UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

In the matter of:	:	
	:	Docket No. 50-70
GENERAL ELECTRIC COMPANY	:	Operating License
	:	No. TR-1
(Vallecitos Nuclear Center - General Electric Test Reactor)	:	(Show-Cause)

Holiday Inn - Golden Gateway
Van Ness at Pine
Crystal Room
San Francisco, California

Friday, June 5, 1981

The above-entitled matter resumed at 9:00 a.m., pursuant to adjournment.

BEFORE:

HERBERT GROSSMAN, ESQ., CHAIRMAN,
Atomic Safety & Licensing Board Panel

GEORGE A. FERGUSON, Ph.D., Member

HARRY FOREMAN, M.D., Ph.D., Member

APPEARANCES:

DANIEL SWANSON, ESQ.,
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Appearing for the NRC Staff.

EDWARD A. FIRESTONE, ESQ.,
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-and-

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Appearing for the Licensee.

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Appearing for Intervenors Friends of the Earth, et al.

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P R O C E E D I N G S

JUDGE GROSSMAN: The eighth day of hearing in the show-cause proceeding is now in session.

Mr. Edgar, do you have any more questions?

MR. EDGAR: Not at this time, no, I don't.

JUDGE GROSSMAN: It is now time for the Board questions, and I will start off --

MR. CADY: Excuse me, your Honor. May I introduce as Intervenors' Exhibit 8 the 1979 Staff SER, with the conclusions included into the record?

JUDGE GROSSMAN: Do you have the requisite copies for the reporters?

MR. CADY: Yes, I do. She has been presented with them.

JUDGE GROSSMAN: Any objection?

MR. EDGAR: No objection.

MR. SWANSON: None.

JUDGE GROSSMAN: What is that marked as?

MR. CADY: Intervenors' Exhibit 8.

JUDGE GROSSMAN: Admitted.

(The document previously marked Intervenors' Exhibit 8 for identification, was received in evidence.)

MR. SWANSON: As long as we are on the subject

of exhibits, I have been doing some thinking about the various charts we are using, and we have been doing a lot of marking of just one copy, Staff Exhibit 4. I think what might be the preferable route, if no one else needs Staff Exhibits 3 and 4 in evidence, is to perhaps identify the -- we'll wait until the end of the day and see if there are any more markings on the chart, but take the completely marked-up version of what was Staff Exhibit 4 and ask that that be marked Staff Exhibit 7, and we will just have to make copies and distribute them to the Board, and that will probably be the only one we would then offer of the series of blow-ups of the trench logs. The others are just simplified versions of the same diagram, and I believe every mark that's on Exhibits 3 and 4 is now included on this latest and most complete version.

JUDGE GROSSMAN: Any objection? That sounds like a reasonable course.

Mr. Cady, along these lines, have you offered all the exhibits you intend to offer into evidence?

MR. CADY: All except for the testimony of Dr. Rutherford, who is our structural engineer.

JUDGE GROSSMAN: There were a number of -- well, not a number, but a few documents that were mentioned, I believe your first three documents on the offer of proof, and I'm not sure that you offered any more than the first

document which is the one you just offered now. Are you satisfied to let the record stand this way? Have you -- and if not, have you laid sufficient foundation, you believe, to offer any other documents into evidence?

I'm just pointing that out to you now, and I will proceed with my questions, but you ought to decide some time as to whether you have your documents in order.

MR. CADY: Yes, sir. Thank you.

EXAMINATION BY THE BOARD

BY JUDGE GROSSMAN:

Q Dr. Brabb, it appears to me as though there were certain assumptions that were made in the probabilistic studies that were submitted to you for your opinion, which may or may not be realistic, and I assume that these studies were submitted to you for the purpose -- for one purpose, of determining whether the geologic assumptions were realistic. Was that one of the purposes, sir?

A (Witness Brabb) I can't recall, Judge Grossman, what the purpose was. There was certainly a purpose to make certain that we had all of the information that was being used by the NRC Staff in coming up with their final interpretation, so that was certainly one purpose, of particularly anything that had geologic information in it as one of the inputs.

To some extent, I'm not qualified to review the

mathematical parts of the probabilistic analysis, and I have therefore been somewhat distant or somewhat less than thorough in the review of the complete analysis. In the beginning, as I mentioned previously, I had looked at the geologic parameters, and I felt that the figures that were being used were unrealistic, and I so commented.

In the latter documents on probability analysis, I felt that the figures were more realistic in terms of the geologic parameters, but I had not in fact reviewed every one to make certain, nor was I asked to, to make certain that they do conform to the geologic information.

Q Dr. Jackson?

A (Witness Jackson) I'd like to respond a little more additionally. Since the probability study was done at a later date in a different type of approach, we thought it would be best to assign one geologist to work with the probability team to make sure he worked closely with them on the assumptions, of the validity of the assumptions used in that, and we asked Dr. Slemmons to maintain that role, since Drs. Herd and Brabb were busy with the rest of the project and other duties.

So I think we tried to put Dr. Slemmons as the focal point on that, and questions then on that should go to him.

Q Dr. Brabb, was one of the assumptions used in the

probabilistic studies the assumption that the existing shears within the Verona Fault zone had already been discovered? And let me restrict that to the younger soils. I don't think we are interested in the older soils.

A (Witness Brabb) Yes, I believe that information was taken into account.

Q I'm sorry, that information was what?

A Was taken into account.

Q Well, what I'm asking you is, was it the basic assumption that the existing shears had already been discovered within that fault zone in between the two trenches I believe that the studies were directed towards?

A I'm not sure how to respond to that. I simply don't know the answer to that question.

A (Witness Justus) I think that you may have implied that shears between the existing shears exist, and were they taken into account? Is that -- that may be a point of confusion on our part. If that is -- could you rephrase your question, please, I think is what --

Q Well, perhaps if the answer needs some elaboration, we can elaborate; but my question, I thought, was pretty specific as to whether one of the assumptions made in the probabilistic studies was that the shears that had been discovered were the shears that exist within the parameters of the -- or within the perimeter of the two trenches.

A That is correct.

Q That is one of the assumptions?

A Yes.

Q We have heard some suggestions made by the panel and as directed to Dr. Brabb -- Dr. Jackson, did you have anything to add?

A (Witness Jackson) No, I think -- I really believe that the probability panel that's going to be on knows what the assumptions were better than us, and we have not focused on them, and I want to make sure that we are talking from our supposition. We are not as close to that aspect as that panel would be.

Q Well, let me preface my remarks by saying that all I want are some basic observations on the probability studies in order to lay a foundation for asking the geologic questions, and I think it would be a little unwieldy to put the probability panel on, and then put this panel back on, and so I think where the answers have to be qualified, such as Dr. Brabb has already qualified his answer by saying he can't go into the mathematics, I think it would be appropriate to qualify the answers, but I don't think I'm going to venture into any detailed discussion of the probabilistic studies.

Dr. Brabb, we have heard some testimony to the effect that to the extent that evidence of seismicity is

discovered in an area, it would suggest that there is increased seismicity. That is the extent -- let me rephrase it.

To the extent that shears are discovered, it suggests that there might well be other shears in the area. Is that a correct assumption? Dr. Herd?

A (Witness Herd) In other words, the fact that we have found three shears already, doesn't that mean that there might be more?

Q Yes.

A Indeed, I think that's an accurate assessment. The Verona Fault zone would appear to be not just a single fault plane, but one of complexity. I believe the -- when I first went and mapped in that area, I envisioned the Verona Fault as basically a simple strand along the hill-front. It was in the course of the excavation of the additional trenches that we saw more breaks in that, so it would appear that the Verona Fault zone is quite complex, and that apparent complexity certainly would allow for even more breaks to be found, or to exist that just have not been encountered heretofore in trenching.

Q And in fact, the more breaks you find, the more you expect to find in future trenching, if you were to trench further; isn't that so?

A Well, that might be true. I would think that

there would be a point of --

Q A plateau?

A Right. But characteristically, if you would look at an active fault zone in say just the San Andreas Fault zone, although that is a strike slip fault zone -- well, let's just talk about San Fernando which is a thrust fault zone. If you look at the outcrop pattern of the rupture which occurred during that event, it is quite complex. There are a number of small, little discontinuous breaks. So if we were afforded the opportunity to plane off the surface of the ground in the GETR area, we might see quite a great deal of complexity, not necessarily continuous throughgoing faults, but a number of smaller, intermittent, short-length faults.

Q Dr. Justus?

A (Witness Justus) I believe you mentioned the word briefly "seismicity" when you asked the question, and that captured my attention, too, and in the context of the answer I think it would be important to add that whether more faults are found or not, would not seem to change our view of the seismic potential for that fault zone, such as -- and this is a perspective to the answer, and I think it might relate to the discussion of faulting and seismicity.

Similarly, for the San Andreas, or other faults,

if we find one, yes, it is likely we will find more, but that needn't change our opinion of the hazard.

A (Witness Jackson) I would like to add a brief comment, too. I agree with what Dr. Herd said. However, we had a very extensive trench that was dug in many -- from B-1 trench, especially, and B-2 trench, and between the existing shears we did not see, at least in those soil layers or those stratographic layers, that kind of faulting along that strike of the fault running perpendicular to the strike of the major faults that we did see.

We also can use discovery tools which lead us to request the trenches in the location where we put them, based on our geologic knowledge and judgment, especially the use of aerial photographs now.

So I believe you have to -- I would qualify and say it is possible, and I think it is very reasonable, especially in the thrust fault zone, to expect other shears. However, there are techniques which allow you to make some value judgments on that.

Q Drs. Brabb and Herd, was one of the reasons why you had reservations about the use of the probabilistic studies or the reliability of the probabilistic studies, the fact that there hadn't been, in your opinion, enough investigation of the existing shears in that area in which you could -- so as to allow you to rely on that

assumption made underlying the probabilistic studies?

A (Witness Brabb) Yes, sir.

A (Witness Herd) Can I, just for the completeness of the record -- I did not participate in that statement. As I have said previously, I didn't consider the probabilistic assumptions -- the probability model.

A (Witness Devine) May I make a comment on this?

Q Certainly.

A I think we need to add to these answers the fact that I'm not sure any of our people here are aware of the sensitivity that these assumptions carry. As I recall some of the discussions that I was only peripherally involved, but overheard, was the techniques that the probability people used is not heavily dependent on just exactly how many shears they would find.

So there is a sensitivity as to what it means if there is one found or not found, and that we can't judge.

A (Witness Brabb) I can judge it in the light of whether or not the information that we have to go on, on a geologic basis, is adequate. We have said that we felt that the information was not adequate, and that's how I responded to Judge Grossman's question.

Q Now it also appears to me that certain assumptions were made with regard to the amount of slip within the Verona Fault area, and it appears that that was also done on the

basis of the slip that had been observed in the existing -- in the discovered shears. Is that also correct, that that assumption was made, Dr. Brabb?

A Assumption by who, Judge Grossman?

Q Underlying the probabilistic studies at the rate of -- that the amount of slip or the amount of offset observed in the existing shears is the amount of offset that had taken place within that fault zone.

(Panel conferring.)

A (Witness Jackson) Mr. Grossman, the problem, I think -- and Dr. Brabb may not even know about all of the reports, there are three -- there are two probability studies by Jack Benchman Associates for GE, and two probability studies and reports done for the U.S. NRC by TERA Corporation and Lawrence Livermore National Labs. There are a variety of different assumptions used in those different reports.

Now I haven't talked to Dr. Brabb about this aspect, but I think that you, to be more specific, should comment on which probability study.

Q Well, I want to be general, and I understand that this isn't a probabilistic panel, but nevertheless, Dr. Brabb had certain studies submitted to him for his opinion, and I'm basing my questions on the studies that he's aware of.

A Those studies were not submitted to Dr. Brabb for his review.

Q Well, whether they were submitted to him for his review or his knowledge of them from some other source, I prefer to have his opinion on it, so that we can get on to the geologic foundation for certain of the assumptions.

Dr. Brabb, could you answer that?

A (Witness Brabb) To the best of my knowledge, the amount of displacement on the shear would have been one of the factors in the probability analysis.

Q And that displacement was determined on the basis of observations from the discovered shears; isn't that correct?

A To the best of my knowledge,yes.

Q Is it possible that if there were existing shears that had not been discovered, that the amount of offset within that fault zone could be considerably in excess of what was assumed within those probabilistic studies that you have seen?

A I don't have any basis to make an answer to that question. I don't recall.

Q Well, basically, wasn't the methodology used that the amount of offset observed on each shear was added to the offset observed on the other shears, until there was a cumulative amount of offset determined?

A That is certainly one of the possible scenarios that could be used in the probability analysis. I don't know if, in fact, it was.

Q Well, it seemed to me that the USGS offered an opinion with regard to that at one point, and I recall questioning a 1.5 foot offset that was included in the studies.

Does that -- am I wrong, or does that refresh your recollection as to whether you were ever asked to comment on that total amount of offset?

(Panel conferring.)

A (Witness Brabb) Sorry, your Honor, I don't remember that.

Q Okay. Well, then, maybe I'm wrong.

MR. EDGAR: Judge Grossman, it may be gratuitous, but there is a fundamental point here, that there are two -- there are methodological differences between the GE and TERA studies which the probability panel can point to at some length.

Furthermore, there is another significant distinction. The GE probability analysis calculates the probability of any size offset beneath the foundation. It could be a micron. The GE analysis is independent of size.

In contrast, the TERA, NRC, Livermore analysis

calculates the probability of a one meter offset. So if that's helpful, that's an important distinction.

JUDGE GROSSMAN: Is this along the lines that GE takes the position that it doesn't really matter as to the size of the offset within certain limits, of course?

MR. EDGAR: No, I don't think that's the implication. The purpose of the analysis was to determine what the probability would be of any offset reaching the foundation without regard for its size, and if you do that, then logically one could conclude that if the probability of any offset is low, then the probability of one meter is low.

BY JUDGE GROSSMAN:

Q Well, let me rephrase my question.

If the amount of offset were a significant factor in a probabilistic study, would you consider that there was sufficient information with regard to that Verona Fault zone on which to base a conclusion?

A (Witness Brabb) That's a good question. No, this is one of the elements of our concern, that the critical information needed to predict the future behavior of the Verona Fault, both in the sense of time and in the sense of the amount of displacement, and in the sense of where this displacement will occur, are some of the elements of information that we feel uneasy about.

A (Witness Jackson) I'd like to add a comment, and again put the clause in there that we as geologists, and I as a geologist on this panel, would indicate that in my knowledge and interaction with the probabilistic people, is that the geologic assumptions that we feel are so important are not always as important as we'd like to feel that they are.

That doesn't mean it changes our conclusions, necessarily. In fact, that's what led us to require some level of offset under the plant, even though there was low probability of any offset based on that. But that again could be explored with the probability panel.

I think I am making comments as a geologist.

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Q. I think we have established, Dr. Brabb, that with regard to the probabilistic studies, you are somewhat uneasy with regard to the underlying information that was utilized, or the extent of the information. I recall that yesterday there was some extended discussion with regard to a sentence in the SER in which the geologists indicated their uneasiness with relying on probabilistic studies to a disputed extent.

It appears to me that some or all of the information that you feel that might be inadequate with regard to the probabilistic studies would be very helpful, not only for probabilistic studies, but also for a deterministic study of the seismicity in the area and the ramifications of that.

Is that correct, Dr. Brabb?

A. (Witness Brabb) Yes, sir.

Q. It also appears to me as though, if you had all of the information that you would consider necessary to have a realistic and valid probabilistic study, that you could pretty well make a deterministic study that would conclusively determine to your satisfaction everything that you would want to find out from the probabilistic study.

Could you comment on that statement, sir?

A. I think I agree in general terms with what you are implying, with the possible exception of the word "everything." I don't think in any investigation of the geology it is possible to determine "everything."

What we are trying to do is to get enough information to allow us to proceed with a substantial degree of confidence in making a prediction about the behavior of a geologic process. The end concern is how this process, in this case thrust faulting, is going to impact a facility. And as geologist it is our responsibility to have enough confidence in all of the geologic information that can be reasonably gathered in a reasonable period of time to make a prediction about that process.

We have stated in our report -- and this is the reason for the words that you must have read -- that we have reservations about the amount of information needed to accurately predict that process. If that information was there, yes, on a deterministic basis we would be able to have a higher degree of confidence about the process and its implications.

Q. I overstated that, but I will allow Dr. Jackson to comment.

A. (Witness Jackson) I was going to make that comment, that it was overstated that it's a better way

to do things. In my seven years working as a geologist in the regulatory environment, I have never been involved in a site where there has been enough information to make an estimate that satisfied everyone.

So the use of probabilistic methods -- and I doubt on this site if we would ever achieve, because of the complexity, ever achieve a level where we were all, first of all, in concensus as scientists.

I would like to make another comment, that again the USGS was not asked particularly to look at the probability study; and it has not been my understanding to date that it was a component of their decision and recommendation to us on the one meter of offset being not conservative enough.

A. (Witness Devine) Sir, I would like to comment on that. As I heard your question, there is a part of it I would have to answer in the negative. That is, as I understand the reason for using probability studies is to enable you to assess what you don't know from a deterministic evaluation. And as I understood your question, you indicated that everything you would need to make a good probability study would be sufficient to make a good deterministic study. And I don't believe that's true. That's the reason for using probability studies, is to assess beyond that which you can do

deterministically.

Q. Let me retract somewhat from my statement and put it this way: Would it be your opinion, Dr. Brabb, that if you had sufficient information to make a valid and realistic probability study, that you would have sufficient information to make a deterministic study and leave the -- and therefore have any further probabilistic study of only marginal value with regard to coming up with an ultimate conclusion?

A. (Witness Brabb) I have great difficulty with that question, your Honor, because I have not made the probabilistic study, and therefore I have no basis on which to make a conclusion. Simply, I can't answer the question.

Q. Okay. It was a poorly phrased question, anyway, so we will go on to something else. Did anyone else have a comment that they wanted to make with regard to this entire area?

A. (Witness Slemmons) I have I believe a somewhat different position than Dr. Brabb has stated with regard to the adequacy of the data in order to come up with at least some sort of an assessment by probabilistic methods.

The data for all three fault zones, the B-1, B-3, the B-2, and the H zones all show rather similar

relationships with the most recent offset. All three show at least one Holocene offset. The amount of displacement in each case was about 1.5 or 2 feet, to a maximum of 3 feet. The amount of displacement is for the B-2 and also the B-1/B-3 explored by a number of trench sites, so that I feel there is a reasonable basis for assuming that the greatest probability of the next event will be in terms of something of the order of magnitude of somewhere between 2 and 3 feet.

The probabilistic approach -- well, the second point is that the very long pair of trenches, the B-1/B-2 trench series, gave a very fine and continuous exposure across the two zones in the entire block between. And this was near the GETR site.

So that there is a good data base -- granted it is not a complete data base, in that trenching on the south side of GETR, or trenching on the strike to the east would have given a fuller record -- but at least this gives enough of a basis, along with at least a general knowledge of the fact that new ruptures only occur in a very minor percentage of the cases, something on the order of a percent or two, or perhaps even less.

So that I feel, in my opinion, there is an adequate base for making a determination at this time.

Q Dr. Herd?

A (Witness Herd) I just wish to make a peripheral sort of summary comment in terms of what I believe Dr. Brabb was trying to state.

In our April 1980 report, to which Earl has alluded a couple of times, we make a statement that we felt that the information on fault potential was incorrect, and we felt that since a decision was pending and that there was going to be additional geologic information, we had to make a summary of the data at that point.

I think Earl and I share a concern that, as geologists trying to understand the geology of the site, there are certain unknowns that are not fully developed to the point of our personal satisfaction of understanding as geologists in terms of where the faults are and in particular the amount of displacement that might be in the fault zone.

I think Earl and I look at the cumulative offset in the zone as being something of particular different perception than worrying about the single displacements on single breaks. And it is the fact that we can add two feet in B-1, three feet in B-2, and at least 1.5 feet in H across the zone in a perpendicular line crossing the break, that would suggest that there

is at least -- two plus three is five -- 6.5 feet of displacement that appears to have occurred at least in the Holocene across the zone. And we see similar amounts of displacement in excess of 5 feet, or so it would seem, at T-1 in the same zone that we were uneasy about making any sort of assessments about limits, or understandings of displacements on the breaks.

I don't believe that Earl wishes to imply that this has not been factored into a probabilistic study. It is just that we have uneasiness as geologists in trying to understand that before going on to the next step, which is to try and apply that information.

Q. Do you have reservations not only with regard to the amount of cumulative offset, but to the amount of offset that may have occurred at any one time?

A. Not to dig a deeper trench --

Q. What?

(Laughter.)

A. Not to dig a deeper trench here in terms of the discussion on this point, or not to belabor the point -- that was a bad pun, and I am sorry, but it has just been a long time.

(Laughter.)

If there were additional breaks in the zone, the actual offset that's occurred in the Holocene might be greater in trying to calculate the total offset. That's uncertain. H, for example, doesn't have an upper soil from which to assess the amount of offset that actually occurred there. It is at least 1.5 feet. So there might be a little bit more in the Holocene offset. That's an uncertainty and an unknown.

If there were other breaks, as well, in between, we might add a foot or so of additional displacement. These are unknowns of that sort.

A. (Witness Jackson) Could I add an additional comment?

Q. I think Dr. Brabb wanted to comment first, and then we will be glad to hear that, Dr. Jackson.

A. (Witness Brabb) I thank you, I did have a comment but I got distracted by Dr. Herd's comment, and therefore I wonder if the recorder could repeat the question?

(The reporter read the record.)

Q. I think -- let me rephrase it, if I can, or at least indicate -- in view of the difficulty in locating the last question, my question related to whether you had some reservations not only with regard to the cumulative amount of offset, but also with regard

to the amount of offset that may have occurred at any one event or episode?

A. Yes. That is what I wanted to respond: Yes, that we have reservations about the amount of movement that can take place on any one splay in the fault zone.

Q. Dr. Jackson?

A. (Witness Jackson) Yes. I wanted to comment that one of the observations that we have made is that and try to make when we're doing a site review, and especially one like this, is to look at consistency, reasonable consistency or something that is an anomaly that would stand out as an example.

If we are looking and observing one meter offsets, they would generally -- during each earthquake event, if you like -- those would generally related, based on our knowledge of worldwide data, to a magnitude about 6.5.

I think if you were observing extensively greater amounts of movement, and that we know from the dispersion in the data that you could get up to 2.5 meters based on what occurred at San Fernando, over a zone of some distance, or across a given single fault splay, if you were to look at something greater than that then you would have to be looking at a greater magnitude earthquake occurring. And I think it is our

belief in general, looking at this, that that then gets -- if you were looking at much greater offsets than that, you then begin to have difficulty in justifying the credibility of a magnitude on that particular fault zone.

In other words, the ability of that fault to sustain that kind of magnitude. So what you have to look at is consistency. That doesn't mean that because you haven't seen everything you can't make a reasonable estimate by ridiculous type arguments.

Now in the earth sciences you can't preclude anything. That is one of the problems we have. And I think that a consistency would lead us to believe that you need much bigger magnitude earthquakes to get greater offsets.

Q. Dr. Brabb?

A. (Witness Brabb) I'd like to make it clear that the limits of my reservations, if you will, are not to the extent that I would consider them ridiculous. That is to say, we are not talking about 100-foot displacements. We are not talking about San Francisco 1906 type earthquakes.

Realistically, our position is reasonably close to the NRC Staff position, but it still differs. And this difference is not in terms of hundreds of feet, or even tens of feet. The difference is relatively

small. And I think you will have testimony later today from Dr. Pichumani who will say that this amount of possible difference between us and the NRC Staff makes no difference with respect to his calculations in terms of offset beneath the reactor.

A. (Witness Jackson) I agree with Dr. Brabb. I think the differences are within the dispersion of the data that we know about for a fault of this type.

Q. Okay. I don't want to belabor it, in view of what you've just said, but just one final question along these lines.

Do you agree with Dr. Slemmons' position that the observations along three shears, I believe, are sufficient to allow you to assume some consistency with regard to the amount of offset at any particular event?

A. (Witness Brabb) No, sir, I don't agree with his statement.

Q. Regarding your April 1980 report, I have some difficulty understanding what was meant in one sentence. I would like to read that sentence, but I will read the prior sentence first. That is in the summary, the second paragraph, and I will read both sentences:

"We concluded previously that the number, location, length, width, geometry, and age of these

thrust faults have not been determined adequately, and therefore that the potential for future surface faulting or vibratory ground motion at the reactor could not be adequately or reliably assessed." I'm sorry, there are two preliminary sentences, and I will read the next one, too: "None of the new information provided by the General Electric consultants has changed this opinion."

But then I come to the sentence that I don't understand. That says: "However, inasmuch as the consultants have provided information on fault potential that we believe to be incorrect, and inasmuch as a decision regarding reactor safety could be made without obtaining the additional geologic information we feel is necessary to assess fault potential, we provide herein a preliminary interpretation of some of the critical fault parameters."

I will start off with the easy part of that, and that is the last part of that sentence. What did you mean by: The decision could be made without additional geologic information?

A. Well, just that in terms of the focus on gathering information about the width of the fault zone, the way this fault zone projects beneath the ground, beneath the reactors, the total length of the Verona Fault, the relation between the Verona Fault and some

of the other faults in the area, we felt that that information for our purposes to try and predict the future behavior of the Verona Fault was not sufficient; and that it appeared that in our conversations with NRC Staff, that for a variety of reasons that this information would not be obtained.

Therefore, in that light we tried to do the best we could to summarize from our perspective wnat information was available that might be helpful in making the final decision.

Q. Well, it appears from what you are saying now that you believe that there was sufficient information to make a negative decision with regard to recommencing operations there. Is that correct?

A. I'm sorry? I don't understand the question.

Q. It doesn't seem to me as though the lack of information could contribute to a decision to begin operations again at the reactor site, but I am asking for your opinion as to whether the decision you thought there was sufficient information to make was a negative decision or a positive decision with regard to the ultimate conclusion?

A. It didn't make any difference to me whether the decision was negative or positive.

Q. Dr. Herd, did you have a comment on that?

A. (Witness Herd) Well, I think that when this was written, the context in which the sentence was written is not in the sentence in which it is now cast in terms of the weight by this Board as to whether the work that has been done is or is not adequate in your estimations.

The questions that were unclear to us were ones of geologic parameters of the site. And there was at least an impression that there was going to be additional work continued at the site, and I believe it was a time after one of the ACRS meetings that -- well, I don't know if there was the impression, but it looked like we were still in the course of an investigation. And then there was a point that came that we needed to make some assessments.

It is just the idea that -- well, how to restate this. There isn't an attempt in this sentence from my perspective to try to make a decision for the Board as to whether the information is or is not adequate. It is, rather, a comment on the geologic information which was available to make larger assessments of questions which contribute to this decision.

And I don't personally wish you to judge this sentence, of which I am a co-author, that we are trying to conclude on the safety of that reactor.

Q. Let me refer you again to that sentence. It is very specific about a decision regarding reactor safety. It wasn't a decision regarding geologic parameters.

A. (Witness Jackson) Mr. Grossman, could I maybe help out a little bit? I may be wrong, but my reading of the sentence went to this end: The review of the GE Test Reactor had gone on from August of '77 to October or November of '79, with investigations, a tremendous amount of work, need for additional work -- I think we would all admit that as geologists we can always gain more information.

It was the judgment of the NRC Staff and management that the problem and the possibility of further information contributing to a different conclusion, or the ability to reach a decision which could be litigated, that the time was ripe to do so.

Now we had then made a decision to go forward with the first September of '79 report, which went to the Advisory Committee on Reactor Safety. It was at that point in time that we had the first position. That was then reviewed, and additional requests by ACRS to implement the probabilistic studies.

Those were then done. The Advisory Committee on Reactor Safety leans strongly toward the Staff

utilizing probabilistic methods. And indeed, in the absence of I believe their conclusion that it was unlikely that the ranks would close, so to speak, by additional information, that one way to approach this was by the use of probability.

So I think it was at a point of diminishing returns on the investigations. We have argued for several years on whether or not this is a landslide or tectonic in origin. Now although that is stipulated in these proceedings, I think that that is still symptomatic of the differences that still remain here between competent professional people, from a vast array of organizations.

So it was just felt that it was time to reach a decision. Now I believe that this report here was written because we told the USGS: It's time to go forward and put together what we currently have and reach a conclusion for licensing purposes.

It was apparently clear, too, that the General Electric had completed the investigations that they had intended to do. We had requested some -- we had been party to the investigations that had been put in to date, and each set that we had requested we felt would lead to the necessary information.

So I personally don't think that another

Phase III study would necessarily reach complete resolution, either. So it is at a decision point. And that is what I believe that decision was in this document, what was being referred to.

Q I believe what you're suggesting now, Dr. Jackson, is that you informed the USGS that a decision was going to be made at that time without any additional geologic information?

A That's correct.

Q However, the import of the sentence and what the sentence says is that a decision regarding reactor safety could be made. And I'm not sure that what you're suggesting is what Drs. Brabb and Herd had in mind when they wrote the sentence. And so, with what you said as background, I would like to direct them again to that sentence and ask them to indicate what it was that they did have in mind: That there was sufficient information to make the decision at that time? Or, as you suggest, that regardless of whether there was, a decision was going to be made anyway and that is what they meant by that sentence?

Dr. Herd?

end JWB 1-2

(Panel conferring.)

A (Witness Herd) For my own, I was trying in the context of this statement to state that we felt that we did not have the geologic information to answer certain questions about the geology at the site in terms of fault offset.

Remember, we had, with the cover letter, said that we weren't sure that one meter was conservative. Okay? Uncertainties about displacement. We have talked about the possibility of faulting beneath the reactor vessel, uncertainties there. The questions about the existence of other faults, and the cumulative displacement.

It was these uncertainties in terms of ones on a geologic basis where we were, did we have our complete picture that we were at in trying to talk about this point.

But I have -- we had no contribution and no input -- excuse me, we had no involvement whatsoever in terms of doing anything more with our information than just simply providing it, and we were not in the decision-making role of deciding whether the data by itself was adequate, coupled with other probabilistic studies and the like. It was just a comment directed towards the geologic information which, for myself, I did not feel to be full and complete to my satisfaction.

So I am trying to separate myself very carefully from an issue which I don't wish to cross; that is, to imply

that I am making an assessment of reactor policy licensing. That is the Nuclear Regulatory Commission. We are only ones providing an assessment of the geologic data and a review of that geologic information. It was information that would be contributed towards that decision by the NRC that we were commenting on.

Q I understand your reluctance to get involved in that area, but apparently you were involved in the area, and I am not asking you now to make that --

A Can I make a clarification? I'm not sure we were involved in that area. The point is that we acted as independent reviewers of the geologic data, and we were asked by the NRC to provide input. We had no role -- I certainly wasn't asked by Dr. Jackson or anyone to make calculations of the expected displacement underneath the reactor. We just contributed the geologic information to Mr. Devine and Mr. Morris, who reviewed the material, made their own contributions and forwarded it on to the NRC. The Survey's role has been and remains one of providing a review capability in part, as well as geologic assessment and, I guess, seismological ones, too.

Q By the way, you mentioned one of the -- one item of missing information, the fact that there might be a fault underneath the reactor, but from your understanding of the probabilistic studies, wouldn't the result be affected

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by the existence of shears that would not be directly underneath the reactor, too?

