from a question by Dr. Trifunac yesterday where I agreed to show a view graph for a Monticello like earthquake record propagating up through a small column, and discuss the transfer function question. Perhaps I can show that view graph

G5pw

now.

JUDGE GROSSMAN: Yes, please.

MR. KNOTTS: Do we have paper copies of that?

VOICE: No.

MR. KNOTTS: May we supply the paper copies later, Judge?

JUDGE GROSSMAN: Certainly.

DR. MARTIN: You may recall on the presentation I made of the vertical propagating SH wave analysis that the results represented in terms of Fourier modulus plot and a transfer function. Looking at the transfer function for these plots, they all have the same characteristic shape and it was observed by Dr. Joyner that for a linear or near linear systems that the shape of the transfer function will be independent of the frequency character of the earthquake record. We agreed with that observation.

At the same time, Dr. Trifunac observed that for the transfer function chart, the values in the frequency range of 20 to 25 hertz was slightly less than 1. In his view, this would mean that the accelerations recorded on the surface in that frequency range could be slightly greater at the bedrock level. In response to that question I indicated that the studies we had done with Monticello type records which had high frequency ranges, the transfer function was more closely equal to 1. The question then arose as to why there was this difference

G6pw

and I think this view graph will explain it. This shows a rock input motion more characteristic of a Monticello type event. One can see that the frequency characteristics for the 20 to 25 hert range dominates the motion. The other analyses which were done the dominant frequencies were lower, in the 10 to 15 hertz range. This is the surface motion and for this particular case , the input maximum acceleration was 0.15 g. The output motion was 0.17 g. linear site magnification. This was reflected since the transfer function in the 20 to 25 hertz range was close to 1. However, the interesting observation here is that the transfer function, whereas it is very similar to the transfer function for the other analyses in the lower frequency ranges, diverts or differs from the transfer function from a frequency of about 17 hertz onward. The reason for this is that when we did these analyses we did a more refined analysis. In the earlier analysis we simplified the analysis by assuming pure equivalent matters representing the various layers of the stratum. Since we were dealing with higher frequencies in the study, we needed to put in more sub-layers in the analysis to get a truer representation of the column continued. This resulted in greater accuracy in the transfer function in the higher frequency range. That is, the transfer functions reported in the original testimony were in error in the high frequency range from 20 hertz on. These in fact are the correct transfer functions for this system in the high frequencies and one will

G7pw

note that in the 20 to 25 hertz range, there was exactly equal to one. The inference I get from this is it doesn't conflict with our conclusions; that is, there will be little magnification at 20 to 25 hertz range for Monticello type events.

By the same token, I can conclude there will be no de-amplification as suggested by Dr. Trifunac.

MR. KNOTTS: Dr. Martin, I take it that this presentation deals with the soil only, is that right?

DR. MARTIN: That is correct.

MR. KNOTTS: Thank you.

(Dr. Martin returns to his chair.)

DR. MARTIN: I would like to now comment by way of rebuttal on the Monticello accelerograph amplification question. Dr. Trifunac indicated that he did not feel the problem was significant and worth pursuing. I must totally disagree with this position. I cannot emphasize strongly enough the seriousness of the implications of the pull back test results and feel I must again emphasize the nature of these observations.

I would like to first emphasize that despite the short period over which the tests were performed, the tests were very carefully carried out. The tests may sound somewhat improvised; however the test procedure is a recognized standard approach to studying this type of problem. Dr. Woods designed the experiment and I have complete confidence in the manner in which he carried out the tests and of the accuracy of the results.

G8pw

During cross examination, I made reference to Biekoff's Theoretical Works where he concluded that substantial errors in the recorded high frequency components for standard USGS installations; that is, high frequency components for accelerograms, may arise from soil pad indirection effects on ground having low shear wave velocity, that is, relatively soft ground. In response, Dr. Trifunac questioned the values of these high frequencies but the question was left unanswered. The answer of course is that amplification errors will occur for frequencies which are in the vicinity of the natural frequencies of the pad soil system. Bear in mind the frequencies that were observed in the Monticello SMA pad were 12.5, 20, 40 and 45.

As an example, Dr. Biekoff cites one USGS pad design which was placed on soil with a shear wave velocity of 400 feet per second, would lead to amplification factors for 20 hertz SH input waves of 1.6. The 20 hertz, as I said, corresponds to the natural frequency of that pad system which happens to be roughly 90 hertz. I also note that in that analysis, significant amplification also occurred for input frequencies of 10 to 40 hertz. I might also add that such soil would be relatively soft and would not normally be encountered by most USGS installations. I might add that my estimates for the shear wave velocity of the soil in the vicinity of the pad site in question is 300 feet per second. This is generally consistent with the observed natural frequency for the pad.

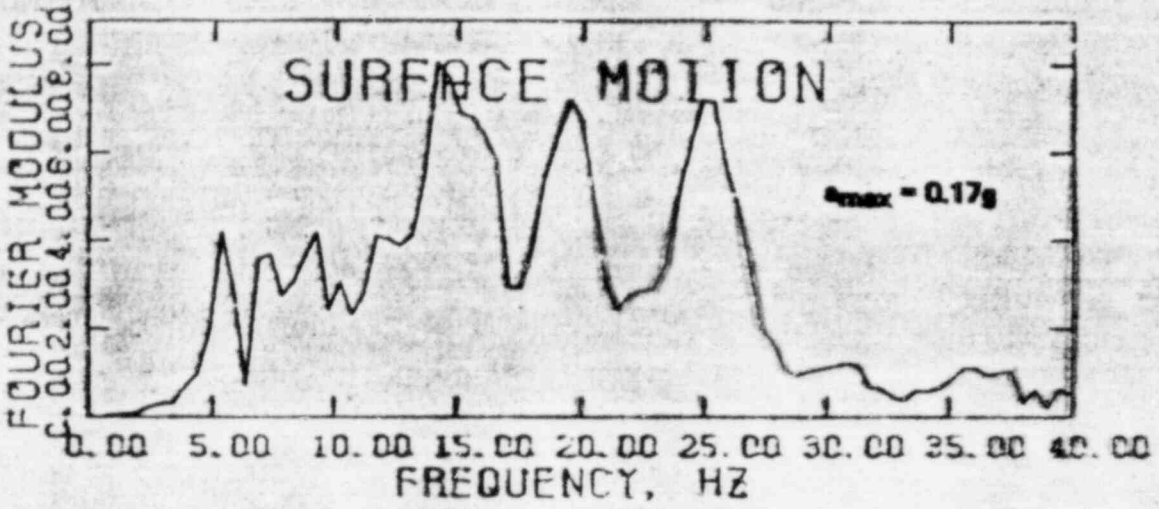
G9pw

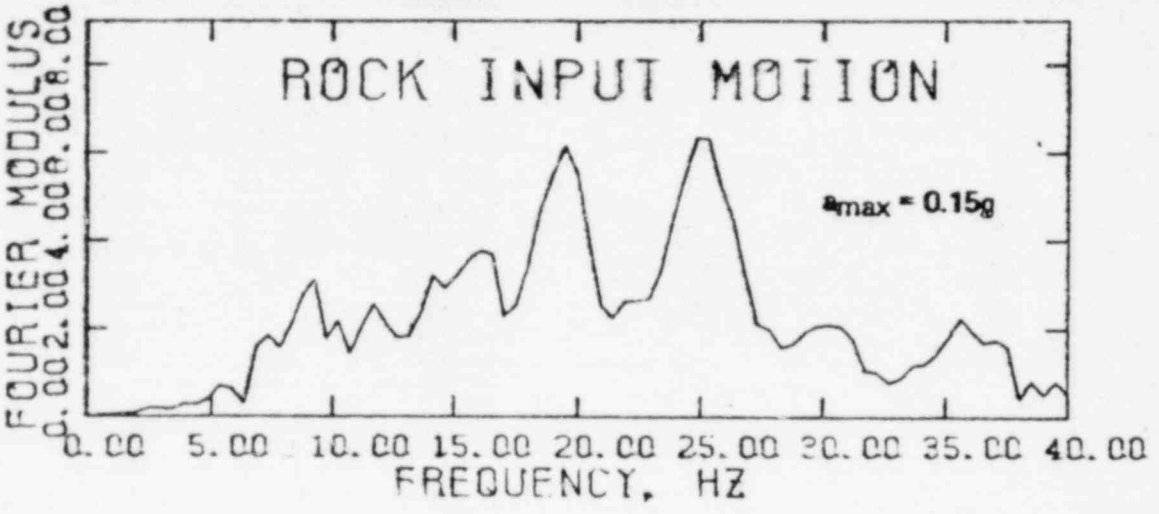
I would like to now make use of the overhead projector in order to briefly re-emphasize the nature of my concerns. Judge, this view graph I plan to introduce is a very simple view graph from a standard text on vibration analysis. Would there be any objection if I made use of it?

