UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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In the Matter of: : Docket No. 50-70
: Operating License
GENERAL ELECTRIC COMPANY : No. TR-1

[Vallecitos Nuclear Center - : (Show Cause)
General Electric Test Reactor] :

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

Monday, 8 June 1981.

The hearing in the above-entitled matter was resumed, pursuant to recess, at 9:00 a.m.

BEFORE:

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

GEORGE A. FERGUSON, Ph.D., Member

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

APPEARANCES:

DANIEL SWANSON, Esq.
RICHARD G. BACHMANN, Esq.,
Office of the Executive Legal Director
U.S. Nuclear Regulatory Commission
Washington, D.C.

Appearing for the NRC Staff.

-more-

APPEARANCES (continued):

EDWARD A. FIRESTONE, Esq.
General Electric Company
Nuclear Energy Company
175 Curtner Avenue
San Jose, California 95125

- and -

GEORGE L. EDGAR, Esq.
JAMES B. VASILE, Esq.
Morgan, Lewis & Bockius
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Washington, D. C.

Counsel for the Licensee, General Electric

GLENN CADY, Esq.
Carniato & Dodge
3708 Mt. Diablo Boulevard
Suite 300
Lafayette, California 94549

Counsel for Intervenors Friends of the Earth, et al.

* * *

I N D E X

WITNESSES:	DIRECT	CROSS	VOIR DIRE	BOARD	REDIRECT	RECROSS
Philip S. Justus				1774		
Joseph Martore				1775		
Christian Nelson				1778		
David Slemmons) Larry Wight) Don Bernreuter) William Vesely)	1797	1810		1822	1893	
Garrison Kost) Dwight Gilliland) Harold Durlofsky)	1906	1921		1967		

E X H I B I T S

EXHIBIT NO.	FOR IDENTIFICATION	IN EVIDENCE
Intervenor's No. 1	--	1896
Intervenor's No. 2	--	1897
Intervenor's No. 3	--	1898
Intervenor's No. 4	--	1903
Intervenor's No. 6	--	1898
Intervenor's No. 7	1900	
Intervenor's No. 9	1901	1901

P R O C E E D I N G S

(9:00 a.m.)

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

Before we get on with the Staff's probabilistic panel, I do have one question for the Staff; and perhaps either Mr. Swanson or Dr. Justus can clarify this for us

Whereupon,

PHILIP S. JUSTUS

resumed the stand and, having been previously duly sworn, was examined and testified further as follows:

BOARD EXAMINATION

BY JUDGE GROSSMAN:

Q We do want an authoritative position on this: In the SER of May 23rd, 1980 -- Is that May 23rd?

MR. CADY: Yes, sir.

JUDGE GROSSMAN: The front page is ripped off.

BY JUDGE GROSSMAN:

Q -- the staff has indicated on page 4 that the Staff's evaluation, or the information developed for the evaluation does not completely meet the investigative requirements of Appendix A of 10 CFR Part 100. And I would like to have an explanation of that statement.

MR. SWANSON: Yes. What we are trying to

find is a response to an interrogatory. That very question was asked and was responded to by Dr. Justus. It would probably be best to refer to that, if you would wait just a moment.

(Pause.)

Would the Board want to return to that a little later? That question was responded to in a specific interrogatory response, and that is probably the answer we would want to point to.

JUDGE GROSSMAN: Okay, that is fine. I see Mr. Martore is also with us, and I had one question for him that I think is important in the context of a probabilistic study, also, and I would like to have him respond.

Whereupon,

JOSEPH MARTORE

resumed the stand and, having beein previously duly sworn, was examined and testified further as follows:

BOARD EXAMINATION

BY JUDGE GROSSMAN:

Q There was one question that I did ask him, and I think I may have forced an answer that may not have been totally correct, and I just want to make sure about that. I am referring to my question with regard to conclusions that may or may not have been drawn with regard to an offset of greater than one meter. And I had noticed that the parameters for the study were -- one of the parameters was that maximum one meter. And I asked whether any conclusions had been drawn with regard to an offset of greater than one meter, combined with the ground shaking. And I apparently suggested that no conclusions had been drawn, and that may not be completely correct.

So I just want to get Dr. Martore to explain what the answer is on that. Could you, sir?

A. (Witness Martore) Yes, sir. As we stated previously, the design criteria did include just one meter of offset combined with the vibratory motion. So no quantitative conclusions could be drawn to an offset greater than that.

Q. Well, did the Staff ask you to draw any cor usions with regard to a situation in which the offset might be greater than one meter?

(Pause.)

A. Mr. Nelson reminded me that at the time of the September '79 SER where we indicated that 2.5 meters may be an appropriate design criteria, we did have discussions then which resulted in the letter which was written indicating that we were not aware of other similar structures that were designed to 2.5 meters of offset. So at that time, yes, I believe there was discussion as to the amount of offset that the facility could withstand.

However, I am not aware of any analyses that were done to anything greater than 2 meters or 2.5 meters.

Q Well, the question really is this -- and I will give you alternatives. It seems to me as though you either did not consider the possibility of greater than one meter; or, you did consider the possibility and decided that you could not endorse greater than one meter of displacement, and therefore came to a negative conclusion without going into a full quantitative analysis.

I am trying to keep from making suggestions here, but unfortunately I did in a way suggest the answer last week, and I want to have your position on it and not my position. So please respond to that statement.

(Pause.)

MR. SWANSON: The Project Manager for the facility, Mr. Christian Nelson, wanted to respond to that question.

MR. NELSON: Sir, at the time of issuing the September 1979 --

THE REPORTER: The witness has not been sworn.

JUDGE GROSSMAN: Okay, he's not sworn? I take it we ought to have an authoritative statement on that, so would you stand, Mr. Nelson?

Whereupon,

CHRISTIAN NELSON

was called as a witness and, having been first duly sworn, was examined and testified as follows:

BOARD EXAMINATION

BY JUDGE GROSSMAN:

Q. Please be seated.

A. (Witness Nelson) At the time of issuing the September 1970 SER input, we had come to a position of 2.5 meters as a likely design basis for surface offset. And in our cover letter transmitting that SER, we expressed an opinion on the engineering design in that we were not aware of facilities designed for that amount of offset, and would not expect it to be, I guess, the design borne out based on analysis. That was our only conclusion or opinion expressed on that magnitude of offset, the 2.5 meters.

Our subsequent SERs addressed only -- from a quantitative engineering design analysis standpoint -- an offset of one meter.

Q. Well, Mr. Martore?

A. (Witness Martore) I am trying to understand the question so I can answer it more clearly. Maybe I can give some background and it might help the Board.

At the time the geological investigations were being undertaken, the structural review was continuing based on a one-meter offset. So we had quantitative analyses to review.

That review was winding down, or reaching its conclusion at the time the geological review indicated that something greater than one meter may be a necessary design criteria.

So it is difficult to say that we considered something greater than one meter. However, based on our knowledge of structures and the way they behave, we did indicate at other meetings -- specifically, the ACRS -- that perhaps the facility could take something greater than one meter. The amount greater than that, we weren't aware -- we weren't sure. We couldn't say quantitatively.

I don't know if that helps the Board or not.

Q. Well, I don't know whether it helps either, but it would seem to me that it's possible in the context of what's already been presented that we might conclude that there would be more than one meter displacement. Now if that is the case, what is the Staff position?

A. The Staff position would be that we would then define the surface offset as would be appropriate, and I would assume that GE would make a decision as to whether they would pursue reanalysis, and the Staff would review then the adequacy of the structure to the new design criteria, which may be higher.

Q. Well, I have a little trouble with that. I understand that GE has analyzed the greater than one meter displacement for the purpose of the cantilever effect, but do I understand now that it has not gone beyond that with regard to a one meter offset? That is the only situation that it has considered?

Could you speak to that, Mr. Edgar?

MR. EDGAR: It is my understanding, and I will check this with Dr. Kost -- and he will be here and can address this directly -- but it is my understanding that the deflection analysis has been accomplished such that a 2.5 meter offset would not affect those conclusions. And the implication of that is that the cantilever loading cases are not a matter of great concern.

The question, then, of the soil pressure cases under 2.5 meters of offset, it is my understanding that there has been no detailed analysis of that particular case. Okay? The loading conditions that would go with 2.5 meters have not been analyzed in detail. But I believe Dr. Kost has looked at the case and has some opinions on the subject. Whether they are detailed, quantitative conclusions I can't tell you at this time.

JUDGE GROSSMAN: Mr. Swanson, I take it then that it is the Staff's position that it would not endorse a resumption of operations unless the Board determines that the one meter offset is the maximum offset? And that it is the appropriate offset to consider for all purposes other than the cantilever effect?

MR. SWANSON: I have to make an assumption in your statement of one meter? You mean, one meter that the building actually experiences, if you just disregard Dr. Pichumani's testimony then?

JUDGE GROSSMAN: Yes. Disregarding Dr. Pichumani's --

MR. SWANSON: Because of course his testimony could accommodate a larger than one meter offset.

JUDGE GROSSMAN: Yes, but I am relegating that to the cantilever effect, which I believe you have limited his testimony to; that it really doesn't have any applicability to the combination of shaking and displacement.

MR. SWANSON: Okay. If we take as a given that the actual physical structure will take one meter, one meter of offset actually intersecting the structure, that would be the maximum amount of offset that the Staff would be able to conclude for licensing purposes at this time is appropriate for restart.

There are I think some subtlties involved in the interaction between the structure and soil conditions that perhaps has not been fully brought out yet, and will be I think in our structural panel. But if we are to take the simplified approach as postulated, one meter actually causing a cantilever effect on the building, that is the answer.

JUDGE GROSSMAN: Okay. And you are not going so far as to say that you considered greater than one meter, and have come to a negative conclusion because your position is that you really haven't considered it, and if the Board were to adopt a greater than one meter, you could go back and determine quantitatively whether under a greater-than-one-meter offset the structure might nevertheless fit the requirements? Is that correct?

MR. SWANSON: I think that's a fair statement. It is not a negative conclusion. It is merely a limit to the amount of review that has gone on thus far by the

Staff.

(Board conferring.)

JUDGE GROSSMAN: Okay. Dr. Foreman just indicated that he wants the Board to qualify the statement to indicate not that it could withstand it at the present time, but it could be modified to withstand it. So take my statement with that modification, please.

Okay. I think that now Dr. Justus is prepared to -- unless, Mr. Edgar, did you have something additional?

MR. EDGAR: May I confer a moment?

JUDGE GROSSMAN: Sure.

(Counsel conferring.)

MR. EDGAR: We have nothing to add.

JUDGE GROSSMAN: Mr. Cady, did you have anything to add?

MR. CADY: No, sir.

JUDGE GROSSMAN: Okay, Dr. Justus, could we have your statement now on Appendix A, Part 100?

WITNESS JUSTUS: Yes. We have determined that in specific -- the specific points of Appendix A that were not required for this investigation in great detail are as follows: The reference is Appendix A to Part 100 of 10 CFR. That is, IV, paragraph A, subparagraph (6); IV, paragraph A, subparagraph (7); and in the same section IV, paragraph B(7); V, paragraph B, which is an introductory paragraph; and then B(1). A small point in VI, paragraph B(3), and I will summarize these.

JUDGE GROSSMAN: Please.

WITNESS JUSTUS: The Section IV is entitled "Required Investigations," and subparagraphs A(6) and (7) refer to the -- well, (6) in particular refers to the correlation of epicenters within a 200-mile radius of the site. We didn't require that amount of detail on the seismic investigation.

In paragraph A(7), Section IV, there is a requirement for mapping faults within a 200-mile radius of the site. This is a major requirement which has so far been applied only to nuclear power plants.

I am reminded that I did omit reference to paragraph A(8) in Section IV regarding mapping capable faults within 200 miles of the site. That was also not a requirement. And I will summarize the reasons for this shortly.

In B(7), there is a requirement for mapping capable faults greater than 1000 feet long within 5 miles of the site and determining various relationships for them.

And in Section V, the pertinent paragraphs require mapping traces which trend 10 miles in both directions from the fault approach closest to the site. This would have been perhaps a requirement to map the Calaveras 10 miles up and down the strike from the GETR.

These detailed requirements we felt were not enforced in detail because early in the mapping of the site, the principal faults and the principal earthquakes that govern the magnitude determinations were rendered early. That is to say, the Calaveras fault was recognized as the main earthquake-producing fault. We felt no need to map it in great detail up and down the strike.

Similarly, for the Verona surface offset on the Verona was the principal hazard that we had determined for the surface faulting aspect. I should -- and we felt that due to trenching, sufficient detail of the extent of the Verona and the surface offset hazard was documented.

Also, these Appendix A guidelines were meant to be applied principally to sites that are much less well known geologically. They are guides to uncovering or discovering a great amount of detail concerning fault movement and earthquake potential for a region.

And in this particular area around the GETR, such information was effectively already known.

JUDGE GROSSMAN: Was there any requirement with regard to -- was there any requirement or is there in the Appendix A with regard to applying characteristics of the Calaveras fault to the area that was not followed? Or was it merely a question of investigating the Calaveras? Doyou understand my question?

WITNESS JUSTUS: I don't think so.

JUDGE GROSSMAN: You don't think that there was any?

WITNESS JUSTUS: No, I don't think I understand your question.

JUDGE GROSSMAN: Oh, I see.

WITNESS JUSTUS: Oh, yes. If you are referring to the control width of the Calaveras, perhaps, and that would be applying Table 2 in -- that is in Section IV -- sorry, Section V, B(1) -- we felt at the time, and still do, that the faults under consideration, the Calaveras and the Verona, were being investigated in sufficient detail to establish the hazard.

JUDGE GROSSMAN: Well, my question isn't with regard to an investigation. I think you have explained that. But whether there is anything in Appendix A that would require applying characteristics of the Calaveras Fault to the GETR site, in addition to applying the characteristics of the Verona Fault that you may not have applied in Appendix A. In other words, the Calaveras Fault is assumed to have a greater potential for displacement and magnitude and acceleration values, I believe, from what I have heard. And the question is whether there is anything in Appendix A that requires applying those characteristics to the GETR site, rather than the characteristics, or in addition to the characteristics of the Verona Fault that you may not have followed under Appendix A?

WITNESS JUSTICE: Well, if I understand your question correctly, I would need to answer it this way: If GETR were a power reactor and it was recognized that the Calaveras Fault were within -- or approximately 2 miles, or 3 kilometers from the site, it would be important in the application of Appendix A to determine the width of the Calaveras Fault Zone, and to consider whether, if future movement on the Calaveras occurred, whether perhaps surface offsets might occur within the design width. And in that respect, or in that specific application was not made.

JUDGE GROSSMAN: Mr. Edgar?

MR. EDGAR: I am a little bit unclear on the question; but as I understand it, the question is: Is there anything in Appendix A that requires the Calaveras to be applied to the site? Well, obviously the Calaveras was applied in terms of vibratory ground motion. I mean, there is no question but that this facility has to take the event from Calaveras.

The next question is surface offset. And as I read Appendix A, what it says is that if you are within the control width, you've got to do an investigation of faulting. In fact, an investigation for faulting was done at the GETR site. And what yo see at the GETR site is that surface faulting that has occurred.

So I would answer the question in terms of Appendix A, that Appendix A has certainly been satisfied as to the control width requirement because all that requires is that you do an investigation on faulting -- you know, on offsets -- and that was done.

The difference here is that we know where the facility is, and on a new site you don't necessarily know that. So if you dig around the site, you are going to get whatever movement can be attributed to whatever is there.

end JWB-1

A (Witness Justus) Well, now that I have had a chance to think a bit more about this particular requirement, we saw no evidence between the Calaveras and the Verona to consider that the Calaveras zone of influence, other than ground shaking, was present at the GETR site.

That is to say, we didn't see evidence for strike slip faults such as the Calaveras, nor any geomorphic evidence that would suggest that what otherwise would be a required investigation of three times the width of the zone would be justified, to that extent in Appendix A.

In the region of the GETR, the Calaveras, obviously an important fault zone, seems well-defined geomorphically, as was already pointed out during the hearing. There are very prominent liniaments or scarps. The Calaveras occurs in a fault valley or rift, as it was called before, and there just didn't seem any indication that that zone would overlap the GETR site.

Q If you had to determine that the Verona and Calaveras were structurally related, would you have to project the characteristics of the Calaveras to the Verona Fault? Is that what you're saying now, in that you determined that they are not structurally related?

A Let me refer back to the definition of structurally related which came up before.

Q Well, I understand it's a definition that says "accompanied by," and that's a problem, I take it. That's another problem that we'll have to cope with, but my question is whether you made the determination that they weren't, and if you have made the determination that they were structurally related, would you have to project the characteristics of the Calaveras Fault to the Verona Fault?

A Well, I'd have to ask you to -- well, we did -- that's a complicated question in this, I think. Effectively we did determine that there was no relationship of characteristics. Now if we determined that there were -- the Calaveras and the Verona were structurally related, would we have to superimpose the characteristics of one on another? Geologically we couldn't do that. What characteristics did you have in mind that we might relate?

Q Well, the characteristics, as I understand, of the Calaveras Fault is for greater magnitude earthquake, for greater accelerations, for greater displacements.

A I see.

Let me continue, then. If we had no reason to assume -- and I'm speculating on your question now -- that we would give equal weight of the characteristics of one to the other if they were structurally related, equal weight regarding say the capability for generating ground motion of Calaveras type, say, along the Verona, there is no geologic evidence for that.

Q Oh, I'm not suggesting that there was, and I think the testimony indicated there probably isn't. The question is whether the requirements of Appendix A are such that you would have to project those characteristics from the Calaveras to the Verona.

A No. The Appendix A is not a requirement or does not require such a transference of information, by any means. Appendix A is the guide on how to approach such an investigation that may actually suggest that a requirement of that type be made, but it does not automatically make such a requirement.

Q Okay. Now I take it to the extent that you didn't follow Appendix A, it was because you weren't required to, because this is not a power plant; is that correct?

A That's also a complicated question with a history, I think.--

(Staff conferring.)

I think as to why we were not required to follow Appendix A as if this were a power plant, I think Chris Nelson could address that point. Then I can get back to other aspects of it.

Q Okay.

A (Witness Nelson) As I understand your question, it was -- it includes what Dr. Justus has already explained as to why we felt these investigations were not necessary, but we were also not bound to make these additional investigations based on the definitions contained in Appendix A to Part 100, which if I can summarize this GE test reactor -- I can't locate the definition I'm looking for, but essentially it implies that the power reactor is designed for producing electricity or for thermal heat output, or required to be -- these requirements apply to those types of reactors, which does not include the GE test reactor.

Q So basically what you are saying is the legal basis for not applying Appendix A was the fact that this was not a reactor that fits the category in which Appendix A must be applied; but that you also want to take note of Dr. Justus' indications as to why on a geologic basis you didn't apply Appendix A also?

A Yes, sir. The guidelines of Appendix A were not ignored in any of these areas, and I think that's what Dr. Justus pointed out.

MR. CADY: Excuse me, your Honor. I would like for them to take a look at Part 100.2(a) and ask them to explain why testing reactors is referred to in that section and why they don't want, or why they don't feel that the full compliance with Appendix A is required.

MR. EDGAR: I have a lot of problems. This is a complicated subject, I believe. I have looked at it at great length, and researched it on both definitions. It will take two hours to argue this point. We can put it in our briefs very effectively.

As I understand it, Dr. Justus says that for geologic reasons or substantive reasons, there were certain provisions that he didn't feel were necessary. Am I correct? Is that what I'm understanding?

WITNESS JUSTUS: Well, not exactly that they weren't necessary; that we felt that the site was sufficiently well documented --

MR. EDGAR: Okay.

WITNESS JUSTUS: -- that these guidelines need not have been applied in the sense that the area was an unknown -- was unknown geologically or seismically, which is the intent of Appendix A, the guidelines for such an investigation in areas that are not known or not as relatively well known.

JUDGE GROSSMAN: Mr. Swanson?

MR. SWANSON: I think what Mr. Cady pointed out is one of the complexities of Part 100. Indeed, it does say at 100.2 that this part applies to applications filed under Part 50.

If you go to Appendix A, however, under "Scope," it clearly indicates these criteria which apply to nuclear power plants. Now, unfortunately, Appendix A does not have its own definition of power plants, but Part 100 does. 100.3(d) defines power reactor, and it does include the phrase that Mr. Nelson was referring to, a nuclear reactor of the type described under 50.21(b) or 50.22 of this chapter, and then the key phrase, designed to produce electric or heat energy. And under the meaning of that definition, the GETR would not be one which is designed to do either of those things.

JUDGE GROSSMAN: I'm not trying to get a legal argument here as to whether or not Appendix A applies. What I do want to get is the authoritative statement as to what the Staff considered when it either applied or did not apply Appendix A, and whether applying Appendix A would result in different conclusions or a different application of the characteristics there.

In other words, whether any of the characteristics of the Calaveras would be projected to the Verona, and I believe the answer has been no, but somewhat qualified.

BY JUDGE GROSSMAN:

Q Dr. Justus?

A (Witness Justus) Well, I think that's essentially correct, though Appendix A requires the application of judgment, and as new findings are made throughout an investigation, certain parts of Appendix A guidelines come into play more heavily, although initially perhaps they were thought to be ignored, it was not so. But it is our judgment that certain sections with detailed sections of Appendix A need not have been applied to this particular site because of our level of understanding of the geology and seismology of the site.

Q Well, let me ask you one final question, then, which I hope is final.

In the absence of making the investigations and coming up with specific conclusions, would some of the guidelines have required applying the characteristics of the Calaveras Fault to the Verona Fault?

Do you follow my question?

A I don't think so.

Q Oh, okay.

A Again, I don't think I follow your question. I'm sorry.

Q Okay, let me give you an example. If you had not investigated the Verona Fault and determined that there were certain characteristics of the Verona Fault, such as maximum earthquake magnitude, maximum ground shaking, et cetera, and all you had was what was on the Calaveras Fault, the characteristics that you determined there, would you have been required to project the characteristics of the Calaveras Fault to the Verona under those circumstances? Under Appendix A.

A Excuse me for one moment.

(Staff conferring.)

A (Witness Justus) Specifically, no. There is -- we would not be required to do that. We have found no evidence to even suggest that we apply Calaveras characteristic to the Verona.

Q Okay. That answers it sufficiently. If, on rereading some of what was said here, you determine that some of it was inaccurate, I would hope that before the end of the hearing, you would indicate to the Board what it is, but I just wanted to get the Staff position on that.

A Thank you.

JUDGE GROSSMAN: Okay, I think we are ready to proceed with the probabilistic panel.

MR. SWANSON: At this time I would ask the Board to call Mr. Larry Wight, Mr. Don Bernreuter, Dr. William Vesely and Dr. David Slemmons to the stand. And, of course, Dr. Slemmons has been previously sworn. I would ask that the other gentlemen be sworn at this time.

Whereupon,

DAVID B. SLEMMONS

was recalled as a witness on behalf of the Staff and, having been previously duly sworn was examined and testified further as follows; and

LARRY WIGHT,

DON BERNREUTER and

WILLIAM VESELY

were called as witnesses on behalf of the Staff and, having been first duly sworn, were examined and testified as follows:

DIRECT EXAMINATION

XXXXXX

BY MR. SWANSON:

Q In addition to the Safety Evaluation, there are two separate pieces of testimony which were prefiled, one by William Vesely and one by Don Bernretuer, and before I bring them into evidence, I will ask if there are any additions or corrections to those pieces of testimony.

A (Witness Vesely) Yes. I would like to correct the testimony for William Vesely. I am Acting Chief of the Methodology and Data Branch, instead of the Meteorology and Data Branch.

JUDGE GROSSMAN: Gentlemen, could you first indicate your full names, in order of Dr. Vesely?

WITNESS VESELY: Yes, that's correct, Dr.

William E. Vesely. Acting Branch Chief, Methodology and Data Branch, U.S. NRC.

JUDGE GROSSMAN: And Mr. Slemmons, we have already gotten your full identification.

WITNESS WIGHT: My name is Lawrence H. Wight, and I am with TERA Corporation.

WITNESS BERNREUTER: My name is Don L. Bernreuter, and I am with the Lawrence Livermore National Laboratory, and I would like to make two slight corrections to my prepared testimony also.

The first is that I'm the leader of the Engineering Sciences Group, rather than as it has here, the leader of Engineering Geosciences for the Lawrence Livermore National Laboratory. I'm just a group leader.

And down a little bit below that, I had studied under a National Sciences Faculty Fellowship in engineering mechanics at Stanford University, not the Douglas Airplane Company.

BY MR. SWANSON:

Q You're referring to page 1 of your prefiled testimony?

A (Witness Bernreuter) Page 1 of my testimony. My attached qualifications on the back are correct.

Q And you're referring first to correction to the answer to A.1, add the word "group director"?

A Leader of the Engineering Sciences Group, that's correct, A.1.

And then at A.2, it should say "and studied under a National Sciences Faculty Fellowship in engineering mechanics at Stanford University," rather than Douglas Airplane Company.

Q And, Dr. Vesely, your correction was, I assume, to the answer to Question 1 on the first page of your testimony, change the word "meteorology" to "methodology"?

A (Witness Vesely) That is correct.

Q So that it conforms with the statement of your qualifications attached to the written testimony?

A That's right, yes.

Q Are there any other additions or corrections?

A (Witness Wight) While I did not have prefiled testimony, we did prepare a report that was attached to the SER, within which there are a few typographical errors, and for the completeness of the record, I have prepared an errata sheet that I could make available. That report was entitled "Seismic Rupture Hazard at the General Electric Test Reactor: A Review and Analysis," and was dated May 1st, 1980.