A (Witness Brabb) It could be. That's to say the cumulative measurement that we are talking about, the addition of trench H, trench B-2 and trench B-1, the cumulative measurements of those fault zones are some of the factors that are used to try and predict the future behavior of the fault.

If there are additional fault strands, additional displacements that are must be factored into there, then the answer is yes.

Q I wasn't specifically referring to the cumulative displacement now. I was talking only with regard to the number of shears, because my understanding is that the probability of there being an offset underneath the reactor is based in part on the number of shears that were observed within the fault zone.

A In fairness, I have to say that I think that if that is a factor, and I don't know for sure that it is, if it is a factor, it's unlikely in my opinion that there are a large number of additional shears. So that to have a bit of caution, we are not talking about larger variations from the data base that already exists. It might be a relatively small variation.

Q Dr. Jackson?

A (Witness Jackson) I'd like to add the additional comment that we seem to be discussing, and the way the questions have been going, a step-function type of consideration that you are on a cliff each time you say yes or no to this answer, in answering.

My limited understanding of probabilistic methods doesn't work in that way. For instance, an additional -- finding an additional shear may affect the probability number and change it. It may change it one way or the other, or it may not have any effect. And such sensitivity studies, as I understand it, have been run.

So what it would do is it may change the probability number from like, say, some hypothetical 10^{-5} to some hypothetical 10^{-4}. The probability panel can testify as to what that may be, but that's not a step function type situation where you are saying that in one case the probability would be 10^{-6} but I find an additional shear, that the probability method is no good.

It would have a gradational effect on the calculation, to the best of my understanding.

A (Witness Brabb) I think I was saying that in slightly different terms, your Honor.

Q Well, now, we're discussing the relative importance of perhaps discovering additional shears, but wouldn't you, as an example, think that the difference

would be significant if, let's say, you based your study on there being "X" number of shears within a certain area, and you discovered that there were 2 "X" or 3 "X" shears in that area? That would be a significant difference, wouldn't it?

A (Witness Jackson) My understanding of the model, if it's important to the model, as the probability experts use it, then it would. But it may not be that significant to them.

A (Witness Devine) I'd like to comment again on that. My brief involvement with the probability people, I am reminded of the fact that I was continually surprised at the things I thought were so significant just were not in their studies, and I think we need to be cautious about this group of geologists and seismologists commenting on what is significant in a probability sense, because I was continually surprised at what was significant and what was not.

Q I understand your caution, and I understand your wanting me to direct these questions to the probabilistic panel, but again all I can say in that regard is that if we want to find out -- if assuming there was a determination that "X" number of shears existed in the area, and we were to wait until the probabilistic panel came on to ask them if that was an assumption, we would never be able to

ask you whether "X" number of shears was valid from a geologic point of view, and so we do have to get into that area here, and I understand your caution, and I am trying to do it as cautiously as I can.

Mr. Devine?

A Yes. If I may, sir. I remember one conversation on that very point. I have forgotten who was in the group, but I remember one of the trenches, I argued suppose it stopped for five feet short of an additional shear. Therefore, you would miss one shear simply because the trench didn't go far enough, and I was quite adamant on the subject. That would add one more shear, and I noticed the probability guy smiling, realizing that I just didn't understand what they were going to do with the data, and yet I was convinced that one more foot or five feet of trench, discovering another shear, would be most significant in my mind. It was not in theirs.

A (Witness Jackson) I would add that Dr. Slemmons will be on the panel. He has worked with -- I don't know if he is in complete agreement with Dr. Brabb on all the aspects of the case, but he will be available to ask the geologic assumptions. I think to try to reasonable portray all of the people here on this panel --

Q Well, from questions that have already been answered, obviously he is not in complete agreement with all

the other geologists here.

Dr. Brabb?

A (Witness Brabb) Can I comment, perhaps, a little more generally?

I think our position in relation to Dr. Slemmons in terms of the amount of offset is very, very close. I think he says, for example, or he has a statement to the effect that in his opinion on any one shear within the Verona Fault zone, a movement of two to three feet is most likely, but that movements up to two and a half meters are possible.

Our position is reasonably close to that statement, so that in terms of the overall perspective, I think that we are in relatively close agreement with Dr. Slemmons on almost all of the geologic aspects of the investigation.

So I don't want to give the impression that my answer to that one specific question indicates that we have substantial differences of opinion. We are very closely in agreement on almost all of the geologic issues.

Q Well, if I understand your closeness and agreement, it is to the effect that Dr. Slemmons bases his position on the fact that approximately one meter offset was observed in three trenches, and your position is based on the fact that approximately one meter of offset was

observed in two trenches, and that there is a possibility that it was five or six feet in the third trench; isn't that basically the extent of your agreement on that point?

A Dr. Slemmons was not in trench T-1, and therefore I don't think he would disagree with our analysis of the data in that trench.

A (Witness Slemmons) I'd like to comment that I agree with the statement just made by Dr. Brabb. When I left on Wednesday, you left a question for me to think about, and that question related to whether if it could be verified that the five, six or seven feet of apparent offset at trench T-1 was in one event, would I change my opinion with regard to the three feet on either faults B-2 or B-1, B-3, and my answer would have to be yes in that case.

I would have to raise the lid. The question that cannot be answered is to whether the Las Placitas Fault is contributing to the displacement that runs through the gap, and then splays out into the two branches. If it were to splay out with movement going on one time on one fault and on another occasion on another fault, or being split from time to time in various proportions, then one would have to assume for conservatism that the maximum amount of offset would be the type that you would see at trench T-1.

On the other hand, if you are dealing with a folding and deformation mechanism of sufficiently plastically deformable materials, the Livermore gravels of Vallecitos Hills, then the two faults or the two shears could have a conjugate relationship and partition in a rather systematic way, so that each of the two shears might split, say half and half of the displacement, and so I think that this is something we cannot answer from the conjectural nature of much of the information that we have from trench B-1, and this would modify my position. And the position that Dr. Brabb and I have is not as great as our answers to your question would indicate.

A (Witness Jackson) Mr. Grossman, could I just add one thing?

Q Certainly.

A I'm sorry to continue adding, but I think it's important for your knowledge.

I don't think there is a disagreement per se among this group. We know the data on this site as well as I think any group that I have worked with can. The problem stems from the fact that there are different viewpoints. We are all professional geologists and have slightly different interpretations of the same data.

What is important, we have looked at the data on trench T-1 and the new constructions that have occurred

even this week which, you know, we've worked together on, and that does not change our conclusion as to the estimate of slip under the plant.

And the reason it does not is because you are discussing -- we have always assumed that somewhere on the fault zone a 2-1/2 meter slip type thing could occur, but that the likelihood of that was low. It was most likely one meter, based on the observations we have seen in the existing trenches, and based on the fact that there is a lower likelihood between the existing shears than on them.

Q Okay. Let's move on to some other areas that won't take quite as long. I know that the parties have stipulated with regard to the rate of slip, and I don't care to do anything to jeopardize what has been stipulated.

But, nevertheless, you arrived at a rate of .0004 feet per year, I believe that is, and it was based on underlying assumptions, and it does appear to me as though some of the underlying assumptions may be in dispute, and I would like to find out whether any of the assumptions that are in dispute would affect that rate of slip such as the offset that was observed in the trench that one party says was three feet, and another says may have been five or five to seven feet. Would that affect the rate of slip?

A (Witness Herd) May I refer you to Figure 14 of our April 1980 report to help answer that point? It's page

34, Appendix B of the May 1980 SER. Have you figure now?

Q Yes.

A Okay, fine. The rate calculation is one that is visually fit to the data, and you will notice that there are a couple of boxes immediately above the zero at the far left axis, apparent dip slip separation, there is a small little black box, which if you'll notice to the right reads Albic horizon/stoneline offset.

This represents the cumulative offset that has been measured in that age timeframe. We are talking about the Albic horizon, that would be the A-2 horizon, and the stoneline at the 17 to 20,000 year old item.

Okay. If you notice, then, just above it, there it says youngest buried soil. That would be the cumulative offset measured in the amount of offset in that buried soil, 70,000 to 130,000 years, and then in the far upper corner is the Livermore gravels.

You'll notice that neither box actually controls that line. That line is fit between them. If you increase the apparent displacement in the Albic horizon/stoneline on the left axis, all you do is you just move it up a few feet along that axis, and the line .0004 feet per year still lies between it. So I don't believe it would have a significant impact in terms of the rate calculation. This average slip rate would still be more or less about the same.

Q And would the line be basically about the same if you change your estimate from the 17 to 20,000 year period to the 2 to 4000 year period for the --

A I already have used the 2 to 4000 years in my calculation here. I misstated that. I was trying to reference it to discussions of yesterday and the positions of Earth Science Associates and Dr. Shlemon.

So if you -- it doesn't appear to me that it would have that much impact to increase that displacement.

MR. EDGAR: Would you point to the table from which you derived the data, just for cross reference?

WITNESS HERD: Sure. That would be our table shown on Figure 12, page 22, and most of the numbers there are in agreement with those reported by Earth Science Associates, except for T-1.

WITNESS JACKSON: I do have an additional comment.

BY JUDGE GROSSMAN:

Q Dr. Jackson?

A (Witness Jackson) There are determinations of slip rate that have been made by four groups and several different methods have been used by some of the groups. So there are cross checks that enter into also, and I think -- we have a table that was presented at the ACRS meeting that might be helpful to show the range, if you

need it.

Q I'm satisfied from what's been said that there wouldn't be any great deviation, and I assume my fellow Board members are satisfied.

Dr. Jackson, I believe in response to some questions on cross-examination you indicated that the lack of knowledge with regard to the entire length of the Verona Fault was not critical because for one example you could use the area of the fault to make appropriate calculations. Was that correct?

A That's correct. And there was another one called slip rate vs. magnitude determinations.

Q For the area of the fault, wouldn't you also need the length of the fault, or is that calculation made in other dimensions?

A No, you need the length of the fault for the area.

A (Witness Devine) If I may.

Q Yes, sir.

A You do, but that's not a very sensitive parameter to the magnitude estimate based on area. That's a very -- a rather crude method to estimate magnitude, anyway. So if you change the area 50 percent or so, it only has minor impact on your estimate of the magnitude. It's not a very sensitive parameter to your estimate of the magnitude.

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So if you change one parameter one dimension, in order to then compute your area to then estimate your magnitude, a small change in that length or width is not very significant.

A (Witness Jackson) I'd like to amend my response. I'm not trying to indicate in any way that fault length is not an important parameter, but it is in -- determination of magnitude by any method is not good, except to go back to first principles. It has a variety of errors and requires a fair level of judgment to make such a determination.

Now I think where it begins to become important, when you are showing vast differences in leggth, in other words. My personal opinion is from what I've observed is, if you're getting substantial differences in the potential fault rupture distance, then you can change the magnitude.

But there are ranges about which there would not be tremendous difference in the magnitude determination, necessarily, and indeed even to go beyond that which is more important to what's trying to be determined for this site is that the relationship of ground motion to the plant and the fault propagation parameters again relates to magnitude in a dispersion type of way.

Q Dr. Brabb?

A (Witness Brabb) Your Honor, would it be possible

to take our morning break at this time?

JUDGE GROSSMAN: Yes, sir. Why don't we take 10 minutes?

(Recess.)

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Q. Mr. Devine, I would like a clarification of your prior answer. I am not sure whether you were saying that a variation in the length which wou d vary the area result would not or could not -- would not be significant with regard to the area input? Or whether you were saying the area input into determining the magnitude may not be that significant, so that any effect would significantly effect the result? Could you clarify that?

A. (Witness Devine) I think both statements are true, sir. The areas itself is -- the magnitude derived from area computations is relatively insensitive to the change in area. For example, you can double the area used and you'll get a magnitude change of about, at most, a half a magnitude.

So the change in the area of 10 percent, 20 percent or so, does not significantly impact your estimate of the magnitude. Consequently, if you change one dimension used to get the area, that's also insensitive. The magnitude is also insensitive to that.

A. (Witness Ellsworth) Could I comment also, your Honor?

Q. Certainly.

A. The relations that have been derived between parameters such as magnitude and other measurable data

on faulting such as fault displacement or fault length have been determined from worldwide data, and there is a great range and variation in the other parameters that would enter into a physical calculation of magnitude based on complete knowledge of the earthquake.

So that when one attempts to carry these relations that are determined on a local basis to a specific site, one has to consider that there is dispersion in the relationship that's being used. There is also an uncertainty which will be generated because of the logarithmic nature of these relationships. For example, in many types of relations that have been derived between a parameter set such as magnitude and fault length, the magnitude depends upon the fault length in a logarithmic way. In other words, one takes the logarithm of the fault length, and it is that logarithm that contributes to the calculation of the magnitude.

So it is a relatively insensitive number.

Q Does everyone on the panel agree with that?

(Panel members nod affirmatively.)

Not having heard any objection, I guess you do. Now wasn't there also some question with regard to the data itself as to variation in length, and possibly area, in that some of the data, while you say were

worldwide data, I believe were specific data related to the San Fernando event.

Now wasn't there a significant variation in that particular set of data?

A. Yes, sir, that is certainly an additional complication; that at any given site we will not have complete knowledge of the event; we will have only some sample of observations, and their mean value or their maximum value may perhaps be representative of the event. That is the assumption that is used to make that type of calculation.

There is an additional assumption that goes into say the application of these formulas to the GETR. When we look at the Verona Fault, we don't know what its downdip depth is. So that's a number that we have to assume based on our experience elsewhere. And that may have an uncertainty of a factor of 2 built into the number. So there is an inherent uncertainty in the magnitude calculation. It is simply calculating area based on the surface expression of the fault and will have an intrinsic uncertainty of perhaps a factor of 2.

Q. Now one matter that Judge Foreman has brought to my attention with regard to your calculations as to the Verona Fault is that it is based primarily or merely

on the length of an area of the Verona Fault itself. Isn't that correct? Without attributing to the Verona Fault any length of any other fault that may be connected to is?

A (Witness Devine) Sir, I believe there were several aspects of that question that are hidden behind the words you've actually said.

One, I personally have not made an actual magnitude assessment, looking at the length or area of the Verona Fault itself. The magnitude that was agreed upon was derived by NRC. I merely checked that magnitude to the sampling of data one would use to estimate, and it appeared entirely reasonable to me and I accepted it without going into great calculations myself. So I can't personally answer whether my own calculations changed, because I didn't make them.

But I could indicate that there are physical limits to how far you can extend the Verona before you run out of space. I looked at that in my assessment of the NRC's judgment of 6.5 magnitude being adequate, and recognized that in my judgment you run out of space to get a fault much bigger -- to generate an earthquake any bigger.

Q Did you have something to add, Dr. Jackson?

A (Witness Jackson) I was going to endorse

Mr. Devine's comment. The original review was done by the NRC Staff seismologist who is no longer with the NRC. Mr. Devine was asked to review that and support it on this panel.

Q Well, now I am a little concerned that we may be getting a circular interpretation here. I was assuming in my questioning that you were arriving at a particular magnitude earthquake through your observations with regard to certain inputs such as length and area.

And now your answer seems to suggest that while that may be so, you then make some intermediate assumption as to magnitude in order to limit the length some way. And I would just like you to clarify that for me, Mr. Devine.

A (Witness Devine) I'll try to.

The reason I approached the problem in the manner that I described was to assess what the maximum could reasonably be. A detailed estimate and evaluation could very probably have resulted in a lower magnitude, but I felt that that was an unnecessary calculation to have to make since it was already being imposed upon this site the higher magnitude.

And recognizing that there are physical limits to how far the Verona -- how much area, or length, or width the Verona can have told me that in my judgment

the stipulated -- or the directed magnitude of 6.5 was indeed conservative. So I did not go through the calculations to see what the best judgment guess would be, or best estimate on the Verona because in my judgment it would have to be less than that.

Q. Now let me ask you whether or not you concluded yourself that it was the length and area that was used based only on the Verona Fault? Or did it include the Las Positas Fault, too?

A. Yes. I looked at that, and in my judgment if you include the Las Positas also you get magnitude estimates of about 6.5.

Q. In other words, the data was based -- the conclusions were based on only the Verona Fault, but in your opinion if you add to it the Las Positas Fault you would come up with figures that are not much different? Is that basically what you're saying?

A. The last half of that is right. I'm not certain what parameters the seismologists at NRC used to originally estimate the 6.5. But in the last half of your sentence, in my review of that that is what I did. I included the Las Positas.

I do not know whether the seismologist originally included the Las Positas when he directed the 6.5 magnitude. I did when I reviewed it.

Q Dr. Jackson?

A (Witness Jackson) The best of my recollection is the seismologist calculating the magnitude used primarily just the analogy to the San Fernando earthquake to get that estimate, and I believe that is what the SER states.

I again caution the Board: A magnitude is an estimate. There are a number of ways of getting to that estimate. They are again not a one-on-one relationship where if a certain parameter is X and there is a magnitude that goes with it, you can go to formulas in Dr. Slemmons' tables, you can go to other people's formulas of fault area, but in the end when you make an estimate it is indeed that: an estimate.

Q And let me point out that the only way we can determine whether it is a reasonable estimate is to take each of the elements of your estimate and see if they can stand. And that's what we're trying to do right now.

Mr. Devine?

A (Witness Devine) Yes, sir. To put Dr. Jackson's comment maybe in a specific perspective, as Dr. Ellsworth just indicated the fault length for example is a logarithmic relationship, as loose as that. The best fit of the magnitude length data is on a

logarithmic basis. So changing the length even by 100 percent is a very insensitive parameter in deriving a magnitude estimate.

So reviewing a magnitude of 6.5, it well encompassed all the area that I could envision on either of the faults.

Q. Dr. Slemmons, it appears to me as though you have some opinions on this area, and we would welcome hearing your observations.

A. (Witness Slemmons) I guess I have also independently taken a look at these figures. I was not involved in their being established.

There are at least three ways that you can come up with an estimate. One is just experience, having seen many zones and getting a feeling for the size and setting of the structure. And from that standpoint and my experience at least, 6.5 seems like a very plausible value, or a very reasonable decision.

Secondly, if you use either individually or the combined faults, and using say the Vallecitos hills with an 8.2 kilometer length and tying it to the Las Positas, you again come up with about 6.5 magnitude.

A third method would be, again using the relatively hard data indicated by shears B-2 and B-1/B-3, if you use 2 feet to 3 feet you come up with estimates

that would be in the range of about 6.4 to about 6.7. So you have internal consistency there.

It is this kind of internal consistency, too, which personally leads me to the conclusion that the Trench T-1 probably has a cumulative effect of two events, rather than being one event.

So there are actually three different avenues that I have used to agree with the 6.5 magnitude estimate.

Q. Now let me ask you specifically, you mentioned an 8.2 kilometer length of the Verona Fault. What would you add to that for the Las Positas Fault?

A. The Las Positas, if you had it rupture from the point of nearest approach to the northern -- to a northern termination against the Greenville, you would have a 15 kilometer length.

Q. In addition to the 8.2 kilometers?

A. Yes.

Q. And are you saying that the addition of the 15 kilometers to the 8.2 kilometers didn't materially -- wouldn't materially affect the result?

A. That's correct, because a 15 kilometer length on a strike/slip fault leads to approximately a 6.1 or 6.2 magnitude. And the amount of energy for that when added to the energy equivalent for about a 6.5, makes

perhaps only a tenth of a magnitude difference.

A. (Witness Jackson) One point of confusion may be the relationship between the Las Positas and the Verona that Dr. Herd discussed earlier, one being a thrust, and the other being a strike/slip fault which can actually be the limiting -- can join with or truncate the two fault systems. So I think the problem -- a bit of the contribution to the problem is the additive nature, whether you can add them one-on-one with each other in terms of length.

Q. Now your result, I take it though, would be significantly different if in your input you use the San Fernando fault, and you assume that the thrust length was 5 kilometers rather than the -- or the zone of faulting was limited to 5 kilometers rather than the original figure that was used? Isn't that so, sir? Wasn't there any testimony to that effect, that the --

A. (Witness Brabb) Your Honor, I think you may have reference to the width of the zone of faulting, and we wish to get in the stipulation that although the displacement did take place over a 200-meter zone, that most of it took place within 5 meters. I think there was a transposition from 5 meters to 5 kilometers in what you were inferring.

A. (Witness Devine) And that is the width of

surface breakage; that's not in any way a calculation of the area or the fault width magnitude determination with the zone of displacement.

Q. But was that one of the inputs into your determination of magnitude?

A. The 5 meters of offset? No, it was not.

Q. It was not.

A. It does not fit.

Q. We have heard some testimony with regard to the method by which GE's consultants ESA determined the vertical acceleration. And my understanding was that they used, at least in part, some data from the Imperial Valley faulting episode; and that they omitted two data points.

I believe you gentlemen were in the hearing room at the time that testimony was given. Mr. Devine?

A. I was not. I believe Dr. Herd was, but that was last week and that was before I got here.

Q. I believe your -- Oh, and Mr. Devine you in fact are the acceleration expert on the panel.

Dr. Herd, was what I said in substance correct?

A. (Witness Herd) Yes, sir. But please don't ask me too many questions, because I'm not a specialist in ground motion.

Q. Well, Mr. Devine, notwithstanding what I said, are you basically familiar with the methodology used by GE's consultants in arriving at the vertical accelerations?

A. (Witness Devine) I believe so.

Q. And basically -- and keeping in mind what I just said, then, would you consider that that was an appropriate method to use to determine the vertical accelerations? To eliminate two data points with regard to the Imperial Valley event?

A. I am caught between "methodology" and "specific points." I have no problem with the removal of anomalous points, with cause. I cannot recall at the moment just specifically which two points were removed from the data. I'm sure I know one of them. I can't be certain of the other one.

Q. Well, one was a 1.74g reading --

A. Certainly. That one I --

Q. -- and I don't believe we had an exposition on what the other point was.

A. That was my concern. I am familiar with the 1.74g data point, and I understand the cause for which it was not included in the data set. And I would agree with the problems associated with the usefulness of that data point.

Q Well, let me make it a more general question. My understanding is that there was a variation between that reading and other readings, and perhaps there is some or are some questionable points with regard to that reading. But is it appropriate in a calculation such as that to single out particular data points and eliminate them, rather than to include them in the overall determination?

A In my judgment, sir, if there is a reason to have serious question on the validity of the data point, I think it is valid to remove it from your data set, if you have sufficient cause.

And in my judgment, there is sufficient cause to be very concerned about the adequacy -- or the value of that data point. The point itself was a good data point in that that instrument did indeed record 1.74g. There's no question on that. But what the use of that data, to then try to develop expected ground motion at other sites is an entirely different point. And there, I would be greatly concerned about using that data point to skew my estimate of what one would get from earthquakes in other areas.

MR. EDGAR: Judge Grossman, I could hand Mr. Devine the piece of testimony that is relevant here, and perhaps that would help him.

JUDGE GROSSMAN: Okay. Though let me first ask a few questions.

BY JUDGE GROSSMAN:

Q. I want to distinguish between the use of that particular data point as the maximum vertical acceleration for which there may be some information that makes that questionable, and the use -- and distinguish that from the use of that data point along with all the other data points in order to determine the mean acceleration for that particular event. And I think your answers are more directed toward that first possibility, or the first use of that data point, rather than to the second use.

I want you to concentrate specifically on that particular event, and whether you consider it justifiable to eliminate that data point and some other data point in arriving at a mean acceleration for that event.

A. (Witness Devine) Yes, sir. As I indicated, that is a data point. It's a legitimate data point, in that the instrument did indeed respond to ground motion of 1.74g. It's not an instrumental problem. It's not a spurious data point in that regard.

On the other hand -- and if one were going to take strictly an arithematic mean of all peak ground

motions, obviously that is a data point that would be included. But if one is trying to make estimates of ground motion that are realistic and apply to conditions that are being observed, or being used at another site, that point is not realistic because it was influenced by a variety of factors that are not present in the rest of the data set. Therefore, it would skew the data incorrectly when trying to say this is what you get under average conditions, or under specific conditions, because that point does not fit those conditions.

Q Okay, now I -- Dr. Jackson, I'm sorry.

A (Witness Jackson) Just one brief comment, and I don't want it lost in the discussion. We are still talking about peak accelerations at a high frequency, and not the total accelerations measured even in that 1.74g record. It has other accelerations at different frequencies throughout it.

So we're still talking about peak acceleration observations.

A (Witness Devine) I can discuss the impact of that if you wish, but your question was directed at "peak," and so I answered it in that manner.

There is a very significant point as to the usefulness of peak accelerations even when they're good.

A. (Witness Jackson) Could I add one other comment?

Q. Certainly.

A. We don't discuss that in the Staff Seismology Safety Evaluation Report, and the reason is, it has only become -- it has not been identified in the past, because the structural engineering group take the horizontal and take a percentage of that and use it for the vertical. And that was done by a recommendation by a number of consultants, and I am not familiar with all the details of that.

It was only as of the recent attention to vertical acceleration, based on observations in more recent records of the last few years, that led to a higher level of attention on vertical accelerations.

I think Mr. Marore and Dr. Hall can comment on the significance of that, also. I think they've done a little bit more work on the vertical acceleration aspects than Mr. Devine has on the applicability of it to the site.

A. (Witness Devine) I was speaking in terms of ground "motion," not in terms of its impact on the GETR structure.

Q. Now I believe you've indicated that the problem with that particular data point was not an

instrumental problem, and I assume then it must have been a geologic variance that was the determining factor as to whether it was a questionable data point. Is that correct?

A. "Geologic" in a broad definition of the term. There are a variety of factors that were not associated with the instrument cell but in the surroundings, the soil, the geometry of the faulting, and so on, under the broad category of geologic.

Q. Well, I think then perhaps it's Dr. Pichumani's area as to whether it was appropriate to question that data point, and I would like to have your opinion on that -- or anyone else's opinion on this panel as to whether the underlying basis for questioning that data point was valid?

A. (Witness Jackson) If I may offer a comment, and I know I am interjecting, but I am familiar with all the backgrounds. The general paper that is being referred to that makes the observations on the differences in geologic conditions was done by an individual at the U.S. Geological survey. The soil properties contribute to that. His view as to the basis for the high vertical accelerations, the soil properties enter into it, but the real parameter that is controlling is the wave propagation path and the rupture path from one type of soil into

another soil.

And Dr. Pichumani, unless I am wrong and he can correct me, has not reviewed the differences in soil properties beneath station six in the Imperial Valley as compared to the GE Test Reactor area, as far as I know. In fact, I don't know if there is --

A. (Witness Pichumani) Mr. Chairman, I agree with Dr. Jackson. I have not reviewed the particular information. I am not conversant with the particular acceleration data.

Q. Well, I guess, Mr. Devine, it is up to you.

(Laughter.)

A. (Witness Devine) I guess by now I don't understand what the question is, sir.

Q. Well, the question is, and you have contributed to the question in indicating that you personally understand that there was something questionable about the use of that 1.74g observation. And I am trying to get to the basis for questioning that reading.

end JWB #4

A Yes. And I thought I had described them in general. I'll continue, if you feel you need more.

Q Please.

A There were two major factors that contribute to this record being very anomalous. One is the station is located between two -- right directly in the apex of the bifurcation of the Imperial Valley fault, and so it is an extremely complicated and unusual geometry of how the ground motion would be arriving at the station.

Secondly, there is a very unique soar of velocity problem, situation in the Imperial Valley that causes strange unique ground motion response, and I can't describe in any detail what this -- it's a function of the velocity variation with depth which causes ray paths to behave in a unique manner. I can't describe it in any further detail except to know that it makes that data very unique and I would hesitate -- particularly the one that's been complicated by the geometry of the two faults on either side of the station. It makes the transferability of that data very unlikely, very unuseful -- not useful.

Q Do any of the other geologists, Drs. Brabb or Herd, want to comment on that particular location of data point 6, if they have any particular knowledge of that? Or Dr. Ellsworth?

A (Witness Ellsworth) I was going to offer a

comment in a slightly different light, your Honor. I should preface it by saying that I'm not an expert in strong ground motion, but I do have a general familiarity both with the Imperial Valley records and with some of the possible interpretations that have been offered of them, and my observation would be that there is not yet a consensus in the seismological community as to the explanation for that record, and I would be perhaps a bit more cautious than Mr. Devine in applying that record to another site.

I think it is possible that such conditions could exist at the GETR site, and I am personally not aware of any information that would say that it's impossible.

A (Witness Brabb) I'll comment in that from my very limited understanding of what Dr. Devine has just said, with respect to the geology, it appears that the geologic analogy between the Imperial Valley event and GETR is remote, for these are two completely different kinds of geologic systems from the standpoint of the geometry of the fault systems, and therefore I would be comfortable with his statement that it would appear that the acceleration information would not pertain.

Q In your opinion, does that hold with regard to horizontal accelerations, too?

A No, sir. Just with respect to what Mr. Devine said in terms of his reason for excluding the data on a

geologic basis.

Q Oh, for just that one data point?

A Yes, sir.

Q Dr. Ellsworth?

A (Witness Ellsworth) Yes, I'd like to bring it to the Board's attention that these are very unique records that have been collected in the Imperial Valley, that we have a very limited set of data observations that are very near the surface break in the faulting event, so that our total data set is only a small handful, small collection of records at this point, and it's very likely that the record from station No. 6 that has been referred to is in fact affected by local conditions, but I don't think that we can disprove other hypotheses at this point.

Q Well, wouldn't you expect that there would be readings in the opposite direction from data points that are affected by other local conditions that would be on the other end of the reading scale?

A These undoubtedly enter into the scatter and the limited data that we have at this point. I think that's true.

Q Would you believe it appropriate to eliminate only the high readings and not eliminate the comparable low readings?

A If I were to conduct such analysis at this

point, I would be hesitant to eliminate any readings, at least as a first pass, unless I had some very site-specific reasons for eliminating one reading or another.

Q Mr. Devine?

A (Witness Devine) Yes. In -- I guess it would depend on the use of data. Dr. Ellsworth's comment about not being very cautious about eliminating data, is indeed a good comment. But the fact was in this particular station, this very high g, very high g, was at a site where there was very moderate damage.

Consequently, in trying to use this data in a practical sense for estimating values in construction of a facility, I factored that component also into reasons why I believe this data point does not transfer.

Q Dr. Ellsworth?

A (Witness Ellsworth) If I could comment further, it's been brought to my attention that there had been other recording at the same site. These are aftershocks of the Imperial earthquake or from other nearby events such as the earthquake that occurred near Westmoreland a month or so ago, and the readings at this site have been substantially higher than other nearby sites.

So this would tend to support the hypothesis of a side effect, site-specific effect contributing to the high acceleration in this record.

So on that basis, if one could demonstrate that similar conditions did not exist near the GETR, then it would be appropriate to down-weight this particular observation in the analysis.

Q Would that lead you to then weight the use of the mean accelerations from that site, or would it influence you in eliminating that particular reading, from determining the mean accelerations at that site?

A It could do either. In determining the mean, it's only one point of many, and it has a small effect.

Q I believe, Dr. Ellsworth, some of your observations went to using data from that site at all, not just limited to the vertical accelerations, and you indicated there was sparsity of data points at the site. Would that affect use of also horizontal accelerations?

A I don't know the specific results that have been obtained for that site, but if it were found that there is a site amplification factor that also applied to the horizontal accelerations, then that would be an appropriate course of action.

A (Witness Devine) I would be cautious about using the horizontal data, too. On the other hand, it fits in with other data in the horizontal directions, so it doesn't appear to be as anomalous. But as a matter of caution, I would -- just because it fits with our preconceived

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idea of what it should read is not sufficient reason to use it. I would be inclined to be very cautious in using even the horizontal data from that station.