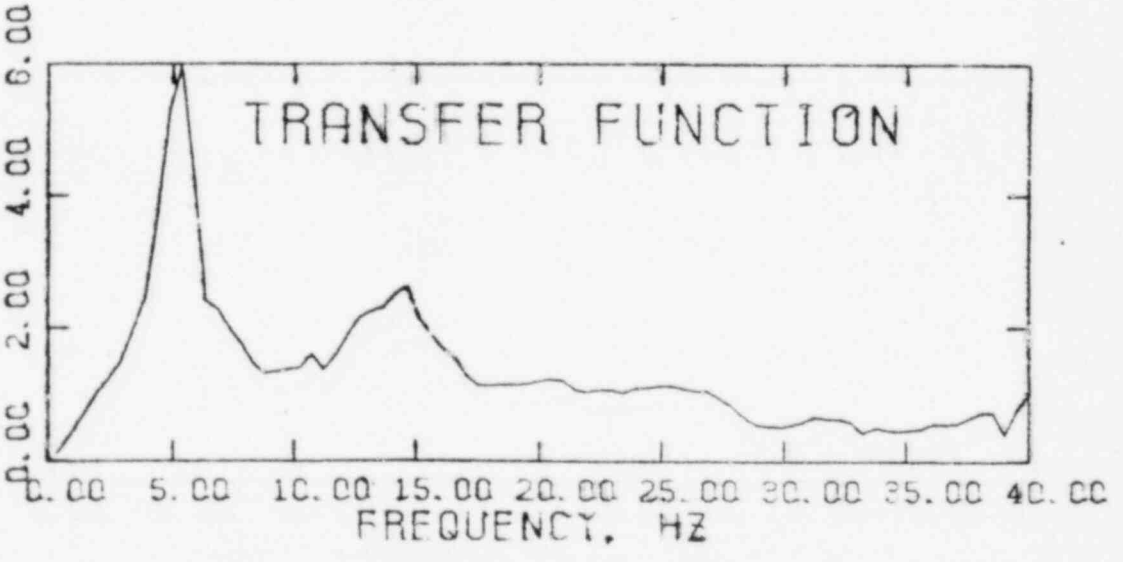
End G.

JUDGE GROSSMAN: None at all.

(Insert.)







RESPONSE STUDY USING MONTICELLO TYPE EARTHQUAKE
WITH BEST ESTIMATE SOIL STIFFNESS.CASE 1

RA h 1

(Dr. Martin at position of View Graph.)

DR. MARTIN: You may recall when I made my presentation, I referred to a simple analogy of a mass spring system representing the pad on the soil. This particular view graph was taken out of a standard text on the Theory of Vibrations by Thomson, 1972. This is a rather elementary text on vibrations.

The text illustrates in quantitative terms my physical illustration. This graph, the lower side of the view graph, plus the ratio of the output amplitudes to the input amplitudes has a function of the frequency ratio, that is the frequency of the input motion versus the natural frequency of the system.

You can regard this, if you like, as a transfer function. One can see, of course, that as the natural frequency of the support system equals the natural frequency of the-- rather the input frequency of the support system equals the natural frequency of the mat soil system, the amplification factors are very large.

On the other hand, if the natural frequencies of the input motion start decreasing, -- then the amplifications reduce quite markedly.

I might also observe that the shank of this transfer function is very similar to the transfer functions which were derived in Weipkoff analysis and I knew that as I matched our sinuous inelastic earthquakes, where you considered amplifi-

cation of the mass arising from incident shear waves. The shape of transfer functions referred to by the analysis of Trifunac would also have very similar characteristics.

This is not surprising because it can be shown that for a mass on a sinuous inelastic medium, you could replace that by an equivalent mass spring dash pot system.

For one of these curves presented in Weipkoff's analysis, in fact for all of the curves presented for analysis of masses on semi-inelastic medium, because of the very high radiation damping inherent in these analysis, he shows these really don't amplify very much.

For example, I mention 1.6, one of Weipkoff's analysis, that means that the damping was very high, maybe 30 to 40 percent. These very high damping ratios characterize analysis of the sinuous inelastic media.

The energy dissipation due to radiant energy is very high.

Lets now examine my concerns then with respect to this particular SMA pad. One of my concerns, and I have already mentioned my concerns in relation to the natural frequency characteristics but the other concern is that one of the damping values demonstrated here was 12.5, 20 hertz was very low, much lower than I normally would have expected.

This of course means that you can get very substantial amplification.

A h3

But first, let me comment on the conerns in relation to other USGS pads, which was expressed by Dr. Trifunac.

For most pad installations, one would expect natural frequency, particularly where they are being put on firm ground, reasonably competent ground, maybe at 70 - 80 hertz. Bear in mind that until very recently, engineers had only been concerned with test year signals, that means this ratio may be one on seven, may be one on ten. The area where they were reviewing this is way down here (indicating), no concern, very little amplification problem. However, as soon as we become interested in higher frequency signals, 25 hertz, even with a hundred hertz pad, we have a ratio of a third. However, if we are dealing with 25 hertz signal and we happen to have natural frequencies of 25 hertz, then we are over here (indicating) really no significant concern, particularly if we only have five percent damping. Five percent damping for this particular system, we get amplifications of about four or five. What does this mean then in terms of pad performance?

Let us take some of the natural frequencies, say on 12.5 hertz. 12.5 hertz, what input frequencies to the pad would be of concern. Well, according to this graph, 12.5, divide that by 2, you are getting down to about six, multiplied by about the root of 2, that would be getting up to 15. This graph would say then, including incident waves in the frequency range of say 6 to 16 would be amplified on that pad. Notice

the curves in this range. That is those frequency components in the Monticello earthquake record, in that frequency range will be amplified by this particular mode of vibration, while that particular mode of vibration was recording.

Take the next frequency, 20 hertz, this would mean that those frequency components in the Monticello record between 10 and roughly 30 would be amplified by that mode of vibration recordings.

The next mode, 40 hertz, this graph means those frequency components in the Monticello record between 20 and say 60 hertz would be amplified for that particular mode of vibration for the pad.

Bear in mind that the first two modes 12.5 and 20 and you have 5 to 10 damping very significant amplification.

The high modes, 40 to 45 hertz had 30 percent damping, that is not quite so much amplification.

The actual amplifications that would occur during the Monticello earthquake record would, of course be a culmination of all the individual pieces of amplification with a curve for each of the central events making up the Monticello earthquake record. So, of course, there would be probably a little bit less, in some of these areas, because of space differences between the various central events.

The other figure I presented during my presentation took this into account and in fact plots a locus of maximental

occasion, as a function of the potential natural frequency of the pad, and that is a wide range of frequencies. From that plot I made an engineering judgment I indicated what I felt it would be reasonable to expect, based on this rationale, to be maximental occasion, for the amplification of the record since the peak acceleration could be about 50 percent.