Q That is the report attached to the cover letter of May 8, 1980, from Mr. Bernreuter to Mr. Eisenhut, which is Appendix F to the Staff's May 23rd, 1980 Safety

Evaluation; is that correct?

A That's correct.

MR. SWANSON: What I would propose to do, rather than go through the corrections, we do have an errata sheet of typos which we will hand out.

BY MR. SWANSON:

Q Mr. Wight, let me get a clarification. The errata sheets that you just passed out, do they change any of the numbers which were used on the analysis, or the conclusions?

A (Witness Wight) No, definitely not. They are completely typographical.

Q Thank you.

MR. SWANSON: Mr. Chairman, the parties have previously agreed there would be no objection to the introduction of the prefiled written testimony. I would now ask, however, that the testimony as corrected and that we include with that the errata sheets for the TERA review which was included as Appendix F to Staff Exhibit 1-B, also be received in evidence and bound into the transcript as though read.

JUDGE GROSSMAN: Mr. Edgar?

MR. EDGAR: No objection.

JUDGE GROSSMAN: Mr. Cady?

MR. CADY: None.

JUDGE GROSSMAN: Admitted.

(The documents follow:)

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

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

NRC STAFF TESTIMONY OF WILLIAM E. VESELY

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

A.1. My name is William E. Vesely. I am Acting Chief, Meteorology and Data Branch, Division of Systems and Reliability Research, U.S. Nuclear Regulatory Commission, Washington, DC 20555.

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

A.2. I graduated from Case Institute of Technology with a B.S. in Physics. I received an M.S. and Ph.D. in Nuclear Engineering from the University of Illinois. I have been previously employed as a Senior Technical Analyst and Statistical Group Leader for Aerojet Nuclear Company, as a Senior Scientist for JRB Associates, Inc. and as Section Leader and Special Assistant for Methodology Develoment for the Nuclear Regulatory Commission. A copy of my qualifications is attached to this testimony.

Q.3. Describe the scope of our participation in the review of the GETR for this proceeding.

A.3. I reviewed the probability analyses and models developed by GE's consultant, Jack Benjamin and Associates (JBA), and Lawrence Livermore Laboratory (LLL) and its consultants, TERA, which were prepared for use in predicting the probability of surface rupture at the GETR. As part of this review, I specifically evaluated the various sensitivity studies that were performed by GE and myself and the critiques that were made to determine those credible results that could be obtained from the probabilistic modelling.

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

A.4. Based on my review, I concluded that the probability models could be used to predict gross probabilities of surface rupture. I also concluded that upper bounds on the probability of surface rupture could be obtained which accounted for various data and modelling uncertainties. The results of my review are contained in Section 8 of the Staff's May 23, 1980 SER.

The probabilistic analyses presented in the JBA reports are methodologically sound. The TERA model presents an alternative probabilistic model which is not as empirical and is more traditional; the TERA model does require more data and more assumptions to be made on rupture parameter relationships. As pointed out in the reviews, available data are sparse requiring sensitivity studies to be performed to gain any confidence in the rupture offset probabilities which are estimated. A wide range of sensitivity studies on variation of parameters were performed for the JBA probabilistic models, which included a variety of sensitivity evaluations performed in the reviews of the models. The TERA model extends the parametric sensitivity analyses by developing a different alternate probabilistic model to compate with the JBA models.

Based on the sensitivity analyses and the alternative model, the probability of a surface rupture offset occurring beneath the reactor building has been shown to lie between 1 x 10^{-6} per year and 1 x 10^{-5} per year (to order of magnitude precision), with 1 x 10^{-4} per year being a conservative upper bound. The probability results for the GETR are credible and should be used to supplement the deterministic evaluations in making a final decision.

PROFESSIONAL QUALIFICATIONS
OF
DR. WILLIAM E. VESELY

Acting Chief, Methodology and Data Branch, Division of Systems and Reliability Research (PAS), U.S. Nuclear Regulatory Commission, Washington, DC 20555.

Responsibilities
Personally responsible for the planning, initiation, and direction of research programs for the U.S. Nuclear Regulatory Commission in the fields of risk analyses, reliability analyses, data analyses, and statistical analyses. Performs risk assessments, analyzes risk implications of data collected at power plants, and develops new techniques for risk and reliability assessments. Directs and coordinates activities of the members of the Methodology Section. Manages contracts issued by the Methodology Section involving several million dollars; directs and coordinates activities of the approximately 50 technical individuals engaged in the contract work. Presents research programs and risk evaluations to congressional committees, governmental agencies and other bodies as required. Serves as a representative of the Commission in international activities involving risk analyses and reliability analyses. Serves as a Commission consultant on risk and reliability matters.

Employment History

Period: March 1974 - September 1980

Organization: U.S. Nuclear Regulatory Commission
Washington, DC 20555

Title: Section Leader and Special Assistant for Methodology Development
Probabilistic Analysis Staff

Period: February 1973 - March 1974

Organization: JRB Associates, Inc.
1600 Anderson Road
McLean, Virginia

Title: Senior Scientist

Responsibilities:
Initiated projects and conducted analyses in the areas of reactor physics, statistical analyses, and risk analyses. Directed individuals involved in the projects. Recommended technical areas for company involvement. Served as consultant for the company on reliability and risk matters.

Period: July 1968 - February 1973

Organization: Aerojet Nuclear Company
P.O. Box 1845
Idaho Falls, Idaho

Title: . Senior Technical Analyst and Statistical Group Leader

Responsibilities:
Developed techniques and computer codes for reactor physics analyses, reliability analyses and statistical analyses. Performed reliability analyses on nuclear systems. Developed theoretical and computer models for fluid flow and heat transport. Managed the statistical group consisting of approximately ten technical members. Served as company consultant for reliability problems.

PRESENT COMMITTEE MEMBERSHIPS

IEEE Committee on Reliability

IEEE Nuclear Systems Reliability and Safety Committee

Centralized Reliability Data Organization Steering Committee, DOE

International Task Force on the Risk Evaluation of Rare Events in Nuclear Power Plants, OECD-CSNI

International Working Group on Common Mode Failure Analysts, OECD-CSNI

International Working Group on Human Error Analysis, OECD-CSNI

Research Review Group on Probability and Statistics for Risk Evaluations (Chairman)

Research Review Group on Risk Evaluations of Limiting Conditions for Reactor Operations (Chairman)

Seismic Safety Margins Research Review Group

Research Review Group on Flooding Analyses for Nuclear Power Plants

Research Review Group on Human Error Modeling in Risk Analyses

Research Review Group on Rish Assessments of Light Water Reactors

Research Review Group on Risk Assessments of the Nuclear Fuel Cycle

PRESENT UNIVERSITY ASSOCIATED ACTIVITIES

Research Affiliate and Thesis Coordinator, Massachusetts Institute of Technology

Thesis Committee Member, Rensselaer Polytechnic Institute

Lecturer, Reliability and Risk Analyses, George Washington University

Lecturer, Reliability and Systems Analyses, University of Washington, Seattle

Lecturer, Navy Safety School, University of Indiana

Lecturer, Reactor Safety School, Massachusetts Institute of Technology

PRESENT SOCIETY MEMBERSHIPS

Americal Statistical Association

Tau Beta Pi (Honorary)

Sigma Xi (Honorary)

Phi Kappa Phi (Honorary)

Reviewer, IEEE Transactions on Reliability

Reviewer, Nuclear Science and Engineering (ANS)

EDUCATION

case Institute of Technology, BS Physics 1974
(Timken Scholarship, Graduated Summa Cum Laude)

University of Illinois, MS Nuclear Engineering 1966,
PHD Nuclear Engineering 1968 (AEC Fellowship, 4.0 average)

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

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

NRC STAFF TESTIMONY OF DON L. BERNREUTER

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

A.1. My name is Don L. Bernreuter, and I am the leader of engineering geosciences for the Lawrence Livermore National Laboratory (LLL).

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

A.2. I received my B.A.E. and my M.S. in aeronautical engineering from the Georgia Institute of Technology in 1958 and 1959, respectively. I also engaged in post graduate studies in mechanical engineering and mathematics at North Carolina State University from 1960-62, and studied under a NSF Science Facility Fellowship in engineering mechanics at the Douglas Airplane Co., as an assistant professor of mechanical engineering and of engineering mechanics at Louisiana State University, and as an engineer and geoscientist at LLL since 1973, including a 2 year assignment with the NRC Staff with the Site Analysis Branch, what is now the Geosciences Branch, as a staff seismologist. Since 1968, I have been involved in the study of strong ground motion from explosions and earthquakes. For the last 5 years

I have been extensively involved in the probability assessment of the safety of nuclear power plants and other critical facilities, and have led a number of projects for the NRC.

Q.3. Describe the scope of your participation in the review of the GETR for this proceeding.

A.3. I directed the LLL review effort, which utilized the services of the TERA Corporation. The TERA review was led by Lawrence Wight. The LLL/TERA review included a review and evaluation of GE's submittals to the NRC prepared by EDAC and JBA regarding the probability of surface rupture beneath the GETR, and also prepared an independent assessment of the probability of such rupture. I personally directed and integrated the overall effort, reviewed and evaluated both the GE consultants' submittals and the independent TERA analysis, and made the recommendations to the NRC which appear in Appendix F of the Staff's May 23, 1980 SER. I concluded that the probability of faulting beneath the GETR is very low, and the use of a mean plus one standard deviation value of one meter for net offset beneath the facility can be considered conservative.

TERA's analysis, which I agree with, shows that on the main Verona fault zone, the probability of the occurrence of a one-meter offset is about 5×10^{-5} per year, which is reduced by a factor of about 6×10^{-2} per year for an estimate that this offset will occur beneath the reactor. This probability is further reduced by a factor which accounts for the degree of belief that in 128,000 years, no observable surface rupture has occurred in the trench between the shears on either side of the reactor.

Reasonable changes in the magnitude of the maximum credible earthquake factored into our analysis (i.e. ±.5 M) and the strain rate (±30%) introduces a factor of only 2 to 3 change in the probability value.

Don L. Bernreuter
Lawrence Livermore National Laboratory
Leader, Engineering Geosciences

1975 - Present - Since returning from a two year assignment at NRC, I have played a key role on a number of NRC projects. I am currently directing several major programs for NRC. I am Project Manager for Project II - Seismic Input - for the Seismic Safety Margins Research Program. In addition, I am directing the effort defining the seismic hazard for all commercial plutonium facilities for NRC and for similar facilities for DOE, as well as directing the project defining the seismic hazard for the nuclear power plants under review as part of NRC's Systematic Evaluation Program. I am also the U.S. representative to the CSNI Expert Group on Reference Ground Motions.

In the past I directed a project for NRC/OSD Assessing the Current NRC Seismic Methodology. I also developed a report which provides NRC a better technical basis to develop the design spectra for the Diablo Canyon site.

1973 - 1975 - On the staff of the Site Analysis Branch, Division of Technical Review, U.S. Nuclear Regulatory Commission. At NRC responsible for evaluating both the seismic design basis and the foundation engineering aspects of several proposed sites. Played a major role in the writing of the Standard Review Plan (Sections 2.5.1, 2.5.2, 2.5.4, and 2.5.5) for the Site Analysis Branch and coordinated the overlapping responsibilities of the Site Analysis Branch and the Structural Engineering Branch. Performed several special geotechnical studies, e.g., a study of the subsidence potential at the proposed site of the Allens Creek Nuclear Station due to groundwater withdrawal.

1968 - 1973 - At LLL responsible for providing estimates of proposed underground nuclear explosion seismic ground shock design parameters (both surface and subsurface) for monitoring hardware. Developed and improved various computer programs to study the soil-structure-interaction between underground structures and ground motion. Led the initial efforts to establish the SSE for the LLL site and the earthquake hazard posed to several complex underground nuclear test programs. Involved in several studies of the structural integrity of various structures to seismic motion and the seismic isolation of sensitive equipment.

1960 - 1966 - Assistant Professor of Mechanical Engineering and of Engineering Mechanics at Louisiana State University. Taught a wide variety of courses, both graduate and undergraduate, in fluid mechanics, gas dynamics, solid mechanics, dynamics, and mathematics. Instructor in both the Mechanical Engineering and Engineering Mechanics Departments at North Carolina State University. Taught machine design, dynamics of machinery, fluid mechanics, dynamics and strength of materials.

1959 - 1960 - Flight Test Engineer at the Douglas Airplane Company. Compiled statistical analysis of low altitude free-air turbulence.

NSF Science Facility Fellowship	Stanford University	Engineering Mechanics	1966-
Post Graduate Studies	North Carolina State University	Mechanical Engineering/ Mathematics	1960-
MS	Georgia Institute of Technology	Aeronautical Engineering	1959
BAE	Georgia Institute of Technology	Aeronautical Engineering	1958

Errata Sheet for the TERA Report
"Seismic Rupture Hazard at the General Electric Test Reactor: A Review and Analysis"
(May 1, 1980)

Page	Location	Expression in Report	Corrected Expression
2-17	First line after eqn. (2-1)	where _ is	where μ is
	Third line after eqn. (2-1)	_AD	μAD
2-18	First line	dN(m)	$\lvert dN(m) \rvert$
2-20	Fourth row of table, "Symbol" column	M	M_ℓ
	Eighth " " " "	_	μ
3-3	Eqn. (3-1)	t_0; n integer _0	t>0; n integer>0
	Definition of $P_N(n/\lambda)$	$P_N(n/\_)$	$P_N(n/\lambda)$
	" " "	given _	given λ
3-4	Second line	parameter _	parameter λ
	" " , second paragraph	variable _	variable λ
3-7	First complete line	$m_i \pm \_m/2$	$m_i \pm \Delta m/2$
3-9	Second summation sign	$\sum_{n=0}$	$\sum_{n=0}^{\infty}$
3-13	Eqn. (3-13)	$\frac{2.34}{2\pi f_o}$	$\frac{2.34\beta}{2\pi f_o}$
3-16	Second paragraph, sixth line	$L_j/2>X_i$	$L_j/2>x_i$
3-17	First and second lines	E_r	E_s
	Final line of eqn. (3-19)	$LnL_j - 2x_i$	$Ln2x_i - LnL_j$
	Two lines below eqn. (3-20)	of$_{_D}$	of σ_D
	Final line of eqn. (3-21)	$LnD_j - Lnd$	$Lnd - LnD_j$

TERA CORPORATION

Page	Location	Expression in Report	Corrected Expression
3-18	First equation	$LnD_j - Lnd$	$Lnd - LnD_j$
	" "	$LnL_j - Ln2x_i$	$Ln2x_i - LnL_j$
	" "	$LnR_j - LnW_i$	$LnW_i - LnR_j$
3-25	Column titles	Offset _ 1m	Offset > 1m
	" "	Offset _ 2.5m	Offset > 2.5m
3-26	Column titles	Offset _ 1m	Offset > 1m
	" "	Offset _ 2.5m	Offset > 2.5m

Appendix C Errata

12	Second equation	$\Gamma^2(1 + 2/\kappa)$	$\Gamma^2(1 + 1/\kappa)$
14	Last equation	$t_s — t_o$	$t_s - t_o$

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15 The second expression for P_{ON} should be omitted.
The $\sqrt{2\pi}$ in eqn. (1) should be omitted.

17 The $P_{ON} \times 10^5$ column in Table 5 should read as follows:

$P_{ON} \times 10^5$
7.95
32.1
1.96
15.92
3.96
1.98
31.8

BY MR. SWANSON:

Q At this time I would then ask the members of the panel to briefly summarize the probability analyses conducted by them, and I think I would ask Mr. Bernreuter to lead off.

A (Witness Bernreuter) The NRC -- I should say Lawrence Livermore National Laboratory does some consulting for the Nuclear Regulatory Commission, and as part of that general consulting agreement with the Nuclear Regulatory Commission, they requested Lawrence Livermore Laboratory to assist them in the review of the EDAC and Jack Benjamin probability studies for offset beneath the GETR reactor, and they asked us to do two things:

One was to review and criticize the documents; and secondly, to attempt to make some sort of independent analysis. And in my capacity as the leader of the Engineering Sciences Group, I engaged several Staff statisticians to help me in the review of the document, in one of the letters attached by Dr. Mensing, and also I asked Dr. Mensing to look over it and see if he could come up with different approaches to do probabilistic analysis. And we also at the same time had a consulting group, TERA Corporation, developing a probabilistic rupture model of the path hazard analysis, which seemed very appropriate to apply to this site, and we requested that TERA Corporation apply that model to this site, and I'll let Mr. Wight just briefly outline that model.

My principle then was to overview very carefully the analysis that TERA made, interface with Dr. Mensing in all the analyses that he and his staff would be carrying over very carefully, and just simply carefully review all these different analyses, integrate them from the point of view of geology and seismology and engineering.

I did not personally conduct an independent probabilistic analysis myself, but depended on Dr. Mensing and his staff and TERA Corporation and Mr. Wight's staff to carry out the detailed probabilistic analysis. Mine was simply review, integration, and then making recommendations to NRC.

Q Mr. Wight?

A (Witness Wight) Thank you.

Yes, as Mr. Bernreuter has said, we have performed probabilistic analysis designed to calculate the likelihood of offsets underneath the GE test reactor. Our model is quite a bit different from the one you have heard about previously in this hearing, and I would like to very briefly describe it to you.

It has basically four separate steps. It amounts to multiplying four separate conditional probabilities. The first conditional probability is the likelihood of an earthquake of a given size occurring on a postulated Verona Fault. We calculate this probability, not by relying on the historical seismicity data, which in itself does provide an indication of that occurrence relationship, but instead relying on a slip rate. There is a well established slip rate for inferring the occurrence of earthquakes on a fault from the slip rate. Rather than use the slip rate from trenches B-1, B-3 and B-2, we independently calculated the slip rate, using the topographic expression between the Vallecitos Hills and the valley within which the test reactor sits.

We used the information in the trenches as an independent qualitative check on our results. That was the first probability.

The second probability, conditional probability, was given the occurrence of an earthquake, what is the likelihood of that earthquake rupturing to the surface?

The third conditional probability was, given an earthquake of a given size rupturing to the surface, what is the likelihood of the fault at the surface rupturing by the facility, by the test reactor?

And the fourth conditional probability was, given the aforementioned, specifically that the fault ruptured by the facility, what was the likelihood of a displacement being experienced at that point on the fault? Multiplying all of those conditional probabilities yields the likelihood of various size displacements occurring on a postulated Verona Fault.

The calculations to that fault did not presume a location of the Verona Fault.

At this point, with the objective of calculating the likelihood of displacements underneath the reactor, we applied a final step that had two parts:

The first part a very simple one, and one you've heard testimony on previously, was the conditional probability of a geometrical argument, the distance between the shears B-1, B-3 and B-2, that distance compared to the size of the foundation. That amounts to an additional probability reduction factor of about .06.

The final step was Bayesian. Everything we had done to this point was generally accepted to be classical probability. At this point we wanted to take account of the fact that no shears had been experienced -- had been observed between the shears for a given period of time, 40,000, 128,000, a variety of interpretations of those data.

At this point our calculation was Bayesian, and it amounted to a technique to distribute the likelihood of rupture on the Verona Fault adjacent to the test reactor to a point underneath the reactor. These probabilities -- the final result, including the Bayesian portion of the calculation, yielded annual probabilities of exceeding one meter displacement underneath the reactor itself, probabilities on the order of 10^{-6} to 10^{-8} per year. The probability, without regard to the Bayesian portion of the calculation, was on the order of 10^{-4} per year of exceeding one meter of displacement.

This calculation was designed to provide an additional check on the results that had been submitted by EDAC and Jack Benjamin Associates, and we found the overall comparison rather satisfying. While we disagreed with certain specific assumptions that were made in the EDAC-Benjamin calculations, we found the comparison to be quite reassuring.

Q Dr. Slemmons?

A (Witness Slemmons) My role within the probability panel has primarily been to review the geological considerations on which the probability analysis has been based.

My viewpoint is expressed in Appendix E of the Safety Evaluation Report. In general, most of the analyses used data that I believe is accurate, within a fraction of an order of magnitude. The major departure from that in all of the probability analyses is the one-dimensional approach rather than the two-dimensional approach, and on Figure 2 of Appendix E I show a cross-section in which I indicate that the geological data for the three shears in the vicinity of GETR have dips which are at low angles, in some cases as low as 9 degrees, and if one considers the third dimension, then you arrive at about 200 percent greater risk, about a quarter of an order of magnitude.

In summary, I think that the various models overall should give results that are within an order of magnitude of the picture that we arrived at from deterministic methods and geological approach.

Q And Dr. Vesely?

A (Witness Vesely) Part of our branch's responsibility within the Commission is to perform probabilistic risk analyses and review these analyses which are submitted to the Agency.

I quote from page 2 of my written testimony:

"I reviewed the probability analyses and models developed by GE's consultant, Jack Benjamin & Associates," as well as those performed by Lawrence Livermore and its consultants, TERA, "which were prepared for use in predicting the probability of surface rupture at the GETR. As part of this review, I specifically evaluated the various sensitivity studies that were

performed by GE and myself and the critiques
that were made to determine those credible
results that could be obtained from the
probabilistic modeling."

And I determined that the probability models could be used to order of magnitude precision. We did not agree -- I did not agree with GE's description of being able to use those models to the precisions that were stated in the reports, but they could be used to determine if the probabilities of surface rupture under GETR most likely lay in the vicinity of 10^{-6} to 10^{-5} per year, with an upper bound of 10^{-4} per year.

Q Thank you.

So that there is no misunderstanding, Mr. Wight, I asked earlier if there was any change in numbers contained in the errata sheet, and if we look at the last item of errata on the second page in reference to your Appendix C, page 17, and a column of numbers is changed. Did you mean to indicate that you had not used different numbers in your analysis than what appears on the errata sheet?

A (Witness Wight) Let me explain that. The table you are referring to from Appendix C of our review is part of the review that we did of the Benjamin-EDAC study. As part of that review, we independently tested components of their model to sensitivity, and that table is one of those sensitivity comparisons.

It is not, however, in any way related to the calculations we performed at the test reactor. It was part of the review.

end 2

MR. SWANSON: Thank you.

I have no further questions. The panel is available for cross-examination.

JUDGE GROSSMAN: Mr. Edgar?

MR. EDGAR: Yes.

CROSS-EXAMINATION

BY MR. EDGAR:

Q For the convenience of the panel, I will hand you Licensee's Exhibits 10, 14, and 16.

(Handing documents to the panel.)

Do you have Exhibits 10, 14, and 16 before you?

A (Witness Vesely) Yes, we do.

Q And are these the GE studies relating to the probability of an offset under the reactor foundation at GETR?

A Yes, they are.

Q One question before we proceed, you used the term, Dr. Vesely, in one of your prior answers "upper bound." Could you explain what that means in layman's language?

A An "upper bound" is a conservative value, for example, for the case of GETR, corresponding to approximately a 95 percent upper confidence level.

Q That is in extreme cases? Is that a fair statement?

A. That is an extreme case; yes.

Q. Now by reference to each exhibit, what I would like to discuss is the nature of the NRC review of each study, and the general thrust of NRC's conclusions in regard to each study. So first, if you could take each exhibit and explain the natu:e of the analysis conducted, and the general nature of the NRC questions that were posed as to each study?

A. Exhibit 10 was the first probability analysis that was done for GETR, and consisted of essentially a Poisson analysis for the probability of occurrence of an offset under GETR.

In our review of this analysis, we were concerned about the assumptions made relating to the Poisson occurrence of random occurrences of offsets. We were concerned about the softness of the uncertainties in data, and so we requested and sent to GE a list of questions regarding their analyses and asked them to do additional evaluations and sensitivity studies.

These questions are in -- I believe they're in Exhibit 14. As a result, GE came back with additional probability analyses in Exhibit 16, which are the responses to NRC questions, in which they performed approximately, including the sensitivity analyses that were performed in Exhibit 10, performed approximately 62 sensitivity analyses by varyin· not only the parameters but changing models. The additional probability models that were done in Exhibit 16, the Poisson assumption was relaxed and a different model where the probability of occurrence of an earthquake increased with time was modeled. There were different data assumptions, different data values used, and there were Bayesian as well as classical analyses used in both these exhibits.

Based on the analyses that were performed -- not only the original analyses but the additional analyses, Exhibit 16 -- as well as the independent evaluations done by TERA -- and I think as TERA points out, Larry Wight points out, those evaluations are quite independent; they use different data and different models than the GE models -- we concluded that the models, the probability results from the models could be used, could be interpreted to within an order of magnitude precision precisely as predicting the expected value for the offset probabilities of 10^{-6} to 10^{-5} per year with an upper bound of 95 percent confidence level of 10^{-4}. 10^{-4} again corresponding to a classical analyses of the data, the 10^{-6} to $^{-5}$ corresponding to the Bayesian analysis interpretations of the data.

That I think summarizes our evaluations and review of the GE analysis.

Q Could you define, again in layman's terms, the expression "sensitivity analysis"?

A "Sensitivity analysis" involves changing not only data values that were used, but also modeling assumptions that were used for the phenomena here. We asked that these two kinds of sensitivity analyses be done not only changes in data such as time until last offset or age of soil under GETR, but also the basic probability models that were used in the analysis; as well as doing Bayesian and classical evaluations to compare the different evaluations.

The data here are quite soft and uncertain, and so we felt these sensitivity analyses were quite important and were necessary to determine the credibility of the results in the probability models.

Q I take it it is your opinion that the GE models were methodologically sound? Is that correct?

A The GE models -- again, there were different methods used, and they covered the different models that could be used to model the phenomena at the site. Again, there were diverse models used even in GE's models as well as TERA's models. So that we felt that the different models covered the spectrum of models that could model the physical phenomena for GETR.

