UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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

HEARING

Veterans Memorial Hall
522 South L Street
Livermore, California

Friday, May 29, 1981

The above-entitled matter came on for hearing pursuant to recess at 9:30 a.m.

BOARD MEMBERS PRESENT:

HERBERT GROSSMAN, ESQ., CHAIRMAN
Atomic Safety & Licensing Board Panel
U.S. Nuclear Regulatory Commission
Washington, D.C. 20555

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I N D E X

P R O C E E D I N G S

9:30 a.m.

CHAIRMAN GROSSMAN: The hearing will now come to order. The third day of hearing in the Show Cause proceeding is now in session.

MR. EDGAR: Mr. Chairman, we have Dr. Reed, Mr. Harding and Dr. Jahns available to provide a brief response to Dr. Ferguson's question yesterday concerning the open file report.

CHAIRMAN GROSSMAN: Thank you. Gentlemen, you may proceed.

WITNESS HARDING: I have asked Dr. Jahns to join us up here because of his many years of experience in the transverse ranges just in case any questions come up on the San Fernando fault.

Yesterday, the Board asked us to review this open file report 81-668, by Robert Sharp, to determine if there were any significant new information in this in which the Board would be interested. While we don't feel that we can really comment on whether or not the Board would be interested in this, we have reviewed it and we find that there is no new information in here which would cause us to change our analyses of the San Fernando data or to change our conclusions regarding the one meter offset which the NRC has recommended as design criteria.

There are several reasons for that. To begin with, we don't base our final conclusion on the offset criteria mainly on the San Fernando data. We feel it is more appropriate to use the slip rates calculated for the Verona fault based on the geologic information we have right there on the site. That slip rate is a maximum of 2 feet every 10,000 years for any one single shear and 4 feet every 10,000 years across the entire Verona fault zone. That is our main basis for the conclusion that one meter is a conservative design criteria.

San Fernando was used mainly for comparison purposes only, kind of as a check on that. If you will recall some of my comments in the last two days, the San Fernando and Verona faults are really two different faults. They are both thrust faults but they are in different tectonic regimes. The San Fernando fault is a small segment of a large fault that is more than 100 miles long. The segment which broke was 15 to 19 kilometers. Compare that to the Verona fault, which has a total length of we feel 8 kilometers and possibly as much as 12.

The stresses that cross the faults we feel are much different. By any comparison, including the slip rate, we would expect then that offsets on the Verona would be much less than what occurred on the San Fernando. If you use a slip rate comparison it would be one-sixth. So on that basis it does not really change our conclusions regarding the off-

set.

Dr. Reed I think can discuss some of the data points which are presented in the chart paper and how that fits in with our analysis.

WITNESS REED: I would first like to give the Board a little bit of the background leading up to the open file report. Initially the NRC staff analysis was based in part on data by Barrows, which is given in a California Division of Mines and Geology report. This data by Barrows consists of 179 data points in the vertical direction, vertical offset, of which a mean value was calculated to be .34 meters. Also from the Barrows report there were 40 data points in the horizontal or lateral direction that had a mean value of .4 meters.

Subsequent to that -- that was published in the Staff's SER -- subsequent to that during the deposition that was taken on March 25, 1981, Dr. Earl Brabb stated that he felt that data by Sharp were preferable because they were based on direct measurements of net slip and taken at the same location. I think the Board needs to understand here that the data that was given by Barrows consisted of components of the net slip, not the net slip directly, but components -- the vertical and the lateral and the dip angle. And they were taken many times at different locations.

The Sharp paper, which was published, is also really

a composite of individual components. The net slip was not measured directly, but rather components were measured generally close together, in one case up to as far away as 250 meters.

Now after Dr. Brabb made this statement in his deposition we felt that it was necessary to go back and look at the Sharp data and, in this process of doing this, we did an analysis that is given in Appendix B of Exhibit 1. Now that analysis in Appendix B includes not only the Sharp data as given in the paper by Sharp -- that paper is referenced in Appendix B -- but in addition to that, the Barrows data and data from many other sources, but principally the data comes from three sources: Barrows, Sharp, and an author by the name of Kamb.

Now the analysis that we did for Appendix B used the data by Sharp as reported in his technical paper. Now at the time we did this analysis, which was a few weeks ago, we asked Sharp to give us a copy of his raw data because we were making some inferences from his data. He gave us -- we completed our analysis. He gave us a copy of his data and at that time he brought to our attention that there were some discrepancies or changes that he would like to make to those data. We took a look at those changes and found that the analysis that we had done for Appendix B was not affected.

Now as you know, the other day the USGS open file

report came out with an analysis by Sharp using his data and using his corrected data. So that sort of is a chronology leading up to the open file report.