Quite contrary then to the opinion of Dr. Reiter who indicated that he felt a decision could not be made on the basis the evidence supported. I feel that some rational conclusions can be made, namely, those that I have just made.

(Dr. Martin now returning to panel location.)

To conclude then by stating again that I firmly believe that the results of our studies indicates that the Monticello records are unreliable and that they cannot be a true or correct record of the Monticello earthquake event. In particular, the maximum recorded horizontal accelerations will unquestionably have been amplified. This conclusion also would apply to the other Monticello earthquake records recorded in 1978. Thank you.

JUDGE GROSSMAN: Thank you. I understand Dr. Martin has to leave soon so could we ask him questions now?

MR. KNOTTS: Please do.

JUDGE GROSSMAN: Let me ask you first whether it is important what the force was with regard to the ropes that were put around the concrete pad that you indicated was 500

pounds of force?

DR. MARTIN: That would not be important, Judge. The intent was to excite a natural vibration in the linear elastic range which was characteristic to the earthquake.

JUDGE GROSSMAN: So it doesn't make any difference to what extent you were excited as long as your exciting the pad, is that correct?

DR. MARTIN: That is correct.

JUDGE GROSSMAN: You said you have great faith in Dr. Wood, is it who ran that experiment?

DR. MARTIN: Yes. Dr. Woods is recognized amongst the geophysical profession as being one of the leading experts in his field. From personal experience, I also know that he is a meticulous experimenter. He is very careful.

JUDGE GROSSMAN: You know we don't have Dr. Wood here to examine and, as a matter of fact, we don't have anyone who is there at the experiment to examine. You are aware of that, aren't you, Dr. Martin?

DR. MARTIN: I understand many of the applicant's consultants were at the site, at the final test and observed the results at the end of the test.

JUDGE GROSSMAN: I understand you didn't inform the staff that you were going to run these tests, is that correct?

DR. MARTIN: I would defer that question to--

JUDGE GROSSMAN: Mr. Knotts, is that correct, Mr. Knotts?

MR. KNOTTS: That is correct. We did not inform the staff.

JUDGE GROSSMAN: We have had a number of months since the first session of hearings here which were held in July and if I understand correctly, we have had over a month since the explosive tests were run and it would seem to me as though if we are going to run a test which seems to have such significance in the view of your seismic panel that something like that ought to be brought to the attention of the staff. Do you agree, Mr. Knotts?

MR. KNOTTS: I think in the ideal situation, it should have been and would have been brought to the staff. What we were operating under was not the ideal situation and the record will reflect. I don't want to testify but just to outline it very briefly.

My understanding is that it wasn't until after the blast tests were conducted and after Dr. Martin's theoretical work was done, then the idea that would explain the comparisons of the two might very well be something peculiar in the 25 hertz range, or the 20 to 25 hertz range and maybe that could be the pattern and that really, quite late last week, I believe it was on Thursday or Friday that I first heard it, that there was even thought being given to it and as of Friday, if I recall the sequence of events, they were still trying to find out whether someone from Dr. Blume's company or another company

might be made available and flown in with instruments to run a test and it was a stroke of good fortune, as I recall my conversation with Dr. Martin, that he was able to locate not only Dr. Woods at the University of Michigan as an expert in these matters but that Dr. Woods had some instruments already at a site in North Carolina, could fly to North Carolina, pick up the instruments and get down here on Saturday morning.

JUDGE GROSSMAN: Was this the best kind of test that could be run in order to determine this, Dr. Martin, or was it only a function of the short time that you had that you required that you do this test in this manner or that Dr. Woods do it in this manner?

DR. MARTIN: It was the most unique type of test that one can run, that is a conventional test to get an estimate or to get values of the natural frequency to vibration. Another type of test that might be done is a force vibration test where small vibrator will be mounted on the pad and then excited over a range of frequencies and the forced vibration response analyzed. That in turn will give studies of natural frequencies and damping.

JUDGE GROSSMAN: Do you have some uncertainty as to what mode of vibration was apparent in that test?

DR. MARTIN: We made--two kinds of vibration were excited. Rocking modes and rocking modes namely the footing rocking like so (indicating) and a horizontal sliding mode.

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Both of these modes excited by earthquake and amplified by an earthquake would of course contribute to the amplification of horizontal acceleration. We could not distinguish implicitly from the tests which mode was associated with each of the frequencies. Additional tests would ahve to be done to confirm which mode was which. Nevertheless, my general conclusions still apply.

JUDGE GROSSMAN: Well, that's a good point, Dr. Martin. I think if you, if the significance you attach to it is something that we ought to attach to it that those confirmatory tests ought to be run which would distinguish between the different modes of vibration and I would ask Dr. Jackson if it would be inconvenient for the staff to participate in those kinds of tests, recognizing your usual audit function, Dr. Jackson?

DR. JACKSON: Fortunately, this area does not fall within my branch. (Laughter.)

Mr. Knight could probably assign someone from the geotechnical engineering branch which is hydrological computer technical engineering branch which is the group that would handle this and we interact as branches and so on, being a bit facetious.

JUDGE GROSSMAN: Well, I would expect then that considering the nature of the tests that was run, that that could be duplicated and that a more substantial testing could

be carried out to verify your conclusions here and I take it, Dr. Martin, there would be no objection to having the staff participate in that?

DR. MARTIN: No, I would have no objection.

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DR. MARTIN: No, I have no objection to the Staff participating. On the other hand, I might add that such additional tests would indeed refine the answers, distinguish the modes, but nevertheless would not change the general conclusion derived from the tests to date.

JUDGE GROSSMAN: Well, if there's a minimum of inconvenience to the Staff to monitor it and to the Applicant to rerun that, why don't we agree that that will be done if the Board is to--that that will be done, period, unless there's an objection?

MR. KNOTTS: May I have a moment, Judge?

JUDGE GROSSMAN: Sure.

MR. KNOTTS: Judge are you saying if we want to take credit for it, run the test again; if we don't want credit for it, we don't have to run the test again?

JUDGE GROSSMAN: I think that's basically my view. I don't know if my colleagues agree on that. Do you have a point to make if that's our view?

MR. KNOTTS: Well, without being argumentative about it at all, the question is naturally raised "When is enough enough?"

I don't hear the Staff insisting that they should have been there or that the fact that they weren't there contaminates the test and so on. I don't see any reason to quarrel with the test, based on the fact that the Staff wasn't present; and given what Dr. Martin has said about what he is likely to learn from a repeat of the test, I don't know how significant it is.

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The reason one raises such concerns, as you know, is you never know when you're buying a schedule problem. For all we--

JUDGE GROSSMAN: When you're buying a?

MR. KNOTTS: A schedule problem. That is to say, for all we know Dr. Woods has to run some tests in Padagonia or something and we won't be able to get hold of him.

JUDGE GROSSMAN: Well, you know, I haven't been able to find anyone on the Staff or Board panel who would give a high degree of confidence to the tests that were run, and I've heard some real objections to that. Now, I understand unless they want to come out and endorse that test and those conclusions on the basis of what they've heard--are you prepared to do that, Mr. Knight?

MR. KNIGHT: No, sir, the Staff is not prepared to adopt the results of those tests. I think an honest answer would be that a number of the Staff members have questions regarding just what can be done with those results, how they can be used.

I think, in all honesty, we see it as a very difficult experimental problem. We're not at all certain that redoing the tests--well, our very, very early, very quick assessment--and I'd like to be very careful with that--it is a difficult problem, and we're not certain that redoing it in a more elegant would overcome any of those problems. But I must say that's an early assessment, and by no means would I mean to prejudge a methodology that might be developed.

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I would say one other thing, that if the Staff were to be involved, we would be most reluctant to be involved only on a very last moment basis. In order to be effective if we were to be involved, we would have to understand the basis for the tests and the considerations that went into it.