Q Now I recall yesterday -- going to another line of questioning -- that either Dr. Brabb or Dr. Herd mentioned that trench H had been bulldozed on the top. Could you explain what happened there?

A (Witness Herd) Yes, I can. Trench H was excavated right near building 102 near the edge of the building complex in the GETR site facility. Apparently in the construction of the building or adjoining facilities, the bulldozer had made a pass over the area where the trench was subsequently dug for this investigation, and as a consequence, the uppermost soil horizon, that one which we would use to judge the offset, was stripped and truncated. Consequently, we don't have the full upper soil preserved from which to assess the displacement.

5-A Q Okay. I also recall, Dr. Herd, that you mentioned that it was your impression that in one of the trenches, the consultants had agreed that the A horizon -- one of the A horizons, I'm not sure which one -- had been offset, but that it didn't show up on the log. Is that a correct statement?

A I think you are referencing a statement probably made by Dr. Brabb, and it was in reference to trench T-1.

Q How do you account for the fact that there was this agreement and yet it didn't show up on the trench logs?

A I'm not sure that's an appropriate question to ask me to speculate on motives.

Q I'm sorry, I didn't mean to go that far.

A If there was any motives involved, if it was a difference in observation, I think that may be the point.

Q I wasn't asking for motives, actually. All I was asking, really, was within the framework of the art or science of geology or a combination of both, is it usual that you would have certain observations in the trench that might not appear on the trench logs?

A (Witness Brabb) I think I mentioned in response to another question yesterday the difficulty of interpreting the features in young soils. We are talking about materials that appear to be very similar, and it's been my observation from going into a number of trenches with Dr. Herd and others that these features are difficult to see, and that, furthermore, the use of soil science in interpreting trenches is very new, in terms of general practice and examining trenches and fault zones.

And therefore, I also recall that Dr. Shlemon was not there at the time, for example, and that it's entirely possible that in our discussions we thought we had agreement on displacements, but in fact it may have

been with respect to the older horizons which are more clearly offset and the fault figures are more clearly seen in the older horizons in contrast to these more subtle features in the soil.

Q Along those same lines -- I'm sorry, did anyone --

A (Witness Jackson) I was going to make a very brief comment, that the consultants have, through GE, spent a great deal more time in the trenches than we do. I personally fly out from the East Coast and go through them in a day or two, and Drs. Herd and Brabb spend more time than that in them. I'm not trying to diminish that, I'm just saying there are relative times and availability of information in the aging of the trenches that we talked about that they also can contribute.

Q I'm not directing, by the way, these questions toward motive, as you have indicated, but I am trying to find how exact a science we are dealing with here, with regard to observations in trenching, and in that context I have prior to this referred to one part of one of your reports which indicated that -- and I'm speaking to Drs. Brabb and Herd -- which indicated disagreement with ESA on a number of data points that you requested by sampled, and in particular there was a statement made about seven of eight observations regarding a fault in which you were in disagreement with ESA, and three of three observations

regarding landsliding, and I would like to hear some discussion as to how there could be such a wide disagreement in that area.

A (Witness Brabb) Well, I wrote this statement, so I guess I should respond.

MR. EDGAR: Dr. Brabb, could I have a reference to the statement so I could understand where it is?

WITNESS BRABB: It will take me some time to dig it out.

WITNESS HERD: I think it's some place in that 1979 report prepared by the Survey.

WITNESS BRABB: The report, unfortunately, is not paginated, so it's difficult to refer to, but it is in our 1979 Appendix A to the SER, and it starts the back of the report and just before Figure 1, above the section on regional fault tectonics.

BY JUDGE GROSSMAN:

Q I'll read the statement I referred to:

"The existence of the fault was tested by the current GE consultants, ESA, in eight places, and in our judgment confirmed in seven of those places" --

By the way, the "confirmed" means confirmed opposite conclusions to ESA.

Let me begin that paragraph and read the whole

paragraph.

"In summary, the existence of the Verona Fault has been determined independently by a number of investigators using different methods, including two consultants for the Licensee, General Electric, in 1958 and 1973. The existence of the fault was tested by the current GE consultants, ESA, in eight places, and in our judgment confirmed in seven of those places. The sense of movement in all places is consistent. The landslide hypothesis, in contrast, was tested in three places without success, in our judgment. In our view, the information provided by the Licensee establishes firmly the existence of the Verona Fault, and does not support the landslide hypothesis."

Now let me indicate what my understanding of that was, and that is, that at the time that ESA contended that there was no Verona Fault, if I understand it correctly, even though it's not an issue in the case, individually those consultants still contend there was no Verona Fault.

There were data points tested in which the geologic survey believed their judgment that there is such a fault was confirmed in seven of the eight places, and their belief that there was no landsliding or that the movement

could not be explained by landsliding was confirmed in three of three places.

And my question goes to how there could be such a wide disagreement which apparently affects 10 out of 11 data points, and could Dr. Brabb expand on that, and expound on that?

A (Witness Brabb) I'd be happy to. The difficulty is to try and do it briefly.

This relates to a site concern expressed by the Intervenors and perhaps by the Court itself, and if I could defer it for just a couple of minutes, first I would like to expound on the response I made in the question to Mr. Barlow yesterday about whether I was pressured to alter my views.

As scientists, we are continually interacting and testing our ideas and hypotheses, and very often we do this vehemently and interact with each other in using hand gestures and facial gestures and strong words to try and probe the analysis that was done, the interpretation of the facts.

At times this leads us even to disregard the sensitivities of other people and even politeness. The end result, I think, though, is a better and sounder understanding of the information and hopefully ideas that are closer to geologic truth.

This relates in part to this sentence as well, so I wanted to get that on the record first.

In our analysis, or in our review, we have to try and test the hypotheses that were proposed, and the landslide hypothesis was one of those that was proposed to explain the features that were seen in the trenches.

We tried hard with a variety of different methods, not only the trenches that are mentioned here, but with a lot of other information as well, to understand the ideas that were being proposed and to test them to see if their hypotheses were correct and whether the data were correct.

In the specific instance we are talking about here, there were nine trenches that were dug to test the validity of a hypothesis that there is a fault going along the base of the hill called the Verona Fault.

In eight of the nine trenches, we think that the information from the trenches does demonstrate that the Verona Fault exists, and is a tectonic feature that must be contended with. The ninth place was trench E, where the consultants have maintained that the fault was tested in that area and they could find no evidence for it.

As we have pointed out, we think there is a possibility that the fault was not tested in the right place, and that it may exist in a different area there, so we would not accept the ninth case as demonstrating that it

does not exist.

Q Let me make a slight correction here. I don't want to throw you off. I think it's seven of eight rather than eight of nine.

A Thank you for that correction. Seven of eight.

In the case of the landslide hypothesis, it was critical to us that in this story that we be able to see some concrete evidence for this, other than what was being explained in the trenches. If there is a landslide there, we want to go up on the hillside and see some place that we could convincingly understand was part of that hypothesis.

This is why we asked that some of the trenching that was done in the hillside be done in the area of the so-called head wall scarp. If there is a landslide in that area, regardless of whether it's very old and regardless of whether most of the evidence for it should have been removed by erosion, we would expect to see some surface that had surface on the ground that could be examined and would show an indication of displacement and rotation.

In the three cases that are specifically referred to here, they trenched for that purpose and found no significant displacement; none in two trenches, and the one trench, the amount of displacement was insignificant.

Therefore, we felt at the time this statement was made that in terms of testing the hypothesis of faulting

vs. landsliding, that the testing that had been done confirmed faulting and disproved landsliding.

Now they take exception still to some of the other information. I think they are still convinced that there is landsliding in that hillside, but it derives from the basic data and how you go about testing that data.

This is why there is still a substantial difference of opinion.

Q Have you satisfied yourself conclusively that there is that Verona Fault and there is not that landsliding?

A Yes, sir, I have. I am absolutely convinced.

Q And Dr. Herd, I take it, you are, too?

A (Witness Herd) Yes, I certainly am.

A (Witness Jackson) I would like to add just a slight qualifier to that, in that I am not absolutely convinced. I have seen many things evolve on this site over the last several years. I leaned heavily toward the landslide hypothesis myself in the early reviews. It was based on some of these trenches dug later that switched my personal opinion over. I think for the insight of the Board, the thrust faults can often be accompanied by landslides, and faulting and landsliding go together pretty closely. So that the fault movement creates the topography which creates the landslide.

So I think I would be a little hesitant to rule

it out completely. I think we have taken a position, obviously, in the Safety Report that it's tectonic in origin, but I don't think you can dismiss the possibility totally. I think the data heavily favors tectonic in origin.

Q I take it, Dr. Jackson's statement speaks for himself and you, Dr. Brabb, hold to your prior statement?

A (Witness Brabb) Yes, sir.

Q And Dr. Herd, too?

A (Witness Herd) Yes. Just as a point, Dr. Slemmons was hired by the Nuclear Regulatory Commission to perform an independent assessment. Perhaps you might wish to ask him of his opinion as well.

Q Did you care to give an opinion, Dr. Slemmons?

A (Witness Slemmons) Yes, I would.

Q Please do.

A I believe that nearly all of the data give strong support for tectonic fault origin. I have minor reservation. I feel that perhaps in some way that we can't picture that there is a landslide contribution, but I think that I make the assumption and I strongly believe that it is a fault.

Q Thank you.

There was some presentation made with regard to the possibility of a shear shifting because of the presence of some structure in the area. I don't -- I guess it's

Dr. Pichumani who is the expert on that, and let me first ask him whether that theory is based upon the single instance that we have seen reported, some bank building in South America, is it? Banco something or other. And that's my first question. Is that so, sir, that that is all based on that one example?

A (Witness Pichumani) No, Mr. Chairman. Actually our conclusions were not based on that field case at all. It was purely based on soils mechanics principles and analysis of the earth beneath the reactor being shifted by the thrust fault forces because of the heavy weight of the reactor itself.

Q Are there any other examples that you can point to, other than that one example, that one observation, where this has happened?

A I know of no other field case other than the Banco Centrale in Nicuaragua, but there has been one case of a laboratory test performed on a similar structure beneath which there was faulting, but under shaking similar to a fault, there also the faulting was away from the structure.

This was, of course, a model study. It was not a prototype field case.

Q So, in other words, you had one field observation and then a laboratory model which you believe might confirm

that field observation?

A Yes, actually.

end 5

Q. Now I believe I went a little far in saying that you had observed that that actually happened in that one example. That still would be considered in the category of the theory as to what happened, would it not, sir?

A. Yes, sir.

BY JUDGE FOREMAN:

Q. I didn't hear you say something, or I wasn't sure what you said. You said that not only was there diversion of the offset, but there was diversion or a change in the amount of shaking?

A. No, no.

JUDGE FOREMAN: Thank you.

BY JUDGE GROSSMAN:

Q. Now let me ask the other geologist, or the geologists on the panel whether they have ever observed that particular phenomenon?

A. (Witness Brabb) No.

Q. Dr. Brabb said "no."

Dr. Herd, have you ever observed anything like that?

A. (Witness Herd) No.

Q. Dr. Jackson?

A. By "observed," do you mean did we go in the field and watch it ourself? Or are we aware of

things in the literature that apply? "Observed" is --

Q. What I mean is: Have you ever observed a fault that appeared to change direction, or a shear that changed direction because of the presence of a structure?

A. No, I have not observed that.

A. (Witness Justus) I have observed the deflection of shears around objects, but the -- the analog is not the one that pertains here, but I feel that I ought to just mention it.

I have observed shears in rocks being deflected around imperfections in the rock, or inclusions in the rock. So that I am aware from field evidence that faults can be deflected around objects imbedded in the rocks themselves.

Q. In other words, a shear would generally not be in an exactly straight line? There would be some waivering there? Is that basixally what you are saying, Dr. Justus?

A. Yes.

Q. Mr. Devine?

A. (Witness Devine) Mr. Chairman, would it be out of order to qualify an answer to Judge Foreman's previous question?

Q. Oh, go right ahead.

A. I apolcgize for not saying so sooner, but in Dr. Pichumani's answer of strictly "no" to the modification of ground motion by the presence of the structure, I realize that -- I think it is important for you to understand that the ground motion is frequently, and in fact almost always, reduced in the structure by the presence of the structure.

A free-field strong motion instrument nearby a structure almost always has higher peak motion than a similar instrument in the basement of the structure. The presence of the structure does indeed reduce the peak acceleration that you observe.

Consequently, when we assemble ground motion data, we have to take that into account.

Q. Dr. Slemmons?

A. (Witness Slemmons) Yes. I have walked many a mile along fault zones. I have no observations of deflections of this sort. But on the other hand, most of the active faults have not intersected major massive structures that are similar.

Q. We had heard a theory expounded by the proffered testimony of Mr. Barlow with regard to seismic gap. I believe he directed some questions to this panel with regard to that theory, but I still am not sure as to what the panel actually said about the

possible application of that theory to the Verona Fault area, or that entire area of the San Andreas Fault in the Livermore Zone.

Dr. Ellsworth, did you have a comment on that?

A. (Witness Ellsworth) Yes, your Honor. You asked me several questions, and I will try to provide a little background information which I hope will answer them.

A seismic gap is an observational quantity which has been derived from global data looking at very large earthquakes -- earthquakes typically of magnitude 7.5 and larger -- that occur along the major plate boundaries of the globe. A seismic gap is an area along the plate boundary which has not ruptured in a recent earthquake, and is believed for tectonic considerations to be the possible future locus of another earthquake, perhaps because such an earthquake has occurred at some point in the past.

There has been a global analysis of such seismic gaps along plate boundaries completed in recent years by a research team at the Lamont Daugherty Geological Observatory of Columbia University, and they have continued to update data on this global analysis as new information becomes available.

They have, however, not attempted to apply that technique to earthquakes of magnitudes smaller than 7.0. So that if I were to attempt to apply their methods, I would not be able to use it on the Verona Fault. It would, however, be possible to apply that technique to the Calaveras Fault or to the San Andreas Fault, and indeed they identify two seismic gaps on the San Andreas Fault in California.

The segment of the fault that ruptured in the 1906 earthquake is considered to be a seismic gap, but one of relatively low potential at the present time.

The segment of the San Andreas Fault that ruptured in the earthquake of 1857 is considered to be a seismic gap of relatively high potential.

Now in their classification system, they have devised six categories of seismic gap. Three relate to segments of faults that have produced great earthquakes either as documented from geologic records or as determined from historical observations. They have also defined three categories that apply to faults where no such earthquake is known. And it is those categories that I believe would be appropriate for discussing the Calaveras Fault, for I am unaware of any earthquake of magnitude 7 or larger that has occurred on the Calaveras Fault.

It is certainly considered to be a possibility, given the great length of the feature. Now in applying the criteria that they have developed, one must consider several possibilities.

One is that the fault is capable of producing such an earthquake, in which case they have a specific category that would apply to that fault: a fault that has not moved in the historic record, but is considered to be a capable fault.

Another possibility would be that the fault is incapable of producing such a large earthquake, but lacking any definite proof of that they would place the Calaveras Fault into another category, which would be of-- which they would consider to be of lower risk.

My personal opinion is that we do not as yet have a sufficient data base on the Calaveras Fault with which to classify it precisely into that scheme. So that my observation would be that it is more conservative to consider that such an event might occur.

Q Now it was my understanding from the discussion of seismic gap that it was generally agreed by the panel that the exact plate boundary could not be described, in view of the fact that there were -- the plate boundary of the Pacific and North American plates could not be specifically described. I hesitate

to venture into this area, but in any event that the number of faults that we've discussed here -- the Calaveras, the Las Positas, and the other faults -- might be extensions of that plate boundary; and that the theory of seismic gap would apply to that entire plate boundary in this particular area and not be restricted to exactly the San Andreas Fault Zone itself.

Now am I wrong in that observation?

A. (Witness Devine) May I start the answer? I'm sure I'll get additional support or comment from the group. But as it pertains to the earthquake potential is where I believe I should start.

First off, two comments I think are important. One, I don't see where the concept of a seismic gap or not having a seismic gap has any direct bearing, since we assume the eartnquake is going to occur. So it doesn't matter whether we estimate whether there's a gap or not, because we assume that the earthquake will happen.

Secondly, in describing the plate boundary -- the boundary between two major segments of the earth's crust -- Dr. Herd described the other day that indeed it is not a single, discrete line. However, it is obvious from the data we can gather that the vast majority of the differential plate motion is occurring

on the San Andreas Fault, not on the subsidiary faults. Consequently, we feel confident in estimating lower maximum expected earthquakes, or maximum earthquakes on the subsidiary faults because of that. So we feel there is a scientific basis for not putting the magnitude 8.5s on every strand of every fault that is included in the broad category of the "plate boundary."

Q. Now when you say that you assume that that earthquake is going to occur, you are not saying that with regard to the 6.0 to 6.5 earthquake on the Verona? I take it you are applying that to the expected 7.0 to 7.5 magnitude on the Calaveras? Is that correct?

A. I have applied it to both. We assume that the 6.5 is going to occur on the Verona, also.

Q. Well, the reason I think there is some significance here is, I do not believe that that theory of seismic gap applies to that expected event on the Verona Fault. It was my understanding that it applied only to major earthquakes within that San Andreas Fault Zone, and it would not be affected one way or the other by the occurrence of a 6.0 event on the Verona Fault.

A. In general I agree with that. However, the only time the discussion of gap would have any relevance is if we we-e discussing what is the likelihood of an event on the Calaveras. And we're not discussing the

likelihood. We are assuming that likelihood is one. Therefore, the concept of gap or no gap doesn't apply.

A. (Witness Jackson) Could I add a comment, please?

Q. Certainly.

A. We are interchanging "science" and "licensing." For the purposes of making a licensing determination for the GE Test Reactor site, we have essentially assumed that those earthquakes will occur. That does not mean everyone on this panel would conclude that that is an imminent thing or anything like that. And that is what the difference is.

Based on our review of this plant, we have not spent an extensive amount of time as far as I am aware looking at the probability of occurrence of the magnitude 7.5 on the Calaveras, for instance, and looking at gaps and things. We have made an assumption and gone from that.

So I didn't want the confusion between the difference between a scientific assumption and a licensing assumption.

Q. Okay. This is my last line of questioning, and I guess you can assume as to what I consider it to be of relative importance, but I do want to fully explore what has been brought up. And I think perhaps

we are not characterizing what we understand Mr. Barlow's theory to be the same way, and undoubtedly I am the one who is incorrect. But I do want to explore it so I have an idea as to what the theory is.

Now it was my understanding that his theory postulated major events -- and it is not "his theory," but the theory that he is applying here postulated major events along the San Andreas plate boundary not restricted to the San Andreas Fault Zone itself; that the presence or absence of gap was determined on the basis of that major event occurring along that plate boundary so that if an event occurred somewhere along that indistinct boundary that was not that major event, that would not eliminate there being a seismic gap at that particular station.

So when you postulate an earthquake either in the Verona Fault Zone or the Calaveras Fault Zone that does not qualify as that major event, that would not satisfy the condition for the gap not being there.

I don't know if I am making myself clear, but what I am giving you is my understanding of the theory that has been presented. So it in effect would not relieve the stress along that plate boundary to have what would not be considered that major event.

I am asking you to keep in mind what I

understand to be his theory, and indicate whether that is in fact the theory of seismic gap?

And secondly, whether it applies to this particular situation? That is, the area in the Verona Fault Zone, but not restricted to the Verona Fault of course, restricted to that entire area around the San Andreas plate boundary.

Now, Mr. Devine, I think you would like to attempt that.

A. (Witness Devine) Yes, I will.

The study of seismic gaps and the report referenced by Mr. Barlow concerning the study of seismic gaps listed many faults that in the judgment of that study group have seismic gaps on it. It listed dozens of them. It did not list the Calaveras. So, number one--and with lots of reasons why.

Number one, I don't myself believe that there is a seismic gap incorporated on the Calaveras. Dr. Ellsworth indicated that the area of the San Andreas to the west of the Calaveras, the area that broke in 1906, is considered to be a gap now.

Secondly, I guess maybe my difficulty in understanding the question -- not your wording, sir, but I just don't see where the whole concept of gap or no gap has any impact at all on this subject when you

assume the earthquake is going to occur.

Now the probability of having the magnitude 7 to 7.5 earthquake on the Calaveras in the next X number of years cannot be higher than one. It is indeed very probably less than one. The most you can get by applying seismic gap would be one.

And if I understand the second half of your question, that having one of these events may not remove the possibility of the gap still being there, assuming that it is to begin with, would only then indicate that: Okay, you can have maybe a second event in the next X years. It does not mean you could have a higher event.

Q Okay. The question of course is whether that 7.0 event on the Calaveras is "the event" that is being predicted under the seismic gap theory, or whether you would expect that event to still happen. But I think Dr. Ellsworth probably has a more complete answer for me that might dispose of this area.

A (Witness Ellsworth) I'll try. Let's go back to the concept of the plate boundary. I think it's been brought out that the plate boundary in California, which we call the San Andreas Fault System, is composed of several discrete faults where motion is localized. And the motion along these faults occurs because of the buildup of elastic strain in the rocks near the fault.

The earthquake is a process of releasing that strain. The observations that have been made using geodetic measurement techniques show that the strain is distributed over a broad region, and the San Andreas Fault indeed carries a significant fraction of that motion. There are other faults that carry also major portions of the motion, such as the Hayward Fault or the Calaveras Fault.

For that reason, we must consider that large earthquakes can occur on those faults, and indeed the Hayward Fault has experienced several significant earthquakes in historic time.

Now if a large earthquake occurs on the San Andreas Fault, that does not necessarily reduce the potential for an earthquake on the other faults. So that each fault can be considered in terms of its seismic gaps independently.

Q Okay. Forgive me, by the way, for saying "stress" instead of "strin" when I did, but what I really was getting at was whether relieving the -- or having that event on the Calaveras or the Verona actually relieved the strain, or whichever it is in that area. But I believe we have pursued it enough, unless someone has something else to add onto that? I think that is a sufficient answer for me.

Dr. Ellsworth?

A. That is -- what you just said is correct, that if the event occurs it does in fact reduce the stress that is stored in the system, relaxing the strain.

JUDGE GROSSMAN: I have no further questions.

Why don't we break for lunch now, and return at 1:15. Thank you.

MR. SWANSON: Excuse me, Mr. Chairman. I assume, then, there were further Board questions of this panel?

JUDGE GROSSMAN: Yes.

MR. SWANSON: Okay. One comment which may make a difference on the scheduling, although we will have members of the probability panel available this afternoon as I reported yesterday. We did ascertain that an important member of that panel will not be available today, Dr. Vesely. That is the probability panel. He will be available starting Monday morning.

However, we of course do have before that panel at least two different categories of testimony to be addressed by Dr. Hall and Mr. Martore, so I think we will be able to fill up the day.

JUDGE GROSSMAN: Thank you.

(Whereupon, at 11:54 p.m., the hearing was recessed, to reconvene at 1:15 p.m., this same day.)

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#7 ar7-1

AFTERNOON SESSION

(1:15 p.m.)

Whereupon,

PHILIP S. JUSTUS,
ROBERT E. JACKSON,
ROBERT H. MORRIS,
EARL E. BRABB,
DARRELL G. HERD,
WILLIAM L. ELLSWORTH,
DAVID B. SLEMMONS,
RAMAN PICHUMANI, and
JAMES DEVINE

resumed the stand as witnesses on behalf of the Staff and, having been previously duly sworn, were examined and testified further as follows:

EXAMINATION BY THE BOARD (Continued)

BY JUDGE GROSSMAN:

Q The afternoon session is now begun. I had one more question for Dr. Pichumani. I believe he's the one who can answer this.

We had had some testimony by, I believe, Mr. Meehan to the effect that with regard to the amount of -- that it did not matter what the amount of offset might be, and it also doesn't matter what the reactor is made of. It's only the weight of the reactor and the natural soils

that would determine whether the offset would have any effect upon the reactor, and I believe that that testimony was in the context of what we had discussed briefly this morning.

The fact that the offset deflected around that Banco building and your laboratory model, would you agree with the statement I made, which I'm sure is a very imprecise paraphrasing of what Dr. Meehan said?

A (Witness Pichumani) Mr. Chairman, I think it's a very good paraphrasing of what he had meant to say. As far as the second point is concerned, I agree. The weight of the reactor is the primary, or is the concern in this analysis. Whereas the first point about the fault movement I will not accept that 50 or 100 meters as stated in his prefiled testimony of Mr. Meehan.

I have a feeling that he probably came back in his cross-examination somewhere, that he said something like 13 or 15 meters. I'm not so sure how it got said.

Q Well, my recollection is that he did limit the amount to either 17 feet or 17 meters, I don't recall. I believe it was 17 feet. No, perhaps not.

A Meters, perhaps.

Q But now is it your opinion, then, that you are willing to rely, then, upon the weight of the reactor? Is that it?

A Right.

Q On the basis of that single field observation and your laboratory model?

A Actually, based on our analysis, and I want to mention about this fault movement. I would approach it from two angles:

One, from a geotechnical point of view; and another from a structural point of view.

From a geotechnical point of view, I would get worried about the fault movement if it exceeds say 13 meters or somewhere in that range, which should put the reactor completely above the surrounding ground surface. That is how I arrived at that number.

As far as the structural support for this size, that is actually not entering into my analysis, but I think it is for the structural engineers to say. From that angle, I think if the fault movement goes five meters or somewhere above that, probably it will need investigation.

Q Well, now, let me ask the other members of the panel who are geologists whether in view of their -- the observations they have made in the past as geologists, whether they could rely upon a fault deflecting because of a manmade structure?

A (Witness Justus) Judge Grossman, I think in your

introductory rendition of Mr. Meehan's testimony, you mentioned that the nature of the material was not important, if I'm correct in that, or at least I think we --

Q The nature of the material of the structure.

A Of the structure. Oh, I see.

Q Yes. Not the soils material.

A I see. Fine. I thought you were referring to the soils, which does make a big difference.

Q No.

A (Witness Jackson) I do not have any knowledge of the deflection of faults around structures. I think there is a limited data base that's been indicated, and I've read a number of papers about this, in which poorly designed slabs of structures like foundations of houses and buildings and in the San Fernando volumes, as many examples have been deflected by faults, or ground settlement, which may not be the exact same thing. I would restrict it to that.

Q You mean the structures have been deflected?

A Well, I think you are using -- I think you were using the structures in too loose a sense. I think that Dr. Pichumani used it in the sense of a well-built, heavy, reinforced structure. What I was mentioning was say the floor of a garage, something like that. Those are what the photographs are of.

Q And, in other words, Dr. Jackson, you concur

with Dr. Pichumani in concluding that you would rely upon the weight of that structure deflecting a --

A I have no basis to make that conclusion, no. It takes a knowledge of soil properties and soil structure interaction, which I do not have.

Q Well, I am asking the geologists on the panel, notwithstanding that they may not be soil engineers, whether their observations as geologists would let them be comfortable with the conclusion that a heavy-weighted structure would deflect a fault or a shear, so as not to impact directly upon that structure. And, Dr. Slemmons, I would like your answer.

A (Witness Slemmons) Yes. Our worldwide data base is very inadequate in this regard. I have probably visited approximately 40 to 50 cases of field surface faulting, some cases many years after the event, and nowhere have I observed or have I seen in the literature anything comparable to the Banco Centrale type of example. So our data base is rather -- is very sparse.

You do see many massive reinforced concrete structures in the form of, say, a pavement for a freeway, a large retaining wall, a concrete lined tunnel, for example, in the 1971 or 1951-'52 earthquake, rather, in the Curran County, the thrust fault cut through the tunnel and ruptured rather cleanly through reinforced concrete. But

these are not strictly comparable to the massive blocky structures that you are referring to.

We have a sort of similar example in the case of the city hall in the city of Heyward which is on the trace of the Heyward Fault, but there, even though the fault comes up cleanly to the building and is essentially deflected around the building, this is due to a slow creep, and whether this slow type of movement would be comparable to the rather sudden rupture in the case of a large earthquake, I'm not certain.

So I think my conclusion is that we really don't have enough observational data to come up with a well supported conclusion, as to your question.

Q. Dr. Pichumani?

A (Witness Pichumani) I would like to add here in this problem there are two aspects:

One is the movement. The other is the vibratory ground acceleration.

What Dr. Slemmons said about the damage to the structure, I am not questioning. It could be damaged by the ground acceleration; vibratory acceleration does not deal with that aspect. The assumption is that the structure is strong enough, like Dr. Jackson said, it is reinforced enough that it can take the kinds of accelerations that we are talking about, 1 g or 5 or 7 g, and all that. If the

structure is strong enough to withstand that level, the design level of acceleration, then because of this soil, not soft in this case, but it's not rock -- because of the soil being interposed between the bedrock and the structure, it could be deflected away. Therefore, the fault movement did not cause the damage to the structure. The accelerations could cause structural damage.

Q Dr. Slemmons, I'm sure the record will show that you indicated a clean break in that tunnel example, which I would believe would take that out of the sphere of the ground shaking or ground accelerations?

A (Witness Slemmons) I was referring primarily to cases of the actual fault rupture affecting the structure, rather than the motion, the ground motion effect.

A (Witness Devine) May I comment on his example?

Q Yes.

A I believe I understand which structure he is talking about from the 1952 Curran County earthquake. And I would point out that was a tunnel in rock, and there really was no other place for the fault to be deflected to. It had no choice but to go through the tunnel, so I don't believe it's a comparable example to the Banco Centrale.

A (Witness Slemmons) I agree. It was at a shallow depth and in shattered material, so that it isn't massive rock in the sense of granite, but it certainly isn't

comparable, either, to the kind of gravels that you have in the Livermore formation.

Q Well, since you volunteered that, Mr. Devine, let me put it to you, whether you, from the observations you have seen as a geologist, would rely upon the theory that the weight of the building would result in deflecting a shear that might otherwise surface underneath the building?

A (Witness Devine) I would like to cast my observations as a seismologist, because my professional career has been looking at the seismic side, rather than the geologic side. But I have indeed studied hundreds of photos and data points from earthquakes around structures and away from structures, and with that background I would say that I know of no other example that fits the situation.

There were very few massive structures with any broad definition of massive that set astride a fault that's moved in historic times. So we don't have a data base.

Q And the only example you have is that Banco Centrale example; is that right?

A That's correct.

Q Dr. Brabb?

A (Witness Brabb) I have really too few -- I have made too few observations in relation to this problem to make an opinion. I simply don't have enough information.

(Panel conferring.)

Q Dr. Herd? Oh, Mr. Devine?

A (Witness Devine) I would add one comment in that. Part of the reason for saying we have such a small data base is also reflected -- a result of the fact that it's not always possible to tell ground failure -- tell the cause of the ground failure, particularly around structures and parking lots and so on. It's very difficult to know that you're talking about primary fault displacement or secondary response to the ground failure. So the data record is not very clean there.

I can show examples, for example, a high school in Anchorage, Alaska in 1964, where the foundation fault appears to go right through the building. But it is not an appropriate example, because I think there is a strong argument that it really is not primary faulting, but a result of secondary ground failure.

So there are a lot of examples that appear to be useful, but in fact are not.

Q Dr. Herd, did you have anything on that?

A (Witness Herd) Well, I really have no basis to draw any conclusions, and I was just looking at a recent article that had been written by Les Youd, if only to bring it to your attention for a future reference. There is an article entitled "Ground Failure Displacement and Earthquake Damage to Buildings," by T. Lesley Youd, which was

published in the Civil Engineering & Nuclear Power Conference, Volume II, Geotechnical Topics, 1980.