Q. Is it your opinion that Bayesian analyses can be used to provide meaningful results in regulatory decisionmaking?

A. It is my opinion that Bayesian analysis can be used. It has been used in regulatory decisionmaking. Again, Bayesian as well as classical, one has to do appropriate sensitivity studies, and be careful in using the analysis, but Bayesian analysis is one statistical approach of treating uncertainties. So, yes, I do believe it can be used, and it has been used in the Agency, again with care and caution.

Q. And "care and caution" I assume by that qualification that you mean that if one does the correct sensitivity analyses, or a spectrum of sensitivity analyses, then one can place this tool in some meaningful perspective? Is that a fair statement?

A. That is right. Sensitivity analyses are critical and necessary.

Q. You indicated that Bayesian techniques had been employed in NRC practice. Could you provide any illustrations or examples of that?

A. Bayesian analyses have been used in risk analyses of nuclear reactors. They have been used in developing test guidelines for the components in nuclear power plants, as two examples.

Q Mr. Bernreuter, if I could turn to page 3 of your testimony, there is a sentence which reads as follows: "Reasonable changes in the magnitude of the maximum credible earthquake factored into our analysis (i.e., plus or minus .5m) and the strain rate (plus or minus 30 percent) introduces a factor of only 2 to 3 change in the probability value."

Now in that context, I assume the implication of the "plus or minus 30 percent" is that you selected some value of strain rate about which you did a variance, or about which you varied to test sensitivity? Is that correct?

A (Witness Bernreuter) Yes, that is correct.

Q What did you select as your reference value about which to do the variations?

A We selected as a reference value a value of .02 centimeters per year, or 20 millimeters per year was our best estimate, which was larger than the stipulated value.

Q And is it your opinion that that strain rate as the best estimate is probably a conservative representation?

A Yes. I think that's a fair representation: That it is probably conservative. We tried to be reasonably conservative in doing that, in coming up with a best estimate.

Q So your analysis went on to consider what a 30 percent increase in the conservative value would yield in terms of effect on the probability?

A. That is correct. We actually varied it through larger ranges than that. This was just trying to put it in perspective for the testimony.

Q A question for the panel: The question or the statement in Mr. Bernreuter's testimony that I am interested in having some elaboration on is the following statement that reads: "This probability is further reduced by a factor which accounts for the degree of belief that in 128,000 years no observable surface rupture has occurred in the trench between the shears on either side of the reactor."

Could you provide some elaboration as to what estimates one can make of that factor?

(Pause.)

Any member of the panel.

A. We ran through a number of different calculations, just mathematically through the TERA analysis that ran through the two ends of the extreme from a factor of one over one-quarter to one over 375, and actually could get it even smaller than that if you really put a tremendous degree of belief in that there are absolutely no possibility for any shears being between the two shears. And I guess possibly Dr. Slemmons might give the best geological view of that. He studied the trenches in much more detail than our other staff did, and has a much better feel for that.

Q. Okay, if Dr. Slemmons could contribute?

A. (Witness Slemmons) Yes. Would you rephrase the question for me, please?

Q. Well, the question is: Based, I assume in your case, based on geological evidence in your own experience, how would you estimate the probability which would take into account the fact that no ruptures have occurred off of the shears in 128,000 years? Or, conversely, that the offsets have tended to stay on the shears?

A. Okay. I have previously commented on one aspect of that question. First of all, the trenches that exposed the shears in that area are some distance, primarily on one side, the B-1/B-2 trench is to one side of GETR, and the foundation area itself or the immediately adjoining areas have not been explored. So to some extent, the evidence has to be obtained by extrapolation.

The B-1/B-2 trench in my opinion -- and I think in the opinion of those who visited the trench with me at the same time -- very clearly had in the segment opposite GETR no evidence of shears. There were buried paleosoils which I think both the USGS people and the consultants to General Electric agreed would correlate with something in the range of 128,000 or older. That correlation is indirect, and the numbers are not hard numbers.

From my experience in other regions, as one deforms a fault zone, one has a very high incidence of recurrence on the planes of weakness that have been previously formed. At some point in time, new faults are formed. As deformation continues, there can be rotation of blocks so that eventually a fault may arrive in an unstable situation, at which time a new rupture may occur. But for both reverse-slip, strike/slip, and normal-slip faults, normally you may go through perhaps 100 events breaking essentially the same trace, or nearly the same trace, before one moves off to a new zone.

So that geologically one would not assign a very high probability for a new rupture to occur in between the B-1 and B-2 shears.

(Pause.)

Q. Mr. Wight, in your summary you made

reference to a 10^{-4} per year figure as being associated with the classical analysis. Now my question is: Was this 10^{-4} likelihood associated with movement on the existing shears? Or did it apply to the likelihood of a one meter offset under the foundation? Just what did it apply to?

A. (Witness Wight) One could conservatively assume that there is a Verona Fault underneath the reactor whose exposure had not been yielded either by foundation excavation or the trench. And if that were the case, then my 10^{-4} probability would be the probability of one meter occurring on that hypothetical fault.

In indicated that the way we calculated the slip rate was through topographic expression, and we deliberately held back the trench data to provide another independent check.

So the other part of your question is: Is it related to the information in the trenches? The answer is: Yes; that depending on your interpretation of the age dating, the return period of one meter is between five to eight thousand, up to maybe twenty thousand, and that is not too inconsistent with the probabilistic predictions.

MR. EDGAR: We have no further questions.

JUDGE GROSSMAN: Mr. Cady?

BY MR. CADY:

Q Dr. Vesely, would you consider it to be more conservative to use the 10^{-4} classical result, or the 10^{-6} Bayesian result for the purposes of these proceedings?

A (Witness Vesely) The 10^{-4} classical result.

Q The same question for Dr. Slemmons. Would you use the 10^{-4} as being conservative, or the 10^{-6} as being a conservative figure?

A (Witness Slemmons) I think I would prefer to leave that question to the members of the probability panel that have the probabilistic background.

Q Mr. Wight?

A (Witness Wight) Obviously 10^{-4} is more conservative than 10^{-6}, but the question is to which to use in a given decision and is a question of policy.

Q Mr. Bernreuter?

A (Witness Bernreuter) Well, the 10^{-4} number that is getting somewhat bandied about I think as the classical number is just the probability of having offset somewhere on the site, not necessarily at the GETR reactor per se. So from that point of view, the 10^{-4} number, I like that number, but that needs to be tempered with some geological evidence that there is no shearing --

whether there has ever been any shearing between those two shears. I gather that one point is still somewhat in question. At the time we were writing our testimony, it looked like there were shears between the shears, but then the USGS came in and said: Well, there was a probable fault in the GETR foundation, and such like. So I think that one needs to temper the 10^{-4} number with some geological estimate of what they would put down as that probable fault, which would reduce it somewhat lower than the 10^{-4} number.

A. (Witness Vesely) I would like to add something here. The 10^{-4}, even an upper bound, is not the probability of a large consequence; it is simply the probability of an offset. There is an additional probability for that offset producing a consequence.

Also, to put that 10^{-4} in consequence, even though it is a very conservative number, the Staff has calculated 10^{-4} for core melts for nuclear reactors, and continue to, and have not shut those reactors down and continue to operate those reactors.

So that an ad hoc criterion that the Staff uses for nuclear reactors is 10^{-3} for an unacceptability criterion. So the 10^{-4} very conservative value compares with a nuclear reactor probabilities that have been calculated for core melt with much greater consequences.

Q. Dr. Slemmons, how much weight would you give to the probability analysis in helping the Board in rendering a decision on this matter?

A. (Witness Slemmons) I think it is an important adjunct method that should be used in conjunction with deterministic geological methods; and I believe it gives supporting data that has value. I would not make a decision, nor do I believe the NRC and other federal agencies use it as the prime method for establishing the risk at major vital structures.

MR. CADY: Thank you. I have no further questions.

BOARD EXAMINATION

BY JUDGE GROSSMAN:

Q. In basing the TERA analysis on slip rates, Mr. Wight, what data did you use for the slip rates?

A. (Witness Wight) The slip rate that was used in the term "best estimate calculation" was a slip rate of .02 centimeters per years.

Q. What was that based on?

A. It was derived from the difference in elevation between the Verona Hills to the north, and the Valley to the south. The underlying hypothesis being that those hills formed in response to uplift on a postulated Verona Fault.

Q. But now in determining probability of earthquakes from slip rates, what data did you use?

A. There is a model that has been employed for almost a decade for calculating the occurrence of earthquakes on structures for which the slip rate is known. The model originally proposed by Dr. Bloom revolves around the theory of earthquake moment.

Earthquake moment is another way to express the size of an earthquake. You have heard in these proceedings about the various sized earthquakes for these faults in the vicinity -- local magnitude, surface wave magnitude, body wave magnitude -- they are all basically spectral measures of the earthquake size. They sample certain frequencies of the earthquake ground motions. The earthquake moment is another magnitude scale, and it samples a very large area of frequency component of the earthquake ground motion, and therefore can be correlated with the overall length of the fault.

The foundation of the earthquake moment, the basis for the earthquake moment, is an equation involving the fault area, the slip rate, and the rigidity of the materials around the fault. Knowing the earthquake moment-- excuse me, knowing the slip rate, one can use this relationship to get a moment relationship, and moments are through data correlated with magnitudes, thereby yielding a magnitude of occurrence relationship.

end JWB3

Q Does that model work the same with regard to each local conditicn?

A No, it doesn't. It has a free parameter, the rigidity of the earth median, and so it certainly is capable of being as spec:.fic as one would like with regard to the local conditions.

And, of course, anocher parameter is the earthquake fault area, and that would be fault-specific.

Now because earthquakes in western California occurred about the same depth, 15 to 20 kilometers, where we believed the mechanical properties of the earth are about the same, there has been in the literature no basis provided for using different values at diffe.ent locations. We used the commonly accepted value for the western United States for rigidity.

With regard to fault area, we used the postulated length of the fault, I believe 11 kilometers in a typical depth, and we looked at sensitivity to our results from other lengths and other depths.

Q Dr. Slemmons, do you consider the use of slip rate to determine the probability of earthquakes occurring to be a very reliable method?

A (Witness Slemmons) I can't really assess the reliability. I believe it is a valid method that has a sound basis and seems to fit empirically reasonably well with field observations.

Q Do you mean overall, that is worldwide, or do you mean it seems to be reliable with regard to predicting local earthquakes?

A I was referring more to the local California type of setting. I think it would fit reasonably well the worldwide data as well. My previous compilations are for shallow focus earthquakes, and so the worldwide data typically involves earthquakes of less than 25 kilometers focal depth and contemporaneous surface rupturing. So that I feel that it could be applied both regionally and locally.

Q Now my understanding -- my layman's understanding of tectonics is that strain or stress can be relieved in a number of ways, and that it is not only through tectonic events, earthquakes, that this happens. Am I incorrect in believing that, sir?

A No, you are correct in that observation. In Californi, and both on the segments of the Calaveras Fault, the Heyward Fault and the San Andreas Fault, some of the strain rate is being relieved by creep. That is, progressive movement almost as rapidly as the stress is being applied.

In general, however, that is a small fraction of the total strain, usually about one fourth or less.

Also, the field observations of the faulting that has occurred during historic earthquakes for many of the data points or observation points, you find that deformation has occurred, distortion has occurred, so that, for example, fences that were crossed by the San Andreas Fault in the 1906 earthquake quite often show a bending, perhaps even 100 feet or more away, indicating that there has been some faulting or warping, as well as the fault slip.

The typical field relationships from cases where faulting has occurred is that approximately 60 or 70 percent of the deformation is by fault slip, and 30 percent or so may be in the form of distortion.

Q Do you have a very high level of confidence in that conclusion?

A Yes.

Q I take it then you would have no hesitation in basing a prediction of the recurrence of earthquakes on an analysis of the strain?

A I think it's one of the approaches that should be applied.

Q Exclusively, or --

A No, I think it is one approach that should be conducted along with others, and the data correlated between the different methods.

Q Do you agree with the method of determining the slip rate that was used by TERA Corporation?

A There are -- yes, and I believe their results are conservative. I have applied a similar approach assuming that the present topography has evolved over the last million years or so by faulting without folding process. So I have assumed the entire deformation is by faulting and by using the range of dips that one observes in the shears at the site, I come up with a very similar value. If one uses the Holocene data, which is what the U.S. Geological Survey has also used you come up with a value that is very similar. You come up with values that range from about .01 centimeters per year to a maximum of about .04 centimeters, .03 centimeters per year, and I believe your best fit uses the .03 value. Or was it the .02? You had one as an upper bound and one as a most reasonable or best fit.

Q Well, now, if you were to use the strain rate for predicting earthquakes, don't you have to take into account when the last release or relief of that strain rate occurred?

A That is correct. If one assumes a cycle with an average recurrence interval or time period during which the strain accumulates to build up for the next event, it would be important, and generally not possible, to determine where you are relative to the time of last offset, or how soon the next offset would be.

Q Well, Mr. Wight, was that done with regard to the Verona Fault?

A (Witness Wight) No, it wasn't. Earthquakes were assumed to occur randomly in time, such that the hazard between earthquakes is uniform.

I would just like to make a point here. It may be semantics, but I think it's an important one. We aren't trying to predict earthquakes. I think that was the term you used. We are instead trying to assess the global hazard presented to this site from earthquakes, and one can examine this hazard in many different ways.

One can look at the occurrence of the historical earthquakes. You've heard testimony on this. One can do numerical modeling on the effect of earthquakes, for example.

Another approach -- and I think we are all saying here a complementary approach that provides a different perspective of the hazard -- is to look at the statistical presentation of the historical load, the historical exposure presented to the site.

That's not to say that we're saying that we are assessing the likelihood of an earthquake occurring deterministically or specifically in the remaining life of this facility, but rather on the average there is presented for the site a 10^{-3} probability of earthquakes rupturing one meter underneath the foundation, on the average.

This is in the same vein that civil engineers look at the return periods of floods, for example. They are not attempting to prescribe a flood occurring in the next decade, but to provide the decision-maker a feel for the relative hazard presented to the site, compared to other facilities.

A (Witness Vesely) I would like to add on the strain build-up, the additional GE models in Exhibit 16 did include the recurrence interval approach, and they did vary the time since last offset from 4000 years to 80,000 years, and that resulted in a change of probability of 1×10^{-6} to 1.7×10^{-5}. That's contained in my review in Section B of the Staff's May 23rd, 1980 SER on page 7.

So the additional models that Staff asked GE to perform or to develop did model strain build-up recurrence interval, as opposed to the Poisson random occurrence modeling that was done in their first Exhibit 14 analysis.

Q Dr. Slemmons?

A (Witness Slemmons) May I comment further on the recurrence or earthquakes?

The most satisfactory data base that we have available is for different type of fault, if that's for the San Andreas Fault zone and the work done by Dr. Kerry Sieh of Cal-Tech at Palette Creek. He's excavated a trench which cuts through a number of peaty or organic soils. This has been in a marshy area that has been repeatedly dammed with each new earthquake, and the record there goes almost back to the time of Christ, and in looking at the disturbances, both shown by liquefaction and by fault rupturing, he finds that the recurrent interval, the time between large events, varies, as I recall, from approximately 120 to some 250 years, with an average interval of about 160 years.

So we see that we do not have precisely spaced this, but they on the average fit with a certain value, with a plus or minus factor of perhaps 50 percent.

We have very little data for reverse slip type faults. One might expect that the Verona Fault zone is -- and I want to avoid the term "structurally related" -- must be tectonically related to activity on the Calaveras Fault zone, the Las Placitas, the Greenville and the entire region is undergoing strain which may vary with time, and as you get various sequences of activity from one fault to another, you can place a changing pattern, time pattern of stress build-up.

So that I think the idea of prediction is -- although it is a very attractive one, is one that could not reliably be conducted for a zone of this sort, and I would think that the data which has been commented upon by both the USGS and the Applicant's consultants suggests that for the Verona Fault zone, you have a recurrence interval between events of two or three foot size that may range for any one of those fault strands there, somewhere between 8 or 9000 years to perhaps a couple of tens of thousands of years, and the correct value may not be a precise value, but may better be represented perhaps as a range which can vary for each new increment or recurrence interval.

But in assessing the risk or the hazard at this particular site, the geological information, I think, provides us a better guide as to what the risk might be, than to use the seismological record, because we are dealing with a zone where both for the Calaveras Fault zone and for the Verona, we are dealing with recurrence intervals that are much greater than the historic and the instrumental seismological record.

Nevertheless, you need to tie all of these kinds of information together. I don't mean to disparage the use of seismological methods. But I think I am making these statements to sort of give a perspective to the errors that can occur in either average slip rate or changes in rate that may occur over somewhat longer periods of time.

The data base that one obtains for slip rate as determined by the Holocene offset normally would be the most credible type of information, in that it is the most current.

On the other hand, you are dealing with a sampling period that is approaching perhaps the length of an average recurrence interval.

The Applicant has used the amount of offset of Stage B soils, which gives a little bit longer sampling period, and it gives almost an order of magnitude different val e. This is another valid approach.

The method used by TERA and by the USGS and myself in using the topographic expression is based on certain assumptions, and may be representative of a longer term average rate over a million years or so, and we get a range of almost one order of magnitude in slip rates, depending upon the particular method that you use.

Q Dr. Vesely, I take it changing or plugging in an assumption with regard to the last occurrence would not change the probability on the classical method; is that correct?

A (Witness Vesely) Well, there are various classical methods. One is the classical method which uses the Poisson assumption, simply observing that there have been no occurrences in 120,000 years. There would be no impact.

A recurrence interval approach, though, could also be approached using classical statistics.

The difference between classical statistics and Bayesian is not the modeling of the phenomena, but how the estimates of parameters are obtained, and that's why sensitivity studies are very important here, because the Bayesian approach may give one specific time to last occurrence. Classical will give another. But models, probability models, are valid for either Bayesian or classical. It's the determination of parameters where the Bayesian and classical differ, and changing the time since last occurrence of offset again from 4000 to 80,000 years changes the probability from 1 x 10^{-6} to 1.7 x 10^{-5}, and that's valid for either classical or Bayesian.

Q I thought you gave a value for the classical at 10^{-4} to begin with.

A 10^{-4} is an extreme result which corresponds to assuming an undiscovered fault under the GE reactor, and that corresponding to an age of soil approximately 10,000 years instead of the 128,000 years. The classical result will give 10^{-5} if you assume 128,000 years for the age of the soil under the GETR, and a random Poisson occurrence.

The classical is very sensitive to what you assume for the age of the soil under the GETR reactor in the Poisson modeling; if you assume the extreme case of an undiscovered fault under the GETR reactor, then you get

10^{-4}. That is classical result. But that corresponds to an assumption of an undiscovered fault under the GETR reactor.

Q Does it make any difference with regard to that determination as to when the age of the last event occurred?

A In the Poisson modeling, it does not. In the recurrence interval modeling, it does make a difference with the strain build-up modeling. These are two different probability models of the phenomena.

Q Okay. One thing that puzzles me is we keep coming back to the offset occurring under the foundation, and I want to ask Mr. Wight whether the entire study is based on determining whether that would occur underneath the foundation; that is the offset directly underneath the foundation.

A (Witness Wight) Yes, the objective of this study was to determine the likelihood of that happening.

Q Well, we heard some testimony at the last session that the displacement did not have to occur under the foundation, it could be in the near field, and I assume then your study would not apply to the situation covered by that other than under the reactor; is that correct?

A One could use our model and our results reported in the appendix to the SER a number of ways. One could look at the likelihood of earthquakes occurring on the fault defined by the shears in trenches B-1, B-3, for example. Many people, myself included, believe that is the principal expression of the Verona Fault, and I personally think that our 10^{-4} probability applies best to modeling displacements on that shear. But we seek some way to take the data and use this model to assess the likelihood of displacements occurring underneath the reactor itself.

One conservative approach would be to take that 10^{-4} and apply it to the reactor.

A (Witness Vesely) I would like to point out in our review of both GE's and TERA's analyses, the probability of a fault occurring under the GETR site contributed, as Don said, approximately .06, 1/16th. So if you assumed a probability of a fault anywhere in that field between the shears, you would raise your best estimate from 10^{-6} to a factor of 2/16th, to 10^{-5}. So that that's the occurrence directly under the building contributing again a factor of 16. But that still would be within the 10^{-4} value, as Larry Wight said.

Q Well, now, you're referring to the offset being on any other shear within the area, for any undiscovered but do those results apply also to any offset occurring anywhere within that zone between the shears?

A Yes, a factor of 16 does.

Q And how would that adjust your 10^{-4} result with regard to that?

A It does not. It includes that factor. In fact, that's one of the biggest contributors.

Q Mr. Wight, did your analysis cover any situation in which an offset had occurred at any time within the last 40,000 years?

A (Witness Wight) Our analysis did not require -- I'm not sure if I totally understand the question. Our analysis did not use data in the trenches, age-dating in the trenches, which yielded by various interpretations numbers up to 40,000, 128,000 years.

(Panel conferring.)

A (Witness Wight, continuing) Perhaps you're referring to the Bayesian portion of the analysis, where we assumed that there were zero offsets in that timeframe.

Q Yes.

A Okay, that was observed data, and that was used in the last part of our analysis, the Bayesian portion.

Q And you also used 128,000 years, didn't you?

A Yes, acknowledging the uncertainty.

Q Well, we have heard some testimony to the effect that the offsets could have occurred within the past 1500 to 4000 years. How would that affect your result?

A Specifically I don't know, but it wouldn't move our results closer to this 10^{-4} number we are talking about, that being the likelihood of earthquakes occurring on the Verona Fault.

Q Did you also assume that the strain rate would be relieved by only one meter offset?

A No. And to explain my answer there, let me say we did not uniquely associate one meter with a given sized earthquake, say a magnitude 6. We did use a relationship derived from actual earthquake data relating magnitude to displacement, but we carried into the analysis uncertainty in that relationship, a fairly sizeable uncertainty. So that there was not a unique one-for-one correspondence between that predicted displacement and a given earthquake.

Q Well, how would the results be affected if you were to assume that if any -- any effect if you were to assume that strain could be relieved by an offset of five to seven feet?

A Well, I don't know for sure that five to seven feet were included in the analysis. I believe it was, through our characterization of the uncertainty between magnitude and displacement. Specifically, let me hypothesize that the median or best estimate displacement for a magnitude 6 is say one meter. I don't believe it is, but say it's one meter. Then we acknowledge the uncertainty in that displacement for the same magnitude occurring, such that it's with some probability likely that that magnitude 6

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would result in a quarter meter displacement, but it's perhaps with the same probability likely that it could be four meters.

Q In developing your model for predicting earthquakes on a basis of slip rates, did you use data from all types of earthquakes, including strike slip, regular dip earthquakes, and thrust faulting?

A Was your question with regard to the slip rate portion of the analysis, that is the earthquake occurrence model?

Q Yes.

A I understood it was. It's the model -- let me back up and say the theory of earthquake moment is independent of style of faulting. It does not -- it is not so specific as to prescribe a certain type of moment for a reverse fault and a different type for say a strike slip.

Q Well, that isn't because there may not be a difference, is it?

Well, let me phrase it another way. You've just included all of that data and you didn't distinguish between one situation and another, but that doesn't necessarily mean that you shouldn't distinguish, does it?

A That's possibly true. The data -- and there is a fair bit -- is incapable of resolving at this point by fault type.

Q Do you have a sufficient amount of data to determine a recurrence rate of earthquakes for thrust faulting types of earthquakes?

A We'd have to define "sufficient," and each person would have his own sense of that. I think it is sufficient. I think the model, taken together with the data, is sufficient, but I want to emphasize that acknowledging other persons' perspective of sufficiency, we have included a lot of sensitivity calculations to provide a broader basis for our conclusions.

Q But you haven't modeled anything specifically on a thrust fault earthquake?

A No, that's true.

MR. SWANSON: Mr. Chairman, the members of this panel may not -- are not aware of the Board's earlier offer that at any time they may feel the need for a break or a recess, they may ask for it.

We have been going now for two hours. It might be an appropriate time if the Chairman was in between lines of questioning.

JUDGE GROSSMAN: No, that's fine. Why don't we take a 10-minute break?

(Recess.)

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JUDGE GROSSMAN: We are back in session.

BY JUDGE GROSSMAN:

Q Dr. Wight, you indicated certain probabilities if one were not to assume that the offset were to occur beneath the foundation. I believe your answer went to an offset occurring anywhere between the two shears, or the two trenches that we were talking about.

Did you cover a range of probabilities for offsets beyond those shears, beyond those two trenches where the shears were located?

A (Witness Wight) No, we didn't, because that portion of the analysis relied on the age of undisturbed soils between the shears. That is, the 40- to 128,000 number.

Now with your permission I would like to do a little housekeeping and maybe clarify that point. First of all, before the break I referred to a probability of 10^{-3} as the likelihood of displacements on the Verona Fault. I misspoke there. It should have been 10^{-4}, as I think I had said, or which number I had used previously. Just a small point.

With regard to that, there were some questions about the concept of what I meant by that. Let me try to explain what I mean by that a little better.

We have a model for the Verona Fault for which we are predicting the likelihood of displacements. We use that model and that result, 10^{-4}, to account for the fact that between two shears there are no displacements observed.

Now our model of the Verona Fault is predicting one meter displacement with an annual probability of 10^{-4}. Now I'm not -- When I give you the result, I'm not saying where that's going to occur. What we do know, what we do hypothesize is that there is some deep-seated zone called the Verona Fault on which this displacement will occur.