JUDGE GROSSMAN: And you would expect to have some input into the methodology that would be used, is that it, Mr. Knight?

MR. KNIGHT: I hesitate to say. There's a question of the prerogative of the Staff to say "No, no, I don't want you to do that, I want you to do something else," and it would certainly enhance our participation if we had the opportunity.

JUDGE GROSSMAN: Dr. Martin?

DR. MARTIN: Yes, Judge. I'd like to make one other comment. I might add that in making my judgment I have not taken into account an identification of the modes. That is, I have not said that some modes are rocky and some are horizontal. So, the actual identification of which mode is associated with which frequency would not improve my judgment.

The reason for that is that the input measurements for that pad themselves are very complex and indeed unknown in any great detail.

Secondly, the pad itself, as I've indicated in my testimony, has different natural frequencies in two directions, which severely complicates any theoretical analyses.

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Similarly, the low damping associated with the lower natural frequencies suggests to me very weak soil underneath the pad or possibly roots are contributing to the observed behavior.

In that respect--and I indicated this in my testimony--I am not sure that regardless of how much experimentation is done on that pad that anyone would be able to with any degree of reliability say without a shadow of a doubt "This is the accelerogram that should have been recorded at that site."

My general overall conclusion, as I've stated, is that the records are unreliable and should not be used for analyses.

JUDGE GROSSMAN: Do I understand, Mr. Knotts, that you are in a position where if we say that we're not going to accept these conclusions without further testing that you will just withdraw this as an element in your case? Is that what I understood to be--

MR. KNCTTS: No, I was trying to put the burden on you, Judge. I was trying to find out whether you were going to tell us that we wouldn't get credit for it.

The problem that we have, of course, is that the Staff may very well say to us, even on the hypothetical that we said "Okay, we'll do some more tests," as Mr. Knight suggested a few moments ago, "We're not going to believe you no matter what you do."

And there is some--

JUDGE GROSSMAN: We're not?

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MR. KNOTTS: "We're not going to believe the test results, whatever they may show," because of whatever.

We've had some experience with running empirical tests in this case, the blast tests and pad tests and so on, and we haven't gotten a lot of credit for it from the Staff in a quantitative way. And that's their prerogative, that's fine. And I'm sure they would not want us to run a test, which was the very point Mr. Knight was making, if they could tell us in advance "It's not going to prove anything to us because our problem is different" or "more fundamental" or something.

So, we wouldn't want to be committed to run a test just for the sake of the advancement of science, as much as we'd like to advance science, unless it would get us some credit from the Staff.

JUDGE GROSSMAN: Mr. Goldberg, do you have a comment?

MR. GOLDBERG: Well, I guess we are talking about a difficult proposition. The Staff really has not offered any substantive testimony, nor has it had an opportunity to give any kind of careful consideration. The Applicant has elected to perform this test and present its results and interpretations. I don't know that we want to take an official position to either advocate or not advocate it, nor would I think it appropriate what weight the Board accord that testimony.

There may be another member of the Staff who perhaps can offer an additional comment or observation on the topic, but

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it is a difficult situation because we're talking about the relative way to view a piece of evidence that we really have not had an opportunity to give any kind of substantive consideration to.

JUDGE GROSSMAN: Well, let me say what I see to be your problem here. We have a lot of scientists and professionals here who are somewhat reluctant to directly criticize what some professional may have done with regard to methodology or conclusions. And pehaps unfortunately I'm not a scientist and professional, and I see some matters that appear dubious to me about the methodology used, the haste in which the experiment was improvised, the degree of confidence one can have in the conclusions because of certain uncertainties within the experiment; and I'm not in a position where it is a professional colleague of mine that I'm unwilling to criticize, but on the other hand I would like something hard in the record here if we are to give any weight to what was done. I would like to have the Staff people consider the matter and give a hard opinion with regard to it, forgetting about all the professional niceties that everyone seems to have adopted.

Now, that may be a little outspoken, but nevertheless it should make the point.

Now, Mr. Knight, is there some comment you want to make on that?

MR. GOLDBERG: Can I just ask what the vehicle would be for this additional contribution to the record or to the case?

JUDGE GROSSMAN: What the vehicle would be? I expect

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that the Staff would send in a report. It doesn't have to be a very detailed report, but merely submit a report to the Board as to what they agree or disagree with as far as the methodology, the results, whatever they think is appropriate to evaluating those conclusions. Dr. McGuire?

DR. McGUIRE: Just a small point. Time is getting short for Dr. Martin. If there are no more technical questions, perhaps he cold be excused.

JUDGE GROSSMAN: There may be some questions as to procedure that he might want to--what time do you have to leave, ten minutes ago?

DR. MARTIN: Three o'clock at the latest, Judge.

JUDGE GROSSMAN: Let's hurry, then.

MR. GOLDBERG: Judge, some of the Staff members I think might wish to confer on that matter for a moment. I do have one technical question that I wanted to ask Dr. Martin, and maybe I could do that while they confer.

JUDGE GROSSMAN: Yes, please.

MR. KNOTTS: Why don't you ask that before the break?

MR. GOLDBERG: That's what I'm saying, I'll ask it now.

CROSS EXAMINATION

MR. GOLDBERG: Dr. Martin, could you please compare the vertical and horizontal Fourier spectra from earthquakes recorded at Monticello to your estimates of predicted pad resonances?

DR. MARTIN: Unfortunately, we did not have a record of

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the vertical motion of the pad. In other words, we did not excite vertical pad motion when that measurement was taken of the vertical natural frequency of the pad. Does that answer the question?

MR. GOLDBERG: How about horizontal spectra?

DR. MARTIN: Perhaps you would repeat the question.

MR. GOLDBERG: Yes, eliminating vertical. Please compare the horizontal Fourier spectra from earthquakes recorded at Monticello to your estimates of predicted pad resonances.

DR. MARTIN: Perhaps I could make use of the figure that I identified in my previous testimony. That figure which I submitted for the record showed the peak amplification as a function of pad resonance frequency for the Monticello records.

In effect, we deconvolved the Monticello records through a range of mass spring systems of different frequencies. That plot then is the locus of the maximum amplifications.

MR. GOLDBERG: Can you display that record on the view graph, please?

DR. MARTIN: This plot is--

MR. GOLDBERG: Really, Dr. Martin, in the interest of expediting your departure, could you go directly to the Fourier spectra for the observed records?

DR. MARTIN: I don't have the fourier spectra for the Monticello records. As I recall, the dominant natural frequencies in the Fourier spectra were in the frequency range 20 to 25 Hz and in the vicinity of 12.5 Hz. Bear in mind that two of the

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natural frequencies observed on the pad were 12.5 and roughly 20. A direct comparison then of those frequencies with the Fourier spectra for the record will generate some concern in itself.

MR. GOLDBERG: Can Dr. Reiter ask a clarifying question?

JUDGE GROSSMAN: Certainly.

DR. REITER: I'm just trying to get at the observed record that we see on the Monticello Fourier spectra and how consistently are your predicted resonances; and if there is any deviation, could you explain that? That's what I'm trying to get at.

DR. MARTIN: I see. Well, certainly the Fourier spectra on the Monticello records would be consistent with 12.5 and 20. I think we'll agree there are peaks in the Fourier spectra in that vicinity. At 20 there is a dip. The peak actually is slightly greater than 20 to 25. In that respect, one might question the value of that 20 Hz frequency. On the other hand, bear in mind that these natural frequencies were recorded just recently. The actual stiffness characteristics of the pad itself in 1979 and 1978 may have deviated slightly from today's.

With respect to the higher observed natural frequencies, 40 and 45 Hz, you would have to tell me if there are peaks at those frequencies because I cannot reca''. I believe there's a broad band.

JUDGE GROSSMAN: Do you have that in front of you?

DR. REITER: Yes. I was observing the 180-degree

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component. I see peaks at approximately somewhere between 10 and 15 Hz, another peak centered around 25 Hz, and then some broad peaks somewhere between 35 and 45 on one horizontal.