BY JUDGE FOREMAN:

Q And can you give us the gist that bears on what we are asking?

A (Witness Devine) I have not studied the article, but I'll make an attempt.

JUDGE GROSSMAN: Excuse me for a second.

(Board conferring.)

BY JUDGE GROSSMAN:

Q I believe you have indicated the article. Does anyone want to base any of his professional opinion on that article at this point?

I don't want to open up Pandora's box and have -- I don't know what the conclusions of the article are, and I would prefer that we not just put someone else's conclusions here, unless someone else is willing to base a professional opinion on it and support the article, whichever the conclusions go.

A (Witness Jackson) Could I make a suggestion?

Q Yes.

A There are many articles that relate to observations of earthquakes and building damage throughout the literature, and I imagine this is one article that is one of many, maybe one of the more recent, and so I would

suggest that it falls in that category of broad group, and that may be a better way of handling it.

Q Dr. Herd?

A (Witness Herd) I apologize. I didn't mean to create complexity. I was just aware of that reference.

Q Okay. Now I did want to ask your professional opinion on your observations as to whether you would rely upon the theory that with a heavy structure, a shear would be deflected away from the foundation of the building?

A I have no experience in having observed this phenomena elsewhere, to be able to comment on it.

Q Okay. That finally concludes my examination of the panel, and I will turn it over now to Dr. Ferguson -- Dr. Foreman.

BY JUDGE FOREMAN:

Q Thank you, Judge. I just have a few questions, because many of the concerns I had were quite well answered in response to Judge Grossman's questions, and I did have some inquiries of Dr. Pichumani which I'll start off with, and in referring to the summary of your testimony that you provided us, dealing with the cantilevered -- with the analysis of the possible cantilever conditions, you talk about in the paragraph about a passive Rankin wedge as a means of analysis to establish a version of stresses from structures. And you say:

"This method of wedge analysis is based on sound soil mechanics principles that have been accepted and applied by foundation engineers in the design of earth-retaining structures."

As far as you know, has that ever been applied with respect to nuclear power plants before?

A (Witness Pichumani) We have done several slope stability analyses, even in the areas of nuclear power plants, but the only thing I am not aware is that for a fault movement of this type, I have not done any analysis using this theory. But I still should add that this theory is applicable for this condition because all the fault movement means is a failure plane, just as in any other slope stability problem, we are assuming a failure plane along which the mass can slide, due to a given triggering force.

Here the thrust faulting is another kind of triggering force, and the same theory of soil mechanics applies here also.

But seismic loading conditions have been applied. This theory has been applied even for the seismic loading conditions, the original analysis. It is not new. I myself have not done it, but --

Q I'm not concerned as to whether you have done it. I would just like to know as to what extent this is a common or uncommon mode of analysis, particularly as it

applies to the kind of situation with which we are dealing.

A It is a very, very common mode of analysis, and all the time it is used in stability analysis.

end 7

Q. And as applied --

A. Seismic loadings, and in the area of nuclear source.

Q. And then a couple of other things. On page 6, and I will read the sentence, and what I am asking is an explanation of the sentence. I don't understand it.

"GE performed an extensive set of parametric calculations to demonstrate that fault planes not intersecting the foundation require minimum passive pressure."

What does that mean? And what is the implication with respect to the effect of a tectonic event on the plant?

A. I think in this connection I can refer to a statement by Mr. Meehan in his testimony where he calls it the "line of least resistance." What here I mean by "minimum passive pressure" is the same thing. The planes which are away from the base of the reactor. Those are the planes which require the minimum force to drive the wedge. That means, the fault movement will take place along the line of least resistance.

Q. I understand. But I guess what runs through my mind is that the forces that are being exerted by a tectonic event are so immensely greater -- at least I think they are -- so immensely greater than the weight

equivalent to 8000 tons, that would push the thing out of the way. Am I wrong in that intuitive thinking?

A. You are right, but the point is that that force, the movement along a particular plane would happen as soon as you find a plane which requires the least force, and the total force may be anything. But as soon as it finds the plane which requires the least force, it will try to go there instead of trying to go right under the reactor, which may require a larger force to lift the reactor. That is what is meant by "minimum force required."

Q. Well, to help me understand, then -- and this would always apply to the plane of minimum passive pressure, that means that that could also happen in structures that weren't really very heavy. It could happen -- maybe it could apply to all structures, because there would be a minimum passive pressure that would divert the thrust. Is that true? Or are we looking at boundary limits?

A. No. I think you are right. It could happen if the weight of the structure was different, and that is clearly stated again in Mr. Meehan's prefiled testimony. This analysis is good, or this conclusion that it is deflected away from the mat is good for this particular structure. If you have some other structure

with some other weight, it may not be deflected. It could be going under the reactor foundation.

Q. Well, aside from soil conditions which I --

A. Right. Right.

Q. -- understand is what modulates this kind of thing --

A. Correct.

Q. Aside from soil conditions, what other factors then would determine the deflection, other than weight?

A. Let me again read this statement in Mr. Meehan's prefiled testimony. "In fact, repetition of the analysis for a lighter structure would demonstrate that a fault would surface beneath the lighter structure, as has been observed in many instances in the field."

Therefore, here the weight of the structure is the main consideration which showed in this case of the GETR structure that it would be deflected. If it was a different plant with a different weight, it could go under the structure foundation.

Q. And your analysis bears out that a weight involving 8000 tons --

A. Right.

Q. -- then does provide --

A. -- the necessary --

Q. -- it does provide the necessary conditions,

as such.

A. Yes, sir.

Q. It is not immediately pertinent to our consideration, but where is the cutoff point? 4000 tons? 200 tons?

A. We cannot give you any such cutoff point because it is the result of all the forces there. It is the soil weight and the soil property which is reflected in the angle of friction, as they call it. All those things go into consideration to define the wedge.

Q. I understand that.

So you are satisfied that this Rankin wedge wedge analysis does provide a good basis for your conclusions?

A. Yes, sir.

Q. Okay. I think that answers what I had wanted to ask of you.

I have forgotten which member of the panel had said that a fault had indeed occurred under a building and had caused problems, but this was due to secondary ground failure rather than to the development of an offset beneath the building.

A. (Witness Devine) Yes. I believe I was the one that made that statement.

Q So what is the likelihood of secondary ground failure happening in our particular situation in which we are interested?

(Witnesses conferring.)

A I appreciate the time to consult with my colleagues. We were trying to assemble possibilities to observe secondary failure, and none immediately comes to mind. The two obvious ones are liquefaction and slumping to a free phase, and we do not believe the conditions for either of those exist at GETR. So I would not expect that kind of secondary failure.

Q I think that is all the questions I have with respect to the cantilever situation.

I would like now to ask Dr. Ellsworth if he would turn his attention here. I was interested in, I believe the statement you made, and if I am quoting you wrong, correct me, that you were ambiguous, or at least you were uneasy about the use of the -- about the data from the Imperial Valley earthquake, even the horizontal ground motion data. Did you say that?

A (Witness Ellsworth) I believe that both Mr. Devine and I indicated that there was some possibility of local side effects contributing to the record at Station No. 6.

Q Oh, just the record at Station No. 6?

A. That's correct.

Q. Not with respect to the other data?

A. Well, all of the data will -- all of the ground motions are an interaction between the source and the path effects. So that to the extent that there are different geologic conditions in the Imperial Valley from elsewhere, those have to be considered when transferring that data from one site to another. So in other words, local conditions always play some effect, but it appears that Station No. 6 was anomalous in that regard.

A. (Witness Devine) But I certainly would support the use of the Imperial Valley data.

Q. Well, that sort of blunts my question, because what I was leading up to was the fact that as I understand it the numbers that were selected on a design basis for acceleration came from a regression analysis curve provided by Dr. Kovatch (phonetic) in which the near-field data were the Imperial Valley data. In fact, there were only three events involved at that particular point. And there was a tremendous amount of assurance gained from that, because as I understand it that is the basis for the numbers that were selected.

A. (Witness Ellsworth) That is correct. And perhaps it seems a paradox, but near-field data -- in

other words, data collected within very close proximity to the slipping surface -- structural effects are not a primary consideration there.

A. (Witness Jackson) I might comment a little further. The Imperial Valley earthquake data is probably the best -- one of the best recorded earthquakes that we have contributing to the data base. In other words, it expanded the data base of near-field information greatly as compared to information that was available before that earthquake occurred for that type of magnitude.

A. (Witness Ellsworth) I would agree with that comment, and I would also add that we have a very detailed understanding of the velocity structure in the Imperial Valley, and that is critical in our analysis. So that is why we are confident that the models that have been developed over the past number of years do apply to the Imperial Valley data.

Q. And it is appropriate to extrapolate for them for design-parameter setting in our situation?

A. I'm not familiar with what Dr. Kovatch did, so that is difficult for me to answer.

A. (Witness Devine) I would comment that it is appropriate to use that data in developing your estimates for ground motion anywhere else, trying to factor in the

differences that you can identify between Imperial Valley and the site under consideration, but certainly that data has to be used. It is the best set of data we have, and it is good data. There are so many things about it that are controlled relative to other data points we have that it does indeed, as Dr. Jackson said, reflect a major source of near-field ground data and ought to be used.

But there are some problems of transferring from one site to another where the conditions are not identical. So that has to be kept in mind when applying Imperial Valley data to someplace other than Imperial Valley. But that is true of all data points.

Q Turning now to consideration of vertical acceleration, somehow my feeling is that those were given not very much attention. My recollection is that the Imperial Valley data, excluding the two anomalous points, were used in the analyses. And then for purposes of applying that information for design parameter setting, two-thirds of those values were considered. Am I right in that?

A (Witness Jackson) I think the best person to answer that would be Mr. Matore or Mr. Hall. That is my understanding, yes.

Q I would be willing to defer to them. The

only question I would ask of you, the seismologists here: Is it an appropriate kind of an extrapolation to take data obtained on one site, and then take two-thirds of those data, or two-thirds of those values to apply to another? Is that a good extrapolation? And why not three-fourths?

A. (Witness Devine) If I may, let me recast what I believe you are trying to ask, because I don't believe that is quite what was done.

What was done was to take the horizontal data, and then use two-thirds of that to estimate the vertical.

Q. Yes. I'm sorry.

A. If that is the question, the question requires both a seismic and a structural response, and they are very severely intertwined. They can't be answered independently.

Traditionally up until the last couple of years, we had a strong reflection of the fact that in most strong motion records there was a lower peak vertical ground motion than horizontal, frequently half or less. So when the process was developed by the structural engineers and seismic design engineers, particularly Blume, Newmark, and Kapour in developing Reg Guides for NRC, it was in my judgment an entirely appropriate concept

to use some fraction like one-half, or two-thirds to reflect vertical peak motions relative to horizontal motions.

But the second half, and Dr. Hall I am sure can answer this with far more expertise than I, but I am familiar with response spectra so I will comment, the peak vertical motion in the vast majority of cases occurs at a higher frequency -- not the rate, but it recurs in the higher portion -- I'm trying to separate frequency; there are two terms for frequency. Let me say it accurately.

When one develops a response spectra which is ground motion versus frequence -- not frequency of occurrence but frequency of the oscillation -- the vertical frequencies always are on the higher end of the spectrum, or almost always on the higher end of the spectrum than the horizontal. So in an engineering sense, they have less significance because they are out of the range of interest.

This was true of the peaks at Imperial Valley, also. So from a seismic viewpoint, it is not always the case, but in the majority of the cases from strictly a seismologic viewpoint, the vertical motion has been less than the horizontal.

As we develop a data base of close-in strong

motion records, there appears to be a different data set developing. So I would be less confident for close-in sites to say that vertical should be considered to be less than horizontal by some fraction like one-half or two-thirds from strictly a seismologic viewpoint.

I think it would require an engineering -- structural engineering accommodation to explain that impact.

Q. Well, I believe Dr. Hall is the man to ask those questions, and indeed I will.

A. I am reminded that I used a term that may not be self-explanatory. I said "close-in." Sorry for the jargon. I really meant to say "records recorded in what's called the near field." That is, very near to the fault.

Q. I would like now to turn to Dr. Slemmons.

A. (Witness Jackson) We're having a hard time hearing you.

Q. I'll pull it up closer. Dr. Slemmons, in response to a question that we asked of you, you would indicate that your opinions would change if indeed the faulting in Trench T-1 was greater than -- say it was 2.5 meters or 7 feet -- was greater than 2 feet.

My question to you is: That if your opinion would change and you were responsible for providing the

geologic data for the probability analysis, would your opinion require you to change the data to be put into the probability analysis? That is one question.

And I will use my prerogative of the Board to ask two questions in a row.

(Laughter.)

And the second question is: Would that be likely to change the probabilities?

A. (Witness Slemmons) First of all, I would like to point out that I think my statement was qualified to the extent that if it could be shown that the amount of displacement in Trench T-1 was greater than 3 feet, and was all in a single event. I have reservations as to whether that occurred as one event. I think there is a distinct possibility that what you see there may be the cumulative effect of more than one event.

If it occurred as a single event, then under those conditions you might have to assume that comparable larger events or larger offsets might occur toward GETR. On the other hand, there are other geological factors that might reduce the amount of displacement toward GETR, in that the location of T-1 is sort of at a possible plexis or an area of convergence toward the Las Positas Zone and might be an area in which you might expect to get larger displacements

than you would have at trenches B-1, B-3, and B-2. So based on these assumed geological setting factors, I would personally utilize the information of the two or three feet at Trenches B-1, B-3 and B-2, rather than using the possibly larger data, or possibly larger offset that is inferred for Trench T-1.

Now if you then, making this an assumed situation that you could have a larger offset at Trenches B-1, B-3, and B-2 at or very near GETR, then the use of a larger value would modify the results obtained for the probability analyses of Jack Benjamin and of EDAC, but I do not believe that would affect the results from the TERA National Laurence Livermore Laboratory, and Bill Vesely's analyses.

So in some cases it would affect the results; for other of the probabilistic analyses, it would not.

Q. Could it affect the results to the extent of one order of magnitude? I just want to get some idea of to what extent the analyses are sensitive to these changes.

A. I think my area of expertise is in evaluating the validity and the range of variation in the geological parameters that would go into the analysis. I think your question might more appropriately be directed toward some of the members of the

probabilistic panel.

Q. And we probably will.

(Laughter.)

Now just one last question, and I intend this to be a quick one. I am not asking for an extensive kind of discussion. I am directing this to Drs. Brabb and Herd, and it has been asked several times. But I just want to confirm or unconfirm my impression.

Your mission and your analyses that have led you to make conclusions about drawing inferences with respect to future events -- size, location, intensity, and so forth -- I don't mean "intensity," but with respect to future events, stems from your perspectives as geologists; and that you are looking at this from a technical problem, in a sense a scientific problem, and you would like to have that kind of data in order to write papers that are rigorous and acceptable.

And what you have to say doesn't -- and this is my question -- doesn't reflect your opinion about the conservativeness or the propriety of the design bases for offset that were established?

A. (Witness Herd) I'm not sure I understand the question, either, clearly. I think what you are asking is: Are our concerns focused -- How are they focused? Is that your question?

Q. Yes. Are they focused with respect to the potential safety of starting a plant up again?

A. No.

Q. Or are they focused entirely towards scientific and technical considerations?

A. They are focused at the scientific issue of calculating the displacement and the other -- calculating the displacement and understanding the geology of the site. And that is the context, and the only context in which we have worked.

A. (Witness Brabb) And I agree.

end JWB #8 Bowers fol

JUDGE FOREMAN: You said that several times before, but I just wanted to confirm it myself. Thank you.

JUDGE GROSSMAN: Dr. Ferguson, continue please.

BY JUDGE FERGUSON:

Q Gentlemen, we've been here a long time and I recognize that, but I think it would be proper if we sort of concluded our investigation of this particular area with a general question and I'll tell you what that general question is in just a moment. But before I do that I would like to ask two brief detail questions. The first one is to you, Dr. Ellsworth. Do you believe that the Verona fault is probably active or possibly active?

A (Witness Ellsworth) On the basis of the seismological criteria that I have applied uniformly in the Livermore region, I would conclude that the Verona fault is possibly active.

Q Possibly active.

A That's correct.

Q Not probably active.

A That is correct.

Q I see. Do you attach any significance to the fact that the Greenville fault as you state -- I'm looking at your letter, really, of October 22, 1980 -- do you attach any significance to the fact that the Greenville fault was in fact classified as possibly active but an earthquake did occur in January, 1980?

A I believe that is an excellent illustration of the imprecision of seismological methods when used alone to classify faults according to their potential for activity.

Q Thank you. I am leading up to something and I hope every member on the panel can detect that. Thank you, Dr. Ellsworth.

I would like to turn to you, Mr. Devine, if I possibly can and pick up on a statement you made that I was interested in. I am paraphrasing what you said. You said that as a geologist you have been able to develop certain ideas, certain opinions on certain matters, but in discussing these opinions with statisticians who have been given the opportunity or the job of determining the probability of these events they would give you an answer. I think you said that you were very surprised at times at the answers the statisticians gave you. I infer from that they conflicted with your opinions as to what was likely as a geologist.

Now first of all let me ask the question did I paraphrase your statement correctly?

A (Witness Devine) I believe so, sir.

Q Would you comment, if you can, briefly on what reliance you would then have on anything that the statisticians might say?

A Yes, I will. First off, one minor clarification of your paraphrase. I was not necessarily surprised at their

conclusions of their studies but surprised at what was sensitive to their studies from my input. And then not understanding how they went about assessing or doing their study, I did not have a good basis for fully understanding why it was that my geologic statement did not carry the importance to them as it did to me. So I admit I was surprised at what they viewed was important, the amount of importance to a data point relative to mine. But I am not able to assess, then, how that should impact how I view the answers. I am not a statistician. I've done various regression analyses over the years of research, but assessing the sensitivity of this data input is something that I am not able to do.

So I would not use that, then, to cause me to not accept the results of the probability studies, but I do admit it would cause me to be somewhat cautious in the use of those studies. That is why I supported the statement earlier saying that the use of probability studies as a sole source of arriving at a judgment is something I would not favor.

Q Okay. That's fine. I appreciate that, Mr. Devine. And that's the point that I hope we would have arrived at at this point in our discussion. I want to ask you to do the very difficult thing and I would like a response, if I possibly can, from each member on the panel to the question that I am going to pose.

I want you to assume -- this is each member of the

panel except Dr. Pichumani I think is not a geologist and this question deals with geology -- I have another one for you, sir. Each member on the panel who has expertise in geology, I want you to try to do this if you can -- if you can't then simply say I can't do it and that would be a sufficient answer.

I want you,if you possibly can, to separate your reliance, if in fact you have any on the statistician, and the numbers that they feed you from their probability analysis and go back, if you possibly can, to when that kind of an analysis was not available. You are a geologist now, you arecollecting information and you are trying to form opinions and you do not have the reliance upon a group of statisticians who can provide you with an absolute number.

Now this is the question. Based on that state, mental state that you are now in, can you tell me -- each of you I would like a response -- when, and I'm not asking for exact dates, like near future is all right -- don't say "at any time" because that doesn't mean anything to me -- near future, distant future, when do you feel the next earthquake will occur on the Verona fault and what will its magnitude be?

Let's start with you on the left there, Mr. Devine. You seem to be anxious.

A (Witness Devine) Thank you for that honor.

Q Let me be very candid. What I am trying to get at is a feel for something that is not expressed quantitatively,

but I just want to get a feel if I possibly can from your expert knowledge and experience in the whole field of geology.

A I think I would have to separate when and how big, because I believe they are two different questions.

Q That's proper. I do want two answers.

A I think that the likelihood of a small earthquake, that is, magnitude 3 or so or under, has a higher likelihood of occurring than the larger event, so that is why I would like to separate the two. I think it would be possible in the next few tens of years to have one or more magnitude 3 or less earthquakes on the Verona. The likelihood of that is in my judgment relatively high. On the other hand, the likelihood of a magnitude 6 or greater on the Verona is, in my judgment, very low. Between the 3 and the 6 is in between those two extremes. But by very low, that may be as bad as at any time. So let me try to explain it a little more precisely.

I do believe a 6 to 6.5 is possible, it could occur sometime. I just don't believe its likelihood is -- I think its likelihood is high enough that, as I understand the frames of reference that NRC is using to license in respect to reactors, is high enough that it should be considered. But it is not likely to occur at that size, 6 to 6.5, with -- the likelihood is so low that for scientific investigation purposes I don't believe I'd -- that's certainly not where I'd put my instruments to find one.

Q Mr. Devine, I appreciate that answer. That is the proper spirit, I think, that I would like the responses. I know it is very difficult. But since you aresitting next there, Dr. Herd, would you like to give us your answer?

A (Witness Herd) Well, perhaps I can attempt it from a little bit more of the geologic evidence as well. We have data to suggest that the displacement at the GETR site occurs at an average rate of about .0004 feet per year, which would work out to about a meter event every 10,000 years I believe was the calculation that was done before. If one would make a simple assumption that the last event on the Verona fault was an event of a meter dimension, which certainly the offsets in any number of areas would suggest, then what is the time since the last event and then assume a constant strain rate accumulation and then calculate the time to the next one.

Well, by our investigation I would think that the age of last displacement has been quite recent on that fault, of the order of less than 4,000 or 2,000 years, as I have said repeatedly during this period of time. Such that with those sorts of considerations and the apparent recency of displacement, unless the degree of slip on this fault is much greater than we presently understand, it would seem to me unlikely that the next event would be immediate. But of course there are exceptions to that rule.

The rate of strain accumulation might not be uniform

and constant through time. So that is focused towards the time of the next large event of magnitude 6 earthquake. But I would think that the probability of lesser sized events of magnitude 5 would be expected because on most of the faults that I am aware of, for example, like the San Andreas, prior to the 1906 earthquake it had a great number of earthquakes of lesser magnitude, 6's and 5's and even a 7 probably, and these were arrayed through time before culminating in a final catastrophic failure of the fault.

I would think, from a logical standpoint, that this would imply then that we might well expect magnitude 5 event size in the future on the Verona fault. But I would agree with Mr. Devine that we are probably some distance, if not tens of centuries away from another magnitude 6 event on the Verona fault.

Q Thank you very much, Dr. Herd. Briefly, Dr. Brabb?

A (Witness Brabb) I think my position is similar. If I take a somewhat more cautious view of when the last event may have occurred, say as long as 4,000 years ago rather than more recently than that, if you could expect one meter over 10,000 years then you could speculate on that that you might get a magnitude 6 to 6.5 event in about another 5,000 years. That's a guess. Obviously, the smaller events are much more likely and much more frequent. Magnitude 2 to 3 event is likely within the next five years.

Q That's very helpful, very similar to the feelings that the other two witnesses had. If in fact your feelings are in agreement with what has been said and youare simply repeating what has been said perhaps we could save time. But Mr. Morris, I would certainly be interested in what you have to say.

A (Witness Morris) I would agree with Dr. Brabb's statements and Mr. Devine's statements. I would like to reference mine to a lower magnitude event, magnitude 3 and less. The tectonic area around the Livermore Valley has had several events larger than that recently. The area is in a state of adjustment and therefore I think the likelihood of a magnitude 3 or at least less than 4.5, if you want to put it in terms of some of the recent events, in the next five to ten years is very high.

Q Thank you, Mr. Morris.

A (Witness Jackson) I agree primarily with Dr. Herd's explanation, with one qualifier and I think it is important to express. The treatment of uncertainty, and we are all making a mental treatment of how we handle that uncertainty in our estimate. Just with that recognition, I agree with Dr. Herd.

Q Thank you.

A (Witness Justus) The question, as I view it, the questions are basically seismological in nature, but I will answer as a geologist and as a reviewer of the information to

which I have been a party to. That does not include visiting the trenches I am I think glad to say at this point. I haven't -- my response is based on a compilation of my review of the situation. Small events, less than about magnitude 5, don't produce surface offsets usually. I'm not especially concerned about small events on the Verona simultaneous with surface offset.

I would say that a large event that is coupled with surface offset on the Verona is, in my estimation, not a likely one and is in a general way then, in my view, an opinion that is a reasonable one in this case.

Q Thank you. Dr. Slemmons?

A (Witness Slemmons) My views are not very different from those that have been expressed. I won't comment as to the lower magnitude events which I think can come certainly in the near future. The main bulk of the data would suggest that faulting in the future is likely to have rather largish displacements of two or three feet; although I think the seismological record would normally indicate that there should be some smaller events as well. I would not be surprised if the next event was one of 1 or 2 or 3 inches, perhaps a thousand or two or three thousand years down the road. I think, if you compute the -- calculate the recurrence intervals as I have done, the average return period is something of the order, as has been mentioned by Darrell, of about 10,000 years for the

holocene record. For the earlier record it would be more likely of the range of 20,000 or 30,000 years. So I would predict that the next large earthquake would be approximately a 6.3 to a 6.5 and it would be accompanied by two or three feet of offset and this is likely to be 10,000 or 15,000 years after the last event, in other words, perhaps 5,000 or 10,000 years down the road.

Q Thank you, Dr. Slemmons. Dr. Ellsworth?

A (Witness Ellsworth) Yes, I would like to endorse Dr. Herd's comments and amplify on them in one small way. I recently completed a study of historic seismicity within the entire San Francisco Bay region in which we find that the frequency of events of a size comparable to the design basis earthquake that we are discussing for the Verona within this entire region is about one event in 10 to perhaps 20 years, viewed over the historical perspective. The Verona, of course, is only one element in that system and the probability of an event on an annual basis on that fault is very much smaller than the frequency that we have measured.

So it would be my estimation that the probability of say today of an earthquake of magnitude 6 to 6.5 occurring on the Verona fault is about -- is really quite low.

Q Thank you very much, Dr. Ellsworth. That's very helpful and I certainly appreciate the cooperation of the members of the panel in bringing out I think something that

perhaps has been missed when we have spent so much time talking about detailed quantitative events.

Dr. Justus, this is directed at you. On June 2, when the panel first began its testimony, you undertook to read into the record certain conclusions. The beginning of each of those conclusions is the word "We". When that statement was made that word was used, did the "we" mean you and Dr. Jackson or did it mean something else?

A (Witness Justus) I speak as a representative of the NRC Staff and the "we" is in that context, "we" referring to the Staff of the Nuclear Regulatory Commission.

Q I see. It did not include the members of the USGS nor your consultants, is that right?

A The "we" referred here to the conclusions reached by the Staff. To the extent that the Geological Survey or our consultants' input went into that review, they are included.

Q I see.

(Pause)

WITNESS DEVINE: Sir, I believe a little more answer is need on that and maybe I can't give it all, but let me start. That opening statement by Dr. Justus was discussed with us at considerable length, those of us from the Survey, and suggestions were made to him on how to express items more clearly and so there was an interaction in the preparation of that statement. So I am not sure how precise the involvement

of the "we" really should be. We did have conversations with him concerning that statement.

WITNESS JUSTUS: I interpreted the question in this way.

JUDGE FERGUSON: Dr. Justus, may I interrupt?

BY JUDGE FERGUSON:

Q In order to prevent prolonging this let me simply ask the members of the panel, everyone present, have you had an opportunity to review or is it fresh in your mind what those conclusions are? I simply want to ask whether or not the entire panel supports the conclusions that Dr. Justus has read into the record.

Are you unprepared at the present time to respond to that?

A (Witness Brabb) I'm unprepared, Your Honor. As I recall, we were discussing this information late in the evening and it now has been some time and I haven't looked at it since then. I certainly am in general agreement with the conclusions but if you are going to put us on the record I think it would be advisable for us to look at the written information again.

Q I see. Since these are in fact the Staff's conclusions it would be helpful I think to be certain that all of the experts that we have do in fact support them.

A (Witness Jackson) Could I ask for a clarification? We are using "staff" in several different ways.

Q Let's use it in the way that Dr. Justus used it.

A (Witness Justus) The way in which I used it reflects my response. My response was on behalf of the staff and the staff does not include for purposes of drawing -- regarding the conclusions in this case, the Geological Survey or our consultants.

Q I see. I think I am going to terminate any further investigation of that. And I will turn to you, Dr. Pichumani, just for a very brief clarification. You speak on page 5 of your written testimony about the Rankine wedge and the analysis done regarding stability. And then you infer in so many words that -- and I am now on page 6 of your testimony -- you had indicated that you had checked GE's calculations and you say something to the effect that the analysis is correct except it would not be correct if GE undertook to begin large earth moving operations. Is that correct.

A (Witness Pichumani) Yes, sir.

Q Okay. And in the last paragraph in your answer to Question No. 5 you say that if for any reason a significant part of this surcharge or overburden were removed a reevaluation of the stability of the reactor would be necessary. My simple question is this: in very loose terms -- let me provide a scenario and then perhaps you could respond to that. You speak about an overburden of a certain number of feet, 21 feet, as indicated in your testimony. And you indicate that

the area that you were concerned with was about 170 feet from the reactor building. Now the scenario is this: let's assume that the Licensee decided that he wanted to begin some bull-dozing operations at that distance from the reactor. At what point would you begin becoming concerned during that operation? That is, how much of that overburden, based on your calculations, would have to be removed before you would begin to be concerned?

A Unfortunately I do not have a really quantitative answer for this. Earth Science Associates I think have a computer program which pursues to do extensive calculations and I meant when I did the review of the work that the same program would be used and actual numbers would be obtained.

Q But do you have any feel? Is it one foot or ten feet? Or twenty feet? Surely if it is 21 feet you would be concerned.

A It would be in the same order of magnitude as I was talking earlier about the fault movement going beyond five meters would give me concern. It is that order of magnitude.

Q You would be concerned if it were five meters of overburden, is that what you are saying?

A Yes. I'm sorry. It is the fault movement of five meters, which would come to about 2 or 2-1/2 meters of over-burden, like six or seven feet of overburden.

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JUDGE FERGUSON: You would be concerned if six or seven feet of overburden were removed, is that correct? And you feel then a re-evaluation of the stability of the reactor would be necessary, if six or seven feet of overburden were removed, is that correct?

WITNESS PICHUMANI: I meant the re-evaluation of the stability analysis of the type performed for this, not for the reactor structure itself, but for the stability analysis that was performed for this deflection of the fault.

JUDGE FERGUSON: All right that is helpful. I have no further questions. I want to thank the panel.

WITNESS PICHUMANI: Before closing, if I may, I want to go over a word I used earlier. I said "shaking of a modern structure," in reference to the Chairman's question earlier, which was to say about the acceleration of the basement. I misspoke the word "shaking." I meant only the fault movement, the amount that is subjected to fault movement, and not the shaking. And those fault movements are also slightly different in nature than the GETR fault movement.

JUDGE GROSSMAN: Well, I am not sure I understand what you are saying now, but I assume it will be clear in the transcript, when I get it, though it may be too late to question you on that again. But I do have a few follow-up questions, before we allow Mr. Swanson to have his redirect.

All of you on the panel were asked about the likelihood of a large seismic event, and of course, you all

answered that -- to the effect that it was unlikely, but I would like to put that in the context of the show cause proceeding, and licensing proceedings in general, and the question is directed specifically to Dr. Jackson:

Even if the possibility of a large seismic event that would damage the foundation were so unlikely to occur within the next 50 years, so that you could consider the odds to be 100-to-1 against that occurring, would you recommend that the reactor recommence operations?