What we are trying to analyze or consider is where that one meter will emerge at the surface. I said earlier that I believe that the principal expression of the Verona Fault, and therefore the most likely place for that one meter to emerge, is on the shears exposed in trenches B-1/B-3, but it might be convenient for you to think of that one meter as being expressed at depth with an annual probability of 10^{-4}.

Then, as another matter, trying to assess where that one meter will emerge. What I said earlier is that one can conservatively assume it emerges underneath the reactor, in which case that likelihood is 10^{-4}. Or one can allow for the geometrical setting --

that is, the ratio of the foundation size to the distance between the shears and the age of undisturbed soils between the shears -- in which case, the probability is reduced.

Now with that, we get to another small point. I sense perhaps some confusion between the age of the undisturbed soils between the shears, what I have referred to as between 40,000 and 128,000 years, and the age -- or better yet, the recency of that one meter displacement in certain of the trenches. That number has been testified to be, by the USGS, between 2- and 4000 years, and by other people up to 20,000 years. That, again, is the recency of one meter displacement in the shears, a very different data point than the age of soils between the shears.

We used in our sensitivity calculations the numbers 40,000 and 128,000 to assess where this one meter might emerge. And I also use the 2- to 4- to 20,000 years regarding the recency of that one meter displacement to provide a qualitative check on our results.

Our one meter displacement has an annual probability of about 19,000 -- 1/19,000. There was one other small point, if you will bear with me. There were some questions before the break about the probability of 5 to 7 feet of displacement. I tried to answer that in terms of the input that we used, how we carried along the uncertainty in the displacement magnitude relationship.

Just to further emphasize that point, we do calculate in our report the likelihood of five to seven -- we report up to almost 3 meters displacement probability in our report. For 2 meters, that annual probability is in excess of -- is lower than 10^{-5}.

Q I have a little trouble understanding how, if you calculate a probability of 10^{-4} of the shear offsetting underneath the reactor, that if you now have to determine the probability within the entire zone in between the trenches, the probability of that occurring wouldn't be higher. That is, 10^{-3} or 10^{-2} or something in that direction?

A Okay. The 10^{-4} is the probability of one meter displacement occurring let's say at 20 kilometers at the hypocenter of this postulated set of earthquakes that might occur. That could emerge at the surface anywhere. And as I have said, I believe that it will likely emerge on a shear that has been mapped already, but we think the actual probability of it occurring underneath the GETR is even less because of the age of undisturbed soils and the absence of shears. And when we model that as our report describes, including both the age of the soils and the geometric effect, that probability is reduced from 10^{-4} to 10^{-6} or $^{-7}$ or even^{-8}.

A. (Witness Vesely) That's right. The 10^{-4} corresponds to an offset occurring anywhere between the shears or on the shears. If you assume that when an offset occurs conservatively it is going to curve right under GETR, you get the 10^{-4} again. But that is going to be reduced. The 10^{-4} is, again, for the probability of an offset occurring anywhere in the region. If you conservatively assume that any time I have an offset occurring it is going to curve under GETR, then that's the only way I get the 10^{-4}.

If you take into account the geographical effects, you reduce that 10^{-4} to a factor of 10 or 100 depending upon the approach you take.

BY JUDGE FOREMAN:

Q. Could you just repeat that last couple of sentences about the probability of occurring under the GETR of 10^{-4}?

A. (Witness Vesely) The 10^{-4} corresponds to an offset occurring anywhere in the region. Now if you assume that whenever an offset occurs in the region, it is going to occur under GETR because of some undiscovered fault that is under GETR, then the probability of an offset occurring under GETR is also 10^{-4}. You take no credit for, as Larry said, the undisturbed age of the soil under GETR or the soundings.

If you take into account the fact that when an offset occurs it is most likely to occur on an existing shear, then you reduce that 10^{-4} by a factor of 10 or 100 to account for the fact that GETR is on undisturbed soil. The factor you count, that extra factor depends on the age you give to that undisturbed soil, whether it's 40,000 or 128,000, reduces that 10^{-4} by a factor of 10 or a factor of 100.

BY JUDGE GROSSMAN:

Q But that reduction would also depend on your assumption being correct that there are no shears directly underneath the GETR.

A That is correct. That is right.

Q My problem was with your terminology of saying "an offset under GETR." I assumed that to mean an offset surfacing under GETR, not that the epicenter of some event would be somewhere under the GETR area.

Dr. Vesely, you -- I'm sorry. Before we get to you, Mr. Wight it seemed to me that in your report you correlated fault length with magnitude in order to arrive at your overall conclusions. Is that correct, sir?

A. (Witness Wight) That's true.

Q. And you relied upon data supplied by Dr. Slemmons for that?

A. It was published data, not personally communicated, but that is right; it was Dr. Slemmons' data base.

Q. Dr. Slemmons, is that the data base that you have since revised, or which required some subsequent revision now?

A. (Witness Slemmons) I don't know in reading the TERA report -- let me divide my answer into two parts.

First of all, I am not certain in looking through the material on page 3-12 of the TERA report whether the utilized the worldwide data base, or the data base for reverse and reverse-olique slip faults.

I have in the last few weeks revised the data base for reverse and oblique-slip faults, but I have not recompiled the entire worldwide data base. I am in the process of starting that study. So the answer is: Yes, in part.

A. (Witness Wight) Thank you. We used the worldwide data base.

Q. Well, even revising that to the --

A. (Witness Bernreuter) I just wanted to add that in our sensitivity studies we noted that we could make fairly significant changes without it really affecting -- to that particular relation, I'm sure, much larger than the few changes that Dr. Slemmons knew, or shuffling in a few earthquakes, and shuffling them out had, telling us about the percent change, and it only changes our results less than a factor of two by making these very large changes in that particular relationship.

So that the fact that we used a worldwide data base which had both strike/slip and thrust in there than if you tried to just segregate it down to, you know, the hypothetical set of just thrust earthquakes, that the order of change -- the order of magnitude change that we're talking about and probability of offset is less than a factor of two. So that these are not producing significant changes, any changes in this data base.

A. (Witness Slemmons) I agree with the comments there. And even for the ones that I have revised the reverse slip, the normal differences are only in a tenth of a magnitude. So that it is not a significant change.

Q. Well, I had thought that you had some sigiificant change based on the San Fernando data that would relate to that, Dr. Slemmons?

A. My earlier 1977 paper did include the San Fernando data. So I don't know if you're confusing the analogy that has been made between similarities between the Vallecitos area of the Verona Fault and the San Fernando? Or whether you are referring to the worldwide data base. Perhaps you could clarify.

Q. Well, I had just understood from prior testimony that there were some new observations made on the San Fernando event which related to the rupture length that might be significant. And I had assumed that it might be significant in this particular application.

A. In regard to San Fernando, I think that in my original data base I used a 12 kilometer length for the San Fernando earthquake on the published accounts at that time. Newer publications indicated a 15 kilometer length. I did use the 2.5 meters, which is the maximum according to current observations. So that there are only minor changes in that regard.

The San Fernando is not a strictly analogous case to the Vallecitos area. And if you plot -- if you were to solve for the San Fernando event from my worldwide data base, you would come up with only about 1.6 or .7 or .8 meters for a rupture length of

15 kilometers. So it is not a close fitting point.

Q Mr. Wight, I noticed in your report that you indicated that you relied upon the Verona Fault not being connected with the Calaveras or Pleasanton Faults. Is that correct? That you did rely upon that assumption?

A (Witness Wight) That's true.

Q What was the significance of your reliance on that assumption?

A The manner in which that assumption was made was with regard to the Verona Fault length, which best estimate we took to be 11 kilometers, and we examined a range of about that from 7 to I think 18 or so kilometers. Theoretically, if one were to connect these two, one would therefore examine a longer fault length and larger possible earthquakes.

Now while we didn't specifically consider that an element of our sensitivity analysis was addressing this point, in fact it was because we looked at sensitivity on the size of earthquakes that could occur on the Verona Fault up to magnitude 6.5.

Q Well, I am not sure what the size of the earthquake has to do with your entire probability study. Can you tell me that?

A Sure. It is contained in the earthquake occurrence model. That earthquake occurrence model predicts or estimates the number of earthquakes occurring per year of different sizes from magnitude 3 on up to the maximum earthquake considered for the structure. That is, the Verona Fault. Our best estimate upper magnitude earthquake maximum earthquake was magnitude 6, but we did examine sensitivity to that 5.5 and 6.5.

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Q Well, do I understand correctly again that if you were to postulate a greater length for the Verona Fault, that you would have a greater frequency of occurrence of the larger magnitude events that you were attempting to determine a probability level for?

A Yes.

Q How would your results differ if you were to postulate that the Verona Fault were connected to the Calaveras or Pleasanton Faults?

A If one assumed that that amounted to increasing the size of the earthquake, maximum earthquake that could occur on the Verona Fault, I don't know specifically, but I do know that going from a magnitude 6 to 6-1/2 increased the probability by a factor of about 33 percent, very small relative to the magnitude of probability being considered.

Q Dr. Vesely, reading the Staff critique of the TERA report, I didn't find a ringing endorsement of that report, and I did see that there was some criticism of the report. Could you briefly summarize what you found in the report that you couldn't entirely endorse?

A (Witness Vesely) We certainly didn't offer a ringing endorsement of the precisions claimed for the probabilities in the report, specifically the use of the report to justify 10^{-6} probabilities or lower. We felt that the data in modeling uncertainties could not justify the probabilities to that precision.

We did, though, feel that the models and the data did allow one to estimate probabilities to order of magnitude; that is 10^{-6} to 10^{-5} as an expected or best estimate value, and as high as 10^{-4} in extreme cases.

We felt that the models and data could not go beyond that. Beyond those kinds of precisions, the different modeling assumptions and data gave you factors of 2 or factors of 3 kinds of changes, and those could compound and give you orders of magnitude differences.

We felt this model and these approaches couldn't be used to justify 10^{-6}. They could be used -- and as we stated in our reviews, could be used to justify 10^{-6} to 10^{-4} kind of range, with a 10^{-4} being extreme upper bound. And all those probabilities were useful, and we recommended that range of probabilities be factored into decision-making.

Our main concern was on the statement -- the indication in the report of the -- I guess the overuse of the models, the models because of limited data, and the different probability models were consistent to orders of magnitude, but that's all.

And I think that's our principal concern, and that was our reservation. This is the reason we asked for sensitivity studies which were performed, we felt, to our satisfaction. As in any probability models, I think this is true, not only for this model, but the other probability models that are used in risk analyses of nuclear reactors, and the other probability models that are submitted to the Commission.

It's been our general policy to use these as one source in decision-making, but not certainly the principal source, and to base the judgments on probability models, based not on precise numbers, but on sensitivity analysis and the range of results that are obtained from these models, not on any specific number.

Q Dr. Slemmons, you did review the EDAC report. Did you also review the TERA study?

A (Witness Slemmons) I did not formally review it, no. I have examined the TERA report, but I haven't given it critical review.

Q Could you tell me what you consider to be the deficiencies in the TERA report?

A In the TERA report, I generally concur with the geological parameters that were used by the report. I believe the geological basis is reasonable.

Q Do you consider that there was sufficient input of local conditions in that report in order to make a reliable probability study?

A Yes.

Q Now if I understand correctly, we still have the EDAC report before us; is that correct, Mr. Edgar?

MR. EDGAR: I need a clarification. We still have that before us?

JUDGE GROSSMAN: Well, what I mean is, that is part of your case, isn't it?

MR. EDGAR: Yes, sir.

JUDGE GROSSMAN: Okay. So we do have to explore that report.

Well, I'm not asking you whether you're requiring me to explore it.

MR. EDGAR: That's your judgment.

JUDGE GROSSMAN: I mean it is still a live issue.

MR. EDGAR: I would suppose so.

BY JUDGE GROSSMAN:

Q Dr. Slemmons, you have certainly critiqued that report. Could you tell us generally what your dissatisfactions were with that report?

A (Witness Slemmons) My first comment -- and I perhaps am not a valid judge of the first comment -- and that was that I questioned the lack of the discussion as to whether the Poisson distribution model was appropriate.

The second feature that I criticized was the fact that the report utilized the indirect correlation with the marine sea level change scale of Updike & Shackleton, and by utilizing the features that are indirectly determined by correlation there, the ages of the various paleo soils were given to three significant figures, and it seemed to me that this gave an aura of greater precision than perhaps should be used.

I felt that the counting back rings essentially type of technique for the paleo soils could include mis-identification or miscorrelations if, for either some local climatic tectonic reason, soils would be either skipped or an additional soil added, and therefore there would be the possibility of having a whole number error in the number of stages that are involved.

Geologically, it would be more likely to have a missing soil than to have an extra soil, and so errors of this sort would generally tend to be conservative, but would not necessarily be conservative.

The numbers used for displacement in my opinion are -- the number used for displacement or displacement rate is based on, I believe, the stage 5 correlation and gives almost an order of magnitude lower rate than if one uses either the Holocene or the type of approach used by both USGS, TERA and myself.

So there is likely to be some variation in a nonconservative direction, although the data point used is a valid one, and should be considered as well.

In other words, the number used by EDAC for the --

I believe it was the 70,000 year correlated soil, does give an accurate appraisal for that particular interval of time.

I guess one of my most critical comments had to do with the third dimension of the 20-foot depth of the GETR foundation, which is, I think, Figure 2 indicates would give a depth at a low dipping fault plane, would have not only the intercepts, it would have linearally along the trench B-1, B-2 type profile, but would in addition have an additional opportunity to intercept the foundation due to the low attitude, and this as a cross-section indicates it is up to 200, 220 percent or so added risk of having the foundation intercepted over and above the normal linear relationship that was used in the EDAC report, and this would give perhaps an error of up to 1/5th or 200 percent in the final analysis.

I don't believe these critical comments would affect the overall results by an entire order of magnitude, but it would certainly, within a range of order of magnitude, modify the results

Q Well, were the probability studies by EDAC based on a determination, or were they for the purpose of determining the possibility of the offset of surfacing directly at the foundation?

A Yes.

Q So that to the extent a surface offset might occur somewhere within the entire zone, the EDAC studies just would not be applicable; is that correct?

A I believe that it would be applicable, but the numbers should be modified.

Q Well, would it be applicable to an offset occurring within the zone or only with respect to a particular shear within the zone?

A It was, as I understand it, and perhaps you should question the other members of the probability panel, it was to determine the probability of a new rupture intercepting a foundation having the width of the GETR foundation in the zone between the B-1 and B-3 shear and the B-2. And so it essentially considered the possibility of a new rupture occurring anywhere within that interval, and then allowing for the probability of that in proportion to the width of the GETR foundation.

Q With regard to inputs to that EDAC report and assumptions made, was one of the assumptions the total offset that might occur at any one event?

(Panel conferring.)

A (Witness Slemmons) No. I don't believe it utilized total offset within the analysis.

Q Okay. I believe it did, though, factor in the total offsets that had occurred in the past; isn't that correct?

A That's correct.

Q Did you agree with the figures used?

A The figures used involved some variation in interpretation. For example, the U.S. Geological Survey position for the time period, for the first time period, differs from the 8000 to 15,000 years utilized in the EDAC report. I believe their figures are something like 2000 to 8000 or thereabouts, and I think that these represent the kinds of variations that two independent, competent soil stratigraphers might have for reviewing age data of this sort. Subject to the possible errors that I have mentioned due to counting back paleo soils, I think the numbers are reasonable.

I am referring to the observed offset data, table 4-1, on page 4-2 of the E. report.

Q Didn't you also indicate in your critique that one of the assumptions you used that might be incorrect is the existence of faults no closer than certain distances from the GETR site?

A I wonder if you would clarify that point. I don't recall that.

Q Yes, it was a badly-phrased question.

Did you also indicate that you were dissatisfied with an assumption in the report that there were no faults within a certain distance of the GETR site?

A I don't recall that comment.

Q Well, you don't -- okay, I guess the report will stand for itself on that.

Mr. Bernreuter, you indicated in one of your reports that the probabilistic studies did not seem to fit the spirit of Appendix A, Part 100. Can you indicate what you meant with regard to that, sir?

A (Witness Bernreuter) Well, what I was saying was, or trying to say there is that when you do a probability study, like TERA did or GE did, particularly the type of study that TERA did, one gets a probability that you get one-meter offset, at some other probability you get a two-meter offset, and so on, and it becomes then very difficult to choose a given number as required by -- for design purposes from that type of analysis.

If I choose one meter, which is in the 10^{-4} range, somebody can say, "Well, why not 2×10^{-4}," something like that, which would give you the meter and a half, and I was trying to indicate because of the difficulties, it becomes very difficult just to choose a number, a hazard number.

What one really needs, then, in the spirit of Appendix A, or actually in the spirit of our analysis, is really to do an entire risk analysis, in which you factor in all possible hazards to the site in response to the structure, to all those hazards and then you can finally come up with in a sense a final risk number to the public which then you can judge whether that is acceptable or not acceptable, which would then account for some probability of having a two-meter offset as well as a probability of having no offset. Normally such analyses are not carried out for particular power plants, because it's very difficult to do that.

So I said then I'd try to go to the spirit of Appendix A in choosing a reasonably worst case from these numbers, and so you only use basically judgment to try to locate yourself and choose what offsets you should then use for your design value, and so it seemed like a probability analysis was telling us that around one meter was about a 10^{-4} type of event, somewheres on that side, and that was also tempered by the geologist's observations in most trenches that the offsets seemed to be occurring as one-meter offsets.

Those two coupled together seemed like this is the type of number that you should use for design, to couple together, and that was the only way you could get by Appendix A.

I don't know whether I've answered your question or not.

A (Witness Vesely) It's important to point out, too, that the larger the offset, the smaller the probability, and the probabilities decrease by orders of magnitude.

A (Witness Bernreuter) That's correct, yes.

Q By the way, I take it that the numbers we have been using, 10^{-4}, and in some cases, 10^{-6}, these are all per year probabilities? Is that correct?

A (Witness Vesely) That's right, yes.

A (Witness Wight) That's correct.

JUDGE GROSSMAN: Judge Ferguson has some questions.

BY JUDGE FERGUSON:

Q Let's start off with a very few simple questions.

Mr. Bernreuter, in your testimony, you indicate on page 3 that reasonable changes in the magnitude of the maximum credible earthquake factored into all analysis -- all right, let me paraphrase that.

Factored into all analysis and the strain rate introduced a factor of only two or three change in the probability value. I simply left out the parenthetical expressions there.

I simply want to establish the fact that you used as the maximum credible earthquake as what? What was the maximum credible earthquake?

A (Witness Bernreuter) The base case that we based our analysis on was magnitude 6, so we ran it up to magnitude 6-1/2.

Q I think Mr. Wight suggested that -- that's the TERA analysis, is that correct?

A That's correct.

Q Do you have any feel for what that factor would be if the maximum credible earthquake were larger than 6?

A Well, we ran it up to 6-1/2, and that reduced the probability something like say for one meter of offset to 2×10^{-5} per year to something like 1×10^{-4} per year, about a factor of -- a little less than a factor of two. But there are some interesting tradeoffs, unless you change a lot of the other parameters along with it; as you make the maximum magnitude larger and larger, it's not going to progressively affect the probabilities that much, because it tends to reduce in the number of earthquakes that you are having at the site. Because you could get larger offsets for any given event, there would be no need for so many events to have the same total offset in the hills and such like.

And so I guess -- my point is that there are interesting tradeoffs, and so the parameters become somewhat desensitized. It changes the probabilities, to change the parameters.

Q You were making a statement a moment ago, and Dr. Vesely commented on your statement, and Dr. Vesely, your comment was -- and we are talking about offsets now --

your comment was the larger the offset, the smaller the probability. Is that correct?

A (Witness Vesely) That's right, sir.

Q What's the basis for that statement?

A The TERA model calculations which give probabilities vs. offset size, not simply pure probabilities, and that is again based on data and models where the larger the offset, the smaller the probability.

Q Does that agree, in your opinion, with what has been observed in geologic events?

A Yes.

Q When you said yes, we don't observe probabilities, of course, in geologic events; is that correct?

A That is right. We observe occurrences, but the more frequent occurrences, of course, are the smaller size offsets, the smaller magnitude.

Q Dr. Vesely, are you familiar with the, or have you reviewed the EDAC report?

A No. As my testimony -- as in my testimony, I reviewed only the reports that are stated in Section B of the Staff's May 23rd, 1980 SER, which does not include the EDAC report you're speaking of.

Q Have you reviewed it, Mr. Bernreuter?

A (Witness Bernreuter) Yes, I reviewed EDAC's report.

Q Okay. There has been some testimony given already in this hearing regarding the fact that that EDAC report has been characterized as a one-dimensional model. Are you familiar with that testimony or that characterization?

A I believe so. That was Dr. Brillinger?

Q Yes.

A Yes.

Q One of the things that was discussed was, it was thought that that did not really explain reality. Do you agree with that, or do you not agree with the characterization?

That is to say, do you feel that a study of more than one dimension should have been carried out?

A Well, they could have easily carried out more than one dimension, as Dr. Slemmons did in his report, which is listed in the SER. He showed the effect of including the depth of the foundation, what effect that would have on the probabilities that they calculated, and I think he came up, as I recall, with a maximum factor of 2.3 difference. That is more probable -- yeah, the probability should be multiplied by a factor of 2.3, and so that's a sort of insignificant change.

So from that point of view, there was a deficiency in the EDAC report.

Q Would that be including one more dimension or two more dimensions?

A That would be primarily the depth of the foundation and in the geometry of the faulting. There is still a possibility for one further dimension to be included in there, but it's not quite clear how that could be factored in.

I think we agree that those changes would be very minor.

Q Would not affect the probability?

A No. Well, might change it to 2.3 -- well, I shouldn't put it in numbers, but it would be a very small additional change.

Q So it's your feeling that the one-dimensional analysis, together with the amendment that Dr. Slemmons has done, is satisfactory?

A Yes. We had -- when we reviewed it, we had some reservations about the EDAC report which are documented in our various reviews, but in the end we felt -- this is in part -- these reservations led to us recommending to the Nuclear Regulatory Commission that we do an independent analysis, which the NRC concurred, and thought that was a good idea, and that led to the TERA analysis, trying to take a different approach.

We felt that one of the deficiencies or problems with the approach taken by EDAC, there's sort of a fortuitous combination in any of those models you take, where the sensitivity of different parameters tend to cancel out on one another, and so that the results become very insensitive to changes in the parameters, and we thought in some ways it's very comforting, but on the other hand, it could just be an artifact of the simplified assumptions that are being made, that have to be made in an analysis like this.

So we thought it would be very worthwhile to take a totally different approach, where we tried to factor in, shall we say, the dynamics of what's going on in the fault into the model, to see where this would lead us, and then when we finished doing that analysis and TERA's analysis, we found it very comforting that they were in general agreement with the results that EDAC and Jack Benjamin & Associates were getting and we found this, you know, to be confirmation of their analysis and our analysis comforting.

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Q Let me continue along those lines for just a moment.

The impression that I get from hearing these analyses discussed is that although you approach the problem from different directions, it seems that the end result is always the same, or very nearly the same. Is that correct?

A Within an order of magnitude, that is reasonably correct; yes.

Q You even, I believe in the Appendix of the SER, make the statement that you were in -- you felt that there were some very serious errors in one of the analyses, but because of self-cancelling, I think those were your words, mistakes, the number comes out to be about what you think the correct number to be. Is that a fair characterization of what you said?

A That's correct; yes.

Q What do you feel that these analyses are so insensitive as to whether or not you make errors and they cancel one another? I am just trying to get some feel for the amount of weight we should put on the analysis.

A Yes. What we felt is that it was necessary to reinterpret or possibly Dr. Vesely also commented that he didn't necessarily agree with our total characterization of the EDAC and the -- we criticized two reports, the EDAC report and the Jack Benjamin Associates' report. I believe that we found that we were suggesting the self-cancelling areas in the Jack Benjamin Associates report, if not in the EDAC report.

Q My question is: If you approach the problem from many different directions, or several directions, and even if you make errors in the approach and you always wind up with the same answer or very nearly the same answer --

A Yes. And I was about to explain that we interpret there were self-cancelling or possible errors in the Jack Benjamin Associate reports. It might have been possibly a misinterpretation on our part of what the Benjamin Associates people were trying to say, that they did not say it very well, and that they were not precise in their definitions, and they meant something slightly different than what they actually said. So we have carried it to the conclusions and had to conclude that they had made errors.

But going back and reinterpreting their results, as Mr. Brian Davis did in his detailed analysis which I think is Appendix C of the TERA report, it showed that by reinterpreting what they had written, at least from our viewpoint, that the equations then worked out to be reasonably correct.

A. (Witness Vesely) I would like to add something here. In the Staff's review of the models, it is not the errors in the mathematical sense where I calculated or multiplied wrong. It was different interpretations. There were different models and a lot of different assumptions, and when we say that they came up with the same answer, they came up or agreed within one to two orders of magnitude. That is a factor of 10 to a factor of 100.

In a traditional geologic sense, that is a very large spread. These different models and different assumptions caused a factor of 10 to 100 difference, but in risk analysis and probability analysis, as the Staff has used these analyses, a factor of 10 or a factor of 100 is not a large spread when you interpret probabilities.

So that the models in details and contributions did give differences and did not end up in the same results, but they gave the same order of magnitude; and I think that is what is important here. The order of magnitude kind of agreement. Anything better than that, no, the models gave different results and different contributors, and different interpretations. But to the order of magnitude precision, all these different approaches did agree to that precision, which is a very gross precision.