On the other horizontal component, I see a peak around 12.5 Hz, a peak centered around 25 Hz, and then I cannot find another--the rest looks noisy.

I wanted to just ask you if you think that's consistent or inconsistent with the band of uncertainty of what your predicted resonances were.

DR. MARTIN: The predicted resonance of 20 Hz is not appearing on the Fourier spectra for the 1979 event. There are some quite high values, nevertheless, in the 1978 event. So, with respect to the 20 Hz peak, the peak in the Fourier spectra does not occur there. Nevertheless, that still doesn't remove my overall concerns.

I might observe that the dominant frequency of the latter part of the Monticello earthquake record appeared to be 12.5 Hz on the record itself.

MR. GOLDBERG: Thank you. We have no more questions.

JUDGE GROSSMAN: Thank you, Dr. Martin.

MR. KNOTTS: Can Dr. Martin be excused now, Judge? We can talk about what we're going to commit to after the recess.

JUDGE GROSSMAN: Yes, that's fine. Thank you for appearing, Dr. Martin.

MR. KNOTTS: Can we have just time enough to talk with

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Dr. Martin for a minute or two?

JUDGE GROSSMAN: Certainly.

(Whereupon, a brief recess was taken.)

JUDGE GROSSMAN: On the record.

MR. KNOTTS: In the ancient tradition of the profession, I have a proposal that we resolve it between the Staff and the Applicant and get back to you in about a week?

JUDGE GROSSMAN: That's fine. If the Staff, by the way, has a conclusive evaluation they want to present that might resolve the matter, perhaps in mind--no? Okay.

We can proceed.

DR. McGUIRE: Dr. Alexander will now make his statement, and I think we'll go through the rest of the statements; and then if anybody has any questions, that's all right.

DR. ALEXANDER: Yes, I'd like to make a few remarks in rebuttal to the suggestion that the explosive tests were not a valid indicator, at least in the sense of reduction between the SMA site and the auxiliary building floor. And I speak to this on the basis of long experience with surface waves, in particular starting with my Ph.D. thesis some twenty years ago. I'd like to make a couple of observations.

Dr. Trifunac suggested that perhaps a great deal or nearly all of the reduction seemed to be attributed to the reduction of fundamental mode Rayleigh waves being conserved in the auxiliary building at some equivalent depth below the surface

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versus the surface itself at the auxiliary building, and to the extent that that happens--and I concede that if there were a lot of fundamental mode Rayleigh waves that might be a reasonable proposition--then point one is that any such reductions would apply equally to any earthquake surface wave fundamental mode energy that should be excited, regardless of the depth of that earthquake. And that's consistent with what Dr. Reiter stated in his testimony a short time ago.

Second, with regard to the actual data themselves, if one looks at the actual signatures generated by the explosive tests and as recorded at the SMA site and in the auxiliary building and looks at the portion of the signal that consists of the compression or P wave portion, which contains no surface waves at all, one sees a reduction comparable to that which was indicated overall. That is, it's about the same as the portion of the record which contains the surface waves. That is to say, up until the shear wave arrival time, there can be no surface waves. They have a travel time which is slower than the S wave itself. So, the early portion of the signal, which is the P waves, contains no surface waves. And by virtue of the fact that the reduction was comparable for body waves as it was for the portion that contains S waves, higher mode waves and fundamental mode waves, I would argue that in this particular instance the reduction observed cannot be caused by the mechanism that Dr. Trifunac proposed.

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Second, I would note that in that connection in the FSAR itself there were some explosive tests reported in which it was indicated that fundamental mode waves were not generated to any observable extent by explosions in this particular area. And in particular I refer to FSAR Section 2.5.4.4.4. And I think this is not inconsistent with the fact that the explosions themselves were approximately 3 kilometers from each of the two sites at which the data were recorded.

So, fundamental mode waves at the frequencies that we're dealing with here, in the heterogenous environment that exists, it's not surprising that fundamental mode waves don't make it as fundamental mode waves that far with a coherence contribution.

Therefore, what I would conclude from this is that the explosion tests, while not necessarily simulating earthquake type response, the reductions observed cannot be explained by the mechanism which Dr. Trifunac suggested in this particular case.

So, that concludes my statement with regard to the explosion tests, with the conclusion that the reductions observed do give an indication that one would experience attenuation or reduction of amplitude between those two particular sites. That is, the auxiliary building will show lower amplitude than recorded at the SMA site.

JUDGE GROSSMAN: Are you suggesting that there were sufficient P waves arriving in advance of the surface waves for you to make a qualitative -- I'm sorry, a quantitative estimate as to the reduction in the P waves at the auxilliary building?

DR. ALEXANDER: I cannot give you an absolute figure, but the envelope of the P wave portion of the records did have significantly large amplitude, approximately equivalent to the surface wave portions, and the reduction in the envelope of amplitudes in that portion of the record from between the two sites was comparable to the reductions seen in the surface wave portions of the record between the two sites.

My point is that the surface wave portion in this case I do not believe contains a great deal of fundamental mode energy; energy that is arriving in the surface wave portion would consist of S waves and perhaps higher mode surface waves, but not fundamental mode waves.

JUDGE GROSSMAN: Do you have any further presentation, Dr. Alexander?

DR. ALEXANDER: Not on this subject, but I would like to -- Mr. Knotts asked me to make a statement with regard to a suggestion, not in this hearing, but in the earlier hearing as to the appropriateness of using Dr. Bolt's book entitled EARTHQUAKES, A PRIMER, as a basic reference for the subject that we're dealing with, and I would simply state that there

is no question that Dr. Bolt is an emminent seismologist and authority in this field;however, this particular publication is written for a general public audience and is not one which would be used by experts to evaluate reservoir-induced seismicity, and insofar as definitions of terms et cetera go I believe that in the testimony that the applicant has filed and others in this case there are adequate definitions in the record as it stands.

JUDGE GROSSMAN: Let me say at the time that we suggested receiving Dr. Bolt's book in evidence, and at the same time did actually admit into evidence the ACRS reports, the record was completely -- was considerably different than it is now, and I am not sure that we would have done either of those two things had we thought we would have a substantial record, but we thought we might at that point be stuck with only what we had.

MR. KNOTTS: The best we can tell, Judge, the state of the record is that both items that you have just mentioned, to be precise, the ACRS transcripts and the Bolt book were marked but never received, and our suggestion is that the Bolt book remain marked but not received and will therefore be in the record but not in evidence, and that I would withdraw my objection to the admission of the ACRS transcripts on the understanding, having looked back at what the Board said it was doing and not having perhaps appreciated it at the time, but those ACRS transcripts were admitted for the limited

purpose of showing what the staff considered. I believe that was the way it was characterized at the time.

JUDGE GROSSMAN: I'm not sure that we didn't actually admit the ACRS transcripts. What's your opinion on that, Mr. Goldberg?

MR. GOLDBERG: My recollection is that they were admitted.

MR. KNOTTS: The transcript does not so reflect, and we've been trying to pin that down for some time.

My suggestion would be to go ahead and admit them to be sure that it is clear that they're in, the ACRS --

JUDGE GROSSMAN: I'm -- Pardon me, Mr. Knotts.

MR. KNOTTS: I'm sorry. My suggestion would be that you go ahead and make clear that the ACRS transcripts are admitted for the limited purpose of showing what the staff considered, and that the Bolt book is not admitted, it's just marked.

JUDGE GROSSMAN: Mr. Goldberg, do you have a problem with that?

MR. GOLDBERG: No, I don't have a problem with that. I don't even have a recollection of the Bolt book being marked for identification.

MR. KNOTTS: I think it has a number if I'm not mistaken.

JUDGE GROSSMAN: I think there was some speculation as

to a number, we weren't sure what number it ought to be marked with, but it could well have finally received a number, but in any event let me just check with my colleagues.