WITNESS JACKSON: Excuse me, for pausing. It is a very difficult question. I think the reason it is is we do not have a quantitative estimate of hazard that we find is either acceptable or not acceptable, but I can make that judgment relative to other plant reviews that we have done over the years, and that generally tells us that an event which has a possibility of occurring roughly on the order of 1-in-a-1000 to 1-in-10,000 should be considered as a design basis event, in the area of earthquakes and geology.

So I think it would be warranted to consider these events as a design basis event, both the ground motion that has been estimated, and the surface -- the possibility of fault.

BY JUDGE GROSSMAN:

Q If I can paraphrase thatin my own layman's understanding, the answer to my question of the possibility being

100-to-1 against would result in your not recommending that we permit the reactor to restart. 100-to-1, as I understand it being ter to the minus two, not ten to the minus four, or five, or six.

A I try to avoid making a recommendation as to whether the reactor would start or not start. Our element is put into description of the design perameters that should be used to make that decision, but that decision rests on a total compilation of input from the people on this panel, as well as the structural engineers, the soils engineers, the probability panel, and other groups who have been involved in the review.

So I would defer that, saying that based on my experience these events have sufficient likelihood of occurring to be considered as design basis events for making that final judgment.

Q In other words, if the event is likely to occur more often than 1-in-10,000, or 1-in-100,000, you must take that event into account, in the design basis of the facility.

Is that basically what you are saying?

A Yes.

Mr. Morris reminded me there is good reason why we specified a design basis, because we have -- speaking in general terms -- do not have the knowledge of the structural aspects, which would allow us to conclude if the plant can

take it -- take the input perameters that are specified, it would not matter.

Q Now, we had some testimony with regard to observations of the ratio of vertical accelerations to horizontal accelerations from worldwide data, I believe, or other data sets. And the answer was to the effect that it had generally been observed to be one-half or two-thirds.

Was it Mr. Devine who had given that answer?

MR. DEVINE: Yes.

BY JUDGE GROSSMAN:

Q Did that relate to thrust faulting movements, as would be expected on the Verona Fault?

A (Witness Devine) I am uncertain as to how many of our strong motion records can be shown to be from thrust faulting. One prime example I recall is the data from the San Fernando earthquake, and in that instance, we had a very unusual record at Pacoyma Dam, which is about the only record on the hanging wall -- the upper wall of the thrust fault, to work from. And in that case, there were so many complications of geometry of the point of rock the instruments set on, and failure of the foundation of the pad that the instrument sat on, that the ratio between those two is very difficult to assess accurately.

So that data point, I would not be able to use right now, without analyzing that question specifically, prior to my

answering, and that is one of the few data points I know of that applies directly to thrust faults. Beyond that, I don't have a data set in mind that is -- that applies directly, other than that record, and I am reminded by my colleagues that the peak that that instrument was indeed horizontal -- the maximum, not vertical. But there were complications on that specific record, and that is one of the few we have that fits thrust faulting.

So I guess in summary my answer is that the data base is very weak for assessing that -- the answer to your question.

Q Would Dr. Ellsworth have an answer to that question?

A (Witness Ellsworth) I appreciate the problem that you are addressing, and I think my answer would have to be that I am not a specialist in strong motion seismology, but I believe that it is true then on a worldwide basis vertical accelerations are generally smaller, and it is also my belief that where they have been observed to be substantially larger than the peak horizontal accelerations, they are generally a very high-frequency character. That may carry specific implications to the structural engineer, who would be more qualified to assess the importance of those high-frequency peaks.

Q No, I am sorry, I thought you had something to contribute on whether the fact that it might be thrust faulting

that we were concerned about would have any impact on whether we ought to use the general observations, with regard to ratios of horizontal -- vertical versus horizontal. But if you have no observations, that is fine.

A I have no observations.

Q Dr. Jackson, did you have something to say?

A (Witness Jackson) I am not a seismologist, but I am aware that there are other records, maybe one or two, and one that comes to mind is called the Gossli record, which is on a reverse fault, which may have had higher accelerations, but also thought to be due to fault geometry and station characteristics, but I am not familiar with the record. I just wanted to make sure that -- I believe the Licensee -- G.E. has provided information on that, if I am not mistaken.

Q Mr. Devine?

A (Witness Devine) Yes, I am somewhat familiar with the Gossli record, and there are more complications to it than geometry and geology. There is considerable concern about instrumental response characteristics for that record, and so it is suspect for a variety of reasons, including instrumental.

Q I see. But that was one of the few major events in which we do have readings with regard to thrust faulting, isn't that correct?

A Yes, it is.

Q And in that case, the vertical exceeded the horizontal acceleration, isn't that correct?

A If the records are accurate, yes, and I suspect -- I have serious doubts about the accuracy of the record. But for the trace it was written, yes.

JUDGE GROSSMAN: Mr. Swanson?

MR. SWANSON: Mr. Chairman, I want to maximise the use of Dr. Hall. I wonder if I could just have a moment or two to talk with him. I want to minimize my redirect.

JUDGE GROSSMAN: Would you like a recess now?

MR. SWANSON: Well, if I could just have a couple of minutes, it might be more appropriate to have a larger recess between panels, but if we could have just about five minutes now.

JUDGE GROSSMAN: All right, let us take a recess for five minutes.

(A short recess was taken.)

JUDGE GROSSMAN: On the record.

Mr. Swanson?

MR. SWANSON: Yes, thank you, I have just a few questions:

REDIRECT EXAMINATION

BY MR. SWANSON:

Q Mr. Devine, could you briefly describe what the extent of review is of the open-file reports, open-file USGS

reports, which are attached to the staff safety evaluation, as well as the the Darrell Herd map of the Livermore Valley of 1977, that is open-file 77-689.

A Yes, those open-file reports, and the map to which you refer, received a full technical review from at least two technical reviewers, and an overall review comparable to any professional paper or bulletin that would be published by the Survey.

Q Thank you.

Dr. Brabb, was it your testimony that you did a study of the modeling or the statistical analysis of any of the probability reports that have been done for GETR?

A (Witness Brabb) No.

Q Did you mean to i-ply that you had done a thorough review of the geologic data that went into the probability analyses?

A No.

Q Dr. Herd, there has been reference in this proceeding to a trace of a fault, indicated, I believe, on your '77 map, which is east of the Calaveras, and southwest of the GETR.

ARe you familiar with that trace?

A (Witness Herd) Yes, I am.

Q And you are also familiar with a trace of a fault to the north of that, mapped by Mr. Harding, is that true?

A That is correct.

Q Have you mapped the area in between those faults?

A I have.

Q Have you found any field evidence to support a northward continuation of your map fault to connect with that map by Mr. Harding?

A I have not.

Q Is there any indication that these two faults are of the same age?

A No.

Q Have you found any field evidence which would connect your map fault or that of Mr. Harding to the Calaveras Fault?

A None.

Q Finally, Dr. Slemmons, two questions:

Would you please indicate your preferred interpretation of the Verona Fault, as to its characteristics?

A (Witness Slemmons) Yes, I had three listed in the Safety Evaluation Report. I list co-equally one that would involve a 8.2 kilometer length of a reverse slip Verona Fault, with a 6.5 magnitude, and a similar segment coupled with the driving mechanism along the Las Positas fault, giving a comparable but somewhat higher magnitude, and I assign low priority, or low weighting to my third alternative of a 12 kilometer length to connect to the Calaveras Fault, because of differences in mechanism and difficulties in the dip of the fault planes involved.

Q Thank you. My final series of questions, Dr. Slemmons:

Is it not true that you have extensively studied, personally, and through the literature, the evidence of surface rupture throughout the world?

Is that a fair statement?

A That is correct, yes.

Q Have you observed a single case of surface offset which has actually intersected a massive reinforced structure, such as the GETR, resting on soil?

A No.

MR. SWANSON: I have no further questions of the panel.

JUDGE GROSSMAN: Mr. Cady?

MR. CADY: I have no questions, Your Honor.

JUDGE GROSSMAN: Mr. Edgar?

MR. EDGAR: No questions.

JUDGE GROSSMAN: I am sorry. I have one simple question. There was a question put to Dr. Ellsworth, with regard to his categorizing the fault, the Verona Fault, as "possibly or probably active," and I just wanted to get Dr. Brabb's and Dr. Herd's statement as to whether they believe it is within the definitions used, whether it was possibly active or probably active.

Dr. Herd?

WITNESS HERD: Well, as I understand those definitions, they are unique to Dr. Ellsworth's paper. The fault, from geological classification, would be one that has evidence of holocene activity. It would be one which I would consider to be recently active.

JUDGE GROSSMAN: Dr. Brabb?

WITNESS BRABB: I concur in that opinion.

JUDGE GROSSMAN: Thank you.

MR. SWANSON: I would ask that the panel be excused.

JUDGE GROSSMAN: Yes, and we all would like to thank the panel for very forthright testimony, and for the very fierce independence of the views of the members there, and it really is gratifying to know that a good many of you work for the government and do maintain your independence.

Thank you.

(The panel was excused.)

MR. EDGAR: Judge Grossman, the question was never asked, to follow up on what Mr. Barlow was asking. Mr. Barlow was asking whether staff had unreasonably pressured Dr. Brabb. The real question is whether Dr. Brabb unreasonably pressured the staff.

MR. SWANSON: My response to that is that I thought the follow-up comments by Dr. Jackson were evidence to the answer to that question. Fierce independence.

At this time, while the panel is getting off, I would

also ask the Board to call Dr. William Hall, and Mr. Joseph Martore to the stand.

JUDGE GROSSMAN: Would Dr. Hall and Mr. Martore please stand?

Whereupon,

JOSEPH A. MARTORE, and

WILLIAM J. HALL

having been first duly sworn, were called as witnesses herein, and were examined and testified as follows:

JUDGE GROSSMAN: Please be seated.

Could you state your full names and addresses for the reporter, please?

WITNESS MARTORE: My name is Joseph A. Martore, M-a-r-t-o-r-e, and the Division of Licensing, United States Nuclear Regulatory Commission, Washington, D.C., 20555.

WITNESS HALL: My name is William J. Hall, H-a-l-l. I am a professor of Civil Engineering at the University of Illinois, in Urbana Champaign. I am also a self-employed consulting engineer. My home address is 3105 Valley Brook Drive, Champaign, C-h-a-m-p-a-i-g-n, Illinois.

DIRECT EXAMINATION

BY MR. SWANSON:

Q Dr. Hall, again, I will refer to a piece of testimony entitled NRC Staff Testimony of William J. Hall. It is five pages long, with attached biographical data.

That was the testimony prepared by you for this proceeding?

A (Witness Hall) Yes.

MR. SWANSON: In the light of the agreement we have, rather than further authenticate the document, and I believe in the absence of objections, I would then offer into evidence the testimony of William J. Hall, to the Board, and ask that it be bound in this transcript as though read.

MR. CADY: No objection.

JUDGE GROSSMAN: Mr. Edgar?

MR. EDGAR: No objection.

JUDGE GROSSMAN: Admitted.

(The statement of Dr. William J. Hall was inserted into the record at this point.)

/ / /

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

In the Matter of	)	
	)	
GENERAL ELECTRIC CO.	)	
	)	Docket No. 50-70
(Vallecitos Nuclear Center -	)	(Show Cause)
General Electric Test Reactor,	)	
Operating License No. TR-1)	)	

NRC STAFF TESTIMONY OF WILLIAM J. HALL

Q.1. Please state your name and present occupation.

A.1. My name is William J. Hall. My position is that of Professor of Civil Engineering at the University of Illinois at Urbana-Champaign, and I am also an independent consulting engineer.

Q.2. Please summarize your educational background and relevant work experience.

A.2. B.S. in Civil Engineering, University of Kansas, Lawrence 1948 M.S. and Ph.D. in Civil Engineering, University of Illinois at Urbana-Champaign, 1951 and 1954, respectively.

As the University of Illinois I have been involved in teaching and research in structural engineering and structural dynamics for over 30 years. In recent years I have been principal investigator on several large research programs concerning earthquake engineering sponsored by the National Science Foundation. In addition, my consulting activities in structural and seismic engineering have included, among many assignments, the following: (a) nuclear

power plants since 1964, (b) trans-Alaska Pipeline since 1970, (c) Canadian sector of the Alaska-Canada gas line since 1980, and (d) the uranium hexafluoride gas centrifuge enrichment plant since 1973. I was a member of the Applied Technology Council projects ATC-3 and ATC-6 dealing with the development of seismic design criteria for buildings (1974-1977) and bridges (1978-1981), and have consulted on military system design in the area of structural dynamics since 1958. A summary of my educational and professional background is attached and is made a part of this testimony.

Q.3. Please describe the scope of your participation in the review of the General Electric Test Reactor for this proceeding.

A.3. At the time of the initial Show Cause review, the Staff contracted with N. M. Newmark Consulting Engineering Services to recommend the proper seismic design criteria to be used for the GETR and to provide a recommendation, based on a review and evaluation of analyses submitted by General Electric, as to the seismic adequacy of the GETR facility to meet the appropriate criteria. During this review period, I carried major responsibility for reviewing the GETR seismic issues. My recommendations and evaluations have provided the basis for certain portions of the Staff's SERs.

Q.4. Please summarize the results of your review.

A.4. After discussion with a number of persons and a review of reports, documents, and letters from NRC, the U.S. Geological Survey, and the TERA Corporation, studies for Diablo Canyon, and recognizing the lack of correlation of damage to structures and equipment in relation to peak acceleration, in the light of our judgment and experience Dr. Newmark and I recommended the

use of the criteria described below for the seismic evaluation of the GETR site and participated in the review of safety-related structures and equipment at the site.

On the basis of considerations of the type noted, we recommended that the most reasonable value of acceleration to use for anchoring the spectra for effects arising from the Calaveras fault would correspond to 0.6 g (consistent with a magnitude in the range of 7.0 to 7.5), but for design or review conservatism we suggested a value of 0.75 g. This value reflects the fact that there is some degree of uncertainty in estimating such motions and that the hazard specified by the USGS corresponded to a magnitude 7.5 earthquake. We noted that we did not expect fault motion of significance to be transferred to the site from activity on the Calaveras fault.

In a similar manner, in the case of the Verona Fault, we stated that, from the information available, an acceleration value of about 0.40 g (consistent with a magnitude in the range of 5.0 to 6.0) was the most reasonable value for anchoring the response spectra, but for conservatism we recommended use of a value of 0.6 g. The margin between the most likely value and the recommended value here is larger to account for a greater degree of uncertainty as to the nature of the seismic motion and for the fact that the USGS specified the hazard to be that associated with a magnitude 6.5 earthquake. Also we noted that the motion was to be taken as acting simultaneously with a fault motion of not more than 1 meter, interpreted to be the resultant (net) motion in any arbitrary direction.

With regard to effective acceleration, the instruments that are used for free-field ground motion measurements are strong motion accelerographs for

the most part. Acceleration, as a measure of ground motion, can be interpreted as an item of engineering interest in the sense of force, through Newton's second law, namely that pertaining to mass and acceleration. Of equal interest to the earthquake engineer are the velocities and displacements arising from the excitation which can be obtained on a time basis through integration of the acceleration record. Reliable instruments do not exist at present for recording velocity and displacement as a function of time in the frequency ranges that are associated with earthquake excitation.

Actually, extremely high accelerations can occur on a localized basis with no damage to structures or equipment. Many types of structures as well as equipment are designed to resist very high frequency accelerations in the range of hundreds to thousands of gravities, as for example in the case of military structures and equipment (submarines, missiles, ground vehicles and underground structures). If one strikes a building with a structural wrecking ball, localized damage and high accelerations occur in the region where the ball strikes the building; generally, such localized loading for a well engineered structure does not lead to building collapse or even any type of gross damage. Accordingly, earthquake excitation with a few high frequency acceleration peaks, characterized for design and analysis purposes by Reg. Guide 1.60 spectra, would not be expected to produce significant damage.

The concept of effective acceleration has been defined by Dr. Newmark in the following manner:

> It is that acceleration which is most closely related to structural response and to damage potential of an earthquake. It differs from and is less than the peak free-field acceleration. It is a function of the size of the loaded area, the frequency content of the excitation, which in turn depends on the closeness to the source of the earthquake, and to the weight, embedment, and stiffness of the structure and its foundation.

This definition of effective acceleration describes the term as used by Dr. Newmark and myself during our review of the GETR.

As employed herein for nuclear plant design and review analysis, the term effective acceleration is associated with the significant part of the ground motion as characterized by the repetitive motion portions which possess strong energy content. This portion of the ground motion obviously is of primary importance in evaluating the response and behavior of the structure or equipment elements, and thereby of importance in design and in assessing damage potential. In this sense, then, in accordance with the definition given by Dr. Newmark, the effective acceleration normally is not that value connected with the high spikes of instrumentally recorded high frequency accelerations commonly found to occur close to the source of seismic energy release, such as in the case with GETR with respect to the Verona and Calaveras faults. On the other hand, the effective acceleration would be expected to be very close to the peak instrumental acceleration for locations at significant distances from the source, zones where such high frequency acceleration peaks normally are not encountered. Accordingly, for design purposes, the effective acceleration value is used to anchor the design response spectrum. As indicated, for GETR we would expect an effective design acceleration value of 0.75 g, consistent with the NRC Staff position for peak vibratory ground motion of slightly in excess of 1.0 g.

The results of our review, as well as our conclusions regarding the earthquake ground motion design critiera are contained in Section C and Appendix A of the Staff's May 23, 1980 SER and Appendix A of the October 27, 1980 SER.

November 1980

BIOGRAPHICAL DATA

William J. Hall

William J. Hall, Professor of Civil Engineering at the University of Illinois, Urbana, Illinois, has been a member of the faculty at the University since 1949. During this time he has been engaged in research and instruction in structural engineering, structural dynamics and materials.

He was born on 13 April 1926 in Berkeley, California. After attending the University of California at Berkeley in 1943 and 1944, he entered the U.S. Merchant Marine Cadet Corps and served in the Pacific War Zone and at Kings Point until September 1945. He received the degree of Bachelor of Science in Civil Engineering from the University of Kansas, Lawrence, Kansas, in June 1948. While a senior student he held a teaching assistantship and worked summers for the Kaw Valley Drainage District and the Phillips Petroleum Company Kansas City Refinery. Upon graduation he received the ASCE Kansas Section Award for the Outstanding Civil Engineering Graduate of 1948.

From July 1948 through August 1949 he worked as an engineer in the field and operation sections of the Sohio Pipe Line Company, a subsidiary of the Standard Oil Company of Ohio. He joined the staff of the Civil Engineering Department, University of Illinois in September 1949, holding successively the positions of Research Assistant (1949-52), Research Associate (1952-54), Assistant Professor (1954-57), Associate Professor (1957-59), and Professor of Civil Engineering from 1959 to date. He undertook graduate study at the University of Illinois and received the degrees of Master of Science in Civil Engineering in 1951 and Doctor of Philosophy in Civil Engineering in June 1954.

He received the A. Epstein Memorial Award in 1958, the Walter L. Huber ASCE Research Award in 1963, the Adams Memorial Membership Award of the American Welding Society in 1967, and the Halliburton Engineering Education Leadership Award of the University of Illinois College of Engineering for 1980. He was appointed an Associate Member of the Center for Advanced Study, Graduate College, University of Illinois for 1963-64.

On 1 April 1968 he was elected to membership in the National Academy of Engineering and in 1979-80 served as Chairman of the Membership Committee.

At the University of Illinois his duties have involved teaching and research in structural engineering and structural mechanics; he carried major departmental responsibility for graduate student and research affairs (1958-1973) and serves on many high-level university policy committees and boards. Specific areas of formal research have included such topics as fatigue machine design; effects of blast forces on model submarine hulls; design, construction, and test operation of protective structures at the AEC Nevada Test Site; static and dynamic response of beams and connections; shear strength of steel beams; brittle fracture behavior of welded steel plates; properties of metals under static and dynamic loadings; seismic hazard evaluation and earthquake engineering.

He is currently principal investigator of a large research program sponsored by the National Science Foundation in the area of earthquake engineering with application to improvements in analysis and design of structures and equipment.

He is the author or co-author of over 115 formal publications (books and articles) in the fields of structural engineering, structural mechanics and dynamics, soil dynamics, earthquake engineering, plasticity, fatigue, brittle fracture mechanics, civil defense and education. He is the co-author with H. Kihara, W. Soete and A. A. Wells of a book entitled "Brittle Fracture of Welded Plate" published by Prentice-Hall in October 1967. In addition he is the author or co-author of over 150 major consulting reports, many of public record and wide distribution.

He serves (or has served) as a consultant to a number of industrial organizations and governmental agencies, including for example the U.S. Army Office of the Chief of Engineers, the U.S. Army Waterways Experiment Station, the U.S. Army Construction Engineering Research Laboratory, Naval Civil Engineering Laboratory, the U.S. Navy Bureau of Ships, Stanford Research Institute, Union Carbide Corporation, Alyeska Pipeline Service Co., Foothills Pipelines (Yukon) Ltd., Woodward-Clyde Consultants, and Structural Mechanics Associates, Inc. On his own, and as an associate with N. M. Newmark, he has carried major consulting engineering responsibility for projects in such areas as development of design criteria for hardened protective structures, including missile facilities, physical vulnerability studies, vibration studies of missile test stands, reactor containment structural design and analysis, nuclear field test studies, review of structural criteria and designs for nuclear power plants and equipment for seismic loading for the U.S. Atomic Energy Comission and the Nuclear Regulatory Commission, and development of seismic design criteria for the uranium hexafluoride gas centrifuge plant. He has been a principal consultant since 1970 on the trans-Alaska pipeline and since 1980 on the Alaska-Canada gas line. He is currently a member of the M-X Nuclear Hardness and Survivability Audit Group, an independent panel charged with technical oversight review of M-X system development.

In 1964 he participated in Project HARBOR, a study of the national civil defense posture, and in 1967 participated in the Little Harbor review. In 1964 he was selected as one of the five U.S. scientists and engineers to participate in the first Seminar on Brittle Fracture held in Tokyo, Japan under auspices of the U.S. - Japan Cooperative Science Program. In 1965 he was selected as one of 30 scientists and engineers to participate in the Meet Modern Sweden science tour held under auspices of the Royal Swedish Academy of Science and Royal Swedish Academy of Engineering. In 1966 he served as a member of the Commerce Technical Advisory Board Panel on High Speed Ground Transportation and was Chairman of the Panel on Guideways, Suspension, and Aerodynamics. From 1970 to 1973 he was Chairman of the Materials and Fabrication Subcommittee of the Ship Research Committee, NRC. In 1974-76 he served as Chairman of the NMAB Ad Hoc Committee on Application of Fracture Mechanics Analysis Techniques to Marine Systems. In 1975-76 he was a member of the Panel on Earthquake Prediction of the NRC Committee on Seismology. From 1974-77 he was a member of two committees (seismic ground motions, and structural design provisions) of the ATC-3 project of the Applied Technology Council, a group working to develop national comprehen-

sive seismic design provisions; currently he is a member of project ATC-6, studying the seismic design of bridges. He was a member of the Committee on Seismology, NAS/NRC in 1976-1979 and was Chairman of the NAS/NAE/NRC Committee to provide recommendations for improving the siting of critical facilities. Currently he is a member of the NSF Advisory Committee on Earthquake Engineering.

He was a member of the U.S. delegation on Earthquake Engineering and Hazards Reduction that visited the People's Republic of China July 24-August 13, 1978 under auspices of the National Academy of Sciences.

He is active as officer and member of many professional and scientific groups and societies: Fellow, American Society of Civil Engineers; Member, Structural Division Executive Committee, 1971-75 (Chairman 1973-74); Chairman, Structural Division Research Committee, 1960-64; Awards Committee 1975-79; Member and past Chairman, Committee on Plasticity, EMD; Member, Committee on Dynamic Forces and Committee on Nuclear Structures and Materials, 1975-79; Member, Committee on Gas and Liquid Fuel Pipelines, TCLEE, 1976-; Member, Executive Committee TCLEE, ASCE, 1980-; Secretary-Treasurer, Central Illinois Section, 1956-59; Vice President, President and Director, Central Illinois Section, 1965-68; Fellow, American Association for the Advancement of Science; Earthquake Engineering Research Institute Director, 1979; American Concrete Institute; American Society of Mechanical Engineers; American Welding Society; American Society of Engineering Educators; the International Institute of Welding, 1959-74, Expert on IIW Commissions IX and X; Seismological Society of America; American Society for Testing and Materials; Society for Experimental Stress Analysis; International Association for Bridge and Structural Engineering (Reporter, 1968); Illinois Society of Professional Engineers; National Society of Professional Engineers; Structural Engineers Association of Illinois (Chairman of Seismology Committee, 1973-77); Honorary society memberships include Tau Beta Pi, Sigma Tau, Phi Kappa Phi, Sigma Xi, and Chi Epsilon. He is an editor for a series of texts in civil engineering and engineering mechanics for Prentice-Hall, Inc.

He is a Registered Structural Engineer and Professional Engineer in the State of Illinois, and is a Registered Professional Engineer (Civil) in the State of California. He is listed in Who's Who in America, Who's Who in Engineering, Who's Who in the Midwest, Who's Who in Metals, Personalities of the West and Midwest, American Men of Science, Engineers of Distinction and Who's Who in Engineering.

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MR. SWANSON: Before we get into his oral presentation, I see two courses we could pursue, and the reason I am bringing it up, of course, is the lateness of the day, and the fact that Dr. Hall will not be available after today.

The one subject that he is testifying to that I think is essential that we complete is effective acceleration.

The other subject which he participated in very heavily, but for which it is not essential, but very important that he be allowed to answer questions on is the structural analysis of the GETR. In the event that the examination of that subject is not completed today, Mr. Martore could return next week to answer further questions.

If the parties have designed questions, and the Board, which would easily separate the subjects of effective acceleration and structural engineering, which is extremely difficult, then I would, I guess, propose that we proceed with effective acceleration and complete our examination of that, and if there is time, then go into structural.

If the questioning is really mixed, then perhaps we should put the two subjects in together and just allow questioning on both effective acceleration and structural.

I offer this as a proposal, because the subject obviously overlap, and it is difficult to separate the two, but I did want to assure the parties had a complete opportunity to examine on effective acceleration today.

JUDGE GROSSMAN: Mr. Cady, do you have any...?

MR. CADY: Well, it is my feeling that we will be able to cover the structural aspects with the full structural panel.

I talked briefly with Mr. Martore and Dr. Hall, and the structural aspects that aren't incorporated into Dr. Hall's presentation and his testimony can be adequately covered by Mr. Martore. I would prefer that we deal with the effective acceleration aspects of Dr. Hall's testimony, prior to getting into the structural aspects, just to make sure that we do get it all covered and on to the record, before he has to leave.

JUDGE GROSSMAN: Mr. Edgar?

MR. EDGAR: I am prepared to accommodate that. No problem.

JUDGE GROSSMAN: So, go with effective acceleration.

MR. EDGAR: Well, either. All of my questions for this panel -- and there are very few -- relate to the interface between structural and effective acceleration, so I don't have a problem.

JUDGE GROSSMAN: I am not sure I understand that, but I -- the respective positions -- but I guess we can do it according to Mr. Swanson's preference.

MR. EDGAR: I was a little bit obscure. I am sorry. What I meant was that all of the questions that I would ever have for Dr. Hall, I will ask this afternoon.

JUDGE GROSSMAN: Okay, that is fine.

MR. SWANSON: The only reason for separating the two subjects is to insure that when we are done with effective acceleration the parties can indeed state that they have had their opportunity on that subject, whereas, if we combine the two, questions may become mixed, and by the end of the day the parties may not be able to truly say that they have had their chance on effective acceleration. So we can separate the two then, and proceed--

JUDGE GROSSMAN: Yes, you may, and please proceed.

I assume you are going to have a summary.

MR. SWANSON: Yes, at this time, I would ask Dr. Hall to present a summary, pursuant to the agreement that we reached among the parties.

WITNESS HALL: Your Honor, and Mr. Swanson, I have a problem: I can take one of two approaches. I can present a very brief summary, and then respond to questions, or I can make the summary somewhat longer, perhaps take care of some of the questions as a part of my presentation. I need a little guidance.

JUDGE GROSSMAN: I personally would prefer the longer summary, but I believe my fellow Board members--

WITNESS HALL: I am not talking about an excessively long summary, but it might take 20 minutes to get through all of the points that I would like to make.

JUDGE GROSSMAN: I think it would be more effective to hear you narrate, than to have that all come out as a result of questioning.

WITNESS HALL: All right, thank you.

I would first like to place on the record the items that my late colleague, Dr. Newmark, and I prepared, as part of the information made available to the NRC, in our capacity as consultants to the Nuclear Regulatory Commission. These consisted, first of all, as Appendix A to the May 23rd, 1980 SER. It contains, in Appendix A a letter by Dr. Newmark and myslef, dated 14 April, 1980, which presented our recommendations, which I shall summarize in a moment.

Subsequently, in Appendix A to the October 27, 1980 SER there is another letter -- in fact there are two letters therein, one dated 29 September, 1980. This particular letter report, dealing with effective acceleration, was prepared as a result of the ACRS meetings that were held last summer, and Dr. Kerr, the Chairman of the Committee, asked that if possible we prepare some additional back-up material, which we did, and that is the subject of this 29 September, 1980 submittal that we made.

Also, as a part of Appendix A of the October 27, 1980 SER is another letter, dealing with the evaluation of structures and equipment, and we will take that up, of course, at a little later time.

All right, to be fully -- place things in perspective, what I intend to do is the following:

I would like in a moment here to essentially read the succinct two paragraphs that place our position in perspective, and I would like to follow this with some elaboration which will go in the following manner, to provide some kind of a roadmap or guide to what we are going to do.

There is a little bit on the prepared testimony that has been submitted. I shall try to not go through it in great detail, because you can of course read this, but I would like to paint the picture, going through a little bit of the material pertaining to observations on buildings, a little about ground motions and their importance, a little bit on acceleration and its importance, and a little bit pertaining to a definition of effective acceleration, prepared recently by Dr. Newmark, prior to his death, and I want to elaborate on that, and then at the very end of this, I would like to pass out a -- I will call it a plot, which may or may not be entered into the record, as attorneys see fit, but I want to use it to demonstrate some of the principles that we have been discussing, with regard to effective acceleration.

So that is what I plan to do.

As a summary of the position that Dr. Newmark and I arrived at, over a year ago, as reflected on our April of 1980 submittal, it can be stated succintly as follows:

On the basis of the considerations of the type noted -- This is taken from Page 3, incidentally, of the prepared testimony -- we recommended -- referring to previous reports -- that the most reasonable value of acceleration to use for anchoring the spectra for effects arriving from the Calaveras Fault would correspond to 0.6 G (consistent with a magnitude in the range of 7 to 7-and-a-half) but for design or review conservatism, we suggested a value of 0.75 G.

This value reflects the fact that there is some degree of uncertainty in estimating such motions, and that the hazards specified by the USGS corresponded to a magnitude 7.5 earthquake. We noted that we did not expect fault motion of significance to be transferred to the site from activity on the Calaveras Fault.

The second paragraph reads: In a similar manner, in the case of the Verona Fault, we stated that from the information available an acceleration value of about 0.4 G (consistent with a magnitude in the range of 5 to 6) was the most reasonable value for anchoring the response spectra, but for conservatism, we recommended use of a value of 0.6 G.

The margin between the most likely value and the recommended value here is larger, to account for a greater degree of uncertainty as to the nature of the seismic motion, and for the fact that the USGS specified the hazard to be that associated with a magnitude 6.5 earthquake.