Q. One or two orders of magnitude?

A. One to two orders, a factor of 10 to a factor of 100. But they all lay in 10^{-6} to 10^{-4} per year.

Q. Let me direct your attention, anyone on the panel, to a statement made in the TERA Corporation Appendix to the SER. It has to do with predicting the return period of ruptures of one meter. To be specific, it is on page 3-20 of, I believe, Appendix F.

I simply want you to help me to understand what this sentence says, and I quote. It says: "As we can see from Figure 3-2, the model predicts ruptures of one meter with a return period of roughly 19,000 years. Age dating soils in both B-2 and B-1/B-3 trenches indicates that one meter displacements have occurred within the last 20,000 years."

Now is it correct to paraphrase that sentence to say that every 19,000 years the chances are that we are going to have a one meter displacement; and one has in fact occurred in the last 20,000 years?

A. (Witness Wight) Yes, that is correct.

Q. Can you, Mr. Wight, tell us when the next one will occur, if one has already occurred within that return period?

A. I cannot.

A. (Witness Vesely) One has to be careful in these return periods. It's not a periodic -- these phenomena are certainly not periodic in the sense that one recurs every 19,000 years, and having one recurred I expect another one in 19,000 years.

We are speaking of an average interval between events. This average has large uncertainties. The uncertainty can be as large as the average itself. So that you can get anything between zero and 38,000, for example, comprising 90 percent of the events that might occur.

So I think this is the reason why probabilistic analyses are useful here, in that these events are not deterministic and are not periodic, and there is a great deal of randomness as to when they occur.

Q. "Return period" does not mean "cyclic" or "periodic"?

A. No, it does not.

Q. That's very curious.

Dr. Slemmons, let me return to something that you said earlier today, if I understood you correctly. You said that the San Fernando earthquake is not a good model to use to talk about events on the Verona?

A. (Witness Slemmons) It provides a very useful analogue, but it is one that is not scaled in exactly the same proportions. For example, in many places along the San Fernando fault zone we find displacements of more than a meter, and up to a maximum of 2.5 meters. The several trenches at GETR show a maximum offset of one meter, if we ignore the data from T-1. I am referring primarily to the Trenches I have examined at the B-1/B-3 and B-2 and H.

So the scaling of the amount of displacement seems to be less, at least for the most recent events, on those three shears. The San Fernando Fault Zone is part of the Sierra Madre Fault Zone, Santa Suzanna System, which is perhaps 100 kilometers or more in length. It does seem to be rather segmented. And the length of the segment that broke in 1971 very clearly as a length of about 15 kilometers.

We have no hard data for any lengths of that sort for the GETR -- or for the faults of the Verona system. So the length seems to be less at GETR. The hills, the amount of offset of units that are two or three million years in age which are present in both areas are much more highly deformed, and much more markedly offset in the case of San Fernando. They are -- on the footwall side of the fault zone, there is 10,000 feet of downward movement of those tertiary materials, and as I recall about 1000 feet or more uplift in the hills, in contrast to the much smaller displacement of units of similar age at GETR.

So that I think that the 6.4 magnitude for the San Fernando event represents a larger kind of event than is likely to occur at the Verona Fault Zone.

Q. So you would think it would be improper to assume that surface displacements like those that occurred in the San Fernando event in 1971 would not be comparable to what you would expect on the Verona?

A. The probability -- and I'm using it in the geological sense -- is much lower that you could get a 2.5 meter offset. The worldwide data has scatter and standard deviations that suggest a spread that could, for a magnitude 6.5, give 2.5 meters. But the much greater likelihood is for something of the order of a meter, a meter and a half. And certainly the hard geological data at Trenches B-1/B-3 and B-2 are suggestive of a lower magnitude.

JUDGE FERGUSON: Thank you, Dr. Slemmons. I have nothing further.

BY JUDGE GROSSMAN:

Q. I just want to ask one question. Those 2.5 meter offsets at the San Fernando earthquake, were they at the zones of thrust faulting?

A. (Witness Slemmons) Yes. I may need to be corrected on that. I believe the 2.5 meters is on the western Sylmar segment, which had a larger strike/slip component, although it is on a reverse fault system.

I observed after the earthquake in one of the Canyons, one of the thrust faults have had about 2 meters of height, and some significant, perhaps a meter or two, strike/slip component as well. And so on that particular zone which was more of a reverse fault, there was something comparable to the 2.5, but I don't think it is a 2.5 figure as plotted by Bob Sharp in his report.

JUDGE FOREMAN: I just have one question.

First a point of clarification, Mr. Edgar. It is not clear to me -- and it may have been pointed out, though -- as to whether the information presented by Dr. Reed represented that from the EDAC report, or the Benjamin report, or aside from any of those?

MR. EDGAR: There is a great deal of confusion, because the terms "EDAC report" and "Jack Benjamin Reports" and other reports were used interchangeably. I think I am going to have to go back through -- there are three reports in question, one of which was reviewed by Mr. Bernreuter, one of which and only one was reviewed by Dr. Slemmons, and Dr. Vesely reviewed all three. I can straighten it out with a few questions, I think; or I can hand the panel the documents over lunch, and I think they could comment on it and get it straightened out after lunch. But we overlapped three different things at various times.

JUDGE FOREMAN: You see, you made reference in that testimony to answers to discovery questions and said the references were given in those answers, and I didn't happen to have those references. That is why I didn't understand where that information came from.

MR. EDGAR: I have given the panel all three documents earlier this morning, and maybe they could look at it and explain which comments were made relative to which documents.

BY JUDGE FOREMAN:

Q I have a question of the panel, and it is a little different thrust, if I may use a pun --

(Laughter.)

-- a little different thrust from what was being asked, and I suspect that Dr. Bernreuter and Dr. Vesely are the people that might want to speak to this. It deals with, in a sense, the significance or the meaningfulness of the probability numbers.

A good deal of attention has been directed to determining the probability of a surface offset beneath the reactor, and giving the impression that of course that this is of very great hazard importance. But it seems to me that it is also of very great significance with respect to hazard, and in turn with respect to the setting of design parameters to know about offsets away from the foundation of the reactor because in the design parameters the offsets are combined with acceleration considerations. And acceleration considerations come about whether the event occurs under the reactor, or whether it occurs away from the reactor.

So to me it seems important to evaluate the meaningfulness of offsets away from the reactor in that context. My question is: In the considerations of NRC, what is the relative significance, if you can, of probabilities for, in this sense, probabilities relating to offsets away from the reactor versus those occurring under the reactor? How do you weigh those? Or is that too vague a question?

A. (Witness Vesely) The GETR analyses did not explicitly compute consequences given offset occurrences in nuclear reactor analysis. They sometimes are. In our review, the probability of an occurrence of an offset near GETR, in the sensitivity study that was done either by GE or by TERA, increased the probabilities by anywhere from a factor of 4 to 10. So in the field around GETR, you came up with, instead of with 10^{-6} to 10^{-5}, you came up with 10^{-5} to 10^{-4}, the 10^{-4} again being an extreme case where you postulated all the faults being under. Those probabilities, again to order of magnitude, are not much different than probabilities under the reactor. Again, it is my opinion, based on the probability analysis, that the probability analyses, the probabilities obtained, and particularly on the conservative assumption, is 10^{-4} to 10^{-5} and are comparable to those probabilities that are obtained for nuclear reactors which are deemed acceptable.

So that again just looking at the probabilities, you would say -- we would say that the probabilities are acceptable. We would deem GETR to be acceptable from a risk standpoint. Care must be taken, again as pointed out by the Staff, that the probabilities are only one factor that has to be brought into decision-making, along with geologic factors of course that are important, as well as other structural factors. The acceptance on the probability point does not come from any specific number, but the range that the probabilities below 10^{-4} on a conservative basis that the Staff has used that on other evaluations such as power reactors and have judged those probabilities to be acceptable.

Again, it is very important to bring up the point that how the probabilities are weighed against the other factors and the other analyses is based on the judgment of the Staff. There have been cases in which probabilities have been low, and yet decisions have been made to modify or correct designs in spite of the probabilities. There have been other cases where the probabilities have been taken as being the principal factors. The Staff is still trying to resolve a more systematic approach of how probabilities enter into decisionmaking. Until that time, it is a case-by-case decision.

Q You wouldn't want to give some impression as to your feeling or your evaluation as to whether an offset beneath, directly beneath the reactor is of much greater hazard than an offset some distance away when one has to consider the fact that the offset from a distance away has to be considered along with vibratory or shaking motion?

A Well, it is certainly my opinion that an offset under the reactor is certainly a greater threat and poses a potentially greater risk than an offset away from the reactor. And I think that this is why our concern for the analysis concentrating on offsets occurring under the reactor.

But in addition to doing sensitivity studies, looking at the probabilities of offsets occurring in the field not only around the reactor but in the whole area between the shears, this is where the 10^{-4} probability comes from. This is why our request for doing sensitivity studies which included offset occurrences not only under the reactor, but in the whole area between the shears.

If you want to pin a hard number, the hard number that seems to come out of all these probability analyses is 10^{-4}. What you can say is it is below 10^{-4}, and that is about all; that the number that you choose below 10^{-4} depends on specific assumptions, but the 10^{-4} seems to be a fairly hard upper bound that comes out of all of these different models.

Now that includes the probability of an offset occurring in the field, and not only under GETR.

BY JUDGE GROSSMAN:

Q Dr. Vesely, it is very interesting that everyone on the NRC panels caution us against relying primarily or solely on the probabilistic studies, and everyone seems to think that we ought to take both a deterministic and probabilistic study into account. But the fact remains that on a deterministic basis the NRC recommended that the plant not resume operations; and that it was solely on the basis of the probabilistic studies that the NRC has taken a different position. Isn't that correct, Dr. Vesely?

A. (Witness Vesely) I would have to defer to the Staff on that, to Chris and Dan.

MR. SULLIVAN: I think what the Board would probably want right now is testimony, rather than a statement of position. I don't believe the Board's characterization was accurate.

JUDGE GROSSMAN: Well, let me ask Mr. Swanson. Are you going to put on testimony indicating to what extent we ought to adopt the probabilistic analyses, and to what extent we ought to rely upon the deterministic? Because I don't see anything in the Staff report which indicates how much reliance ought to be given to either, except to the extent of the conclusions that I have stated now, that it appears that on the basis of the deterministic you go one way, and on the basis of a probabilistic you go the other way.

MR. SULLIVAN: Well, there is a statement as to how it was used in the Staff analysis. It was in the section prepared by Dr. Justus and Dr. Jackson on page 15 of Section A of the May 23rd, 1980, Safety Evaluation where there are a couple of sentence which deal with that. It reads:

"Deciding the proper surface offset design basis for a facility within a fault zone by use" or as Dr. Jackson more correctly interpreted it, "the sole use of the proposed probabilistic methods is not favored by any of the geological personnel involved in the review of this site. Several specific areas of concern were outlined above." And they point out some of the uncertainties.

However, it was used -- Well, starting on the next page, the top of 16: "The probabilistic calculations do, however, provide a frame of reference for making a judgment on geological offset parameters that are not at the upper bound for the dispersion of the available data. Furthermore, they help provide a perspective of the type of data which is needed and which is most critical to making a conservative estimate of the surface offset displacement."

That, I think, summarizes the way in which it was used by Dr. Jackson and Dr. Justus in factoring that into their overall assessment in assignment of design values.

MR. EDGAR: I would also refer the Board to Dr. Jackson's testimony the other day. I happened to have reviewed the transcript pretty carefully over the weekend, and I just don't read him as saying that the sole basis for the change was probability. I believe he referenced a number of points of information that led to that change.

If that is the Board's conclusion, I would like a chance to be heard on that on briefs before that conclusion was reached.

JUDGE GROSSMAN: No, that wasn't a conclusion; that was just throwing it out for comment on the part of the panelists. I think Dr. Justus has a comment on that whole area.

MR. SWANSON: If the Board would like, perhaps Dr. Justus could take another crack at explaining this point, perhaps immediately after we return from lunch. I think it is very important that the Board get a correct perception of the role that it plays.

JUDGE GROSSMAN: Fine. Why don't we have that when return at 1:45.

(Whereupon, at 12:25 p.m., the hearing was recessed, to reconvene at 1:45 p.m., this same day.)

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AFTERNOON SESSION

(1:45 p.m.)

Whereupon,

DAVID SLEMMONS,

LARRY WIGHT,

DON BERNREUTER and

WILLIAM E. VESELY

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

JUDGE GROSSMAN: We are back in session.

I believe Dr. Justus was going to explain to us how the probabilistic studies influenced the deterministic evaluations made.

MR. SWANSON: If we could, this is an important point. I know it's been gone over before, but I think at this point it might be helpful to lead off with a brief historical perspective from Mr. Nelson, the project manager, who does have the continuity in this case, and then I would ask Dr. Justus to again summarize the factors that they considered important in arriving at the current Staff position of one meter of offset.

MR. NELSON: I would like to just present a general overview of the history since the GETR was shut down in October 1977.

The first year, '77 through '78, was primarily spent arguing the origin of the offset of observed in the trenches, earthquake vs. tectonic origin was the issue.

During that year, GE also proposed structural analysis assuming a one-meter offset, and that way, as far as timewise, those two values, or the value of one meter and the origin of landslide, were presented.

In 19-- in the fall of '78, the Staff did come out informally with its position using the comparison for lack of site-specific data with San Fernando, noting that it was conservative, but this was the best source of information we had for taking a position at that time.

And that resulted in the postulating of two and a half meters surface offset at the GETR. At this time, the fall of '78, GE proposed an extensive trenching program which resulted in the gathering of site-specific information for GETR.

Following this trenching program, and also GE presented probability arguments in the, I think April 1979. Following our review of the trenching results, the Staff still felt or more strongly felt that tectonic was the origin of the features observed at GETR, and GE in, I guess, pursuing their trenching program, was trying to demonstrate that it was of landslide origin.

So at the time, October or September 1979, with the Staff feeling more strongly that the faults were of tectonic origin, with the consultant, I guess, input representing greater than one meter, our review of the probability studies showed that we had difficulties with those initial studies, and we didn't feel that they would significantly affect our conclusion.

The Staff again reverted back to its comparison with San Fernando as being the one with which it was most comfortable. That resulted in the two and a half meter surface offset.

In November of '79, we went before an ACRS subcommittee. The Staff left that subcommittee with the strong feeling that, one, it was being a little too extreme in its use of the San Fernando data, and that it should consider the probability studies in conjunction with the review of geologic parameters from the site-specific research program or trenching.

In the pursuing of the probability arguments, as well as a review of the San Fernando data, to, I guess, make a more appropriate comparison with the Verona Fault resulted in the Staff's May 23rd, 1980 SER, which has the position of one meter surface offset.

MR. JUSTUS: To answer your earlier question, which further considering -- in how we considered the importance of the probabilistic analyses in rendering our judgment --

JUDGE FERGUSON: Dr. Justus, might I interrupt for just a moment?

MR. JUSTUS: Yes, sir.

JUDGE FERGUSON: Before we get too far away from the statements that have just been made, sir, you had indicated that coming out of the ACRS meeting, the Staff had a modified view of the 2.5 meter offset. Is that what you said?

MR. NELSON: No, sir. We didn't modify our view at the ACRS subcommittee meeting. We came out with the impression that we were being viewed as much too conservative, or extreme, in taking in that position.

JUDGE FERGUSON: Can you recall what it was about that meeting that made you feel that way?

MR. NELSON: I don't remember specific ACRS comments. However, at that meeting the Licensee did present its probability analysis, or the results of its probability analysis, and the Staff presented its comparison with the San Fernando data, and the Verona Fault, or drawing the information from the San Fernando data that we did in making our postulated two and a half meters.

I would have to go back to the record itself, though, to come up with the specific comments that were made at ACRS.

JUDGE FERGUSON: Are these comments, before we transfer the microphone there, the comments that you are referring to, were they made -- or can you tell us who made the comments?

MR. NELSON: No, I can't.

JUDGE FERGUSON: I'm trying to understand, sir, what it was, what happened, what occurred at that meeting that gave you a feeling that you were a little too conservative in your estimate?

WITNESS SLEMMONS: I believe it was primarily the chairman -- I believe it was the chairman of the ACRS subcommittee, Dr. Okrent, from UCLA that gave a very strong endorsement of the need for probabilistic approach to supplement the deterministic approach that had been used.

And as I recall the meeting, it was almost a mandate that you will do this for future studies of this type.

JUDGE FERGUSON: Is that your understanding?

MR. NELSON: Yes, it is. I would just add that Dr. Kerr was the chairman of that subcommittee, and Dr. Okrent was a member.

JUDGE FOREMAN: It has been said that new information came up at the ACRS meeting that led to further investigation, and to the Staff reviewing its position.

Was there new information in addition to a request that you review -- that you consider probabilistic information, probabilistic studies? Were there other kinds of new information that came out?

(Staff conferring.)

MR. NELSON: I'm not sure what that new information was. I know that one of the problems was that a lot of the information was presented in a different form that had been seen previously, and I know of that term "new information" being related to some comments by the USGS, but I'm not sure of the specific points that were new or brought out.

I'm sure some things were brought at the committee that had been submitted in writing to the NRC for review, but I'm not aware of any specific points.

MR. JUSTUS: I can add a little to that so-called new information. Although I was not present at the meeting, I did review the transcripts and subsequent discussions. The meeting was to discuss, among other things, various aspects of the geological work that had been done, especially that material that had been submitted in writing to the NRC and its consultants for review.

The presentation given by the ESA, consulting group to GE, at that particular meeting contained new interpretations, apparently new approaches concerning landslide hypotheses, trench data, and the like, which completely -- well, which to a certain extent, at least, was a surprise to the NRC Staff and its consultants.

I think the impact of that presentation by ESA to the ACRS also amounted to or led the geologists on the board, the ACRS, to, I think, feel that the Staff had been extremely conservative in establishing the 2-1/2 meters point of view.

That, by the way -- I should reemphasize the 2-1/2 meter statement in the September '79 SER was not a final Staff licensing position. It was an input, essentially a status report up to that point.

That's, I think, all I can add concerning the ACRS meeting associated with the input of new information to that group.

(Board conferring.)

MR. EDGAR: May I make one comment, just to direct the Board's attention to transcript pages 1389 to 95 for future reference? Dr. Jackson -- this information is not presented in the historical context, but it summarizes the substantive factors which bear on the Staff's decision to select one meter as the criterion.

I read that the other night and went up at lunch and located that. It is a very succinct summary of a very complex subject, but in my mind, it is worthy of citation in that it does put the Staff's position in the correct substantive perspective.

MR. SWANSON: Actually the type of summary that was just referred to is exactly what Dr. Justus was about to do when the Board required a further explanation of Mr. Nelson's statement, and if the Board would like Dr. Justus could summarize from --

JUDGE GROSSMAN: Well, if what you're going to do is just a repetition of pages 1385, et seq., there's no point in having it again and taking our time. If you have anything to add, especially with regard to the effect of probabilistic studies on the determinations, we would welcome it. But if it's merely repetitious, there's no point to it.

MR. JUSTUS: I have some statements to make that aren't repetitious, but do overlap, to a certain extent.

JUDGE GROSSMAN: Okay. Well, fine. Give us the whole thing, then, so that we don't get it piecemeal.

MR. JUSTUS: The probability studies demand that we reevaluate all of the input data to the extent that the probability analyses suggested to us that we did not need to consider only the largest or maximum values that had been determined over many decades of study, of worldwide or even local data.

The worldwide data set referred to is based on

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the largest values for a particular event. We reinterpreted or reassessed our use of the maximum offsets and worldwide data sets, maximum magnitude relationships, and felt that we didn't need to utilize that, those maximum values, particularly as simultaneous to the probability methods we were reevaluating on our own, the San Fernando data, which was heavily relied upon for the initial input where we stated 2-1/2 meters was a conservative value.

Indeed, 2-1/2 meters was the maximum value, the largest achieved at one point on the 12- to 15-kilometer long San Fernando rupture.

Most of the readings -- in fact, with a much more detailed and statistical reanalysis of the data -- one meter is the most characteristic rupture at the surface -- surface offset of the San Fernando, and that is confined, we feel that one meter of offset can be applied to a more or less narrow zone, a narrow zone on the order of five meters wide, and we need not consider the whole 2-1/2 meters found at one point to cross the entire zone to be representative. And besides, as we further have elaborated, and I certainly won't repeat that -- it's been repeated multiple times -- the conservatism of the San Fernando is, I think, now established, and the maximum would be comparing an extreme.

The trench data, which was known for the

September '79 input status report, showed that one meter presumably was exceeded, actually based on the trench data -- I'm referring now to T-1 -- and that -- we gave weight to that upper bound or upper or largest observation.

Again we recognized in reassessing the data at hand that the characteristic values, the two or three feet, or perhaps one meter which is more than that, more than three feet, actually, is the more appropriate value, and we felt when comparing all of the factors, that went into our final judgment that compounded values that I spoke of when the geology-seismology panel of NRC was introduced -- there were multiple conservatisms in that one meter or the two to three feet observations would be appropriate in that case.

So, the distribution of values that we had to consider, we initially considered at the maximum or certainly at the -- let's say the upper or perhaps extreme tail end of the spectrum. The probability analysis suggested to us, in addition to the ACRS, geological, geologists, consultants' reports and letters, that it's actually the characteristic values of these data, data sets, that we should be using as we are compounding or multiplying conservatisms in the final analysis.

I will repeat, if you don't mind, one point that Bob Jackson did make, and that is it's more complicated even than that.

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JUDGE GROSSMAN: Thank you.

I think we can now return to the probabilistic panel, and I believe it's time for Mr. Swanson's redirect.

REDIRECT EXAMINATION

BY MR. SWANSON:

Q I just want to clear up one point.

Dr. Slemmons, in one of your responses, you referred to a term "tectonically related." I just want to make sure that we understand what you meant when you were talking about that term in the faulting. Did you mean that in a regional sense, in terms of faults sharing a common external regional stress? Or did you mean to imply that you were imposing a structural interconnection with various faults?

A (Witness Slemmons) I specifically did not include the term "structurally related." I had the connotation of a regional balancing and disturbance in the regional stress and strain fields, and I did not imply any simultaneous type of activity between the different structures involved.

Q Thank you.

MR. SWANSON: That's all we have on redirect.

JUDGE GROSSMAN: Fine. Thank you very much.

MR. EDGAR: Judge Grossman, I have one item that I'd like to straighten out with the Board's permission, and that was there was some confusion about the so-called EDAC report and the Jack Benjamin report, and I wonder if I could get that identified to exhibit numbers, and get the panel to explain which documents they were talking about when they testified, because I don't think it's going to be clear in the transcript.

JUDGE GROSSMAN: That's fine. Is that what Mr. Bernreuter had in mind?

WITNESS BERNREUTER: No, I had just one slight point I wanted to make.

Mr. Wight pointed out to me at lunch that the question was directed to me, or I responded to a question about how much change in the probability, increasing the probability of occurrence from one meter of offset would occur if the upper magnitude was changed from magnitude 6 to 6-1/2, and I think I indicated around a factor of two or so, or a little more. And actually, the particular number I happened to remember at that time, I envisioned or thought that I had, the particular factor of two not only had a half a unit of change upper magnitude cut-off, but it also had some increase in strain rate.

I think Mr. Wight had testified just slightly earlier that just changing the magnitude from 6 to 6-1/2 resulted in a 36 percent change, and that's correct, and I just sort of remembered the figure slightly wrong, and went to the very bottom line, where we increased the strain rate from .02 to .03, and the upper magnitude cut-off from 6 to 6-1/2, and that gave a factor of two.

I just thought I'd bring that to the Board's attention, to just get those two straightened out.

MR. SWANSON: Actually, Mr. Edgar, there's something I meant to do, and that is to clarify what documents the gentlemen did review. Actually it's in the written record of the SER what documents they reviewed, but I think there was at least one point of confusion as to what document.

WITNESS VESELY: Yes, I want to correct that. I did review all three reports, that is Licensee's Exhibit No. 10, Exhibit No. 14, and Exhibit No. 16.

MR. SWANSON: That's the EDAC report?

WITNESS VESELY: EDAC report, and the Jack Benjamin report, as well as GE's responses to NRC's questions. Those are the three reports.

MR. SWANSON: And those are the documents --

WITNESS VESELY: Those are the documents.

MR. SWANSON: -- referred to on your cover page of the input to the SER?

WITNESS VESELY: Yes.

WITNESS SLEMMONS: My basis of the review is primarily the EDAC report, which is Exhibit No. 10, and the

Jack Benjamin report, Exhibit No. 14, and I believe I also had access to Exhibit No. 16.

WITNESS BERNREUTER: I reviewed all three reports. However, my comments there on Appendix F were dealing solely with the Exhibit No. 14, which was the Jack Benjamin report. Elsewhere in the report, some of the other subappendices, I also referred to the other two reports, but my cover letter dealt solely with Exhibit No. 14.

JUDGE GROSSMAN: Thank you, gentlemen. You are excused now.

(Panel excused.)

JUDGE GROSSMAN: Mr. Cady?

MR. CADY: Your Honor, at this time I would like to introduce into evidence certain exhibits that have been marked. I reviewed the transcript over the weekend and found some exhibits that had not been admitted.

Exhibit No. 1 was the 1978 map prepared by Dr. Herd. That showed the intermediate fault between the GETR and the Calaveras Fault zone.

JUDGE GROSSMAN: Any objection?

MR. EDGAR: No objection.

JUDGE GROSSMAN: Admitted.

(The document previously marked Intervenors' Exhibit No. 1 for identification, was received in evidence.)

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MR. CADY: Exhibit No. 2 was the Licensee's Figure No. 1 attached to its Exhibit 2 that we had superimposed that intermediate fault from Dr. Herd's map onto that exhibit, and we have an excerpt copied and copies were given to the Board and to the reporter, and I would like to offer those into evidence.