(The Board confers.)

JUDGE GROSSMAN: Well, whatever may have been the situation, it doesn't appear to us as though we want to burden the record with ACRS reports if we have a substantial record as we think we have now. I think it would save some eye strain on the part of counsel as well as the Board if no one sees any purpose for it that we -- if we had admitted the ACRS reports, that we strike them at this point. If you have a problem with that, Mr. Goldberg, we won't do it.

MR. GOLDBERG: I don't believe I have a problem. I believe they were proposed as Board exhibits. I take no position on it.

JUDGE GROSSMAN: I take it, Mr. Knotts, --

MR. KNOTTS: I don't have a problem with that, Judge. I was trying to find the numbers for you, and unfortunately the only page I have here shows the Bolt book was Number 6.

JUDGE GROSSMAN: That was my recollection too for attempting to get a number.

MR. KNOTTS: It would have been --

JUDGE GROSSMAN: But in any event, that is a moot point now. It is the Board's decision without objection by counsel to strike both of those exhibits, and so they may have been

identified, but that is the extent of their inclusion in the case file.

May we proceed further, Dr. Alexander?

DR. ALEXANDER: That concludes my statement.

JUDGE GROSSMAN: Dr. McGuire.

DR. McGUIRE: Dr. Blume will make a statement.

DR. BLUME: I propose speaking to three subjects as breifly as I can, and the three subjects will be: First, damping; second, reduction factor for RIS; and the third, anchor point for RIS spectrum.

The NRC Regulatory Guide 1.61 allows 7 percent damping for reinforced concrete structures and has been in use for many years, and has been used in many nuclear plants.

Dr. Trifunac suggested a lesser value of damping for the Summer station because the strength of the materials were more than specified. That's my understanding of his reasoning for that requirement.

I would like to comment as follows: That in a plant that has not been subject to reanalysis for RIS or for any other reason, the allowable 7 percent is not questioned, 7 percent damping that is, because the test values are not re-examined as has been done for the Summer station.

However, I point out that essentially all nuclear plants, in fact most modern engineered structures do have material strength values that are far in excess of the specified values.

Thus, to reduce the allowable damping value for Summer because of its extra strength of material would be discriminatory in my opinion, and certainly is not required on a technical basis.

Speaking of actual values, I believe when Dr. Trifunac was asked as to why he wanted to reduce the amount of daming he referred to tests that he had been connected with I believe at CalTech, and apparently these tests were either ambient or very low in stress tests, and he mentioned something about 5 percent damping as I recall it; it would be in the record.

Dr. Luco in turn in prior testimony or summary talked about tests at CalTech where he had dampings of as high as 12 percent including radiation under the assumption that the structure had only two percent which they arbitrarily held constant.

The point is that Drs. Luco's and Trifunac's experience with these damping tests has apparently been in a low or medium stress range where 3, 4 or 5 percent would be perfectly logical. We as the applicant have presented data that's in the red book that shows stresses taken, amplitude yield and where the damping values are on the order of 9 and 10 percent. We feel that the values already in use at Summer should not be changed.

That is the end of the damping portion. Would you like to question now or shall I go on to the other portions?

JUDGE GROSSMAN: Please go on, unless you have some clarifying questions, Mr. Knotts.

MR. KNOTTS: I don't beleive I do.

DR. BLUME: All right. The second item is reduction factor for RIS.

The applicant has conservatively proposed a reduction factor of 0.5 for RIS records, for the use of one-half the values recorded on a concrete pad at the dam abutment in October of 1979.

This reduction would apply to the base of large structures bearing on rock, and ask you can recall it has in part -- not wholly, but in part the concept of the data from the explosion tests.

I wish to emphasize, however, that the explosion tests are by no means the only reasons for this suggestion. Other means would include the matter of effective acceleration which as I testified was .65 at Diablo Canyon, and has been generally considered within the engineering field.

The staff has stated it considers a reduction factor indicated, but it has not yet developed a numerical value for this factor.

Consultants Trifunac and Luco propose complex 3D, SSI or soil structure interaction analyses which would very likely lead to similar results, but take a considerable time and great expense for reanalysis of the plant.

It is my suggestion that the staff develop its own reduction factor now that it has been supplied all the

applicant's data, and that these be used in lieu of additional analyses.

That is the end of the second item.

The third item is the anchor point RIS spectrum. The applicant has proposed to increase the anchor point for the rock conditions from 0.15g for SSE to 0.22 for RIS. Dr. Trifunac, on the other hand, suggests two and a half times 0.15, or .375g, an enormous value for an eastern nuclear power plant.

The high frequency, short duration spikes of the Monticello RIS do not justify any such increase in my opinion on the basis of the following items:

A) the record of possibility of potential damage; b) precedent with many other plants and projects; c) the small amount of energy input from a disturbance that lasts less than one-half of one second, and; d) the fact that the displacement associated with the maximum motion in October 1975 was less than one millimeter in amount. That is pretty hard to conceive, but one millimeter is not going to shake down or even affect grossly any major structure such as we're talking about.

MR. KNOTTS: Dr. Blume, I believe you misspoke. October 1979 rather than '75? Is that what you meant?

DR. BLUME: No, I meant '79.

MR. KNOTTS: You said '75, sir, I just wondered.

DR. BLUME: Pardon me. If I said '75 I was mistaken,

I meant October '79.

So we're dealing with micromotions, the kind that you'd have to see in microscope.

Another reason is the record of nondamage such as the hydro plant which also not designed for seismic forces -- I'm speaking now of its equipment, not the building -- the equipment was undamaged by greater than .36g in 1979, assuming for the moment that the record obtained was correct.

I also cited the results of the nuclear event Ruleson where with .36g in a town with very weak buildings and no seismic designs the damage was essentially trivial.

I also cited the record of the El Centro steam plant which in spite of the walls that Dr. Trifunac mentioned had equipment and piping and pumps and so on which were not damaged as they should have been on paper.

In San Francisco in 1906, the Esso refinery in Nicaragua, the Chile steel plant all suffered tremendous accelerations with nominal damage.

It is proposed that the RIS spectrum that has been submitted by the applicant be used for the RIS problem, and in my opinion it is entirely adequate.

JUDGE GROSSMAN: I do have one or two questions.

DR. BLUME: Certainly.

JUDGE GROSSMAN: It is my understanding, though, Dr. Blume, that it isn't because of a 2.8 magnitude event that we're

concerned about damage to the equipment, and so when you project the effects of the October 1979 event to a postulated higher magnitude event, you're not exactly taking into account what it is we have to consider, and specifically you talked about the duration of half a second and the lack of damage to the auxilliary building or the -- I'm sorry, the --

DR. BLUME: Hydro.

JUDGE GROSSMAN: -- the hydro plant from the October 1979 event, but don't you recognize that a higher magnitude event would ordinarily have a longer duration?

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DR. BLUME: It would tend to have a longer duration if it were at the same distance and depth but I am relying upon Dr. Nuttli's work and the work of others that indicates that we have already measured essentially the maximum motion that we can expect from an RIS event. The reason being that as we get into larger magnitudes they go deeper into the ground and we have lesser response.

JUDGE GROSSMAN: I was talking about duration of strong motion regardless of the --

DR. BLUME: If the strong motion is less, the duration becomes less important. The only time that duration is important is when you're beyond the yield point of stress.

JUDGE GROSSMAN: But also, Dr. Blume, aren't you assuming in your entire projection that what was recorded at the accelerometer at the SMA-1 I believe was the instrument, that the motion really was only half of the motion that you would expect from another event of the same reading at the foundation of the reactor building. And so how can you compare the lack of damage in the one case with the lack of damage in the other case? Do you follow my question?

DR. BLUME: Not exactly, I don't follow how the event migrated to the reactor building.