Also, we noted that the motion was to be taken as acting simultaneously, with a fault motion of not more than one meter, interpreted to be the resultant (net) motion in any arbitrary direction.

That is a statement of our position.

Now, the next point I shall go through, as I indicated, first of all, I will talk for a moment about observations of damage and lack of damage in buildings in earthquakes.

There is no question that well-designed and well-constructed buildings have survived earthquakes characterized by significant ground motion. Unfortunately, most of our earthquake reconnaissance reports emphasize very strongly the damage that we see in earthquakes.

For a number of years, many of us in the research field have felt that we have been missing part of the picture by not paying equal attention to those building which are not damaged. In other words, when you read an earthquake reconnaissance report, you see primarily pictures of damaged structures; you do not get a picture of the other many, many, many buildings which have survived and stood there, and are standing.

Moreover, from a research point of view, we think that in some respect, we don't understand the picture as well as we might. There is no question that the studies of the damaged buildings have rendered information of value, but I

think we could be further ahead today, if we had been willing to spend additional money and effort on analyzing also buildings which were not damaged. Some of this has been done, in the case of school buildings, in the case of the San Fernando Earthquake. In the case of a few lightly-damamged buildings, in the case of the Caracas Earthquake, in 1957, I believe, but for the most part, not too much of this has been done.

You might be interested to -- It might be interesting for me to state that on Monday and Tuesday of this week, in and Advisory Committee meeting in the National Science Foundation, dealing with advice on the $18,000,000 earthquake budget for this coming year, we made a point of trying to get more work of this type going in the future. And this meets with support from the whole community, as far as I can tell.

The second point: We sometimes seem to deal with acceleration as it was one of the few things that existed in this world. From an engineering point of view, we are concerned with the acceleration, and the velocity, and the displacement, and we use these as part of our design process, in particular, they enter into it in the sense of the interpretation which I shall get to, as reflected in response spectra, but we deal with it, in general, in the sense that acceleration, of course, gives us some measure in general of force, in the sense of accleration times mass leading to

force, in connection to Newton's Second Law. The velocity we are interested in from several standpoints, in the sense that it gives us some general feeling of the measure of energy, and displacement we are interested in primarily from the standpoint of what are the displacements or strains in the structure.

So from an engineering point of view, I want to make it clear that we have an intense interest in all three of these particular parameters.

Now, the reason that we hear so much about acceleration is -- There are many reasons. One of them is it is one of the things that we can measure. And we have instruments for measuring acceleration, and these instruments do a very good job, so this is one of the things that we measure, and therefore we have this kind of data available to work with.

A great amount of time, effort, and funds have been spent on developing velocity-measuring instruments, in the military field, and area in which I have worked for some 30 years very heavily. To this date, we have no good instruments for measuring high levels of velocity in dynamic excitation situations. And displacements are harder yet to measure in the transient sense, although we can make inferences about them from the standpoint of relative motions, occasionally, as we do in faults and things like this.

But the important thing about acceleration also is that we use it in a design sense, perhaps a little bit too heavily, but we do use it as a perameter to base our response spectra on that we use in the design process.

Now, on the other hand, acceleration is not something that necessarily is a measure of damage, in all cases, or trouble, or however we want to characterize it. We design structures in the military field, and I have been connected with these heavily, myself, such designs for 20 or 30 years, not only for a few G's. Recently I was involved with a design where we were worrying about 40,000 G's of acceleration.

I have been also associated with many other types of military structures where hundreds to thousands of G's are common, and we do this on a routine basis.

I gave some examples, in the testimony, of a wrecking-ball striking a building. This leads to perhaps localized very high accelerations, but doesn't lead to a building falling down. And accordingly, when we look at the results of damage in earthquakes, it has been perceived for decades that there is a very poor correlation between the accelerations and the damage that is observed.

As one little bit of explanation, please observe, and I am taking out a pocketknife. I am going to strike the table like this. I am sure that exerted some tens of gravities of acceleration on the table. My knife is still in

pretty good shape. The table is still sitting here in pretty good shape. So the point to this simple classroom example is that a high acceleration does not mean that something is going to fall down.

There is most assuredly -- as the last point to this part of the presentation -- a statistical aspect to the acceleration situation, with regard to earthquakes. We have heard from this very outstanding panel that preceded us here discussions of the nature of some of the motions that might be expected in an earthquake.

For the most part, the discussion was about the peak accelerations, the high accelerations. If one takes the time to look at the data from many, many earthquakes, and we shall address this a little bit later here, you find out that there is a whole spectrum of values, of course, ranging from high values to low values. And from an engineering point of view, this is of great importance to us.

In the engineering field, from the standpoint of design, we do not design for the peak value of everything that we deal with. We don't design for the highest wind forces that have ever been observed or inferred. We do not design for the highest forces that arise from accelerations associated with earthquakes.

This whole philosophy of not designing for the very highest whatever it may be, for all types of natural

hazards, and/or man-made hazards is implicit in the engineering design field. We design for values that are expected -- that are of such a type that we believe they can be accommodated in an economical way, in arriving at structures that perform the function for which they are intended, but high enough to ensure that the level of safety is something that we are willing to accept as being reasonable. So I make that particular point.

In October of 1980, a few months prior to his death, Dr. Newmark prepared some direct testimony for the proposed Little Coho Bay site, for the liquid natural gas terminal that is located about 40 miles west of Santa Barbara, California. This testimony is available in the public record. It was presented to the California Public Utilities Commission, and anybody who wants to refer to it, can obtain a copy, I am sure.

In this testimony, he made a very short succint statement about effective acceleration, which is just a few sentences long, which I should like to read here at this point:

"It is that acceleration which is most closely related to structural response and to damage potential of an earthquake. It differs from and is less than the peak free-field ground acceleration. It is a function of the size of the loaded area, the frequency content of the excitation, which in turn depends upon the closeness to the source of the

earthquake, and to the weight, imbedment, and stiffnes of the structure in its foundation."

Now, I would like to refer briefly to the document that we prepared, dated 29 September, 1980, if you will allow me a moment to get this available. I would like to make a few statements about that, and then I am going to be pretty close to the end of my opening statement.

JUDGE GROSSMAN: While you are taking those few minutes, we do have this hall available tomorrow, if we need it.

Did you anticipate having either of the witnesses here tomorrow?

WITNESS HALL: Your Honor, if I may address this?

JUDGE GROSSMAN: Yes.

WITNESS HALL: I have been gone from home since 5:00 o'clock Monday morning. If I leave on the 7:15 flight tomorrow morning that I am scheduled to leave on, I will have exactly 12 hours at home before I leave for seven more days away from home. I would truly like to get finished today, if I could.

JUDGE GROSSMAN: I would assume that it would apply to Dr. Martore, but do you think that it would be profitable to--

MR. SWANSON: Well, of course, Mr. Martore is available next week also. The real concern is with Dr. Hall's

availability. I guess I wanted to see where we were at 5:00 o'clock. I have indications from Counsel that there is not a great deal of examination on effective acceleration. There might be more on structural, and of course I have no appreciation of what the Board's examination might be on effective. It could be that we will get well into structural today.

JUDGE GROSSMAN: I didn't understand that you said Dr. Hall won't be available again, period. I thought there was some--

MR. SWANSON: Well, he pointed to the constraint. I told you that the availability problem was with Dr. Hall tomorrow, in that he would prefer if we could go into an evening session tonight, if there is a problem of finishing up with him. What he is indicating is that he could be available tomorrow. It would be at a sacrifice.

WITNESS HALL: Then I will have six hours at home, okay, before I leave.

MR. SWANSON: My point is -- maybe you were making reference to it when I said that it was essential that we get through with effective acceleration, because that part of the testimony was done by Dr. Hall. The structural review was done jointly between the two gentlemen on the panel. Now. Mr. Martore is available next week. We anticipated that the examination on structural would not be completed today,

and then Mr. Martore then could resume his natural order of presentation of evidence next week. But Dr. Hall's availability is the problem, after today, and that is why I suggested that perhaps we could take a look at things at 5:00 and see if we can assess where we are, and determine whether or not it would be profitable to extend the session today. I don't know where we will be.

JUDGE GROSSMAN: Well, right now -- I didn't ask about having the room beyond 5:00 today, but I assume someone on the staff could find that out. They did indicate that the room would be available tomorrow, if I told them by 4:30 today, but that is out of the question, so perhaps at the next break we will check on holding the room longer today, and that may be necessary, or I will check before 5:00.

Okay, I don't want to take out time discussing it, so why don't we allow Dr. Hall to complete the presentation.

WITNESS HALL: Thank you, Your Honor. I appreciate the Board's efforts in this matter.

MR. SWANSON: Thank you.

WITNESS HALL: In our -- in "our," in this case Dr. Newmark's and my September 29 submittal, we presented an overview of the process of getting at this business of effective acceleration.

The first thing to realize in a succint and short manner, of course, is the occurrence of earthquakes is

certainly a probalistic process. Where they occur, when they occur, and where the strong energy sources occur, even on a given fault, whether the earthquake would be, for example, immediately adjacent and opposite to the GETR reactor. It couls be down the Calaveras Fault 50 kilometers. This is certainly a matter of probability.

In recent years, there have been a number of studies directed towards looking statistically at the earthquake data that is available from instruments around the world, as our data base increases. And we pointed out on Page 6 of this particular submittal "an example," is the word I would use of some of the approaches that one can take to this.

Our evaluation, in terms of arriving at the value of effective acceleration is one in part through looking at some of these statistical summaries, and in part through judgment of our experience in the earthquake field over many, many, many years, and also in the related military field, because it is very closely related in the sense of the effects that we work with.

We pointed out in there, for example, that one of the early studies by Dr. Neville Donovan of Dames and Moore Company, presented, I believe, in proceedings of the Fifth World Conference in Earthquake Engineering, in Rome, in 1973. It was one of the first large studies that involved

worlwide data, and also included the San Fernando data, and over some period of months or years, we had come to find that we could estimate fairly well for some of these close-in effects these values, through the procedure that we described there on Page 6, and I am not going to go into great detail on this.

We gave there some estimates of what one would get out of these kinds of relationships, in the sense of magnitudes six-and-a-half to seven. We said that the mean accelerations were found to be .35 to .4. I would like to make a correction here, which really has no significance on anything that we are dealing with: In rechecking these, sitting in the back of the room the last few days, I think that number .35, should be .30. I caught this on the airplane, realizing that the range there was a little bit small.

And we go ahead, and we say for magnitudes 5.5 to 6, the mean accelerations were found to be somewhere between .2 and .25.

And the mean plus standard deviation values, or perhaps the 16 percent excedence type values at a higher level would be on the order of 1.6 times these values, and would get up to about six-tenths, in the first case, and about four-tenths G, in the second case. And these are the numbers that I had read earlier in the statement of our position.

This is one example of the type of approach that one can take to get at these particular values.

I would like to comment about several points about this now:

You will notice that in what we presented that we used a range of magnitudes. We did not use a magnitude. We are not -- neither one of us are siesmologists. On the other hand, we work in this transition between the information provided by the geologist and the seismologist, and try to interpret it in a form which can be used in engineering design. That is what the role of an earthquake engineer is. And to go forward in offering advice in oversight with the design.

There is some uncertainty, very definitely, in the magnitudes as they are calculated and as they are estimated, and you have heard this brought out in the testimony in the last few days.

So we always prefer to look at these things in a little broader scale, in terms of ranges of magnitude, and ranges of numbers. And this is the reason that we presented it in this particular manner.

Now, as a second point to this, in the intervening time, since even this was prepared, there have been other studies available, at the time this was prepared, and since then. For example, we attached to the back of this report

a plot which you can study, which was based on another study by Dr. Donovan, attached to an abstract that he presented in April of 1980, at a meeting in Toronto, and we have given the reference here, which leads to an estimate of values close-in of the type that we gave.

There is also a study prepared by the Tera Corporation, by Dr. Campbell, and Larry White is present here in the audience, and will be before you on the probability panel here shortly, which supports this same level of acceleration, in general, and you can ask him firsthand about this.

And lastly, I have been part of a study on this liquid natural gas facility, down 40 miles west of Santa Barbara, in which the magnitude of the earthquake selected for design purposes, and the other conditions are very, very similar to the condition that we are dealing with here in GETR, and and independent study made by a geologist/seismologist named Dr. Jeffrey Johnson, and that particular case, which has been made available to the California Public Utilities Commission, incidentally, recently, lends support, again, precisely to the level of numbers that we are giving here.

So what I am trying to get across is that it isn't just one calculation, or one estimate. There are a number of people who have made various types of studies, regression

studies, and used different amounts of data in making these studies, and they are not widely different in the conclusions, is the point I want to get across.

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WITNESS HALL: At this point, Mr. Swanson, I would like to pass out a plot, if I could, or have you pass it out. I am going to do some work here on the easel. Other people can look at this while I do this for a minute. I want to get across a few simple concepts and then I will be through.

MR. SWANSON: Just to clarify, you mean by way of illustration?

WITNESS HALL: This is by way of illustration.

MR. SWANSON: With the Board's permission.

JUDGE GROSSMAN: Let me ask you, Mr. Swanson, while that is being done whether this in effect is going to constitute the bulk of the Staff's case. I know I asked you the question earlier in the proceeding whether the amount of offset would affect the Staff's conclusion and your answer was to the effect that no, it wouldn't matter one way or the other. Now we are up to the point of ground accelerations and the question really is is this the bulk of the Staff's case at this point.

MR. SWANSON: I want to make sure you understand. When I indicated that the offset, that the geologists would come up with as a design value, when I indicated that that offset at least within the values that were being discussed was not an essential part, that was because of the effect of the testimony by Dr. Pichumani. Now the structural panel that is here today, Drs. Hall and Martore, do address -- nevertheless is a design analysis, the effect of a one meter of offset

on the plant. What I wanted to make clear was that in the final analysis, taking into consideration the testimony of Dr. Pichumani as well, we feel that the offset that geologists would predict because of geologic or design principles is not likely to actually affect the plant. But this panel indeed can discuss the subject of offset and its affect on the plant in terms of cantilevers and structural design principles, and that is indicated in their testimony.

JUDGE GROSSMAN: Mr. Edgar, you looked puzzled.

MR. EDGAR: I missed the point. I wonder if I could have a clarification. I just didn't understand.

JUDGE GROSSMAN: Well, the point I was trying to get clarified was whether this is the heart of the staff's case at this point. From reading materials we had understood that at one point the staff's position was based on the expected offset from a highest magnitude event and it was because of that that the staff was recommending that the site not be reactivated or that the reactor not commence operations again.

Now I asked some question of Mr. Swanson earlier in the hearing in which his answer indicated to me -- and maybe I was mistaken -- that that was no longer a critical element and it left me with the impression that maybe the critical element in the case was confined now to the expected accelerations. So I am asking Mr. Swanson, since I don't know what his case is, whether that in fact is the bulk of his case.

MR SWANSON: I'm glad we cleared this up now before questioning really has started of this panel because there are a number of -- I don't want to testify, but to explain the case, there are a number of factors which the previous panel indicated were what they considered to be conservatisms, what Dr. Jackson and Dr. Justus indicated were conservatisms, one of which is that they believe, based on the testimony of Dr. Pichumani, that regardless of the offsets that are predicted, perhaps differences that may be predicted as to offsets that should be used in a design value, that in fact, based on Dr. Pichumani's testimony, a deflection would render that concern to be moot.

This panel is, however, prepared to take as a design parameter as stated in the staff's age evaluation, a structural design of one meter of offset. And they should be -- the Board and parties should feel free to question on that as well as the results of acceleration.

JUDGE GROSSMAN: Do I understand now that you are saying if we don't accept Dr. Pichumani's testimony that the fault would be deflected that then the amount of offset does become a critical factor in our evaluation?

MR. SWANSON: It is a consideration that this panel is prepared to address, yes.

JUDGE GROSSMAN: Mr. Edgar?

MR. EDGAR: I might just state our position. Our

position -- the staff has basically specified criteria which consist of a set of accelerations and a meter of offset. That is what they have done. Those values have been fed into the structural analysis, through Dr. Hall, to derive an effective acceleration. The structural engineers then analyze the building, including response to surface offset. One meter of surface offset.

Our position is that one meter, all things considered, is a conservative value. We have at least four reasons for that, one of which is Mr. Meehan's analysis, which indicates that even if a fault whose upward projection would intersect the reactor foundation was trending up toward the foundation, it would deflect around and it becomes a rather insignificant thing in the context of the structural analysis, therefore requiring that the one meter of offset be considered is indeed a conservative assumption.

JUDGE GROSSMAN: Yes, Mr. Swanson?

MR. SWANSON: Perhaps I should defer to Mr. Bachmann for an explanation on the structural review. We have split up our lines of responsibility on that matter and I think the more precise definition of the staff position is important at this time.

JUDGE GROSSMAN: Mr. Bachmann?

MR. BACHMANN: Well, as Mr. Swanson said, and I agree that I don't want to testify here, but to give you an

idea of what we are heading into, the acceptance of Mr. Meehan's and Mr. Pichumani's testimony merely eliminates the problem we considered of a cantilever condition forming under the foundation mat. It just eliminated one part of the problems you have with one meter of offset. There is related piping, there is a lot of other things that would be affected. And that is all part of our structural analysis.

WITNESS MARTORE: Your Honor, if I could attempt to clarify in two short sentences, the question of offset and vibratory motion is no more nor no less critical than the other design factors. It is a design factor and it was part of the criteria that we used to judge the structural evaluation.

JUDGE GROSSMAN: Okay. And --

WITNESS MARTORE: And our conclusion to that evaluation determines then whether it is critical or not.

JUDGE GROSSMAN: Okay. My understanding now from what has been said is that the combination of offset and accelerations are taken into account in the design basis and that you can't separate out one of them without taking into account the other and they are interrelated. Is that a correct understanding now?

WITNESS MARTORE: For the Calaveras event it was postulated only vibratory motion. For Verona there were two inputs to the design criteria. It was vibratory motion and surface offset, concurrently.

JUDGE GROSSMAN: Okay. Now I understand what you indicated last week, Mr. Swanson, was that merely the amount of offset by itself, but not taking into account in any way other than the cantilever motion, is something that would not be critical if we were to accept Dr. Pichumani's testimony, but even if we were to accept his testimony, the amount of offset nevertheless remains a critical factor in determining the appropriate design basis with regard to an event on the Verona fault because of the interaction or interdependence of the vibratory motion and the offset in determining the design basis for that event.

Is that correct, Mr. Bachmann?

MR. BACHMANN: That's correct, sir.

MR. EDGAR: I don't agree with that characterization. I'm sorry. But I am trying to be helpful.

JUDGE GROSSMAN: Let me state to begin with I am not characterizing that way. I am trying to understand what the staff position is.

MR. EDGAR: Well, but the point is that the structural engineers will take a vibratory ground motion and a one meter offset and apply that as input conditions for their analysis. If you do not accept Mr. Pichumani's testimony, then the structural engineers have still analyzed offset and ground accelerations and it remains to consider whether the analysis of those loading conditions which would obtain, absent Mr.

Pichumani's testimony, are indeed or whether the facility can take it. I don't think they are interdependent. They are independently selected and they are both considered in the design.

JUDGE GROSSMAN: Well, with that further clarification -- did you have anything to add, Mr. Swanson?

MR. SWANSON: No. I think we are about to agree. I was going to wait for the Chairman to finish with that clarification. I think what we have is a problem of words and I think the most precise response probably should come from Mr. Martore. But I believe that the clarification is an appropriate one for the staff position. But I think in this case we are talking about a very important point and I would like Mr. Martore's response.

JUDGE GROSSMAN: Fine.

MR. SWANSON: As to whether or not he would agree with the clarification.

WITNESS MARTORE: Yes, I agree with the clarification.

JUDGE GROSSMAN: Okay. You may proceed.

WITNESS HALL: Before I proceed, let me make very clear about what I was attempting to do. I was trying to present very simply -- as simply as I know how -- a very complicated subject, incidentally, in terms of the transfer of the seismological information with regard to ground motion to a form which is of use to the engineer in designing or re-

analyzing the structure. And I am dealing here solely with the subject of the shaking problem is what I am trying to get across at this particular moment. That's what I am doing. And you have opened up other topics that we will get into of course.

So by way of completing my discussion, I am going to have to hold this up, I guess, to make a few points. That is better for your hearing? You have in front of you a plot and I shall refer to this and I have tried to put some of the lines on there and I will try to keep this simple. This is a plot of a response spectrum. I will indicate the source of this at the moment and indicate this is Figure 18, if anybody can find it that wants to find it, out of a report by Dr. Newmark in the Diablo Canyon case entitled "A Rationale for Development of Design Spectra for Diablo Canyon Reactor Facility", 3 September 1976.

I hasten to add that I am not bringing into this hearing any aspects of this Diablo Canyon case. It just so happens that this particular figure, which you will note doesn't even have the name Diablo Canyon on it, is precisely the one that we need to explain what we are doing here because this particular figure that you have in front of you is anchored to 0.75 G, I found out as I looked at it.

This is a busy figure. It has lots of things on it. First of all, what is a response spectrum? A response spectrum

is a plot of the response of a number of simple oscillators that have damping in their system. These oscillators obviously have various frequencies and periods. We are interested in their response in general in terms of the acceleration of the mass, the relative velocity between the base and the mass in the sense of energy, and the relative displacement in terms of strain.

Now this is very complicated and I am trying to make it simple. But a very good description of this, Your Honor, is presented in the direct testimony of Dr. Kost, which you have before you somewhere in your papers. If you are interested in going into more depth about how these are obtained and what they mean, I refer you to that particular testimony. It is an excellent discussion.

So we are trying to take a single degree of freedom model, if you will, an oscillator, subject the base to a transient excitation which, in this case, is the earthquake excitation, and study the parameters of this particular model that are of interest to us from an engineering design point of view. And one of the many ways of depicting this information is on a tripartite plot of this particular type.

There are many other ways, I might say, and there are engineers in the audience here that use other forms of plots for analysis purposes. But this is one convenient way. And it is governed for the most part, if I may summarize now,

in the righthand portion it is governed by acceleration considerations, in the central region it is governed by velocity considerations, on the lefthand portion it is governed by the displacement forces.

Now most of my comments from here on are going to concern this righthand portion pertaining to the acceleration problem which has been the subject of so much discussion.

MR. SWANSON: Excuse me. If we are going to make extensive use of this perhaps we should have it marked as -- although he mentioned another document, I don't think we have actually used the number 7 yet for Staff exhibits, so perhaps we should at this time mark this Staff Exhibit 7, a document which --

JUDGE GROSSMAN: In view of what has been said in the record, maybe we ought to mark this 8 and leave Defendent's Exhibit 7 for your adjustment and modification of Staff's Exhibit 4.

MR. SWANSON: Okay. I think the previous discussion was off the record, but that is fine. Okay. Maybe I should indicate then, for clarification of the record, that although it is out of place that we formally have marked the complete blowup of Staff Exhibit 4 with notations that have been made during the course of the proceeding and have that marked as Staff Exhibit 7 and that we have -- the document just passed out by Dr. Hall, which is a response spectra for the Pacoima

Dam, February 9, 1971, figure and have that marked as Staff Exhibit 8.

JUDGE GROSSMAN: So marked.

(The documents were marked for identification as Staff Exhibits 7 and 8, respectively.)

MR. SWANSON: Thank you.

MR. CADY: Your Honor, we may run into a problem with that, is that if Dr. Hall makes any adjustments on his graph that he is working on on the easel it may change the effect of proposed Staff Exhibit 8.

JUDGE GROSSMAN: We will take that into account when and if it happens.

WITNESS HALL: I'm going to make a few lines on it, let's put it that way. Okay. Now to get to the point of this. The upper solid straight line on this plot is what we call a smooth response spectrum. It is a representation of studies of a large number of earthquakes. It originates from some statistical studies carried out in the 1952 to 1954 timeframe by Dr. Newmark, myself, and some of our associates and by the J.A. Blum Company, Mr. Sharpe of EDAC, and Dr. Delal -- is that right? -- and it was sponsored by the NRC Commission. This is the basis, incidentally, of the Reg Guide 1.60 spectra that are used today.

This plot is anchored on the righthand side, in the

plot that you have before you, to 0.5 G. If you will look at the scale on this, for those of you who want to study this, you will see that the righthand portion down here is precisely at 0.75 G, if you want to look at this, and the rest of the spectrum reflects the amplified response that one would get of the simple oscillator. A number, a series of simple oscillators, incidentally, having different periods as one goes across and this is precisely the way in which it is calculated.

In fact, this upper smooth straight line is precisely, as close as I can tell, the response spectrum that the EDAC company people used. I think it is in your report, if I am not mistaken. This is a 5 percent damping and I think at least the key values are the values that are in your testimony.

This is almost identical to a Reg Guide 160 spectra. I would point out to the Board that the only difference would be you would find that the Reg Guide spectra has a slight slope in here and there is a slight difference here and a slight difference over here, but for all practical purposes this response spectrum is identical to what you would find for a Reg Guide 160 spectrum. All right?

Now, superimposed on this in the upper wiggly line -- there are some other wiggly lines on here, but I will get to those just briefly in a moment -- the upper wiggly line that goes through here is the response spectrum for the Pecoyma Dam record characterized with a peak acceleration of 1.17 G.

If you look at the righthand side you will see that that starts to flare out here at about 1.17 G. It is the response spectrum plotted to the same scale on this particular diagram and you will see that in almost all instances it falls below or at or slightly above, in a few places, the smooth response spectrum which is anchored at .75G.

Now this is interesting because at this particular time this was the strongest earthquake excitation record we had in which we had good data. It points up several things. It points up that from many aspects the Pecoyma Dam record which we always think of as describing a record of something in excess of 1 G really in most, perhaps even the significant parts of it, is more closely aligned to that which would be characterized by a three-quarter G spectrum.

Now I can tell you from a research point of view a few other things about response spectra from some years of work in this field. Let me make the point this way. I am going to draw a line now and whether you need this line on the plot is beside the point. But if you were to take the Pecoyma time history, acceleration versus time, which is a wiggly earthquake record like this, and you were to place in this a small, sharp peak -- it has a small sharp peak now which has a peak of 1.17 G. Suppose I were to put in it a small sharp peak of 2 G -- I'm just going to pick this out of the clear blue sky -- what would happen to that response spectrum? If that particular

peak had a response in the 30 Hertz range or something, what it would do is it would come along like this and raise up here -- and let's assume that this is 2 G here -- it would come up like this and go out like this, is the point I wanted to make.

Now if you were to anchor the design spectrum to that high value for some reason, where would it come? And this is the point I would make. All you would do is shift this thing up and youwould have yourself a curve which goes like that over here like that, very much higher and gets into accelerations which are really unreasonable from a design point of view.

Second point. We know from observations, from looking at earthquake damage and lack of damage in buildings for years and years and years -- and this is not something that is quantified by an equation -- but we know that the damage in structures, as I said earlier, is not characterized well by acceleration and in fact it has been observed in many, many journals, we cited some of the references in our responses to interrogatories, that the damage is just not commensurate with the peak accelerations that are observed. And this has been true all over the world.

I am going to try to answer one question here. I don't know when the first use of effective acceleration was actually used in the literature. All of the concepts that pertain to that particular idea are embodied in the words that

are contained in USGS Circular 672, which was put together for the trans-Alaska pipeline. This was two years before the Diablo Canyon project. If you look at pages I believe it is 3 and 4 and study this, you will find all of -- many of the words and terms I have used are part of this. I can remember as a young researcher in 1967 the discussion of the Koyna Dam in India, which was designed for a very low seismic coefficient, was subjected to a rather high shaking, there was much discussion at that time how come the high acceleration and shaking of the dam and nothing happened.

In the Pecoyma Dam, similarly, here is 1.17 G. I do not know the precise number to which the dam is designed. The dam was undamaged. A caretaker's cottage roughly a half mile from the base of the dam which had a brick chimney -- you could see pictures of it in the San Fernando reports -- stands there just as pure and simple as it was constructed with absolutely no damage. So there is evidence galore to support the fact that these high peaks of acceleration are not the indicator of the damage.

At present we are trying to quantify this. There is a series of very large studies going on in the United States -- and this is the last point I am going to make about this -- the biggest one is being sponsored by the Nuclear Regulatory Commission, being carried out by Woodward Clyde Consultants with a number of other firms involved, trying to find out what

it is in the time history records as reflected in the response spectra and the response of structures that can better identify what it is that leads to the response and the damage in the structures. We have already learned many things and I am pleased to tell you we are relearning some things. The study is concentrating in this area of high acceleration as you would find in rigid structures with frequencies somewhere between 2 and 10 or 12 Hertz and it is very clear already that it takes a repeated series of pulses containing significant energy content to create the damage in the structures that we can associate with what actually happens in earthquakes.

We suspected this for some time and I think we are starting to get our hands on what it is. I have several students at my university working on that subject and there are, I am sure, a number of students working on this also. It is a very difficult problem because you arenot only trying to work with the theoretical aspect, you are trying to relate this to what we see in the field.

I might point out as the final point -- I draw another line on here, which might interest the audience and the Board -- and I have made a few comments in my prepared testimony about current existing building codes. What about current existing building codes versus what we are talking about here? I made a rough calculation yesterday for the UBC -- Uniform Building Code, 1979 -- for California for a structure that would be

rigid in the sense of being a shear wall, took a soil coefficient -- without going through all the details -- a soil coefficient on the upper side, an importance factor of 1.5, which is as high as you would go, which would be something very important, like emergency facilities, and sketch this on there. The sketch would go something like this, just figuratively. It would go something like this down here like that and over here like that.

The point I want to make in this particular case -- and it is immaterial whether this is 0.28 G or 0.2 G because we know that the coefficients from the code to which these kinds of structures are designed is somewhere in that range. The point I would like to make is that the ratio between that acceleration and this acceleration which is 2 G, incidentally, is a factor of about eight and a half. And you will find that for this ratio between code-type structures and what we are dealing with here in terms of a strong facility is a big margin in terms of the point.

I would close by saying that in fact from an engineering point of view the fact that this structure is as strong is of course comforting, from the engineering point of view and particularly the earthquake engineering point of view, one of our concerns is that a structure not only be able to be strong in the sense of resisting forces, but we are very concerned that it be able to absorb energy and have some ductility.

And so this is another concern that is involved in the process. I would point out to the panel that it is not solely one of can it resist so much force. The studies and the observations reveal that properly designed structures can not only resist force but can absorb energy and can accomodate reasonable amounts of deformation.

Thank you.

JUDGE GROSSMAN: Mr. Swanson?

MR. SWANSON: I did just have a couple of questions in the way of rebuttal or response to a couple of issues that have come up. I think just three questions, I should be pretty short.

BY MR. SWANSON:

Q Dr. Hall, would you please comment on the role of vertical ground motions with respect to the seismic design criteria that you and Dr. Newmark have recommended in this proceeding?

A (Witness Hall) All right. I am glad to respond to that. I have several very short points I would like to make to answer that particular question. Yes, there have been records, as we heard repeatedly in the last few days, in which the vertical accelerations are equal to or greater than the horizontal accelerations. But that is not the case in every case. Again, we come back to the concept of what you design for. The maximum of everything? No, we don't design

for the maximum of everything. So one of the first things I would like to -- in an engineering sense -- one of the first things I would like to point out is -- and this is not anything that was preconceived -- there was a study that Dr. Newmark and I made some time ago in which we had 56 -- I've got some numbers here -- 56 stations from 22 seismic events which were picked to include a spectrum of earthquakes ranging from I think about 1932 up through San Fernando and so forth, trying to get a range of earthquakes. This study was done in 1976.