JUDGE GROSSMAN: Any objections?

MR. EDGAR: None.

JUDGE GROSSMAN: Admitted.

(The document previously marked Intervenor's Exhibit No. 2 for identification, was received in evidence.)

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MR. CADY: Getting to Exhibit No. 3, the Licensee's Answer to Intervenors' Interrogatories to Licensee, dated 4-3-81, dealing with the amount of investigation performed by Dr. Horvath in reviewing certain data from various earthquakes around the world, and we would like to have those answers to interrogatories introduced.

JUDGE GROSSMAN: Any objections?

MR. EDGAR: I've never heard of Dr. Horvath.

MR. CADY: Kovash. Excuse me.

MR. EDGAR: Oh, I'm sorry. You're introducing the whole set of interrogatories?

MR. CADY: We would like to introduce, to be specific, Interrogatories No. 8 with the answer; 9 with the answer; 10 with the answer; 11 with the answer; 12 with the answer; and No. 7 with the answer.

MR. EDGAR: No objection.

JUDGE GROSSMAN: Have you Xeroxed or reproduced those?

MR. CADY: Yes, I have, your Honor.

JUDGE GROSSMAN: You have? Admitted.

(The document previously marked Intervenors' Exhibit No. 3 for identification, was received in evidence.)

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MR. CADY: And Exhibit No. 6, which is a list of the documents that Dr. Brillinger reviewed prior to coming to testify here today.

MR. EDGAR: No objection.

MR. SWANSON: None.

JUDGE GROSSMAN: Admitted.

(The document previously marked Intervenors' Exhibit No. 6 for identification, was received in evidence.)

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MR. CADY: And last is the testimony of Glenn Barlow that was submitted as an offer of proof.

JUDGE GROSSMAN: Well, that, I understand, has been objected to.

MR. EDGAR: Yes, and we maintain that.

MR. CADY: Right. We note the objection. We just wish to have it admitted subject to further ruling by the Board.

MR. SWANSON: The Staff also has objected to that.

MR. EDGAR: It's just a minor matter. The document was never marked for identification, apparently, according to our check of the records. So we might want to be sure that's done.

MR. CADY: Glenn Barlow's testimony?

MR. EDGAR: Yes. It didn't get marked. We find no record it was marked for identification, and we would have no objection to having it marked for identification, obviously.

MR. CADY: Well, then, could we have it marked for identification as Intervenors' No. 7?

JUDGE GROSSMAN: And do you have the requisite copies?

MR. CADY: Yes, I do, your Honor.

JUDGE GROSSMAN: Fine. So marked. And the Board renews its ruling, reaffirms its prior ruling with regard to that, which is not to accept the exhibit as admissible evidence, but the Board may reconsider at the end of the testimony.

MR. CADY: Thank you very much.

(The document referred to was marked Intervenors' Exhibit No. 7 for identification.)

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JUDGE GROSSMAN: I believe, Mr. Edgar, we are up now to the structural panel, unless there is some more business.

MR. EDGAR: One other item. Have you made an offer on No. 9?

MR. CADY: No. 9 was the map that Dr. Herd brought out on examination from Mr. Barlow, and it is a larger map of Dr. Herd's 1978 map. The Intervenors' Exhibit No. 1 was a scaled-down version of our proposed Exhibit No. 9. Exhibit No. 9 is more extensive and more detailed and shows the reservoir to the southwest of the GETR facility, and I guess there was a line of questions as to the possibility of the Las Placitas Fault going towards the reservoir.

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In reconsideration we would like to have that map marked as Intervenor's Exhibit No. 9, and admitted.

JUDGE GROSSMAN: Any objections?

MR. EDGAR: No objection.

MR. SULLIVAN: No objections.

JUDGE GROSSMAN: Admitted.

(The document referred to was marked as Intervenor's Exhibit No. 9 for identification, and received in evidence.)

MR. SULLIVAN: Just so we have it clear, we have two Darrell Herd maps, and I just want to make sure I've got the right numbering for that. The one you just described could easily have been the '77 map?

MR. EDGAR: Yes. Exhibit No. 9 was the '77 map. Exhibit No. 1 is a section that was Xeroxed of '78.

MR. CADY: Okay.

MR. SULLIVAN: Okay, where does Exhibit No. 4 fit in, then, because I've got that marked as the '77 map, also.

MR. CADY: Exhibit No. 4 was an epicenter map taken from your USGS Open File Report 66-689 that had various epicenters in the vicinity of the GETR site.

JUDGE GROSS: Is that that Sharp report?

MR. EDGAR: No. It was -- it came out of Darrell Herd's Open File Report.

MR. SULLIVAN: Okay, you must mean 77 rather than 66.

MR. EDGAR: It has to be 77-dash-something.

MR. SULLIVAN: 77-689 is the map. That is what I am trying to clear up.

The report that accompanies the map, do I understand it that that is Exhibit No. 4?

MR. EDGAR: Yes.

MR. SULLIVAN: And the map itself -- that is what I am trying to clarify -- is Exhibit No. 9? The Open File map, the one that we provided the copies of, the big one which is 77-689?

MR. CADY: Right.

MR. SULLIVAN: That is Exhibit No. 9?

MR. CADY: Correct.

JUDGE GROSSMAN: Is Exhibit No. 4 in?

MR. CADY: Yes. Exhibit No. 4 is in.

MR. EDGAR: We don't have a record of that.

MR. SULLIVAN: I didn't, either.

MR. CADY: Well, I have reviewed the fro

the transcripts and it has No. 4 marked and entered.

JUDGE GROSSMAN: Well, I don't think it will disturb the reporters too much to admit exhibits on two separate pages of the transcript. So just to be sure, are you offering No. 4 again?

MR. CADY: Yes, I am.

JUDGE GROSSMAN: Is there any objection?

MR. SULLIVAN: No objection.

MR. EDGAR: No objection.

JUDGE GROSSMAN: We will admit it.

(The document referred to, previously marked as Intervenor's Exhibit No. 4 for identification, was received in evidence.)

JUDGE GROSSMAN: Now let me ask Mr. Swanson to refresh my recollection on the Sharp report. Is that an admitted exhibit?

MR. SULLIVAN: No, it is not. It was never offered. It has been discussed extensively, but of course we did not have the author as a witness so it was never offered as an exhibit. It has been discussed I guess as a reference document, just as many other documents have been that are authored by scientists who have not testified, but it is not in the record at this time.

JUDGE GROSSMAN: And does any party want to offer that exhibit at this point?

MR. EDGAR: No, your Honor.

MR. CADY: Let me hold in reservation that offer. I will go back tonight and take a look at the report and confirm it with Mr. Barlow to see if we feel it would add anything to the record, and I will let the Board know tomorrow morning.

MR. SULLIVAN: Again, let's make sure we are clear. There have been two Sharp documents that have been referred to. I am assuming we are talking about the very recent one that the Staff provided to the Board and parties on I believe the second day of the hearing?

JUDGE GROSSMAN: That's the one I was referring to, yes. Is there any other Sharp report that is in evidence?

MR. SULLIVAN: No. But there is earlier Sharp data that was discussed, I believe 1975 as well as at least two or three other reports on the San Fernando that were not in evidence but were referred to.

JUDGE GROSSMAN: Fine. I believe we can proceed now to the structural panel of General Electric.

MR. EDGAR: GE calls to the stand Dr. Garrison Kost, Dr. Harold Durlofsky, and Mr. Dwight Gilliland.

Whereupon,

DR. GARRISON KOST,

and

MR. DWIGHT GILLILAND

were recalled as witnesses on behalf of the Licensee, General Electric, and, having been previously duly sworn, were examined and testified further as follows:

and

DR. HAROLD DURLOFSKY

was called as a witness on behalf of the Licensee, General Electric, and, having been first duly sworn, was examined and testified as follows:

JUDGE GROSSMAN: Could you identify yourselves again for the reporter, starting with Dr. Kost.

WITNESS KOST: My name is Garrison Kost, G-a-r-r-i-s-o-n K-o-s-t. I am a principal with Engineering Decision Analysis Company, or EDAC, and our address is Palo Alto, California.

WITNESS GILLILAND: My name is Dwight Gilliland. I am an employee of General Electric. The address is Pleasanton, California.

WITNESS DURLOFSKY: I am Harold Durlofsky, and I am with Structural Mechanics Analysis in Sunnyvale, California.

JUDGE GROSSMAN: Mr. Edgar?

DIRECT EXAMINATION

BY MR. EDGAR:

Q The panel each has a short oral summary of their testimony to make.

A (Witness Gilliland) The criterion design bases were discussed earlier by Licensee's panel one and two, and has been considerably gone over since that. However, now we are to consider whether or not the facility as modified can achieve and maintain safe shutdown under the design-basis conditions.

Earlier I have made reference to the dramatic difference in size between the GETR and a modern power plant, the ratio being variously 60 to 70 times different. So also goes the decay heat load which is of interest in these considerations now, which for the GETR is about 2 percent, or a little less than 2 percent of that of a modern power reactor.

Within 40 hours after shutdown, it is at a level of about .1 megawatts, which is about equivalent to the heat load one would find in the radiator of a large trailer truck rig when it's in operation.

Insofar as the reactor facility is concerned, there are two requirements that need to be met with respect to the seismic design basis.

The first of these is that the reactor must be scarmmed. The second is that the fuel elements remain covered with water. Now the first of these is achieved by the use of seismic trip switches which are set at .01g. That is for either of two horizontal or the vertical for either of two switches. This set point is at about 1 percent of the design basis value which is .75g as you have heard earlier.

The control rods disengage within 180 milli-seconds, .18 seconds, after the seismic trip switch. Then the reactor is shut down within about .48 seconds, or 480 milliseconds of that same trip time. Therefore, the important systems do operate in advance of any consequential accelerations.

The second requirement is that we must keep the fuel elements covered with water. We do this by assuring two things. One is that the fuel element containers remain intact; and the second, to provide water to make up for the loss due to boiloff and evaporation.

If you would turn to the first figure that is in the handout, it is Figure A-1 and it is on page 32 of -- it is Exhibit No. 22, actually, but it is the first page of the handout. The two containers are located one in the canal, the canal storage tank, and it is on the bottom and to the outside side of that part of the facility in the canal. The second is the reactor pressure vessel, which has earlier been noted to be in the center of the 9-foot diameter pool.

Of course the concrete core structure, which contains both the pool and the canal, must remain intact. The reactor pressure vessel is kept intact by assuring that no consequential loads are induced on it by the piping, and that missiles do not interfere with that integrity.

Similarly for the canal storage tanks, protection is provided so that missiles do not impinge upon it either. There has been a new system added for water makeup. It is referred to as the "fuel flooding system." Conceptually you will see it in Figure D-1, which is the second page of this handout. It is also page 110 of Exhibit No. 22. In it, you will see that there are two separate trains which supply water from a reservoir to the canal and pool. Actually, these lines go directly to the canal storage tanks and to the reactor pressure vessel. The flow is low, approximately 5 gallons per minute.

Then if you would turn to the last figure in that first set, Figure D-2, page 111 in Exhibit No. 22, you see the layout of the tanks in relation to the reactor building. The reservoirs are located on the hills above the GETR such that only gravity flow is needed in order to supply water. Therefore, no power is required. The flow is initiated by the same seismic switch that produces the scram. So a means is provided for an early shutdown of the reactor, and for keeping the fuel elements covered with water.

Now Dr. Kost will describe briefly the structural and mechanical analyses that have been performed for the concrete and the core structure -- that is, for the concrete core structure and for the reactor pressure vessel.

WITNESS KOST: The structural and mechanical analyses were performed to show that these safety-related structures and equipment meet the NRC Staff's design basis seismic criteria. My introduction briefly describes the investigations performed for the concrete core structure, and the reactor pressure vessel. The emphasis of what I describe will be on what happens physically when an earthquake occurs, and I will describe the phenomena in a qualitative fashion, and the actual details of the analyses are given in the testimony and in the various backup documents.

I think it is worthwhile first to review the criteria at this stage. It has been previously shown that the probability of a surface rupture offset beneath the reactor building is so low that the offset should not be considered as a design basis.

Secondly, the soil/structure analysis shows that the fault plane will deflect from beneath the reactor building. But even so, in spite of these two factors, the surface rupture offset was very conservatively assumed to occur beneath the reactor building, and the structures and systems important to safety were evaluated accordingly.

We adopted the NRC criteria which were two. First, all of the earthquake on the Calaveras Fault which would produce a ground shaking at the site with an effective ground acceleration of .75g.

The second criterion was an earthquake on the Verona Fault which produces ground shaking at the site with an effective ground acceleration of .6g, combined with the surface rupture offset of 1.0 meters.

I think it is worthwhile to put these criteria themselves in perspective, and note that the historic earthquakes that have occurred recently at the GETR site have produced maximum ground accelerations in the range of 0.02 to 0.10g. Actually, these numbers were measured on the structure, and the numbers on the ground themselves would be less in the free field.

To give you another idea of the magnitude of the numbers that we are dealing with here, the conventional three-story building in the San Francisco Bay Area would be designed for roughly .2g according to the Uniform Building Code.

I think one thing, too, as we go through the structural investigations, we have to keep in mind here, and that is that we are dealing here with structures and components, piping systems, and so on, that are inherently very tough in themselves. They have significant reserve strengths, and ability to absorb or dissipate energy. We have used some fairly restrictive definitions of the word "capacity" here, and we will have to keep in mind that even if one were to exceed capacity which we don't believe will happen, we are still nowhere near what one would envision as a collapse situation. We have very, very conservative "capacity" definitions here.

Well, I would like to next summarize what we did in examining the integrity of the concrete core structure. I have given you a handout of some larger figures, which are the same as in the testimony here. I would like to refer, first, to page 32, which is Figure A-1, which shows the reactor building concrete core structure. This is the same one that Mr. Gilliland referred to a few minutes ago.

I think it is worthwhile, in looking at this picture here, to note several things about this structure. The core structure is outlined by the dark line. It is shown in the vertical cross-section, as well as the plan view there. The structure itself is 70 feet in diameter. There is very heavy, massive construction, as you can see there. The foundation mat is 4'8" thick. The vertical walls that make up the sizes of the concrete core structure are 6'6" thick.

In sum, it is really a short, squat structure. The height-to-diameter ratio for that portion that is above grade is about .65, and it is roughly one-third embedded, so it is well embedded in the structure. It is noted that structures of this type respond well in earthquakes. The motions are not amplified very much as one goes up to increasing levels in the structure.

If you will refer, then, to Figure A-6, which is on page 39 of the testimony, this figure here is meant to illustrate the effects of ground shaking on a typical building. Here in this Figure A-6 we see the structure in its original position in the dashed lines, and the ground motion which is a vibratory shaking motion shown horizontally for illustrative purposes here. And we show how the flexible, conventional building would behave in an earthquake.

Of course this is an exaggerated scale here. We are actually talking about displacements of only a few inches. These are the types of motions that I think most people envision when they think of earthquakes and conventional structures.

Now the GETR reactor building, however, is different from this very flexible structure shown in Figure A-6, and I would like to refer you to Figure A-8. This is on page 41, and it illustrates the effects of ground shaking for a very stiff, rigid building such as the GETR reactor building here.

In this case, the structure essentially moves as a rigid block, and the deformations are primarily in the soil surrounding and beneath the structure. Here again we have shown an exaggerated scale. The maximum displacement in the horizontal direction of the top of the interior concrete structure, the concrete core structure, is about 2 inches when it's subjected to the Calaveras criteria.

Well, this illustrates, as I said, the vibratory motion and shaking. I would like to next refer you to page 47, Figure A-10. This figure illustrates the effects of a surface rupture offset on the GETR. We can envision in this figure the Region A for illustrative purposes as remaining stationery; and Region B as moving upwards and to the left shown by the arrow that is just above the line that says "fault."

This upwards movement of the wedge of the soil to the right side of the picture then produces the forces on the structure. It is thus the task of the structural engineer in this case to evaluate the effects of the shaking or vibratory motions which I illustrated in one of the previous figures, as well as the effects of the forces on the structure induced by the fault displacement as shown in Figure A-10.

Based on the analyses that were described in the testimony, it was possible to conclude that the concrete core structure will remain intact when it is subjected to the postulated earthquakes.

The next section in the testimony describes the investigations that were performed to demonstrate that the reactor pressure vessel and the associated piping will remain intact.

Beginning on page 69 of Exhibit 22 are described these investigations. The related piping and equipment include those items which are necessary to keep the fuel elements in the reactor pressure vessel covered with water.

Now our basic approach here was to either verify the adequacy of or modify any component required for safety. Now modifications in the case of the piping systems and components were actually very simple. It involved the addition of seismic restraints, which are really braces, to the piping or component to restrict the movements during the seismic events.

Now the basic phenomenon that we're dealing with here in designing supported piping and components is very analogous to what we have done with the building. The motions generated in the ground are transmitted through the structure and to the supported piping and equipment.

This movement of the building has two main influences on the piping or component. The first is relative displacement; and the second is vibrational effects.

Now if you will turn to page 70, which is Figure B-1, you will see a figure that has been prepared to illustrate relative displacement effects. Now again I have used the example of a flexible three-story building. It is a little bit easier to visualize, and it is an example that we are all a little bit more familiar with.

In this figure, two adjacent stories of the building or floors will displace relative to each other. This relative displacement is shown in Figure B-1 in an exaggerated scale In the real structure in GETR we are talking about relative displacements on the order of 0.05 inches, five one hundredths of an inch. So these relative displacements in this very stiff structure are actually very, very small.

When you have -- when the displacements of the floors occur as shown here, a pipe that may be connected between two floors will be distorted from its original position as shown here. It then becomes the goal of the structural engineer to determine the effects of these dostortions on the piping system.

In addition to the relative displacement effects, we also have to consider the vibrational shaking effects, just like we did for the structure. And if you will refer to page 72, Figure B-2, you will see an illustration of these vibrational or shaking effects.

In this figure, the ground motion is shown by the double-headed arrow, indicating that the ground can move back and forth. Again, I am showing only the motions in the plane of the paper here, and we for simplicity are not showing motions perpendicular to the plane of paper or in a vertical direction. It is simpler just to use the single direction for illustration purposes.

As the ground shakes, of course the building shakes, as shown by again the double-headed arrows at each edge at each floor of the building. These motions of the building are then transmitted to the component, as shown in the smaller inset in the upper right-hand portion of this figure, and it becomes our task to evaluate the component for the shaking motions which are transmitted to the supports of the component.

Well, so we have evaluated all the components related to safety for the relative displacement and vibrational effects, or shaking effects. An example is shown on page 77, Figure B-4, which is a figure similar to one you have seen before, which is a view of the primary cooling system which includes the reactor vessel, heat exchanger, and the pumps, and various other components.

We developed computer models of these components, subjected the computer models to the prescribed earthquake motions, calculated the stresses, compared those against our allowable values, and were able to reach the conclusion that the safety related piping and equipment are adequate to resist the motions for the site.

In addition to the items shown on Figure B-4, the investigations were also performed for the reactor pressure vessel and pool drain lines, pcison injection line, safety-related valves, pool heat exchanger, control rod, and in-core shuttle assemblies.

That concludes my summary of the c ncrete core structure and the reactor pressure vessel investigations, and Dr. Durlofsky will now continue with the introduction.

A. (Witness Durlofsky) I have some handouts that you can use, or else I can refer you to the pages in the testimony.

Mr. Gilliland in his testimony described the safety systems that provide for safe shutdown of the reactor, while Dr. Kost in his testimony discussed the analyses that were performed to show that both the concrete core of the reactor building and the reactor vessel remain intact under the design seismic loadings.

I would like to briefly discuss three major safety-related structural modifications that were made to the GETR facility. These three are: the fuel flooding system; the third-floor missile impact system; and the fuel storage tanks -- or I should say, the new fuel storage tanks.

Mr. Gilliland has already briefly described the fuel flooding system, but I will refer again to the figure that he used. That is on the first page of the handout that I just gave you. The fuel flooding system consists of two independent reservoirs, each having an indopendent feed line to the reactor building.

The reservoirs are located in the hills above the reactor building, and flow is provided by means of gravity. Both the reservoirs and the feed lines are made of a synthetic rubber matarial which is highly flexible. Each of the two reservoirs is capable of supplying sufficient water to cool the reactor. So in tendem we have a redundant system which in fact enhances the overall reliability.

The third-floor issile impact system is shown on the next two pages of my handout, pages two and three. This system consists of a series of structural frames that are strategically located on the third floor of the reacto building, and are designed to prevent the overhead train assembly from impacting either the reactor vessel itself or the fuel storage tanks. The frames are covered with approximately 14 inches of honeycomb. The function of the honeycomb is to mitigate the postulated impact of the polar crane assembly, and in this way minimize the loads both on the frames and on the floor of the reactor building.

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The new fuel storage tanks, which is the third major structural modification I wish todiscuss, consists of three inner tanks nested within one outer tank. Both the inner and the outer tanks provide a fluid retaining boundary nearby and near in; there is again a redundancy in this design as there was in the fuel floating systems, since both the inner and the outer tanks are designed to take all of the seismic loadings in addition to normal loadings.

This redundancy again enhances the overall reliability of the system.

In all of these -- in all of the modifications, in all of the structural analysis that was done for the GETR building, the only external requirement to the structures themselves, that is the only requirement that these structures function properly, except for their own ability to carry the loads, is that the core of the reactor building remain intact.

This is because all these modifications stand alone, except for the fact that they are, of course, resting on the floor of the reactor building.

The integrity of the core of the reactor building was discussed by Dr. Kost in his testimony. For all of these structures, the general method of modal superposition was used in the analyses. This is basically a dynamic method which takes into account the dynamic characteristics of a structure as opposed to some static methods whereby the inertia is applied as a static loading. The modal superposition or dynamic methods are generally accepted as being more accurate in describing the earthquake response.

In all cases, the seismic load was combined with any normal loads the structure might have to experience. As a result of the analyses, then, it was possible to conclude that the safety-related components are able to perform their design function under the maximum seismic event.

JUDGE GROSSMAN: Mr. Edgar, is there any rebuttal testimony that you want to offer through these witnesses?

MR. EDGAR: No.

JUDGE GROSSMAN: Mr. Cady.

XXXXX

CROSS-EXAMINATION

BY MR. CADY:

Q What is the general purpose for encompassing all of the related systems in the building known as the -- is it known as the containment building? Is the containment structure -- what I want to know is what is the purpose for having all of the safety-related systems maintained within the containment building?

A (Witness Gilliland) Let me see if I can answer you.

The reactor is the article of equipment that we are interested in here, and it is inside the containment building. It is connected to the prima:y coolant system. The control rods are in it, which is the vehicle by which it is shut down. They are part of the safety system.

I think that the best short answer I can give is that the safety systems are related to the safe operation of the reactor. It's in the building, and therefore that's where they are.

Q Is the purpose for having such an immense structure in case there is an accident, to keep the radioactivity within the structure?

A You're referring to the concrete structure, for example?

Q Right.

A The concrete structure has a number of purposes, as has been described earlier. There is a canal in which fuel elements are stored. The reactor vessel is in the pool, and during the time the reactor is in operation, there is a need to shield against the radiation that is produced, and so the concrete structure is thickened to provide this shielding.

It serves also a structural purpose, but I think one would find in examining that the principal reason for the massiveness of it is in relation to, at least in part, to serve that purpose.

Q Is there any radioactive contaminated water stored outside of the containment building?

A Yes. The water process systems are generally outside the building, outside the reactor building. The water is processed in an adjacent facility that contains storage tanks and demineralizers and pumps and so on.

The primary system does have bypass demineralizers that are located inside the building.

Q Could you give a brief description as to how the cooling water is circulated throughout the system?

A You mean the primary cooling water?

Q Right, the primary cooling water.

A All right. Let me find the figure. Let's refer to Figure 8, that's on page 15, Exhibit 22. This is an isometric of the primary cooling system. Central in the figure is the primary heat exchanger. It's the largest object that is seen.

To the right of it is the reactor pressure vessel, and then in the lower left-hand corner, partially obscured by the piping, is a diagram of the primary pump. If we start with the top of the reactor pressure vessel, which is to the right in this figure, you will see arrows on either side in the connected piping. That indicates the direction of flow.

So water enters the top of the reactor vessel through these two 12-inch pipes, flow is directed downward through the core, and then exits the reactor pressure vessel low in the vessel through again two 12-inch pipes.

And if you will follow, there are two parallel lines, two exit pipes, that eventually join before they come into the heat exchanger. The one comes from the left of the figure and around to your left, down and then back to the right, to join with the exit pipe that comes from the right, goes up around, and then down, and so these two pipes join at the entrance of the primary heat exchanger where the water is cooled.

The exit to that primary heat exchanger flow is in the lower left corner of the lower left of the heat exchanger where it enters the pump. That's at pump suction. The exit of the pump then is to the left and up. The flow is split again and you can follow the lines and arrows in the flow directions, and they come back around to the point where they reenter the top of the vessel.

So, in a nutshell, that's the flow in the primary coolant system.

Q Is it a closed system?

A Yes, it is. It is also pressurized.

Q Is the water at any time changed?

A You mean do we take it all out and replace it with something else?

Q Right.

A Well, as I said earlier, the water is continuously through a bypass demineralizer controlled for purity and, of course, in the process of refueling operations which occur every two to three weeks, normally, when the reactor is in operation, the reactor vessel is open to the pool and during refueling operations all of that water is common, the pool canal and the primary, all of which is kept at high values of resistivity, and there is some exchange of the water in the primary cooling system. But -- and so it is not taken out and changed and hauled away, if that's the sense of your question. There is some interchange with the pool and the canal during refueling operations, but it in effect remains there and remains in the facility.