JUDGE GROSSMAN: Well the point is that the .35 g motion that didn't cause any damage in October '79, is really going to be let's say a .35 g or a .22 g actual motion at the

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foundation for a future event. The lack of damage caused in October 1979 really can't be compared to what damage we might expect having taken into account amplification and reduction factors attributable to the 1979 event.

DR. BLUME: The fact that the duration might be a little longer and the amplitude might be more than one millimeter, would naturally cause more response, but I point out again that the hydroplant which had no design at all and suffered apparently very severe shaking, if these records are correct, was not damaged and the nuclear plant, the reactor building, the auxiliary building and so on, I would expect to be undamaged even if the motion were much more than recorded in October, 1979 and lasted longer and had even more than one millimeter, say two or three millimeters.

Again, I'm referring to the fact that these high, narrow spikes in the high frequency range are not fully effective in affecting a large structure. My analogy in my written testimony was the man striking the building with a hammer.

JUDGE GROSSMAN: Okay, I don't mean to be argumentative at this point, it's late in the day. I was only trying to point out to you that part of your testimony is to the effect that the hydroplant wasn't subjected to severe ground motion and because of the amplification factor and therefore you can't postulate that that kind of severe motion which isn't severe motion is what we're concerned about with regard to the foundation

of the reactor building because then we're concerned with actual severe motion and not an amplified figure. Do you follow me, Dr. Blume?

DR. BLUME: Yes. I know exactly what you're saying and the rebuttal to that, if you'll allow me, is the fact that I've been impressed throughout the whole hearing, everybody is talking about August '78 and October '79, and I know of many other records that have been taken of RIS events that are almost as large where the peak motion was only about nine or ten percent of gravity instead of 25 and 36 or whatever the two were.

In other words, we're just enveloping again, we're taking very high values of two events and if we considered all the Monticello events where we had reliable records on tape, and I think there must be 7, 8, 9 or 10 of them, I find the average acceleration to be much less than recorded at the SMA site for August '78 and October '79. In other words, I think we have recorded whether it's due to the pad or the ground or the event or whatever, I think we have recorded the maximum RIS condition.

JUDGE GROSSMAN: Dr. McGuire?

DR. MCGUIRE: I'll amplify on some statements by Dr. Blume regarding Professor Trifunac's conclusions. Professor Trifunac's spectral multiplication factors are equivalent to a zero period acceleration of 0.375 g. He also states, Professor Trifunac, that background for tectonic earthquakes dominated analysis. So presumably he would make the same recommendation for

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any other nuclear power plant in the same area.

We have also had in testimony the SSE acceleration for the nuclear power plants in the area and I think the comparison that was given was that the SSE at Summer is typical of those other plants in the southeastern U.S. I think that demonstrates that Professor Trifunac's recommendation is really over-conservative. He recommends a value which is two and a half times the value determined to be appropriate at many sites in the same region by many people for the same earthquake history.

I'd like to point out what I consider an inconsistency in Dr. Joyner's presentation. He used in his opening statement a comparison of his derived value of peak velocity which as I recall, was on the order of 10 centimeters per second, with an observation at Gilroy at a rock site. That analogy he justified by his observation of modified Mercalli intensity VII at Gilroy. And we pointed out that that observation of his was in fact conservative, he taking the larger of two horizontal components, the average of those two was more like 7 centimeters per second.

Dr. Joyner now talks about peak velocity on the order of 20 centimeters per second for his recommendation. So I suggest that to be consistent with the example that he himself presented.

I'd like to give you some perspective on the damage or rather lack of damage that was observed for these small earthquakes

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and the reason why we presented those observations. We've had many, many earthquakes of magnitude less than 5 every year throughout the world. For instance, in California we've had tens of thousands of magnitude 3 earthquakes occurring everywhere every day; in towns, in cities, in villages, industrial areas and none of those magnitude 3's caused damage. So some questions to put this in perspective, some questions on the relevancy of those observations came up for the type of earthquakes we're discussing here. There is uncertainty in every engineering analysis we do, but that doesn't mean that we don't do the analysis and arrive at a conclusion. We do this every day in fact. In so doing we use engineering judgment in making decisions at various points in the analysis. In earthquake engineering, for example, we make estimates regarding the appropriate magnitude, distance, source parameters, effective propagation paths, filtering, foundations and so on enter the analysis. If we are really, really concerned about the effects of some earthquake phenomenon we must be very precise and conservative in our analysis and choices of parameters to insure an adequate design. However, if everything that we do leads us to the conclusion that the observations we have are conservative, the structure has ample safety margins and in particular the phenomenon we're studying has never been known to cause damage to an engineering structure, we take great comfort in that and we conclude that our analyses can be more realistic instead of overly conservative at every

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step. It's in that perspective that the observations of lack of damage to engineered structures from the phenomenon that we're discussing is relevant and should be considered by the Board in reaching its decision.

JUDGE GROSSMAN: Anything further on direct, Mr. Knotts?

MR. KNOTTS: Anything further, gentlemen?

(No response.)

MR. KNOTTS: No, Judge.

JUDGE GROSSMAN: Mr. Goldberg has his turn and then the Board questions.

MR. GOLDBERG: No questions, Your Honor.

JUDGE GROSSMAN: I have no questions. The panel is then dismissed. I'd like to thank you for appearing.

(Panel excused.)

JUDGE GROSSMAN: Mr. Goldberg?

MR. GOLDBERG: Yes, Judge. Dr. Jackson would like to clarify an answer he gave to a question Judge Hooper asked and if he might do that, it'll take just a minute.

JUDGE GROSSMAN: And while you're there, Dr. Jackson, would you give me your opinion as to whether the T waves arrive sufficiently in advance so that we can make a quantitative determination of reduction?

DR. JACKSON: I don't have any idea but I could ask Dr. Reiter or Dr. Sobel, either one.

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DR. REITER: I haven't fully examined them, I can't offer an opinion at this point.

JUDGE GROSSMAN: Dr. Jackson, sorry to interrupt you.

DR. JACKSON: That's fine. It's a very minor point. Judge Hooper asked me about stress measurements in the eastern -- in the Piedmont region, it's a minor change but there is a -- I have been informed and I think I was aware but I didn't recollect, there is a hydrofract test hold that has been drilled in the Bad Creek Project which is to the northwest of the Jocassee Project, a Duke Power Project Pump Storage Project, and I understand the stress measurements there are about equivalent to those in the region. I haven't looked at those figures since probably 1975 or 6. It's not technically in the Brevard zone, it's in the Blue Ridge, but it's only a couple of miles probably or half a mile to the west of the Brevard Zone which is the boundary line, so to speak, between the two provinces.

I didn't want someone to look at a figure and see another hydrofract plotted there that I overlooked.

JUDGE GROSSMAN: Does that conclude the seismic presentations of the parties?

MR. KNOTTS: It does, Judge, and I would suggest that the record on seismic matters be closed, being left a little bit open to receive the hopefully joint recommendation of the staff and the applicant on how to proceed with matters concerned with pad testing or plucking.

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JUDGE GROSSMAN: Mr. Goldberg?

MR. GOLDBERG: No objection.

JUDGE GROSSMAN: Well before closing it I would like to comment on the Board's opinion of our having utilized Board experts. I'm not going to ask for or try to pin anyone down on that, I just want to say that we have; that is, the Board members have discussed among ourselves the value of having those Board witnesses. We are very pleased with what we heard from the Board witnesses and the quality of the testimony we heard from the parties which was enhanced by having the Board witnesses. If this is an experiment, though I don't really think it is, I believe it has been done in the past, but if it is looked at as something new, our feeling is that it was a very favorable experiment. I think aside from even just the quality of the testimony presented to the Board, there were inputs by the Board witnesses in this case and the Board did ask the witnesses to be acceptable to the staff and the applicant so that there were effects that may not even have been demonstrated at the hearing.