For those -- this is trictly time history now, first of all -- for those records which were in the free field, clearly in the free field, 12 of 14 or 86 percent of them had accelerations less than a half of the peak horizontal. For those that were in structures, ground floors -- now in some cases these were two or three or so story structures; in some cases they were one story structures -- let's see what we had here. Right here. We had 23 of 42, or 55 percent, were less than half of the peak horizontal, and we had 37 of 42, or 88 percent, which were less than two-thirds of the peak horizontal.

Now in all fairness I must comment to the Board and the people present that of course these data not only included some close in data, like the Pecoyma Dam which was a piece of this, El Centro in 1940, which is perhaps 11 or 12 kilometers from the fault was in there, but we also had data out at some distance, perhaps up to 30 or 40 kilometers. So we didn't

have all close in data. I want to make that very clear.

But this shows you at least some range of the values. And of course we get high values.

Now with regard to the -- the next comment I would make is with regard to the Imperial Valley data. And now I am using my memory in part. We looked at this very carefully in connection with the Diablo Canyon proceedings -- and I helped Dr. Newmark with this a little bit -- and of course we recognize that there are several, several in this case, at least 3 of the 16 -- I'm not sure how many are exceeded. I need to dig out a piece of paper here. Just a minute. Here. Right here -- 2 of 18 exceeded two-thirds of the peak value, I believe. I think the finding was that in terms of response spectra there were three response spectra which exceeded the design spectrum, which was two-thirds of that for horizontal, which is what we allude to.

In other words, you see my interpretation, Your Honor, is not one only of looking at the peak acceleration. From an engineering point of view, I am equally concerned with looking at the response spectra which I am going to use for design purposes.

Now I make a few more comments about this. What was observed in Imperial Valley was not new. I remember vividly in making these earlier statistical studies when I looked at the other records in that region of California, in

the Imperial Valley, dealing, for example, with the 1940 record and some of the aftershocks and so on, that they all had this interesting characteristic of having kind of a double hump -- and I really don't want to get up and draw this if I can help it -- in which they peaked over at one particular frequency -- if I am not mistaken, you will find for even these more recent records they peak at about 10 Hertz. I will tell you in a minute -- maybe I will let Mr. Martore address it -- this is not a frequency, incidentally, that is of great concern to us from a design point of view. But we can say more of that.

It is over -- if I may take a second -- it is over here. It is over here. It is not up in here where much of our design concern is. It is over here.

Q You are indicating on the righthand side?

A On the righthand side, over at --

Q On the righthand side of Staff Exhibit 8.

A Over at 10 Hertz in this particular case. And in fact I believe in the discussion that was held today about omitting data -- and I don't know why any data were omitted and I will let other people address that -- but I believe there was one reason for one bit of data that was not included in perhaps the GE study, if I am not mistaken, and that was that one of the peaks that was at extremely high frequency, if I am not mistaken, it is over here at about 50 Hertz, way over

here. And that wasn't brought up in the testimony earlier today.

I personally have some further comments to make about the --

JUDGE GROSSMAN: Excuse me. Let's clarify. Are you sure about that? Are you testifying to that effect? Or is that some --

WITNESS HALL: This is from my memory. I'll have to give it to you from memory because I do not have anything --

JUDGE GROSSMAN: Well we want to know whether that is something that we ought to take as gospel here. I don't -- if you're just generalizing, you know, say so, and if it is a vague recollection --

WITNESS HALL: The ones at 10 Hertz I can attest to because I looked at one in our report from an earlier study. The 1940 earthquake I can tell you is 10 Hertz. We looked at the -- I think -- go ahead. Let Mr. Martore comment.

WITNESS MARTORE: The 1940 study, looking at the data that we have, did indicate that the high frequency peaks at around 10 Hertz. The other data that we looked at for the 1979 high frequency -- and I am sure it was at Station 8 -- was at frequencies greater than tens of Hertz, that is to say, cycles per second, and I can get the number exactly for you, but to the best of my recollection it was 50. But it was certainly in the tens of cycles per second.

WITNESS HALL: I'd have to hunt some in the document, Your Honor, to find this. But I am sure this can be documented.

JUDGE GROSSMAN: I'm not trying to doubt you gentlemen. I just want to know whether we are getting an authoritative account of that. Let me point out that the only station I recall being mentioned this morning was Station 6, not Station 8. So I don't want to get any confusion in the record here.

WITNESS MARTORE: Let me clarify. Station 6 was the station that was discussed this morning with the extremely high vertical accelerations of 1.74 G. That was one of the two that were left out. It was mentioned this morning that there were two left out. That was one. For the reasons that Mr. Devine mentioned. The other was Station 8, which again had the high frequency peak in the order of 50 Hertz.

MR. SWANSON: Just so there is no confusion in the record, we are stalking about stations that recorded the 1979 Imperial Valley earthquake, is that correct?

WITNESS MARTORE: Yes.

WITNESS HALL: In my case it is from memory, Your Honor. I'd have to check it.

MR. CADY: And this morning there was no evidence given as to any site-specific criteria pertaining to Station 8. The only discussion did pertain to Station 6.

WITNESS HALL: Good point. I would suggest that

Mr. Martore perhaps make a few comments -- excuse me.

JUDGE GROSSMAN: I think now is an appropriate time for a five-minute break.

(A brief recess)

JUDGE GROSSMAN: Back on the record.

WITNESS MARTORE: Your Honor, if I could clarify one point I made just before the break, I had when I referred to two stations, I was referring to two stations out of all of the recordings in Imperial Valley whose spectral accelerations may have exceeded the design criteria spectral accelerations which we specified as appropriate for the GETR. It was confused and it appeared that I was referring to an earlier discussion of two accelerations which had not been considered, may or may not have been considered by GE. So I am sorry for that confusion.

This was an independent analysis that I had done earlier of the Imperial Valley '79 data that then indicated two of the number of recordings that there were at that time, only two showed spectral accelerations greater than that that we specified in the vertical direction. I am sorry for the confusion.

MR. SWANSON: Dr. Hall, did you finish your response to the question about vertical accelerations?

WITNESS HALL: I have another general comment I think I would like to make after Mr. Martore makes some comments.

WITNESS MARTORE: Those are the only comments that I have at this time. To that question.

BY MR. SWANSON:

Q Then I would like you to indicate the effect of vertical ground motions with regard to engineering in structures, response, give us a perspective of vertical accelerations and the role they play.

A (Witness Martore) This is somewhat related to the previous question, but there are some different aspects that I would like to bring out. One is that, as has been mentioned, in most cases where there have been vertical recordings higher than those on a horizontal level, those accelerations tend to be at high frequency ranges which are not of significance to the extent, for structures, that other frequencies would be.

The other point is that where we do see especially in the Imperial Valley '79 records higher vertical accelerations than horizontal, those tend to be isolated peaks and not repetitive peaks which are typically those that are involved in damage to structures.

So the two points are that they are isolated and they are at higher frequencies than we normally consider as of significance to the structural response.

WITNESS HALL: I would make a few more comments in this connection, if I could. It is my understanding that the studies of the differential array in the Imperial Valley

the data coming from the differential array, as they become reported, will show that there is very little coherence in the data. This means that you don't see things that are repetitive in the data. And this is an important observation from an engineering point of view.

I perhaps could picture this best by the analogy of a rough sea state, if I could, in the ocean and get across the point that the size of the building has a very great influence on some of the effects we see. If you were in a small rowboat on a very rough sea you would be subjected to very violent motions, for example, whereas I think you can picture very clearly that if you were in the Queen Mary these motions would be averaged out and it would be much smoother. This effect very definitely -- it's an analogy, but this effect very definitely occurs in large buildings and, quite frankly, in a very heavy structure of the type we are dealing with here, one would see this effect. And incidentally, that pertains to some of those reduced lines on this sheet that I used earlier, but I won't go into that further.

The other observation I would make is that -- and this is perhaps a more general one from an engineering design point of view -- just how important are vertical motions. Sure they are important and we consider them in our design always in recent design, especially for critical facilities such as nuclear reactors. They can of course lead to upper level

excitations of flexible floors and lead to increases in acceleration response of equipment at these levels if you have flexible floors, which we don't in this particular facility. And this is taking into account a normal design through what is called floor response spectra.

But for the most part, in the case of a very strong massive rigid structure of the type we are dealing with here the effect upon the stresses in the concrete, for example, would be very, very small if these are calculated. The biggest effect would be perhaps upon equipment, if it were mounted or tied to one of the walls through which this vertical excitation were excited. But we don't have any situations that we can perceive where this particular type of response problem would arise.

The point I want to make here is it has been examined, it has been considered, and we just don't see a problem in this particular case. That is the end of my answer, Mr. Swanson.

BY MR. SWANSON:

Q Fine. I was just wondering if either of you could comment on your experiences with structures that have in fact experienced peak accelerations higher than those to which structures have been designed and comment generally on the effects of these accelerations.

A (Witness Martore) Very briefly, because a substantial

portion of this is in our testimony, our SER, and in response to interrogatories; however, as earthquake engineers, we have identified a number of cases where structures seem to exhibit an additional capacity above those to which they were designed. Just to cite a few cases, one has to do with the El Centro steam plant that withstood the Imperial Valley 1979 event. The El Centro steam plant was designed approximately to .2 G in an equivalent static fashion. Based on analyses that we have made and records of data near the plant, it appears that it actually saw something two to three times higher than that, on the order of .5 or .6 G.

There are additional cases of steam plants and fossil fuel plants and refineries that again have been typically designed at the same time, of the same vintage or built at the same time that the GETR was built, in the 1950's, designed to .2 G in a static fashion, which is certainly less rigorous than the dynamic analysis that we use now, which underwent higher accelerations during the Managua event, San Fernando, Alaska, Kern County and Long Beach. All I am trying to point out is that there are studies and there are indications that structures can withstand higher peak accelerations than to which the equivalent spectral acceleration that they were designed to.

WITNESS HALL: I have nothing more to add to answer that question.

MR. SWANSON: I would then make the panel available for examination by the parties and Board.

JUDGE GROSSMAN: Mr. Cady.

MR. CADY: According to the stipulation, Mr. Edgar leads off on examining Staff witnesses.

JUDGE GROSSMAN: Oh. Mr. Edgar? I'm sorry.

MR. EDGAR: All of my questions have been discussed.

JUDGE GROSSMAN: Mr. Cady?

CROSS EXAMINATION

BY MR. CADY:

Q Gentlemen, my name is Glenn Cady. I am an attorney for the Intervenors Friends of the Earth and other consolidated parties. I don't expect I am going to take very much time and so hopefully, Dr. Hall, we won't have to call you back tomorrow, with the understanding that the Board may have more questions than I want to pose to you. I am a little bit unclear. First of all, let me refer you to page 2 of your submitted testimony.

A (Witness Hall) Okay. Proceed.

Q I refer you to your answer to Question No. 4 and specifically I am interested in that one phrase that says "and recognizing the lack of correlation of damage to structures and equipment in relation to peak acceleration, in the light of our judgment and experience, Dr. Newmark and I recommend the use of the criteria described below". Could you please clarify what sort of lack of correlation of damage

to structures and equipment there is in relation to peak acceleration?

A Well, first Mr. Martore just gave a description of two or three facilities where such an observation has been made recently where we have information around the facility in terms of measurements, which is unusual in earthquakes. In other cases, it is a matter of judgment on behalf of myself and my past colleague, Dr. Newmark, over the years from what we have seen and read in the literature and so forth. I can't help but emphasize, although you may not perceive this to be related, but our experience over the years in the same types of equipment that are in hardened facilities for military structures and which have been tested extensively, we make precisely the same observation from what we can see in this sense.

So it is based in part on judgmental assessment and years of experience.

Q Okay. Thank you.

A (Witness Martore) Excuse me. If it would be helpful I can state more specifically than --

Q Please do. Please do.

A -- than my previous response.

Q Even though I am directing questions specifically to Dr. Hall, if you want to add anything, please feel free to do so.

A I stated in general terms the number of structures that underwent earthquakes. I should probably do that more specifically. I will only name a few. First was the El Centro steam plant, designed to approximately .2 G, saw -- underwent something on the order of .5 or .6 G. In Managua the Esso refinery was designed to .2 G, UBC; it withstood approximately .39 G. That was in the 1972 earthquake in Managua. In the Alaska earthquake a fossil fuel power station, 50 megawatt station built in 1957, was designed to .1 G UBC, which is static. It withstood the magnitude 8.4 earthquake in Alaska in 1964.

And finally, in the Kern County earthquake of 1952, the Kern County -- the Kern Steam Station designed to .1 G spectra, which was not exactly the Reg Guide spectra that we used, but again, it was used as a dynamic analysis, .1 G. It withstood .25 G.

Q Thank you. Or all of these facilities I am assuming that there were extensive amounts of piping in a steam facility and an oil refinery. During these events did these pipes suffer any form of damage? And if so, to what extent?

A The reports that have been written and that we reviewed indicate that the piping damage was I am tempted to say surprisingly minor. I think the word "minor" or "insignificant" can be used in terms of the fact that many of these were back operating within from ten hours to ten days. So

there was not substantial damage to the piping.

Q Do you happen to recall what type of damage these pipes did suffer through your reading?

A I do recall -- I would just caution that piping is not the major concern in the case of the GETR and I would prefer not to focus on that; however, the types of piping damage that was seen in a number of these cases was either in some cases deformations, cracks, I believe -- and this is from memory.

Q What exactly is a deformation? How would you describe a deformation of a pipe?

A A deformation I would define as movement into the inelastic range.

Q Which is? To the point of breaking? To the point of bending, twisting? Could you be a little more specific, please? I'm not trying to harrass you or anything, I just need to have a more clear understanding for later questions.

A It could be breaks, leaks, or bends in the pipes that would show up upon visual examination.

BY MR. CADY:

Q Any other forms of damage to the piping systems that you can recall, or that you have experienced in your reviews?

A (Witness Martore) I am trying to recall. I think there may have bee- restraints or bolts that may have been pulled cut. In some cases they were caused by the earthquake, or thought to have been caused by the vibratory motion. In other cases, it may have been another cause, for example, a tank, or something falling on to them. I can't be specific on each of these cases.

Q Okay, fine, thank you.

Dr. Hall, on Page 3 of your testimony, the last sentence in the second paragraph, where -- or the first full paragraph, where it states: "We noted that we did not expect fault motion of significance to be transferred to the site from activity on the Calaveras Fault."

Now, does that mean that you do not expect any ground motion from the Calaveras Fault to be transferred to the site in any way?

A (Witness Hall) I find it difficult to answer your question. When you say "ground motion," that can refer to very small motions on the order of milimeters, in connection with acceleration, and things like this.

Q Okay, let me please clarify:

Assuming that there was a 7.0 to 7.5 event on the

Calaveras Fault that did result in surface displacement, I just want to clarify this sentence, what you meant by "you do not expect fault motion."

Are you stating here that an event on the Calaveras Fault, that you do not expect that event to be of significance in transferring--

A. Yes, I understand your question.

Q. Okay.

A. My statement here refers to the fact -- in the sense of "significant," which is the key word, that it would be something -- I would expect it to be considerably smaller than the one meter that we would associate with the Verona Fault, were to it occur, as our design perameter.

Q. Oh, right. And even if there was a one meter displacement on the Verona, that did not directly go beneath the GETR facility, it still would cause vibratory ground motion, is that correct?

A. I am not sure I follow the question. Do you mean -- You are alluding to the fact that the one meter -- The Calaveras fractures, and over on the Verona you have a meter somewhere of slip, is that what we have?

Q. Right.

A. Would that -- If that occurred, there would have been some ground motions associated with that slip, I am sure.

Q Right, and so what you are stating here in this particular sentence is that if there was faulting on the Calaveras, it would have no effect whatsoever on your design criteria--

A That is not what I intend to convey, at all.

What I intended to convey here was the fact that if the Calaveras Fault had an unexpectedly larger earthquake of the type we have characterized, I would not expect to see several meters of displacement, for example, on the Verona Fault associated with it. It would be something small, if anything at all.

Q Okay, fine, thank you. You have clarified it for me.

Can I refer you now to Page 4 of your testimony?

A Page 4?

Q Page 4, yes.

And let me read a couple of sentences. The first one is in the first full paragraph and it states: "Actually, extremely high accelerations can occur on a localized basis with no damage to structures or equipment."

And then the very last sentence in that paragraph it says: "Accordingly, earthquake excitation with a few high-frequency acceleration peaks, characterized for design and analysis purposes by Reg. Guide 1.60 spectra would not be expected to produce significant damage."

And what I am trying to get at -- I am not an earthquake engineer; I am an attorney, and if you can help me out with my questions, to help me phrase them in a way that you can understand it, I would really appreciate it.

A Yes, sure.

Q What I am trying to get at is that with these high accelerations that occur on the localized basis, upon what do you justify your discounting of these high accelerations, when they are in a close vicinity to a structure?

Is that clear?

A Well, I have to go back -- I will try to give a very brief answer, but a simple one, if I can, to explain this:

First of all, the intent here was to convey the point that some high accelerations of high frequency, first of all, would not be expected to lead to -- have an energy content and lead to damage, precisely in connection with the concept I tried to demonstrate on the figures that we put out here, if it occurs on that site. And I think our research is showing that, as well as observations. And that was the point of it.

It is something that does not contain a lot of energy, and incidentally, it is not highly repetitive, in the sense where we are finding that the energy content and the repetiveness, which leads to amplification and resonance.

You see?

And incidentally, this was one of the reasons -- while I am talking about this -- that the verical accelerations perhaps have such a small influence. First of all, they are very high frequency, and in many cases, they are not, as far as we can tell from studying the make-up and characterization of the excitation, they don't have a lot of energy content, and they don't have a lot of repetiveness to them.

Do you see what I am getting at?

That is just a site observation.

The other point that I would make with this, and then you can go ahead ask some more, if you want to, is another aspect, in the sense that these high-frequency motions are in some way filtered by these large,massive buildings, and this is part of the observations that we have made before, too.

I think I will stop there, for a second.

Q Okay, you mentioned the concept of the energy being contained within the frequencies. Is that--

A Well, it depends on whether you are working with accelerations, or velocities, or displacements, what type of ground motions you are working with, but you can calcualte the energy content in several ways. You can make a calculation, if you want to. Energy is a hard thing, though, you can't see it.

Q Right, okay.

A. Let me make another point. There is another point that I was thinking:

In our observations -- Incidentally, over years -- that had to do with transient motions. In this case, I am going to refer to blast-type motions, as go with military work; they are not as unrelated -- I keep referring back to this occasion. They are not as unrelated as you may think.

One of the things that we have observed, in the early -- late 40's and early 50's, for example, and it is documented in all kinds of literature, is that for very short period, high-frequency, blast-type motions, of an impulsive nature, and this is the term we would use: "of an impulsive nature," really, the equivalent static resistance one has to provide to resist this type of motion is really very small. You see, it is the reverse of the situation; you don't have to have much resistance to withstand these very high motions.

As you get to somewhat longer period motions -- and I don't want to mislead you, because some of the motions that we allude to here in some the testimony, like the Milenia Ranch motion, and so on, are not exactly very, very high frequency -- they become -- their periods are a little bit longer, but this brings you up to a situation where you are essentially approaching static considerations. You are really getting to -- it is a very complicated, theoretical situation, but you are getting to a situation where the

frequency content of the loading, and the frequency of the responding system get to be closer together, and if you don't have repetitive motions which can lead to resonance, you are kind of getting to an equivalent static situation.

What I am trying to really say, is I think we are rediscovering the wheel, in some of the research that we are doing. That is the end of my answer.

Q Okay, thank you.

A In static cases, there is no amplification.

Q Mr. Martore, do you have anything that you would like to add?

A No, he was commenting that in the static case there is no amplification. It just doesn't exist.

Q Are you aware of any situation where the amount of energy that is transferred through these near peak field observations -- I am trying to get at where you have a situation where you have the GETR and three kilometers away there is a Calaveras earthquake, and we heard testimony throughout these proceedings that there are certain instances where you have instrumental readings that appear to be exceedingly high, the Imperial Valley, the 1.74 vertical acceleration, and in your presentation here today, you have said that there are occasions where you do have these high frequency observations.

What I want to know is: Is that -- In all cases

are there high frequency observations between earlier -- Okay, from the epicenter, going in the direction of the GETR, would that be an expected occurrence to have high frequency observations, or could you explain it?

A I think I see the nature of your question, and I can answer it very simply:

Calaveras is within, what, three kilometers?

Q Approximately.

A My answer would be that that is near field. There could be some high frequency excitations, but not necessarily.

An earthquake record is characterized by many spikes of high frequency, of course, so in that particular sense that this is a near-field situation, you would expect a record that has high-frequency excitation. Whether it has one great big bump of high frequency, it may or may not, from the observations we have made of many records. It is not an assurance that it will.

Q Is there a possibility that these high-frequency observations could contain sufficient energy to damage the structure?

A On the basis of our observations, to date, very unlikely.

Q Which observations are those? Are those--

A Pacoima, Imperial Valley. We don't have too many

with high spikes.

Q All right, getting to the graph that you have drawn here on the easel, which is Staff Exhibit No. 8, I just have one question--

MR. SWANSON: Let us have a clarification. Staff Exhibit No. 8 would be the diagram you passed out.

MR. CADY: Oh, right, I realize that. But that is a fairly representative diagram of Staff Exhibit No. 8.

BY MR. CADY:

Q I just have one question pertaining to that:

Is the straight line that you have referred to, did that come before or after the Paccima Dam experience, or was that line drawn to conform with the Pacoima Dam observation?

A Two parts to the answer, which are very short:

First of all, that is not exactly a representative situation. I would call the Pacoima Dam record a very unusual record, first of all. Secondly, the shape of the smoother response spectra there was based on a statistical study, as I said, in the timeframe of 1972 to 1976. There are two studies, actually, by us, and one by the Boone Company, that led to those particular lines, but we had arrived at that particular formulation in almost exactly those same lines, many years ago, by curve fitting of the data. We didn't have as much data until San Fernando came along. And if you will look at Dr. Newmark's and my 1968

paper in the Fourth World Conference on Earthquake Engineering, in Santiago, Chile, in 1968, you will find that smooth line drawing there, almost identical to that particular one, three years before San Fernando.

Q When was the Pacoima Dam--

A 1971.

The answer is: The smooth line is based on a statistical study of -- Again, I don't know -- some 21 earthquakes, dating back to 1932, and 54 records, or something like this, several studies like this, by several firms, and it is a representation of many earthquakes, not Pacoima. I mean -- Pacoima was just one small bit of the data that led into that particular -- part of that particular data base.

I am really trying to be helpful--

Q Right, I was getting into what came first, the chicken or the egg, analogy, and if the data was -- If the straight line, the smooth line, was drawn to conform with the Pacoima experience--

A Another thing that I should point out here.

Q Please.

A The smooth line -- There are a whole lot of smooth lines. That is one representation of one spectra.

Incidentally, I should point out for the record, and I neglected to do this for the Board, and so on: That particular smooth line that is shown on this -- What do you

call it? Figure A? Exhibit 8?

Q Exhibit No. 8.

A It is a mean plus -- it corresponds to a mean plus one sigma response spectrum. In other words, it is an 84 percentile estimation, or 16 percent exceedence. We can draw them also for 50 percentile, and so forth, but I just point that out.

I am still trying to get an answer for you. The Pacoima -- The San Fernando data, which there was a lot of it -- A small part of that data, along with a lot of other data were used in the statistical studies -- I am going to restate my answer -- to arrive at some best estimates of these types of straight lines that you have got here.

The Pacoima Dam record was one of the records, it turns out, that was in that particular set. This particular comparison was not made at that time. This particular comparison, incidentally, was made specifically for the purpose of the Diablo Canyon hearing, because there was so much discussion about Pacoima Dam versus the Diablo Canyon case. That is how that came to be. But I want to emphasize again that the shape of the spectra, and essentially the same amplifications, and the same bounds, and so on, can be found in another paper, of which I can give you a copy, if you would like -- I have it here with me -- presented -- published by us, three years before Pacoima Dam ever occured.

Q I would like to look at that paper, but if you could supply a copy of it to Mr. Swanson--

A As I leave today, I will give you my copy that I have.

Q Thank you.

Is there a possibility that the repetitive motion which possesses a strong energy content that initially begins from the epicenter could have enough energy to damage the GETR?

A Well, let us see. There is always a possibility. I mean you are asking a question -- Really the question you are asking is: Instead of a design basis of a magnitude of 7.5, could it be an 8,5, or something like this? A very remote possibility, but there is always a possibility. It is much more likely that it will be a smaller earthquake, as we heard today, in terms of magnitude.

Q But I am just asking, with your experience in the study of earthquakes and earthquake engineering, that are there data sets available that show that close to the epicenter of the earthquake that there is sufficient energy in which to cause damage to a structure, whether or not it be the GETR, or--

A The answer would have to be that if the characteristics of a certain large earthquake were such, it could cause it. But the point I would make to you, as part of

the answer would be: You realize that we have a very conservative selection of magnitude. We have a very conservative selection of the ground motions. I have tried to point this out. We have a very conservative choice of the response spectrum. YOu realize all three of these have been chosen at high levels, high percentile levels, and these are all compounded one upon another.

So quite frankly, I must say that I would expect the likelihood of representation of energy to do the damage that you are discussin as represented by a spectrum that we have chosen here, to be a pretty remote possibility. I think this is pretty -- very large, in that sense, is what I am trying to get across.

WITNESS MARTORE: Let me just add one thing to that:

The procedure that is used to determine effective acceleration considers that likelihood of energy which could be of significance, or should be considered in the design criteria. So that if the likelihood of energy content occurring at certain frequencies, or at certain peak level -- at certain levels of acceleration, if that appeared likely, it would have been factored into the effective acceleration, and thus the effective accelerations levels, for design purposes, would reflect that.

So I think the point that we are trying to make is that while there is a likelihood -- Well, the point I am

tryaing to make is that is considered in the process.

MR. CADY: Okay, thank you.

WITNESS HALL: Yes, I agree; I concur.

BY MR. CADY:

Q Again, on Page 5 of your testimony, towards the center of the page, and I will quote:

"On the other hand, the effective acceleration would be expected to be very close to the peak instrumental acceleration, for locations at significant distances from the source."

What -- The first question is: What do you consider to be a significant distance?

A (Witness Hall) Somewhere between 30 and 50 kilometers, or 40 and 50 kilometers.

Q Would you use the effective acceleration that is obtained from the 30 to 50 kilometer readings to be used to determine what the effective acceleration would be at a point, say, within five to ten kilometers of the source?

A No, within five to ten kilometers, we would normally come down from the peaks, as we have explained here. I considered five to ten kilometers still to be close-in, in my parlance.

Q Fine, thank you.

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Q Just two more questions -- actually it is one question concerned with the two examples that you have given. In your testimony you made the reference to a demolition ball striking a building. Would the amount of damage to that building in the vicinity of the wrecking ball be of great significance on the area where the ball struck?

A It could be. I guess, if you have watched a wrecking ball wreck a building, if they try to destroy a rather massive piece of concrete it takes a tremendous amount of beating at the piece of concrete to destroy the piece of concrete. On each blow the ball, if you look, you will see of course some small amount of concrete that gets crushed. So in that sense that is damage, of course..

MR. CADY: Okay, fine. Thank you. I have no further questions.

EXAMINATION

BY JUDGE GROSSMAN:

Q Dr. Hall, a propos of our preliminary discussion with regard to displacement, I notice on page 5 of your testimony that you indicate that you considered a fault motion of not more than one meter.

A (Witness Hall) Right.

Q Do I understand correctly then that that is the overall limitation with regard to your conclusions and that is that you do not conclude anything further than what would

happen with regard to a maximum one meter displacement?

A I guess the answer to the question is yes. But that's not quite right. Let Mr. Martore comment and then I will comment again. I will answer some more. Go ahead.

WITNESS MARTORE: Are you saying that your understanding is that we didn't consider anything more than one meter?

BY JUDGE GROSSMAN:

Q With regard to your conclusions, yes, that is my question, whether your conclusions would hold for anything beyond one meter. I understand that the limitation here is as to one meter. But if it isn't, I want to have that clarified now.

A (Witness Martore) By inference that is correct, yes. We assume that one meter -- the recommendation was that one meter is the maximum possible, well, as the words indicate here. And so we didn't require analyses beyond that.

Q Okay. That's a propos of what we were discussing before you began the testimony as to where displacement comes into the picture. I think I understand it now. That's fine.

Now did that one meter movement maximum that you were talking about, did that have to occur directly underneath the GETR or could that be anywhere in the near field?

A (Witness Hall) It could be either.

Q All right. Just to clarify that diagram, there was

a question asked as to when that diagram -- I am referring to that response spectrum that was in your exhibit, Staff's Exhibit 8. I wasn't even sure that there was a Pacoima Dam event. Was there an event?

A Yes, Your Honor. This is a plot -- it was the San Fernando event and part of the San Fernando event. There was a record on the crest of the abuttment, on the rock near the abuttment on the Pacoima Dam. This is a calculation from an actual record taken by an instrument located at that location.

Q Okay. And from what else you have indicated in response to Mr. Cady's questions then, you had, if I understand correctly, the basic shape of the straight line response spectrum before the event, but that you then applied that shape to a chart, but after the event occurred. Is that basically the proper perspective?

A Let me elaborate for you and Mr. Cady a little more. I see you need a little more detail, but let me be specific about this. I'm going to have to go back in history and I will make this very, very short. In the early years of designing nuclear facilities the so-called Howsner spectrum was used and it looks different than this -- it is a smooth curve -- it really came about in several ways, but we only had a few records at that particular time from which response spectra were calculated. These were estimated by overlay, again. I

mean by "overlay" I hope you know what I mean in the sense of drawing them on transparent sheets and overlaying them and estimating what these were.

Subsequently, in about 1967, Dr. Newmark and I undertook the same type of an exercise. We had a few more records at that particular time. We arrived at the plot -- just one moment, please -- in this paper which I am going to give to Mr. Cady in a minute. I am going to just show you here. Look at the plot. This is from 1968. It has this shape is what I am trying to convey to you.

It turned out that after we had a much larger body of data and had done the statistical studies that we found that the amplified acceleration region in this case was almost precisely what we had over here. In the velocity case we were at about the 70 percentile value and in the displacement case perhaps a 60 percentile value. We said this in print before so I just tell you. In other words, what I am trying to say is we didn't do as good a job of estimating this part but we did a very good job, almost by accident truly, in estimating this particular part, which is really the significant part normally.

MR. SWANSON: Could you indicate verbally what portions of the spectra you were referring to?

WITNESS HALL: We did a very good job of estimating the acceleration controlled region, a fair job at estimating

the velocity control region, and a somewhat poorer, although I must modestly admit not that poor a job in estimating the displacement region in this particular paper written in 1967. Incidentally, this was also presented in this form in Tokyo in a conference by Dr. Newmark and at that time was actually used by the NRC in the designs at that time.

BY JUDGE GROSSMAN:

Q If I can indicate my understanding of what you are stating, I believe Mr. Edgar was also going to clarify the description. What I see on Staff's Exhibit 8, to which you refer to and which you believe you did a very fine job of estimating, it was a straight line that slopes down beginning at about a frequency of 1-1/2 Hertz.

A (Witness Hall) Correct.