Q Okay. Thank you.

Dr. Kost, in your opening summary, you mentioned the amount of motion that would occur. You mentioned 5/100ths of an inch. Is that what you would expect from the postulated events on the Calaveras or the Verona Fault?

A (Witness Kost) In that case, I was referring to the event on the Calaveras Fault. The motions-- and those were between the first floor and the third floor -- would be less for the Verona event.

Q How much horizontal movement would you expect on the containment building? You were mentioned two or three inches for certain structures.

A I think I mentioned two inches, about two inches at the operating floor level and on the inside of the structure, that is the inside and the operating floor level on the interior concrete structure.

Q If there was a one-meter offset beneath the containment building, how much vertical motion -- what do you expect -- how much vertical motion could one expect to occur to the containment building?

A If there is a one-meter offset at an angle of 45 degrees to the horizontal, the maximum vertical displacement would be on the order of two feet.

Q So the containment building would be lifted up approximately two feet?

A I think it would tilt, it would actually tilt.

Q Tilt?

A So the two feet would be the difference between the original position and the tilted position.

Q Okay. Thank you.

A And that's a tilt of a few degrees, three or four degrees.

Q What effect would that have on the water level of the core?

A (Witness Gilliland) You mean the tilting?

Q The tilting, right.

A It would essentially have no effect. If it were tilted a few degrees from one extreme of one stand pipe to the other side, if you were in line with those, there would be a very modest difference in elevation, but not enough to affect the operation.

Q Would that have any effect on the amount of water it would take to maintain a safe shutdown?

A No. The amount of water that was required is to provide for boil-off and evaporation of water produced by the heating of the fuel elements and that's not a factor in that consideration.

Q Is there any consideration taken for the possibility that some of the pipes might leak or break in determining how much water there would be to keep the system intact, or to keep the system at a safe level?

I'm assuming, of course, that there is a mal-function or a break at one of the valves or at one of the joints in the water system, in the primary cooling system.

A What we have done from a design point of view is to determine that that is not to be possible. That is, we have designed restraints on the piping systems to assure that we do not put those kinds of loads on the reactor vessel.

As you may recall -- you may not, and if you remember the Figure 8 that we were looking at -- the only piping of interest with respect to the reactor pressure vessel was the exit piping, and attached to it are two stand pipes.

Q Excuse me. What is the function of the stand pipe?

A Well, those were added to be sure that should there be leakage in the pool to the extent that water could go below the level of the fuel in the pool, that one could supply water to the reactor pressure vessel and the stand pipes would assure them that the level would remain over the fuel. Before these modifications were made, that is the addition of the stand pipes, there was simply a check valve, a large check valve, in the exit cooling piping near the bottom of the reactor vessel, and if one didn't have the stand pipes, then it would allow the water in the reactor vessel to go below the level of the fuel, and the stand pipes provide a means for assuring that doesn't occur.

Q Dr. Kost, what kind of an effect would a 2-1/2 meter offset have on the tilt of the containment building?

A (Witness Kost) In a hypothetical situation?

Q Hypothetical situation of a 2.5 offset.

A It could double the tilt. This is again assuming that the surface rupture offset occurs as a plane, and not like the photographs we saw earlier that Mr. Meehan was using, where we actually have a zone of failure in the soil, but if one could hypothesize that the structure were lifted up and tilted, it would be about seven, eight, nine degrees.

Q And, Mr. Gilliland, would that have any effect on the water level or the primary cooling system?

(Panel conferring.)

A (Witness Gilliland) To the best of my recollection, we haven't looked at that particular set of conditions. My judgment would be that it would not affect either of the two filter containers. That still represents a very small amount of tilt.

Q What kind of loads would a 2-1/2 meter offset have with the seven to nine degree tilt? What kind of effect would that have on your piping systems and the primary coolant system?

A (Witness Kost) Well, as we said before, this is a hypothetical situation, and we have performed our evaluations for the one-meter case. If one were to have a 2-1/2 meter offset, I cannot see that it would cause any appreciable stresses in the primary system. The reason being that again we are concerned with relative displacements which are very, very small; that is the interior concrete structure is so massive and strong that the deflections would be very small, even in the hypothetical case that you have mentioned.

Q Have you conducted any studies or investigations of offsets greater than one meter? For instance, 1.2 meters? 1.5 meters?

A No, we haven't.

Q Did the NRC Staff ask you to perform such studies of potential design consequences from a 1.5 meter offset or a 1.2 meter offset?

A (Witness Gilliland) No, they did not. We are, of course, fully aware of the data that was in an earlier preliminary SER.

However, we did not do analyses on those bases, insofar as offset.

Q Who on the panel would be able to give me information on the structural integrity of the foundation?

A (Witness Kost) I think I would.

Q Dr. Kost, could you please give us a brief summary as to what design criteria the foundation was built to, including any seismic considerations?

A I think I can, but I need to know what you mean by the word "foundation."

Q Okay. On your mat --

A Are you talking about the foundation soils?

Q The foundation mats.

A (Witness Gilliland) Mat, m-a-t?

Q Yes, the foundation mat, m-a-t, on your Figure 1. Just what kind of specifications were those built to?

A (Witness Kost) The original design was based on a dead load plus live load plus seismic loading, and the average pressure underneath the structure is on the order of -- I don't recall the exact numbers, but on the order of 5000 pounds per square foot, noting, of course, that you have a four foot eight inch thick concrete mat that's simply supporting loads and compression strength of concrete, and compression being quite high.

Q And over the years that strength has increased?

A The strength of the concrete has indeed increased over the years.

Q Do you happen to recall what the seismic specification criteria was for this design mat back in the mid, early '50s, when it was designed?

(Panel conferring.)

A (Witness Kost) I think that the design was basically UVC, but a static acceleration or a static force of 0.1 times the weight was used. So, in effect, like a 10 percent ground acceleration.

Q Would that be sufficient to meet the design criteria that has been postulated of .75 g from the Calaveras Fault?

(Panel conferring.)

A (Witness Kost) I think the basic answer to your question in the general sense would be no. If you were to have a structure that was designed originally for a low g level and it's a conventional building, the answer would be

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no. But here we have a very strong and massive structure that was really proportioned and designed for factors other than its basic load-carrying capability. That is, either the vertical forces or the lateral loads. So we have a tremendous amount of inherent strength due to these very heavy six foot six inch walls and so, in fact, the structure does the criteria.

Q You're saying just because the walls are six foot six inches thick and that the foundation mat is four feet eight inches thick, that because of its inherent size that it will meet the .75 g design criteria? Have you conducted studies to that effect, or is that just an opinion?

A Well, it's a conclusion based on the analyses that we have performed, and these analyses represent the massiveness of the structure that I mentioned, the geometry, the physical geometry, the strength of the materials that comprise the structure.

Q But I don't think you're answering my question.

A I'm sorry, then. Maybe I misunderstood it.

Q I just want to know if you've conducted any studies relating specifically to the foundation mat in regard to having the foundation mat meet the design criteria as postulated by the NRC?

A Okay, the -- I think I know what you're driving at here. If you -- and this will get us into a discussion of the different load cases for the Verona case, and when we envision and assume that the fault occurs underneath the building, there are certain conditions, certain support conditions which will occur which can indeed cause damage in the foundation mat.

Now this damage will be -- and by damage, please understand what I mean. I mean cracking of the concrete and yielding of the steel. And this damage is exterior to the concrete core structure and does not impair the integrity of the core structure.

Q Does it impair the integrity of the containment building, the damage that you have referred to, relative to the cracking of the concrete and bending of the steel bars that I'm assuming are within the concrete mat? Would it impair the integrity of the containment building through the foundation mat?

A Yes, that's possible, recognizing, of course, that we don't need -- actually rely on the containment structure here to meet the safety requirements that Mr. Gilliland outlined earlier.

Q Have you ever had problems with leakage through the canal liner?

A (Witness Gilliland) I don't know that I would count them problems. There has been some leakage in the

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canal liner.

Q Do you consider 250 gallons per hour a significant problem? Or is it just a matter of course situation?

A I believe that in our analyses we used values of leakage that are somewhat higher than that. No, they don't represent a difficult issue operationally or otherwise.

Q Am I correct in assuming that the water that goes through the canal liner, is that radioactively contaminated?

A It has -- there is some level of contamination. It's not high, but it has some contamination in it.

Q Whenever there is a leak in this canal liner, where does the water go?

A It eventually goes to the sumps which are located in the basement of the reactor building from which it's pumped to the facilities where it is demineralized and stored, and then eventually returned to the canal on the pool.

Q Where is the basement in relation to the foundation mat?

A Let's see. It's above it. The sumps are located in -- let's see. Let me look for a minute here.

MR. CADY: Your Honor, while he's looking, can we have a five-minute break?

JUDGE GROSSMAN: Let's take a five-minute break.

(Recess.)

end 10

JUDGE GROSSMAN: Please proceed, Mr. Cady.

BY MR. CADY:

Q Have you had time to find out where in relation to the foundation mat the basement is located?

A (Witness Gilliland) The basement floor is above the foundation mat. I think the thickness of the concrete is around a foot-and-a-half to two feet. That is, the basement is a foot-and-a-half to two feet above what we refer to as the foundation mat, it being four feet eight inches thick.

Q What is between the basement and the foundation mat? Is that a foot-and-a-half of concrete?

A Yes.

Q So in essence there is approximately six feet of concrete from the base of the foundation mat to the floor of the basement?

A That's approximately correct.

(Witnesses conferring.)

Q What would be the extent of the cracking in the foundation mat during a postulated event on the Calaveras Fault and on the Verona Fault?

MR. EDGAR: You said a postulated "event"?

MR. CADY: Right. The assumed design criteria as proposed by the NRC.

(Witnesses conferring.)

WITNESS KOST: We have not attempted to identify the extent of the cracking, other than ensuring that it is outside of the concrete core structure.

BY MR. CADY:

Q When the water that is leaking from the canal liner is -- How does it get down into the basement? Is it a controlled flow? Or is it pumped? Or is it gravity flow? Or just how does that leaking water get into the basement into the sump?

A (Witness Gilliland) Well, it's mostly by gravity, of course. The leaks are from the canal itself and are usually through openings that are left for piping. Those then normally exit into the equipment room, which is the volume that is inside the concrete core structure. And from there it is routed, in most cases, to the sumps which are in the basement.

Q Is a sump a holding container?

A I'm sorry. The sumps are located in the basement because it is the lowest point in the system, and it is the best place to collect the water. They consist of a small, reasonably shallow pit in which a pump is located, and it being the lowest point it is the point at which the water collects and from which the water is pumped.

Q What are these sumps made out of?

A. They are concrete. They're in the -- yes, they're concrete. They're a hole in the concrete. They are a rectangular opening in the concrete.

Q. And how far above the foundation mat are these sumps located?

A. I don't know a specific answer to that. If you need it, I think I can look it up. I'm not sure that we have the information here.

Q. Is it below the level of the basement floor?

A. No. No, it's within the basement floor. In other words, it is within the thickness.

Q. It is within that one-and-a-half foot?

A. Yes, it is.

Q. Area. Okay.

Where is this water pumped from the sump?

A. It goes to the -- Well, normally it would go to a holding tank, which is outside the reactor building but adjacent to it. And from there, it is reprocessed through the demineralizers and put in holding tanks from which it is pumped back in.

Q. Are these holding tanks subsurface, or above ground?

A. The holding tanks -- Well, let's see. They are both -- There are subsurface tanks, and those that are above the surface.

Q Okay. From the pumping of the water from the sump to the first holding tank, where is that first holding tank located? Or are they pumped to several tanks simultaneously at different locations?

A I believe that normally they are pumped to one tank. And again I believe it is one underground. It goes to an underground tank.

Q And what is that tank constructed of?

A I would have to look. If you would like that answer, hang on. I'm not sure I can get it instantaneously, but I can look if I have it here.

Q Could you, please?

(Pause.)

A Carbon steel. The principal containers are carbon steel. The principal piping is aluminum.

Q What is the specification reference for that?

A You mean for the carbon steel?

Q Right.

A I see none here. It is probably in the specification, the design specifications, but I do not have a copy of that.

Q Okay. Thank you.

What type of reaction does a carbon steel container like this have in the event of a seismic occurrence? Is it responsive? Is it 100 percent safe? Or just what exactly are the design limitations?

A. (Witness Durlofsky) Well, to answer that question we would need some more specifics, but generally speaking steel responds very well to seismic activity. It has a great deal of flexibility relatively speaking. Steel structures tend to do well under seismic loadings.

Q. Do you happen to recall in reviewing those documents that you were just taking a look at whether or not there were seams in these tanks in this particular container?

A. (Witness Kost) There would likely be seams, although that is conjecture on my part. They are constructed out of plates, most likely.

Q. Would this particular container -- do you believe it to be designed to withstand the postulated Calaveras event of .75g, or the Verona event of .6g?

A. (Witness Gilliland) Those tanks were not evaluated for those loads. They were not evaluated because they are not part of the systems that are required for the safe shutdown of the reactor. As you may recall from the earlier comments, the principal requirement we have that relates to water is that we keep the fuel elements covered with water. And these systems are not a part of that. The principal system that is involved in keeping water over the fuel is the one of makeup, the fuel flooding system, and of course the containers in which the fuel is located. So these tanks and their integrity with respect to water and/or with respect to their loading, or effect of loading by earthquake, were not considered because they are not a part of that system requirement.

Q Would a breach of containment in this particular container have any effect on the health and safety of the people?

A Well, as I indicated to you earlier, the level of radioactive contamination of this water is low, and you are hypothesizing the failure of a tank or the opening of a tank perhaps because of seismic loadings. I am not even sure the underground tanks would suffer that kind of damage in this kind of event.

But if you were to hypothesize a leak of this low-level contamination, contaminated water into the ground, I would not expect there to be any effect on the health and safety of the public. The hydrology of the area is such that it would be decades before it would get to a site boundary, and by that time would be decayed to the point where it would not be of consequence.

Q To a site boundary? What do you mean by "to the site boundary"?

A. Well to the site boundary, to the periphery of the site on which these facilities are located.

Q. Do you happen to know how deep that water containment tank is buried in the ground?

A. Let's see. I'll have to get the specific number. I would have to look. If you want to do that, hang on.

Q. Please.

(Pause.)

MR. EDGAR: Judge Grossman, while the question is pending, I will register an objection on the grounds that the line of questioning is addressed to the consequences that the Board has made its rulings rather clear on in advance.

MR. CADY: Your Honor, I don't believe it's on "consequences." I believe it is not proper design of this containment tank that is located subsurface.

JUDGE GROSSMAN: Well, the Board's ruling didn't go as far as to exclude releases in excess of the requirements of the regulations, and I believe this goes to that.

MR. EDGAR: I didn't press it initially. I think there will -- in any event, I want to raise it and get a sense of it from the Board.

JUDGE GROSSMAN: Okay. But I assume that that is a point he is trying to make. So as long as there is no specific objection, that is noted for the record.

WITNESS GILLILAND: It appears that that specific dimension is not in this document. By the way, what I am referring to is the Safety Analysis Report. The underground tanks are noted to be located 8-1/2 feet below grade, and I am not sure of the diameter, so they probably estend another, oh, six or seven feet below that point.

One point that is worthy of note is that in these -- underneath these buried tanks is a retention basin, or a catch basin which was put there to collect water that might leak, and it is then recirculated back to -- into the system.

BY MR. CADY:

Q What is that retention basin made of?

A (Witness Gilliland) Concrete.

Q How thick?

A I don't think -- I don't have those dimensions, and I don't think they are in this document that I was looking at. Again, that concrete and the tanks that are above it have not been evaluated for these seismic events that you were describing earlier.

MR. CADY: Thank you. And I want Mr. Edgar to know that I realize what the scope of the proceedings are. I believe a few of my questions did sound "consequences" oriented, and I purposely wanted to try to avoid that situation; but they just came out sounding consequence. I am primarily trying to get to design to protect the public health and safety. Once the event happens, that is when we get into consequences. We are just trying to establish proper design to further the purpose of the regulations.

BY MR. CADY:

Q After the water leaves this underground containment vessel, then it is transferred to various demineralizing containers to -- what is the purpose of going through these other containment vessels?

A (Witness Gilliland) You mean what is the purpose of its going to the demineralizers, for example?

Q Right.

A Well, the demineralizer is a resin exchange bed which is intended to take impurities out of the water for purposes of at least clarity in the canal in the pool, and also because it is beneficial insofar as corrosion is concerned. The water purity is kept high so that once the water has gone to the sumps, it is necessary to recirculate it through these resin exchange beds to remove impurities that may have collected.

Q Thank you.

Getting back to the underground containment water tank, is there a cathodic protection system that is necessary to prevent corrosion incorporated into that containment tank?

(Witnesses conferring.)

A I know of no cathodic protection, but that may only be because I don't know. If that is an important point, I can check it.

Q Okay, let me just ask one question. Have there been any holes discovered in the tank?

A No. No, and they do get examined periodically.

Q I would assume that after 25 years that if there was a deficient cathodic protection system that holes would have developed, so I won't pursue that any further.

Could I direct your attention to page 24 of your submitted written testimony? You mentioned that there are four different kinds of restraints that are or will be installed on the primary piping system to eliminate stresses on the reactor vessel, thus assuring its integrity.

Could you please explain what the four different types of restraints are, and what their effect is on enhancing the integrity of the system?

(Witnesses conferring.)

A. (Witness Kost) I think I can answer the last part of your question first -- that is to say, what the intent of these restraints are.

As I mentioned in my introductory comments, the intent of any restraint, any seismic restraint on a piping system is to brace that piping system and decrease its movements when it is subjected to a seismic event.

We have used several different types of restraint here, but they all have the same basic purpose. That is, they restrain and stop the piping system from moving.

Q. This was referred to as "seismic bracing," among other things?

A. Yes. "Seismic restraint," or "seismic bracing," they are the same terms and are used interchangeably.

A. (Witness Gilliland) If you would like to put your finger in a couple of places, the one to which he just referred, page 24, and then come back to Figure 8, which is on page 15, some of these restraints are multiple, and I'm not sure -- and in fact I'm not sure with the diagrams we have here if I'll be able to tell you pre::isely the location, but I can give you the kind of restraint they are.

The first is a gusset, which is a plate that is connected to the bottom end of the 20-inch elbow that is coming out of the pump. If you look at Figure 8, the lower-left hand corner, you will see that drawing indicates a primary pump. To the left of that, there is a 20-inch line. This of course is somewhat of a simplified diagram. It doesn't show all of the appertenances to it. But below the 20-inch elbow portion there is now added a plate which is attached to the floor and provides the restraint at that point.

The second is a saddle and U-bolt arrangement that goes on the piping. Let's see now, if you look at that same figure where the primary pump is located, there is some piping that returns water from the reactor vessel to the heat exchanges, and it partially obscures the pump.

A. (Witness Kost) Could I interrupt for just one second and perhaps refer you to another figure which might be a little better here in illustrating what we are talking about here? Please refer to page 79, Figure B-5, which is entitled "primary cooling system restraints." Page 79.

A. (Witness Gilliland) This will be a big help.

Let's go back just one step. Is that gusset clear to you on the elbow, the first one that we talked about?

end JWB #11

Q Yes, sir.

A All right. And then the U-bolt saddle arrangement is just to the right of that, and that piping that sort of obscures the pump. There is a member that goes down to the floor and there is a number that says 2-10 which identifies that as a restraint number. That's restraint. There is one of those as well.

The third type is the -- are the trapeze hangers which are shown in the bottom half in front of the heat exchanger identified as HE-101, the tall circular structure, and it's on the two sets of parallel piping just to the left of the center of the primary heat exchanger, and you will find numbers like 1-8 -- let's see. Sorry. Let's see, it looks 1-9. You see those? You see that structure?

Q Yes.

A All right. Those are the trapeze hangers. There are two of those, and then there are 16 piping restraints that attach to the primary piping to the interior of the concrete core structure.

Q Excuse me. On those trapeze devices?

A Hangers.

Q Right. Do they rely on the support of pipes, or other pipes? The way it looks, it looks like they are --

A It looks like they are hanging from there.

Q Right.

A The design that this reflects would show them attaching to the bottom of the canal. That is the underside of the floor of the canal. The canal lies above this point, and this would attach underneath on the bottom side of that floor.

Q I see.

A Okay. We are considering a design which would not attach them to that point, that might attach them to the floor, but it doesn't change their function. It only changes the attachment point.

There is a fourth type of piping restraint then. These are the simplest, I guess, of the collection. There are 16 of them, and they restrain the piping by attaching through clamps and struts to wall attachment points, and you will find these in several locations. If you will look above the primary pump and to the left on the far left-hand side of the diagram, you will see a number 1-5 and 1-11, and above it 1-6 and 1-7, and these -- there are 16 of these located throughout the piping, and that is the fourth type.

I can point each of these out if you wish, as their location.

Q No, that's fine. Thank you.

A All right. You asked about piping alone, right? Okay.

Q Could I direct your attention to page 36 of your submitted written testimony, the last sentence in the top paragraph where it states:

"The earthquake ground motions tend not to be amplified by the structure."

Could you give us a brief explanation as to why the earthquake ground motions are not amplified in that situation, or they tend not to be amplified in this particular structural situation?

A (Witness Kost) Right. The intent of that statement there was to indicate the basic behavior of the structure. As I mentioned earlier, we have a very rigid and massive structure, and it deforms very little when it's subjected to an earthquake, and you don't have the significant motions you would envision that you would have in a more flexible structure, and so I'm trying to point out the contrast between the types of structures that we tend to think about, which are conventional buildings, as opposed to this very stiff and massive structure which is well embedded into the ground.

Q In the walls that are six feet six inches thick, is there extensive rebar?

A "Extensive" is a qualitative term. They are reinforced, right. There is reinforcing in the horizontal and the vertical directions, and in those walls in both phases of the walls.

Q Do you happen to know what the separations are?

A The spacing between the bars?

Q The spacing between the bars.

Both horizontally and vertically, and going the whole width of the wall.

A I don't have that information in front of me.

Q Going again to page 36, the last paragraph, it starts, or it says:

"When seismic waves pass through the earth's crust, the ground at the site, including the ground on which the building is supported, is moved, and this movement varies rapidly with time."

Could you explain the relationship between the movement and its relationship to time? I would just like a clarification, a definition of what was meant by the movement varying rapidly with time.

A The movement of the soil or the ground upon which the building is supported will be of an oscillatory nature. That is, it moves back and forth in an erratic fashion, and you can visualize this motion as a plot or a graph of acceleration of the ground vs. time, and if you refer to Figure A-5 on page 38 where I have given a typical earthquake record, you will see graphical representation of this movement, and you can see here the time plotted on the horizontal axis and the ground acceleration plotted on the vertical axis, and I think this graph illustrates the time acceleration relationship that you are talking about. The character of these motions here, as you can see, on Figure A-5, is that you have very faint vibrations at time T equals zero, and there is a build-up of some three to four to five seconds, and then you have a more intense shaking for several seconds, and then you have a tapering off of the motions, and eventually these motions will taper off to zero when the earthquake has ceased.

Q Do you happen to know which seismic event this graph was taken from?

A It's the 1952 Taft, California earthquake. It's one of the two horizontal components that is indicated on the bottom of page 37.

Q Do you happen to know what the magnitude was of that particular earthquake?

A No, I did not look that up.

Q Thank you.

I direct your attention now to page 58, and it says in the first full sentence:

"It was determined that there may be some cracking and a deformation of the ring wall between the basemat and the first floor due to the soil pressure against the ring wall on the left-hand side of the building."

What did you envision by making reference to cracking and deformation? What type of damage would result?

A Okay. This refers to Figure A-14 which is at the bottom of the page, and this case is one of the several that we investigated for the postulated event on the Verona Fault, and in this case the surface rupture offset is assumed to occur as on Figure A-13, page 57, in case 1-A. There is shown on the Wall A also.

Now in this case the pressure on the right-hand side of the building tends to push the structure and the soil to the left-hand side of the building. This causes a soil pressure on Wall A which is termed a passive pressure, that's the word that's used to describe it. And we have conservatively -- we have made an estimate of that passive pressure and performed an analysis of Wall A, determined that the stresses and the reinforcing bars are above the yield stresses which means that this Wall A could indeed be pushed towards the center of the structure.

Now, recognizing that, then, in all of our stress analyses of the interior concrete structure, we have made the assumption that Wall A simply does not exist, that is we have not relied on its strength at all in the analyses of the stresses and the concrete core structure.

Q Thank you.

Could you please turn to page 76, and what I want to know is basically from where did you obtain your conservative allowable stresses that you based the values that you applied to the construction materials used for the GETR facility?

A Okay, I can answer that question for portions of these components.

The stresses for the aluminum piping, the allowable values were obtained from the ASME Code, Section 3 for Class 2 components.

Now, what that all means basically is that for the piping systems themselves, the maximum ultimate tensile strength that's given in that code is 24,000 pounds per square inch, and we have used a value that I believe was 15,000 psi, or pounds per square inch. So we are below the ultimate tensile strength of the aluminum piping.

Similar values were used for the steel braces, and typically we have used values that are, I believe, either eight or nine-tenths of the yield stress. Now the yield stresses are obtained again from the same code, and these are also basically the same values that are in the American Institute of Steel Construction Code.

I think one point to note here also is that when you achieve stresses that are say 8/10ths or a half of the yield stress, there is a tremendous still reserve margin in the structures to dissipate energy.

That is, they are very ductile structures, and you can have deformations that will stress you beyomd the yield stress before you could ever anticipate any type of a failure.