Now to the extent that the witnesses may have ventured into areas that perhaps the Board and the parties don't feel was part of the hearing and shouldn't have been explored, that was something that we were not really in that good a position to control without having had closer communication with the witnesses that we didn't care to have. We wanted to keep everything on the

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record. But we don't consider that because they wanted to bring in something that they thought was significant to them or within their particular orientation, that that therefore expanded the area of Board consideration and I'm thinking particularly with regard to the probability question and perhaps with the re-analysis suggestions. And let me say also with regard to recommendations that may have been made by the witnesses, we didn't ask for recommendations, we asked for summaries of testimony and to the extent recommendations were made, that's not anything that we're really concerned about and they were presented for whatever they were worth. They just happened to be there. My particular position on having recommendations without even supporting documentation or testimony is that it is something that I wouldn't encourage if I were an attorney and if they were -- the Board witnesses, I certainly don't encourage that. But I just wanted to put that in perspective.

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And, with that in mind, I think we can agree to close the record on seismology at this point, except for the matter of the tests of the concrete pad, and if we can -- I don't want to get into the area again because I will just add on to the ambiguities but I think that in your report to the Board, you can probably resolve a good many of the things that I would be raising if I went into it again, so we will leave it the way it is right now and if there are any problems, we will certainly welcome a conference call and then perhaps a formal report to the Board unless you think you can anticipate what the Board's interested in and just submit a report and then we will close the record even on that aspect.

MR. KNOTTS: Very well.

JUDGE GROSSMAN: Any further business today?

MR. KNOTT: I would suggest that the record having been closed, we need to talk a little bit about the schedule for proposed findings. We now have, with the narrow exception of the re-opened matter which will be taken up next week and an even narrower exception of the pad, we now have a complete record and the seismic decision--I mean the seismic proposed findings should be embarked upon. I would propose then in terms of intervals we follow the intervals in the regulations because although I talked earlier in the proceeding about having a short proposed finding schedule, I don't see realistically how either myself or Mr. Goldberg and colleagues can deal with

this very large record on seismology much less in the allotted time.

We do, however, observe that Mr. Bursey, although here for a few hours on Monday and Tuesday, and as the record will reflect asked a few questions on perhaps one or both of those days, has not been here Wednesday, Thursday, yesterday or today and I really wonder whether he needs or wants to file proposed findings on the whole seismic question, which leads me to suggest that maybe there could be a time saving in there of the staff not having to wait to hear from Mr. Bursey before it replies a reply brief.

JUDGE GROSSMAN: I would guess that the staff would like to take its time, its normal time in any event, whether he files it or not. I will let Mr. Goldberg speak for himself on that.

MR. GOLDBERG: Well, I suppose we would agree to advance our filing date if we can expedite our preparation process, and I am sort of suggesting this, by adopting portions of the applicant findings with which we have no disagreement advancing our own affirmative findings in the areas where there are disagreements or toward different emphasis. I am not advocating reduction of time but I think that would be the only circumstance on which I could contemplate that and, in addition, not to have to address perspective findings Mr. Bursey might raise, otherwise, I think we have to let the--

JUDGE GROSSMAN: If you are asking whether the Board has any objection to your not paraphrasing things you agree with, this Board doesn't. Perhaps an Appeal Board does but I don't believe they've gone into monitoring this case that closely.

MR. GOLDBERG: I agree.

MR. KNOTTS: Off the record.

(Discussion off the record.)

JUDGE GROSSMAN: On the record.

MR. GOLDBERG: With that understanding, the only thing remaining was whether we would have the benefit of seeing, you know, Mr. Bursey's findings in advance.

JUDGE GROSSMAN: On the record. As Mr. Knotts has proposed then, you expedite your requested findings on the assumption that Mr. Bursey is not going to file anything, and you have indicated you could do that if he doesn't file anything.

MR. GOLDBERG: The way I would like to leave it is we will exercise our best efforts to advance the filing date on our findings and, you know, that is more likely to be realized if we learn subsequently that Mr. Bursey is not going to have any affirmative finds.

JUDGE GROSSMAN: That sounds fine. I want to--

MR. KNOTTS: We may try to work something out further with Mr. Bursey so that we can present the Board with a little tidier package of what we are talking about. Lets just leave

it now that we will be using the periods in the regulation and will work to try to improve that if we can.

JUDGE GROSSMAN: Well then, I would like to thank the parties for the excellent presentations they made which the parties may feel was unncessary, however, we think that if they were a matter of concern, which we thought they was, then we think that it was necessary and it was an excellent job. Of course, if there was no matter for concern, then perhaps we have wasted some time but we still go on the former assumption and we would like to thank you for that excellent preparation and presentation.

MR. GOLDBERG: Judge, it seems to me that before we close today that we ought to discuss the prospective hearing I believe on Wednesday regarding an aspect of the Emergency Planning contention. We just had some discussions off the record about the perspective nature of that. I don't know that we can resolve them in Mr. Bursey's absence, but it seems to me that we ought to have a conference call on Monday to see if we can get a clarification of the matters to be taken up at that session. I refer particularly to some suggestion that some unspecified individual or individuals may be offered as witnesses on some unspecified topic or topics that do not fall within the--or were not identified in the motion to re-open, which this Board granted in part, and I say this in order to allow us to accomplish something

next Wednesday. I think it necessary that we have a discussion in advance of exactly what the matters are to avoid the necessity of unnecessary delay at that juncture.

JUDGE GROSSMAN: That sounds agreeable to us to have a conference call Wednesday afternoon if you can arrange it-- I am sorry, I mean Monday afternoon if you can arrange that with Mr. Bursey, and let me say I don't know that we would even entertain the prospect of having a witness testify on a topic that we didn't cover in the order, so there may be a question as to whether someone might come on for a limited appearance statement. I have heard that suggested and let me say that I don't know that we would even entertain a prospect of having a witness testify on a topic that we didn't cover in the Order, so there may be a question as to whether someone might want to come on for a limited appearance statement, I have heard that suggested informally and I would assume the Board would agree to hear that, but that is something I will discuss with my colleages. Mr. Knotts?

MR. KNOTTS: For our part we should be--I think everyone would agree that we should be clear on the scope of the re-opened hearing if we are not now clear, I thought we were.

JUDGE GROSSMAN: We will read our own order before we participate in the conference call.

MR. KNOTTS: Secondly, we think it is a matter of

discovery and trial preparation that if there is direct evidence at least being proposed on behalf of Mr. Bursey, within the scope of the re-opened hearing, then he was obliged to prefile that testimony. That is our argument and you need not respond now. I just wanted to outline it so it would be clear, and that is a matter of trial preparation and discovery we think without a shadow of a doubt, that direct evidence and if the argument is something other than that, then I would like to hear it and listen to it and react to it.

MR. GOLDBERG: I am sure these will be matters that we will discuss more fully in the conference call, but just a preview sort of of our position is that we see that again there has been a motion to re-open which has been granted in part to receive specified direct testimony, one piece of which has been offered. It seems to me that some of the suggestions I have heard off the record are tantamount to an additional motion to re-open, the grounds for which I assume the movement will be able to provide during our call.

JUDGE GROSSMAN: With that in mind, we are adjourned until 9 o'clock on Wednesday.

MR. KNOTTS: In this same room, Judge?

JUDGE GROSSMAN: In this same room.

(Whereupon, at 3:55 o'clock p.m., the hearing was adjourned, to reconvene at 9:00 o'clock a.m., Wednesday, January 20, 1982.)

NUCLEAR REGULATORY COMMISSION

This is to certify that the attached proceedings before the

NUCLEAR REGULATORY COMMISSION

in the matter of:

Date of Proceeding: January 16, 1982

Docket Number: 50-395

Place of Proceeding: Columbia, South Carolina

were held as herein appears, and that this is the original transcript thereof for the file of the Commission.

Peggy J. Warren

Official Reporter (Typed)

Peggy J. Warren

Official Reporter (Signature)