Q And the place where you thought you did not such a fine job was below .2 Hertz, is that correct?

A That's correct, Your Honor.

Q And I forget exactly what you said was -- pardon?

A In the middle region, is that what you are asking?

Q Yes. A fair job, was that it?

A A fair job. Yes, sir.

Q Okay. So it is a fair job on the horizontal line between .2 Hertz and approximately 1-1/2 Hertz.

A Yes, sir.

Q Okay.

JUDGE GROSSMAN: Mr. Edgar, was that what you wanted?

MR. EDGAR: Yes, sir.

BY JUDGE GROSSMAN:

Q Sir, you gave us some examples, Dr. Hall, of instrumental readings on stations which exceeded the design spectrum and indicated in general that there are examples where the accelerations exceeded the response spectrum with regard to certain events. Are you suggesting that we disregard your response spectra or the NRC's response spectra in determining what the design basis ought to be?

A (Witness Hall) No, sir, Your Honor. Really what I really should say is the following: I must have given a slightly wrong impression. When you undertake a statistical study of the type we did in this case to look at norizontal response spectra and arrive at the smooth shape, you must realize -- let's talk about the mean values for a minute instead of talking about something higher than that or lower than that. There obviously are spectra -- no two earthquakes are alike. They all have different time histories and they all leave a response spectra which are different. There are no two alike. Some fall below, some have a shape that cuts off -- can I use the board for a second? Would this be helpful?

Q Yes, that's fine.

A In some cases you will find earthquakes that have

response spectra that look like this; some of them you will find that have something like exceeded in one small part over here and so forth. What I am trying to get across is there is great variation in the response spectra that come from the time histories from earthquakes.

This smooth line is a best representation which is easily usable in the analysis and design process. That's the reason that we do it this way.

Q In other words, it is the best thing we have to work with so that --

A We have two things to work with, Your Honor, without getting too detailed. One is a response spectrum like this. We can also deal with more cost and time and effort using the time histories themselves, if we wish. And we do in some cases. If you do that, you need to use a number of them.

Q Okay. You gave examples of where accelerations exceeded design bases in a number of buildings and events in which -- and I believe Dr. Martore elaborated on the examples in which there were .6 G values were the design basis called for .2, et cetera. You do recall that, Dr. Martore?

A (Witness Martore) Yes, sir. One clarification. It is Mr. Martore.

Q Mr. Martore. Okay. Were there instruments at the foundations of those buildings that you referred to?

(Pause while the panel members confer.)

A The reason for the delay is that we are checking each of them. In some cases there were instruments directly in the building; in other cases, they were not too far from the building, instruments not too far from the building.

Q And were you then talking about actual recordings taken at the buildings of these G values?

A Again, I cited a number of cases. Some were actual instrumental values; others were estimates.

Q Dr. Hall, you also gave a presentation with regard to percentages of data points in which the vertical accelerations were less than a certain percentage of the horizontal.

A (Witness Hall) Yes.

Q And I believe there were some figures that indicated that in 86 percent there was the vertical accelerations were less than one half of the horizontal, figures on that order.

A For example -- yes, Your Honor. Go ahead.

Q Were those thrust fault events that you --

A Some of them were. Some.

Q It was my understanding that these were very old figures, from 1932 to somewhere in the 1950's.

A No, they go from 1932 up to 1974, I think. I can check the date. I've got it right here. I had this handy with me so I did this while I was sitting listening. I can tell you in a minute. It went up through -- well, it looks like 19 -- the upper date is 1971 and the early date is 1940.

Q I had understood from prior testimony that there weren't any or there were only few thrust faulting events until recent years in which there was data taken.

A I don't think there are many thrust faulting -- that's correct, but go ahead.

WITNESS MARTORE: We need to clarify a point. The difference between an event and a recording. An event, we typically use that word to mean that earthquake event as defined by San Fernando or Imperial Valley '79. Within that one event there may be a number of recordings and we tried to break that down. The numbers appear to be -- these, again, are basically -- for San Fernando there were 18 different recordings for that one event and 16 of the 18 were less than -- the vertical was less than two-thirds of the horizontal.

BY JUDGE GROSSMAN:

Q Yes, but my question had to do with whether there was any significant amount of thrust faulting within the examples you gave and if there were, let's hear about it, because it seems to be at variance with some testimony that I had heard earlier which indicated that there was almost no data before the 1970's with regard to thrust faulting events.

A (Witness Martore) When you say thrust faulting, Your Honor, do you mean surface offset or a thrust fault event? Or an event from a thrust fault? I'm not sure what --

Q Well, I'm talking about data from an event in which there was thrust faulting rather than just strike slip movement.

A (Witness Hall) Well, in a tabulation which I just happen to have here by Dr. Johnson that we used in the liquid natural gas studies, he has tried to identify the events or earthquakes, as we are talking about here, with regard to whether they are thrust faulting or otherwise. As you look down through it there aren't many, you are correct, there aren't many. So far as I can tell by cross checking quickly the data I have in front of me here versus what we used to make this statistical summary, I think it is fair to say that the only data I can identify readily that is of a thrust nature in the numbers we gave you is the San Fernando data which comprises -- he says 18 of the records here. I didn't count them.

Q So actually until the 1971 San Fernando event you really can't pinpoint any of those events in which you have data as involving thrust faulting.

A This is somebody else's data and I always hesitate to use something of somebody else's, but just let me take a moment. This is, again, Dr. Johnson's data. As I look down through his tabulation -- and he tried to gain from various people as much as he could -- I see a reverse question mark, which would be a reverse thrust fault, question mark, at Santa Barbara in 1941 which is not in our data here. The

vertical, if you are interested, in terms of the peak value -- it's a question mark, admittedly -- was .07, whereas the peak horizontal was .24. So that is roughly 30 percent.

Q I don't want to --

A No, I --

Q I don't want to get involved in someone else's data.

A No.

Q You just indicated that that is not part of your data set.

A That's not part of my data set. But the point I am making is that there are smaller earthquakes, as far as we can identify them, in which these ratios are not 1 or in excess of 1. They are also smaller. That's the point I'm making. Okay.

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BY JUDGE GROSSMAN:

Q I am not quite clear as to whose work it was, in which you relied upon the particular G values in your study.

Was that your work, or was that based on also input from the geologists and other seismologists involved in this proceeding?

A You mean to arrive at the effective acceleration values?

Q Yes.

A The three or four data summaries that I referred to, and the statistical analyses of these were done by others, very clearly, but I named the people who had done this: Neville Donovan, in two cases, Tera Corporation, in one case, Dr. Johnson, in another case, with regard to the data bases themselves.

Those are the four cases I cited. There are -- those are the major studies, frankly, that have been done to date, on large bodies of data.

Q I am just trying to get the methodology used.

So, in other words, from the input of those people, which resulted in a certain acceleration -- or certain acceleration values, you then used those acceleration values--

A Plus our judgment.

Q --plus your judgment, and arrived at your conclusions.

A Yes, Your Honor.

Q Okay.

JUDGE GROSSMAN: Judge Ferguson?

BY JUDGE FERGUSON:

Q Dr. Hall, I would like to repeat a statement that Mr. Martore made, and ask you whether or not you will agree with the statement.

A Okay, go ahead.

Q He said structures can stand higher accelerations than they are designed for.

Do you agree with that statement?

A In general that is true, sir.

Q Let us then focus on vertical accelerations.

A Fine.

Q You were speaking earlier about vertical accelerations and you related those vertical accelerations to what might be expected to occur at the GETR site, and I think you made a summary statement that: "We don't see a problem, in this particular case."

Is that a correct statement of what you said?

A That is what I said, sir. Yes, sir, that is what I said.

Q What did you mean by that?

A Well, first of all, we have a heavy structure, so I would expect some filtering -- If high frequency, high vertical

accelerations were to be imparted to the base -- Now, I really have to qualify that, because there is some cell structure interaction that takes place. These are some terms that we haven't used before, but you have an item burried in the soil some 20 feet, there is some interaction between the soil and the structure. This interface leads to some filtering right at the interface, in terms of the excitations in the building.

The building -- the heavy concrete core, in this case, which contains the important parts of the reactor system, is a massive concrete structure. These accelerations, in themselves, just would not lead -- in the sense of a forced concept, if you were to interpret it that way, don't lead to stresses or strains that are of really great engineering significance. That is the point in the sense of the structure itself.

Q When you said a "high acceleration," I am only talking about vertical accelerations at the moment--

A It could be on the order of -- Our recommendations were two-thirds of what you see here, so we are talking about -- What is 60 percent of -- Yes, I guess it would be half a G, in the high frequency range.

Q So you are speaking about a vertical acceleration of about .5 G, is that right?

A Yes, and in certain frequency ranges, it is higher than that, because it is amplified.

Q What would you feel the highest acceleration value -- vertical -- excuse me, vertical acceleration value for the lowest frequency that would occur at that site would be?

Is my question clear?

A Oh, wow, let us see.

Q What I am trying to get at -- to be very candid, Dr. Hall, if I can direct your attention to this question is that I think you have made it clear that we are really concerned about the low frequencies, rather than the high frequencies, when we talk about damage, is that correct?

A Lower frequencies. Frequencies in the medium range of frequency, right.

Q And my question really is: In that range of frequencies, what values are you speaking about when you speak about "high accelerations"?

A From the standpoint of the criteria that we would use for purposes of checking, with regard to the structure itself, the base of the structure, and so on, it would be two-thirds of what is shown here in the stright-line plot, and the amplified part, it would be about one-and-a-third G's.

Q About one-and-a-third G's vertical acceleration.

A Correct.

Q Now, let me ask you to assume that there is in fact a one-and-a-third G vertical acceleration at the base of the

building.

Are you knowledgeable enough about that area of the building to determine what you would expect the transmission of that value, through the base, to the inner parts of the structure? I am really asking you, if you can, to give me some estimate of what the acceleration inside of the building would be, if there were the values you just quoted on the outside.

A Let us confer here for a minute. You are taxing my capacities here.

(The witnesses confer.)

WITNESS HALL: In all honesty, I don't have those numbers at my fingertips. I would expect that the G.E. people could address this problem, in terms of the analyses that have been made, sir.

BY JUDGE FERGUSON:

Q But you would not know, at this time?

A I do not -- I hate to speculate what the numbers are, when I really don't know what they are.

Q Well, I don't want you to speculate. I just want--

A No, I don't know. We went through a review of the analyses that were conducted, but this was a year ago, more than a year ago, and I do not have them in my mind at the moment.

Q Mr. Martore?

A. (Witness Martore) I would like to clarify two points, and perhaps it is not necessary, but just for the record: One, is we -- those numbers are calculated, it is just that they are not before us here. So it is not a question of anyone having to speculate what the amplified accelerations would be through the structure. They are in the various reports that are submitted on the docket.

Secondly, when we are speaking of 1.3 G's, that would be, again, the amplified vertical acceleration, at a specific frequency, as specified in our design criteria, not the level of acceleration that we would anchor the regulatory guide 1.60 spectra to.

Q. Okay. Now, you will be with us, hopefully, next week, is that right?

A. Yes, sir.

(A drinking glass falls to the floor.)

WITNESS HALL: You see, that high acceleration didn't even break that glass.

WITNESS MARTORE: Yes, sir, I will be here next week, and available.

BY JUDGE FERGUSON:

Q. I wonder if you would be good enough to review that, and perhaps the question may come up again.

A. (Witness Martore) Yes, sir, I will.

Q. What I am trying to get, Dr. Hall, before you leave,

is as much as we can on this particular point.

A (Witness Hall) Yes, I appreciate that.

Q You did give several examples of how massive structures can dampen out oscillations. You spoke about a ship on a sea. That assumes a certain coupling, if you will, between the ocean and the ship, is that correct?

A It certainly is.

Q Do you think that would be appropriate in the case of this structure of the GETR resting on the soil it is resting on.

A No, I mean that is a kind of crude analogy used, because we certainly don't have the GETR plant floating in a pool of water. So this is not the situation that we have. But the subject of soil-structure interaction, which is really germane to your question, which is the situation of a structure resting on, resting in soil medium--

Q Well, that is just my point, that is what I am focusing on, the soil-structure interaction.

A Yes, exactly. That is a subject that has received extensive research investigation, over the last ten or 15 years. There are various techniques for making calculations of what this coupling is that you are referring to, and the interaction, and the motions that take place, which incidentally are not only translational motions, in the sense of horizontally or vertically, but we also have rocking and

and other motions. All of the procedures lead to estimates -- What I am trying to get across is that they lead to estimates of what these motions may be. None of them really accurately reflect the motions come into the structures, and it takes quite a bit of judgment to interpret these. But soil-structure interaction effects were taken into account in the analysis in this particular plant, quite frankly.

Q. Did you participate in any of that analysis?

A. I participated in the review of some of these analyses, Your Honor.

Q. I see.

Dr. Hall, we have heard testimony in the past few days that an earthquake of perhaps 6 to 6.5 may occur at this particular site.

Are you familiar with the type of ground accelerations that might be expected from that earthquake at the site?

A. We made estimates of them. That is reported in our -- Well, one example of these types of estimates is reported in our September letter, and we used other data to draw our judgment.

Q. Yes, I think we are familiar with that.

Based on -- My question is: Based on the analysis that you have done at this particular sight, and this was sort of a conclusionary question, do you feel that anything has

been overlooked that should be considered, so far as acceleration, ground acceleration is concerned, for an earthquake of the magnitude that we have considered?

A. To the best of my knowledge, on the basis of the information we have available to date, and the experience that Dr. Newmark and I have had over the years, my answer would be I don't -- I cannot conceive of anything that has been overlooked.

Q. And you do feel that if an earthquake of magnitude 6.5 did occur, there would be no damage to the structure, based on ground accelerations, is that correct?

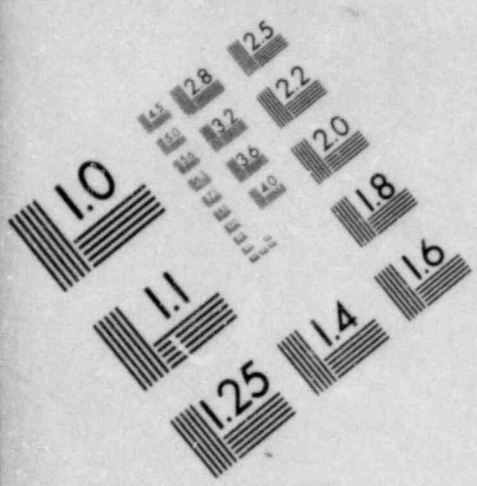
A. That is correct.

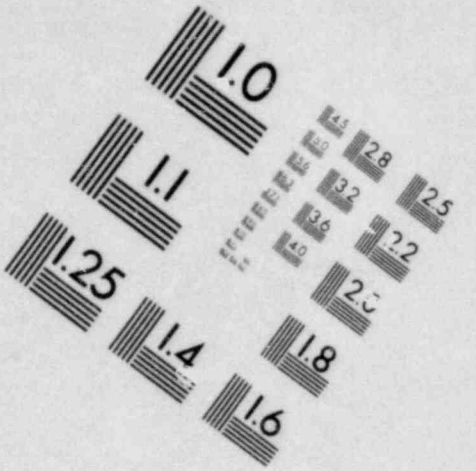
JUDGE FERGUSON: I have nothing further, Mr. Chairman.

JUDGE GROSSMAN: Mr. Swanson?

WITNESS HALI: Well, wait a minute, I should -- Well, why don't I let you make a comment.

WITNESS MARTORE: Again, for clarification, in our safety evaluation reports, and in our testimony, we did indicate that there may be some minor localized damage -- By that, we mean -- I am trying to think of something localized -- The point is that the criteria that all of the safety-related structures, components, and equipment must function; we are assured of that. Localized damage that is not of significance to the safety of safe shutdown, and





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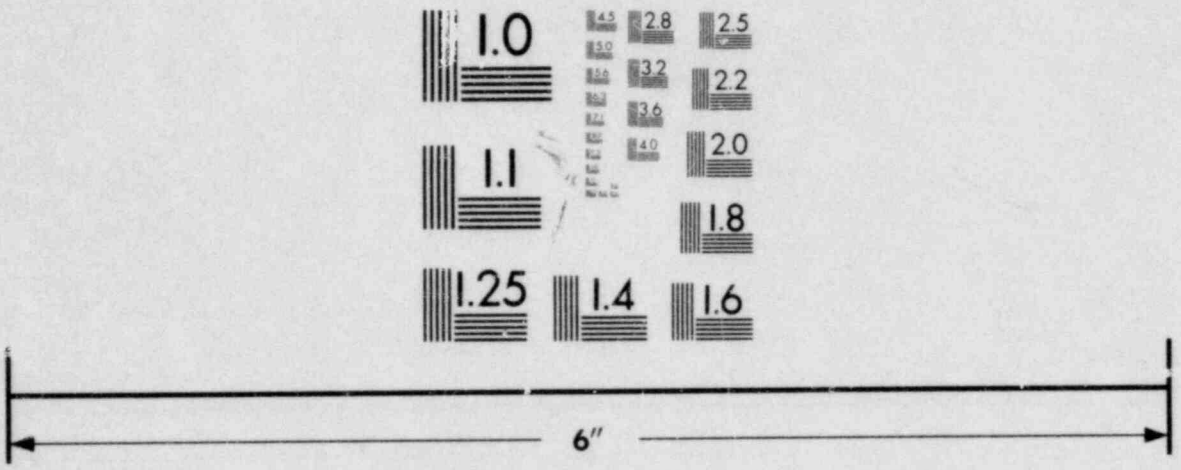

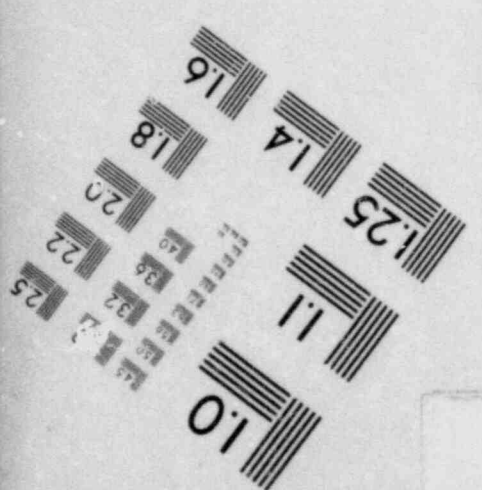

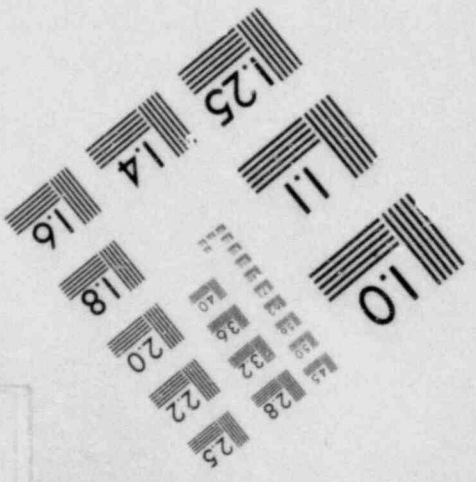

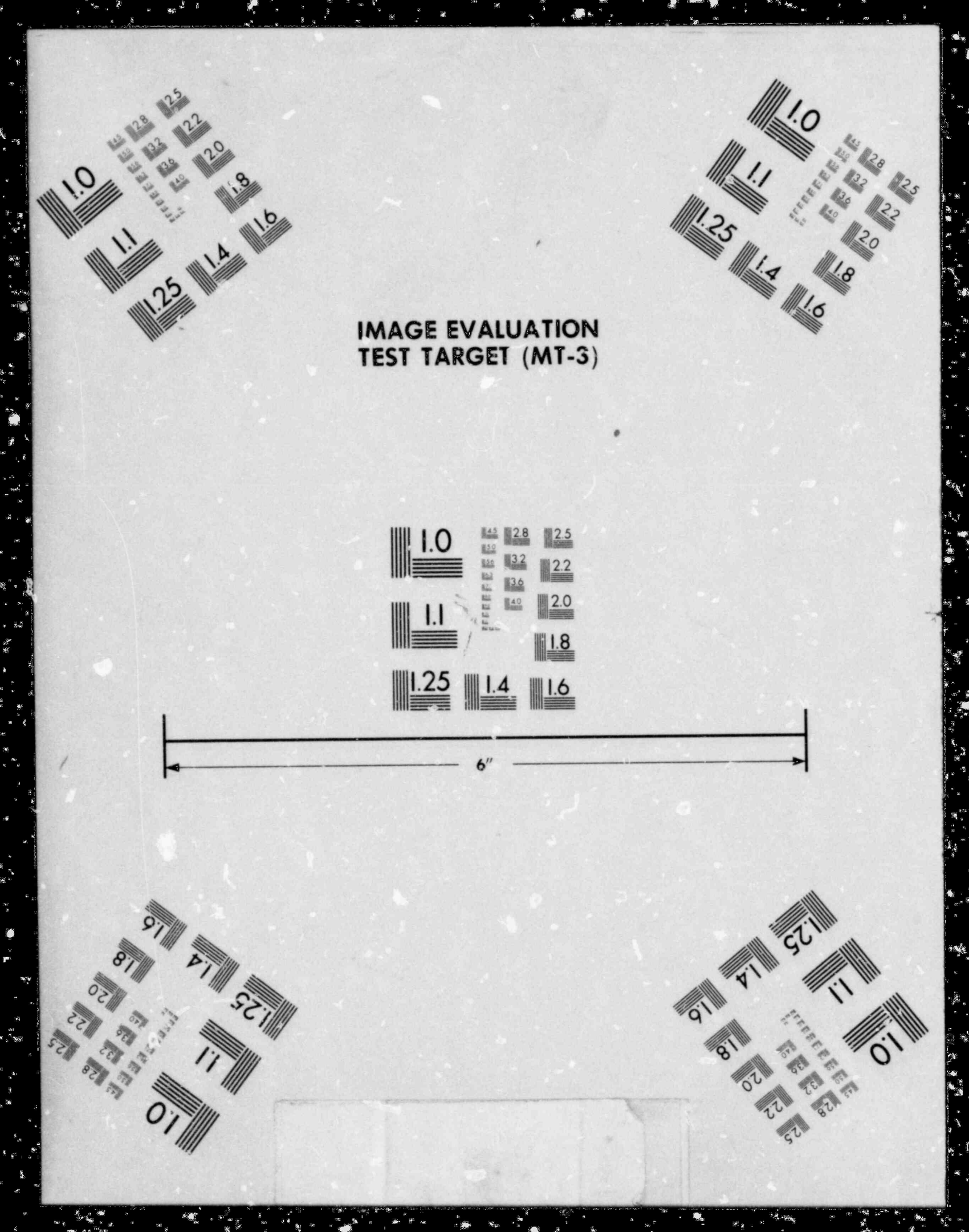
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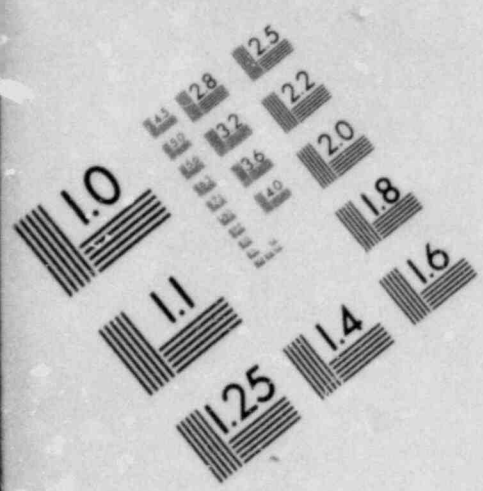

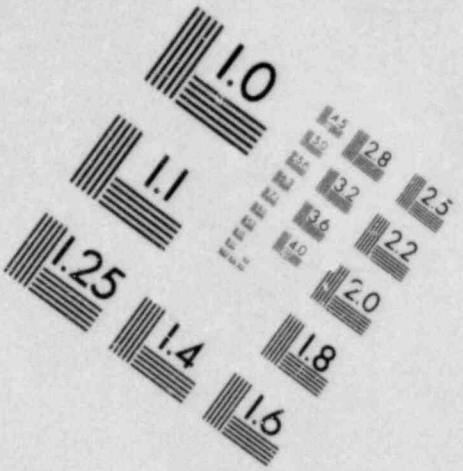

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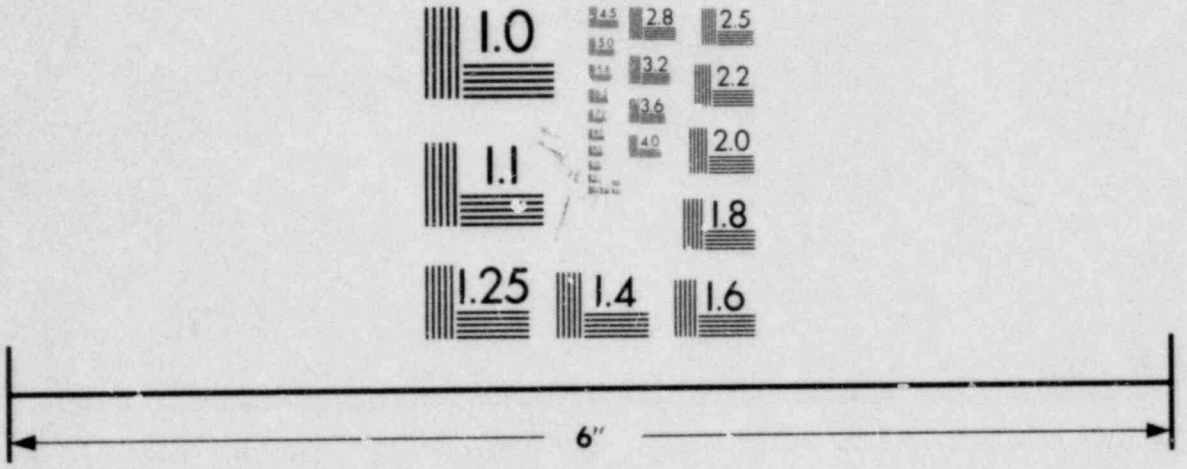

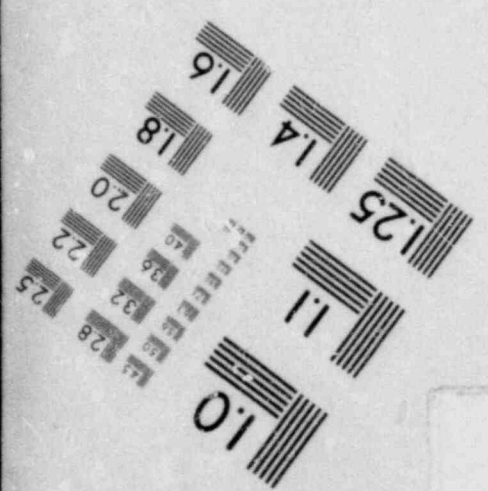

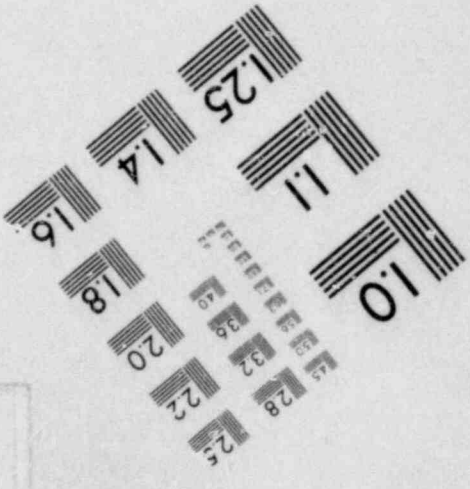

continued safe shutdown may occur, and that is the only reason for the clarification.

WITNESS HALL: Let me amplify my answer to your question, which is a good one, and I will amplify it in the sense of what the Chairman asked a while ago, that had to do with the shaking and the fault motion, in the sense that we consider both of these, which -- I am kind of broadening your question.

If the faulting, for example, were to occur concurrently with the shaking, underneath the reactor, as the Chairman postulated, there is definitely a possibility of having some local yielding, for example, in the foundation slab. But this would not be of -- I guess you would call it -- I would have to say that is damage, yes. It is damage to the structure, but it is not damage of the sort that would lead to impairment, in my estimation, of the functional capability of the system. There is a distinction I am trying to get across here.

JUDGE FERGUSON: Thank you for the clarification.

JUDGE GROSSMAN: Mr. Swanson?

MR. SWANSON: No, I have no redirect.

JUDGE GROSSMAN: Mr. Edgar?

MR. EDGAR: I have nothing more.

JUDGE GROSSMAN: Mr. Cady?

MR. CADY: No, sir.

MR. SWANSON: Maybe before the panel is excused, though, I should take the opportunity now to offer into evidence -- since there was substantial reference to it -- Pacoima Dam Response Vector, which was marked as Staff's Exhibit No. 8, and this was not on our previously identified list, but I would offer it into evidence now, as Staff's Exhibit No. 8.

JUDGE GROSSMAN: Admitted.

(The document referred to, having been previously marked for identification as Staff's Exhibit No. 8, was received into evidence.)

JUDGE GROSSMAN: I would like to thank Dr. Hall for appearing here, and excuse him from further questioning, unless something comes up, of course.

WITNESS HALL: Thank you.

(The witness was excused.)

JUDGE GROSSMAN: And Mr. Martore, we won't thank you yet, because you will be back, but...

MR. SWANSON: Mr. Chairman, maybe a housekeeping item before I let it go: I would like to offer some of the other Staff items that have been marked for identification.

I indicated previously off the record that I think the best representation of the blow-ups of G.E. Figure B-1, of their Exhibit No. 2, is the version that has the complete

notations on them, and therefore, I would offer Staff's Exhibit No. 7, which is the blow-up of Staff Exhibit No. 4, but with the completed notation.

I would also offer Staff's Exhibits Nos. 5(a) and (b). Those are the photographs of the trench at T-1, and Staff Exhibit No. 6, which will be the colored Plates 1 through 11 that were identified, I believe, yesterday, the colored versions of Figure 13, of Appendix B, of our own Exhibit No. 1(b), and we do not have sufficient copies of all those documents, at this time, but I would offer that the Board admit them into evidence, and we shall have to provide -- and ask that the record be held open, at least for these items, and I will provide the record copies to the Board.

JUDGE GROSSMAN: Mr. Edgar?

M EDGAR: No objection.

JUDGE GROSSMAN: Mr. Cady?

MR. CADY: No objection.

MR. EDGAR: And I assume we will get copies, too.

MR. SWANSON: Yes, I meant for the Board, the parties, and the reporter.

JUDGE GROSSMAN: Admitted.

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(The documents referred to, having been previously marked for identification as Staff's Exhibits Nos. 5(a)-(b), 6, and 7, were received into evidence.)

JUDGE GROSSMAN: We will adjourn until 9:00 on Monday.

(Whereupon, at 5:33 p.m., the hearing in the above-entitled matter was adjourned, to reconvene at 9:00 a.m. on Monday, June 8, 1981, in the same place.)

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This is to certify that the attached proceedings before the

US NUCLEAR REGULATORY COMMISSION

in the matter of: GENERAL ELECTRIC COMPANY (VALLECITOS NUCLEAR CENTER)

Date of Proceeding: Friday, 5 June 1981

Docket Number: 50-70 SC

Place of Proceeding: SAN FRANCISCO, CALIFORNIA

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

Ann Riley

Official Reporter

Jane N. Beach

Official Reporter

Valerie Cole

Michael Connolly

Official Reporter