Q Any other comments from Mr. Gilliland or Dr. Durlofsky? Or did that pretty well explain what codes and handbooks were used and the reasoning behind them?

A (Witness Durlofsky) I might just add that in reference 22, which are stress analyses, all the references are clearly called out.

(Panel conferring.)

MR. EDGAR: For cross-reference, that's Licensee's Exhibits No. 26 through 33.

WITNESS KOST: I could add one thing. The reference on the allowable stresses is to the primary system and associated piping and equipment.

BY MR. CADY:

Q All right, thank you.

On page 84, in the last paragraph, it says that:

"It was determined that the stresses in the piping, piping restraints, RPV lateral braces, RPV shell, internals and stand pipes were within acceptable limits."

What acceptable limits were those?

A (Witness Kost) I can answer that question. Again the acceptable lim::cs are basically ones that we were talking about a minute ago for the aluminum and the piping and the steel.

Now there were also other limits which were determined for anchor bolts, where you anchor one of the restraints that Mr. Gilliland showed you earlier, and in that case typically the factor of safety of those anchor bolts is probably a factor of -- is four. So we are using a value that's a third or a fourth the ultimate capacity in our design.

Q Have you made a determination that the primary coolant system is fail-safe, or fail-proof?

A I don't know what "fail-safe or fail-proof" means. I don't think I can respond to that.

Q It means have you made a determination that the prim--- coolant system will withstand any event at any de- . load that is placed on it, or have you just made the determination that it will withstand the NRC postulated design criteria?

A I think I can respond to that in a general sense, and if we have to put certain things in, in perspective, in looking at what we're really doing here.

We have designed -- we selected certain magnitudes which, as we heard, are very extreme events in the sense that they are not likely to occur. We have ascribed to those magnitudes a certain ground acceleration. We have talked about mean plus one sigma values of extreme events. We have extreme values of extreme events.

Then the next step in the design process is to select a response spectrum which represents the response of the structures to a very broad band, frequency content of the ground accelerations.

Then after that has been selected, we have then gone through the structural analyses using the types of allowable stresses or deformation limits that we have talked about before, all of which lead to indicate that there is a sizeable margin of safety in these structures and systems above the criteria that we have selected. But I don't think anybody can go as far as to say that they are fail-safe or fail-proof. That would be unrealistic.

What we can say is that the designs are adequate for very extreme loads, very, very severe level of seismic event for this site.

Q Is there a back-up system, should this primary coolant system fail?

A (Witness Gilliland) As indicated, the primary cooling system has been modified to assure that we do not impart consequential stresses to the reactor pressure vessel, and so it is not necessary to have back-up for that piping system in that sense.

We have gone to almost an extreme to assure that we will not have significant loads on the reactor vessel, so that we can rely on its integrity, and so, too, for the canal storage tanks.

Q This is a hypothetical question:

Is there a possibility or is there any way to design a back-up system to be incorporated into the present system, should the primary cooling system fail?

(Panel conferring.)

A (Witness Gilliland) Well, as you know, we have added a system which is in addition to what we would expect to have available insofar as water to cover the fuel elements. That's the fuel flooding system.

Under the circumstances that we have described, we believe that there would be water supply available in both coolant canals to cover the fuel, but the fuel flooding system has been added and these two containers designed so they are to remain intact, so that we are to be assured there would be water supplies. So, yes, there had been a back-up system added to assure that fuel elements would remain covered and, in fact, it is a back-up back-up system. It's redundant in all ways.

Each of the two reservoir and piping systems supply each of the canal storage tank and the reactor vessel with water, and either one of them has a supply that is adequate for the task. So each of those two are redundant, so we have a back-up for the back-up.

Q You're referring to tank A and tank B for the fuel flooding system?

A Let me see. Do you have it?

I am referring to reservoir A and B, and one thing these diagrams don't show is that reservoir A is made of two tanks and reservoir B is made of two tanks.

Q Of what, 50,000 gallons per tank?

A That's correct.

Q If reservoir A should fail, will reservoir B be sufficient to supply all of the water needs of the reactor?

A Yes, it will. That is correct.

Q And vice versa, if reservoir B fails, they are mutually independent of one another; is that correct?

A That's correct. They are completely redundant.

Q I'd like to direct your attention to page 118, referring specifically to the shield pipe and the supply pipe.

Am I correct in assuming that the four-inch diameter steel shield pipe is embedded in a gravel-filled trench with the base of the trench being outlined in this diagram, Figure D-9?

A Schematically, that's correct.

Q What type of steel is used for that steel shield pipe?

A It is stainless steel, Schedule 80, which if you're not familiar with that nomenclature, is an extremely thick-walled pipe.

A (Witness Kost) Basically the schedules of pipe that are used are 40, 60 and 80. They all have the same nominal diameter, so for this example Schedule 80 has the thickest wall diameter.

Q Okay. What effect would a seismic event originating either on the Verona Fault, having .6 horizontal acceleration, or on the Calaveras of having .75 horizontal acceleration, what effect would that have on this shield pipe?

A In an earthquake on the Calaveras Fault, having the acceleration you mentioned, it would have no effect, it's simply a buried pipe, and the strains on the pipe would be extremely small. That's typically the case with buried piping systems, particularly ductile steel pipes. I would say the same thing for the vibratory portion of the event on the Verona Fault. If one were to have a surface rupture offset that would intercept the steel pipe and try to lift it out of the ground, that would induce certain stresses in the pipe, and we have done an analysis of that pipe and estimated those stresses. I don't recall the specific numbers but we have found that they are within the allowable limits for that pipe.

Q Would the pipe bend? Would it break? What would be the result of a surface offset that happened to intersect the pipe?

A It would bend, but not break, nor kink.

Q The pipe would not kink?

A That's correct.

Q I just have one question concerned with the missile impact system. Were there any field tests conducted to test the sufficiency of this missile impact system?

A (Witness Durlofsky) I assume you're talking about the third floor missile impact system?

Q Yes.

A We did conduct tests on the honeycomb material to be sure that the material is capable of carrying the loads that we use in the design.

Q On Figure C-1, on page 98, are those dark lines the areas where the honeycomb material is located?

A Those dark lines represent the frames themselves. They are all covered by the honeycomb. If you look on the next page you can see an elevation view of the bend in relation to the polar crane assembly. Also in that figure, the honeycomb is designated. Do you see that?

Q Right. Right. I can see that.

A Now the nature of the test that we performed -- perhaps I should diverge for a moment and say the purpose of the honeycomb was to minimize the loading on the bend, so the purpose of the honeycomb test was rather to see how strong the honeycomb was, was to make sure that it does mitigate the impact effect.

To that end, we conducted tests at the Hexell Corporation in Arizona on our specific design. It is the specific honeycomb that we used, with a specific material covering the honeycomb, and we measured the impact loadings on those tests, or the crushed strength of that honeycomb.

Q How did you measure the crushed strength of honeycombing material? Did you drop something on it? Did you just do it mathematically?

A No, it was done in a test, but not in an impact test. It was done by applying a load head to the honeycomb.

I might also say the honeycomb itself is pre-crushed. One of the difficult things in designing an impact-limiting structure is to make sure that your structure isn't too strong, in fact.

Q Why is that?

A Because if the structure is stronger than you think, your loads might be somewhat higher than you think. To that end, we made sure of that by -- well, honeycomb in general makes sure of that by precrushing the honeycomb.

It's actually precrushed before it's put in place, and it was precrushed very slightly before it was tested, the actual honeycomb that we installed in the GETR building was tested, and it was tested from the testing machine.

That test is referenced, I think, in our document.

end 12

A. (Witness Gilliland) They're on the bottom. You mean the bottom of the tanks?

Q. The bottom of the tanks.

A. Yes, on the bottom of the tanks.

Q. So there is pure gravity? There is no pumping involved?

A. Yes.

MR. CADY: Thank you. I have no further questions.

JUDGE GROSSMAN: Before we get to Mr. Bachmann, we will take a 10-minute break.

(Recess.)

JUDGE GROSSMAN: Mr. Bachmann?

MR. BACHMANN: Thank you.

CROSS-EXAMINATION

BY MR. BACHMANN:

Q. I will direct a couple of clarifying questions to Dr. Kost. There seems to be some parts of the record that we have developed so far that need a little bit more clarification.

Dr. Kost, I direct you to page 55 of your testimony, the last paragraph, where you -- I will sort of paraphrase that. Even though you had postulated that the one meter of offset would deflect around the building, you go on on page 56 to say that you analyze the effects of the one meter as if it would not deflect. Is that correct?

A. (Witness Kost) That's correct.

Q. Well, assuming that the one meter does deflect -- in other words, assuming that Mr. Meehan and Dr. Pichumani's testimony is the correct way, in what way, or perhaps you could explain how the one meter would -- would the one meter still affect the structural response of the building, assuming that it deflects?

A. Yes. I have discussed this with Mr. Meehan and, if you recall from his presentation he has shown that the surface rupture offset will deflect and not intersect the base of the foundation. Now there is the possibility -- and this is based on my discussions with him -- that it still could hit and intersect the side of the building.

Now I can illustrate that region that we are talking about here on Figure A-13, which is Case one.

Q. Do you have a page number for that?

A. I'm sorry. Page 57. Case one, and specifically case 1-B. Now it is my understanding from Mr. Meehan that his analyses do not preclude the intersection of the surface rupture offset with wall B. Now as a matter of practical interest here, if the surface rupture offset intersects as it is shown in that diagram -- that is, it intersects the 4'8" thick base slab -- most of the pressure will be distributed to that base slab. And I would envision that the damage to that wall would be wall B above the base slab and would be minimal, although I haven't quanitified that.

To continue on a little further, if we imagine that the surface rupture offset intersects within the top six feet of that wall -- that is, from the ground surface to six feet down -- there will be no damage to that wall. That is the stresses are within the acceptance criteria. There is the possibility for damage -- and again now I am using the term "damage" to mean yielding of the reinforcing bars and cracking of the concrete. There is that possibility if the surface rupture offset intersects the distance six feet down from the surface to about 13 feet down to the surface. So you can see that we have a very narrow target, or a very small target here, where this unlikely event would actually have to intersect the building before we would have any structural distress.

Now of course I have to point out, as we know from reading this testimony, that even if we do have damage in the region of wall B as I have shown, this will not have any influence on the interior concrete structure.

Q. Thank you.

MR. BACHMANN: I have no other questions.

BOARD EXAMINATION

BY JUDGE GROSSMAN:

Q. Mr. Gilliland, did the FSAR place any reliance upon the integrity of the containment being maintained in the event of a design basis accident?

A. (Witness Gilliland) Yes. The accidents that are described in the Safety Analysis Report do rely on the containment for some of them.

Q. Dr. Kost, on pages 58 and 59, you indicated that walls A or B might crack. Is it possible that they might also fall?

A. (Witness Kost) I don't believe so. "Fall" means a total collapse to me. That is, the first floor slab ends up on the basement floor, and I don't envision that will happen. These walls will be moved in by the soil, and the reinforcing will yield. It may actually pull out at its ends, but I can't envision enough of a displacement inside towards the center of the building to actually cause a total collapse.

Q. Did you make a quantitative analysis?

A. Yes, we did.

Q. Did you make a quantitative analysis -- this is for Dr. Kost, again -- of the amplification that might occur to the upper portion of the reactor from accelerations at the base of the reactor?

A. Yes. And I can refer you to a figure. I think just to spend a minute here, Figure A-11 on page 49 gives the schematic view of the process that we use where we have used various computer codes to simulate the behavior of the structure.

On the left-hand side of that diagram is shown a schematic earthquake record, which is an input motion that schematically is a plot of acceleration versus time. Now we have assumed that that motion occurs at the base of the physical structure. That is, the base of the 4'8" thick concrete mat.

Now I might add that there is another conservatism that is thrown into the analyses here, because it is typically shown that the free-field motions at the surface are higher than the motions at the base of the structure. So we could have actually decreased the motions, but we did not choose to, to obtain the motions at the base of the structure.

To make a long story short, then, in Figure A-11B is shown a schematic of the mathematical model that we use in our computer analyses. We have actually input that acceleration time history -- that is, physically mathematically shaken the structure by using mathematical model of Figure B, and then produced the response output that is shown schematically in Figure C. And that output then in the form of accelerations does exhibit the amplification that you were asking about.

Q. What accelerations values did you use?

A. We used the NRC criteria, which are .75g; and then that is the effective ground acceleration that we heard about over the past few days. And then we used, in conjunction with that, the Regulatory Guide 1.60 Response Spectrum. That is a measure of the frequency content of the ground motions. And we actually in our computer analysis used an acceleration time record that would produce that response spectrum.

Q. That .75g, that was a horizontal acceleration value, wasn't it?

A. That's correct.

Q. What did you use for vertical acceleration value?

A. We used two-thirds of the horizontal, which is consistent with the criteria.

Q. And what factor did you use for the upper part of the structure?

A. There is not a factor for the upper part of the structure, as such. The process here is one to define the motion that is at the base of the structure.

That is, that which goes into the structure. And then by the computer analyses, we calculate what comes out at the top of the structure, to use those terms a little bit crudely. But the amplification, if there is any, is automatically calculated in the computer analysis. And for structures such as this in the vertical direction, they are very, very rigid and there is no amplification. That is, if you have a certain fraction of g at the input at the base, you have essentially the same number with only slight amplification, a few percent, at the reactor building operating floor level, which is the third floor level.

Q. That is for the vertical accelerations there would be very little amplification? Is that what you're saying?

A. That's correct.

Q. Are you aware of any observations which indicate that with regard to thrust faulting that there may be very considerable amplification of vertical accelerations towards the upper part of structures?

A. I don't think I can answer that specifically with regard to thrust faulting, but I think I can answer it in general and say that I am not aware of any significant -- well, of historic earthquakes which produced significant amplification vertically with this type of structure. You may find that for more flexible structures that are composed of beams and columns, you may find that in the literature, and I am sure it does exist. I can't recall specific examples now, but not for this type of structure.

Q Well, wouldn't that relate to the amplification of horizontal accelerations, those types of structures that you refer to? In other words, wouldn't there be an amplification of the horizontal because of those particular types, rather than an amplification of the vertical?

A For the conventional type of building?

Q Yes, for the conventional type of building that you just described.

A Yes. The conventional type of building with moderate numbers of stories would certainly amplify more in the horizontal than in the vertical direction.

JUDGE GROSSMAN: I have no further questions.
Judge Ferguson?

BY JUDGE FERGUSON:

Q Let me be sure I understood what you just said a moment ago, Dr. Kost. Let's picture a tall, flexible structure resting on earth that moves as a result of an earthquake event. Did I understand you just now to say that there would be amplification by the structure of the horizontal acceleration, and not the vertical?

A. (Witness Kost) I didn't quite say that. I think I said, and I meant to say, that there would be amplification of the horizontal motions, and amplification of the vertical motions, but not so much as the horizontal motions.

Q. There would be more amplification of the horizontal motions, you're saying?

A. That's correct.

Q. Even if the structure were very tall?

A. That may not be the case. I am thinking of a moderate structure here in terms of say 10 stories, in that range, 5 to 10 stories. If you have a very tall flexible structure, then the converse could be true.

Q. But neither case is true at the GETR? Is that correct?

A. That's correct.

Q. Okay, let me begin a line of questioning that I hope will help me at least understand what has been said.

At the beginning of your testimony you identify several things that you have looked at. Let me start with one. Namely, that you say that in the case of a seismic event, one thing that is important is that we shut the reactor down. And of course you go on to say that the way that is done is by you insert control rods and presumably that shuts the reactor down. These control rods presumably are controlled, or at least the dropping of these control rods are controlled by triggers. I think you called them "seismic triggers." Is that correct?

A. (Gilliland) That's correct.

Q. So the picture I get is that when the event occurs, there is a mechanism called the seismic trigger which essentially releases the control rods, scrams the reactor, and the reactor is then shut down. Is that correct?

A. Functionally that is correct, but there are some intervening steps. But that is correct.

Q. I am trying to keep it as simple as I possibly can. You indicate that you already -- you now have installed on the reactor seismic triggers. As a matter of fact, you have always had seismic triggers on the reactor. Is that correct?

A. That's correct. We have recently installed new seismic triggers which also have a vertical trip. The earlier seismic triggers that we had would actuate on horizontal motion only.

Q. I see.

A. They have been replaced with different triggers that will also actuate on vertical motion.

Q. In your testimony you indicate that some modifications have been completed, and some are in progress. Is this the one that is in progress? Or is this one completed?

A. The seismic triggers, the installation is complete, and all but a very small amount of the testing has been completed.

Q. I see. What led you to change your seismic triggers from one that would trigger only on horizontal to one that would trigger on both horizontal and vertical?

A. In the examination of the seismic records, we found that a number of the records showed vertical accelerations in advance of horizontal. Our observation was that we could and would shut the reactor down quicker were we to -- that is, for some earthquakes, were we to also sense the vertical motion.

Q. Were these accelerations ground accelerations you're speaking about? Or accelerations within the building?

A. Well, the records that we reviewed were records that were free field records. So that they're not exactly the same as one would have in the building.

But we did examine free field records from a number of historical earthquakes. I believe the number of records that we reviewed -- and this was done with a computer -- is in the neighborhood of 100.

Q. I see. Where were these instruments, these seismographs, located, the ones that you reviewed?

A. Let me find a figure. Let's see. Look at page, if you have it, page 59. It is Figure A-15. There may be other figures which would illustrate it as well, but that is a handy one.

A. (Witness Kost) May I ask a question? I am a little confused. You asked where were the seismographs that we reviewed? The seismograph records that we reviewed in deciding to use the three-dimensional triggers were located throughout California in the vicinity of recent earthquakes, and not only California but in other places in the United States. So that was the historical data base that we used as the basis for our decisions.

Now if you're going to talk about specific locations of the seismographs as they exist in the GETR site and the GETR reactor building --

A. (Witness Gilliland) Then that is the answer I was looking for.

Q. Why don't you give us that.

A. If you're at Figure A-15, you will note in the concrete core structure there is a portion of it that is to the right, somewhat pointed to the right. And there is a small void there. These are mounted about six feet up from the floor on those walls. There are two of them. So they are mounted on the wall about six feet above the basement floor.

Q I see. Inside?

A Inside the core structure.

Q Getting back to the larger smapling that you mentioned, Dr. Kost, was it always true that you got a vertical sigral before a horizontal signal in the records that you reviewed?

A (Witness Kost) Just a second.

(Witnesses conferring.)

I can't say specifically for all cases. I think that is the case, but I am not positive. For the recent earthquakes, the records from recent earthquakes that have been recently instrumented, as I understand it prior to about 1970 the USGS instruments only had the horizontal triggers. I may be wrong here, but I understand that it is only the newer installations that have the 3-D triggers, the three-dimensional triggers.

Q Well, what I was really interested in is your statement about the time sequence of the signals. I thought that you said that the rationale for installing seismic triggers on your control rod was the fact that you got information about vertical motion before the horizontal motion, and you canted to trip as early as possible. Is that generally correct, Mr. Gilliland?

A Yes, that is correct.

endJWB #13

Q And that was the motive or motivation that led you to make the change from the old triggers to the newer ones; is that correct?

A That's correct.

Q I want to go back now to about 1958 or about that time, when the reactor was first constructed, and you say that there have been several earthquakes since that time at the GETR site; is that correct? Nine, I think. Something about nine earthquakes.

A We've had vibratory ground motion there. The earthquakes were epicentered variously. I think nine or more miles away. But we did get some vibratory motion at the site for those occasions.

Q Okay. Your testimony says you got nine -- let me make sure I quote you correctly. I'm looking at page 22 of your testimony, and I read:

"Since GETR commenced operation in 1958, a total of nine events have caused the present seismic triggers to operate."

Now when you say "present seismic triggers," you're really not talking about those you now have, but the ones you replaced; is that correct?

A That's correct.

Q Okay. So those triggers were replaced since you developed this testimony, presumably?

A That's correct, and I believe that since the testimony or -- I think we have had not an earthquake since those -- there's been no earthquakes since we installed the new triggers.

Q I see.

A That is, none that we've measured.

Q Okay. Well, again, trying to simplify this as much as I can to get what I'm trying to get at, you say that since -- in that same section, since 1977, there was an earthquake of Richter magnitude 4.1. Presumably this caused a seismic trip and scram of the reactor. Is that correct?

A This is at a time when the reactor was down pursuant to the show-cause order. However, these systems we have kept operational, and so we do receive a trip, but of course the control rod assemblies have been -- the control rod sections have been removed from the reactor, and so we didn't actually achieve a scram, but all the signals that are associated with that were received, and we did operate as if a scram had occurred.

Q I see.

Well, let's go a step beyond that. Let's assume that there is an event that triggers -- scrammed the reactor and the rods are now in their downmost position, but the building continues to shake because of the ground motion.

You say that you want to make sure that the rods don't move as the building moves in this shutdown position, and your statement simply indicates that you have performed an analysis to make sure -- or your analysis says that they do not move as the building shakes. Could you give us some guidance as to just what that analysis was, without going into great detail? Try to give us an overview of what that analysis was.

A There was a dynamic analysis performed for the control rods, using a response data provided by Dr. Kost, and that was input data as well as the weight of the control rod assemblies, and that was evaluated analytically in a dynamic fashion to see what the degree of motion of those control rods wo be in that circumstance, and while they moved a little bit, they came nowhere near to the point where they would be an issue insofar as moving out far enough to be able to start the reactor.

Q But that was a computer analysis; is that right?

A That's correct.

Q Have you ever done any actual measurements, any instrumental analyses? That is to say, you have presumably had the reactor shut down at the time you have had events. Have you ever attempted to determine whether or not this computer model actually represents what's going on?

It just seems to me you have an excellent laboratory here. You do have the building shaking, and you can make measurements. Have you ever done that?

A We have not done that. I guess a couple of things should be said.

One is that the control rod assemblies, as a way of explanation, are a two-part assembly. There is a poison section to which is attached a fuel section, and in order to understand that particular phenomenon, it would be well to test the whole assembly. That's really the question at hand. What happens to the whole assembly. And since the reactor has been shut down and we have been obligated to remove all fuel from the core it's not possible to have those assemblies as they normally are to do it. So that's one thing.

The other is -- and it's perhaps a more practical matter -- the levels of acceleration that we have measured are quite low, and while one in this down condition might be able to instrument, for example, dummy the weight and configuration of the fuel section with the nonfueled section, I suspect that one might wait a good long time -- hopefully not until we have an opportunity to start again -- but would wait a long time before you have an earthquake anywhere in the vicinity that would give you accelerations able to measure. They're pretty low. As Dr. Kost indicated, the highest we have measured is .1 g on the second floor, after it had been amplifed from the ground.

So it would, I suspect, be somewhat of a difficult task to instrument, especially these are normally underwater, and that's again the environment in which you'd like to measure.

So we have not considered it, and we have not made a measurement. It would be a somewhat difficult task to do, and you might wait quite a while before you would get data, if at all.

Q I'm really not asking you to do anything. I'm only exploring some of the things that you say here, and I guess perhaps the reason behind these questions is that as I read through the testimony, I note that a great deal of modeling was done.

With all due respect to those who did the modeling, and who enjoy working with computers, I think that that is certainly one aspect of reality. There is another aspect, and that is a measurement, and it just seemed to me that we would want to support any computer analysis that we rely on with this much instrumental measurement as we possibly can.

Very good. But nothing, you say, has actually been measured as regards the control rods?

A Not in regard to that particular thing. We -- just a second. I want to clarify one point with Dr. Kost.

(Panel conferring.)

A (Witness Gilliland) Judge Ferguson, one additional comment:

We did do a measurement on a control rod assembly with a side load of 1 g. It was laying on its side and we measured the friction force, because we were interested in comparing it with what we considered to be the driving force for its going into the core. I don't remember the numbers, but the friction forces were well below the gravity force, plus the flow force.

Q Friction force between what?

A The control rod assembly and the control rod guide tube in which it is housed.

Q I see.

Is there a watery layer between that when it's in the core?

A Yes, there is, and when we measured that, it was dry. So one would expect the friction to be lower even still in the operating condition.

Q I see.

Let me call your attention to three basic mechanical and structural requirements that you referred to on page 23 and 24 of your testimony, and at the moment I want to focus only on your comment that -- well, maybe for completeness, let me tell you what those three requirements you say are:

The first requirement is that the fuel element containers must be kept intact.

The second requirement is that a water supply for boil-off and evaporation must be available.

And the third is the concrete structure which encloses the canal and fuel tanks must be kept intact.

And then you go on to say that you are making modifications to meet these requirements, but that none were necessary to meet the third requirement; that is the concrete structure which encloses the canal will be kept intact.

Can you give us some background as to why you feel none are required to meet that requirement?

A I'm sure Dr. Kost could elaborate, but the result of his analysis indicated that the concrete core structure was adequate to meet the demands without modification.

Q Okay. That's the basis of all of your testimony, is that right, Dr. Kost? Your analysis of the concrete structure itself, is that correct?

A (Witness Kost) That's correct.

(Board conferring.)

JUDGE GROSSMAN: Why don't we break until tomorrow at 9:00 o'clock, at which time we will continue with Judge Ferguson's examination?

Okay. We can adjourn now and we can discuss the scheduling after that, unless there is some order of business that you want on the record now.

MR. EDGAR: I have none.

JUDGE GROSSMAN: We meet here tomorrow. Okay.

(Whereupon, at 5:00 o'clock p.m., the hearing was adjourned, to reconvene at 9:00 a.m., Tuesday, June 9, 1981.)

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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: Monday, 8 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.

Jane W. Beach

Official Reporter

Ann Riley

Official Reporter