Now I would like to just spend a few moments kind of reviewing the results of the report, the open file report, and the Appendix B that we have included with Exhibit 1.

The Sharp open file report gives a mean offset, mean slip offset, that ranges between .59 and .78 meters. The Appendix B results that we present in Exhibit 1 we feel are better, a better analysis of the data, for several reasons. These are: first, we included all the data points. Sharp in his open file report uses only 18 data points. In the Appendix B, by including not only Sharp's but Kamb's and Barrows' data, we had a total of 81 lateral components, 238 vertical components, and 85 dip angle measurements.

The second reason is that our analysis, we feel, is more statistically rigorous, in that by using proper procedure we are not limited to only data points that measure net slip. We can work with the components and include those in the statistical analysis. This allows us then to use not only Sharp's data, but Kamb's and Barrows'.

The final reason that we feel that the analysis is Appendix B is more realistic is that the analysis fits better the GETR situation. Our results are that we obtain a mean slip offset of .22 meters.

In conclusion, we feel hat the open file report is not inconsistent at all with the analysis that we have done. As Mr. Harding mentioned earlier, we feel the more appropriate basis for establishing the criteria is to look at the data that was actually obtained at the site. As Mr. Harding said, we can't judge whether this open file report will be of interest to the Board; however, we believe that the information that is contained in that has been included and accounted for in our analysis.

DR. FERGUSON: Thank you, Mr. Harding and Dr. Reed, for that information.

(Off the record)

CHAIRMAN GROSSMAN: The hearing will be back in session.

DR. FERGUSON: Mr. Harding and Dr. Reed, I understand that my expression of thanks for your efforts were not recorded, so I want to thank you once again.

I was turning, I think, to you, Mr. Swanson, and trying to recall the words that you entered into the record when you offered or distributed this document, this open file report that we are talking about, Open File Report 81-668 from the U.S. Department of Interior Geological Survey. I am paraphrasing now, Mr. Swanson, what I remember you saying and you can correct me if I am incorrect.

You I believe distributed this report with the

statement that you felt it was recent information and the staff wanted to distribute it so that nothing that was or may be of value would not be known to the parties. That's my paraphrasing of your statement. Is that essentially correct?

MR. SWANSON: Yes. I think that is an accurate paraphrase. The exact words appear on transcript page 258 that, if you would like, I would be prepared to expand on that.

The purpose of bringing that to the Board's attention was I think in line with the teachings of the Northanna proceeding and other cases which clearly place an obligation on the part of the Staff to bring forward information which we consider relevant to the proceeding. I think our position is that it clearly is relevant. And it was recently released, as we mentioned. The morning it was handed out was the first that we were permitted by the USGS to -- were able to obtain copies to publicly release it.

We do not feel, however, that it is material which is significant -- sufficiently significant from the point of being inconsistent with or at odds with the previous testimony that we personally would offer it. We would not object to it being placed in evidence, but we were not personally going to offer it. The results of the Sharp data and analysis is that the San Fernando event caused an average mean plus one standard deviation of surface rupture I believe of -- well,

there are two methods. One was a .70 meter surface rupture; another method came up with the result of .74 meter rupture. The Staff came up with an analysis which produced a mean of about 1 meter. One could argue that the Sharp data points to the conservatism of the Staff analysis. In that sense, it is perhaps material as well as relevant.

But it was not our intention at that time to introduce it. It is new information. It is something that came up at the last minute. Because of its -- the fact that it did not significantly affect the Staff's position, we didn't intend to bring this up at the last minute and offer it into evidence. We did, however, feel that it was our obligation to make it publicly available and that was the purpose for the distribution.

DR. FERGUSON: Thank you, mr. Swanson. By the way, we had a little manipulation by our mechanic or engineer sitting at the desk there on these microphones. Can everyone hear what I am saying?

Very good. Then let's proceed from that point.

Without revealing anything, Mr. Swanson, at this time that you do not want to reveal, you have said that this particular document is relevant. You I don't think used the word "significant". I think you intimated that it had perhaps some bearing on this case. You did not intend to enter it into evidence. May I ask you whether or not you intend to

refer to this document in any of the testimony that you hope to present later?

MR. SWANSON: No, it is not referred to in our testimony and we would not intend to rely on it in our testimony at this hearing or in findings. I did indicate that we certainly would not object to it being introduced. It would possibly necessitate bringing yet another witness. I think that plus the lateness of its availability probably more than anything else dictated that decision to not offer it at this time.

DR. FERGUSON: So it is merely an informational document, is that correct?

MR. SWANSON: That is correct.

DR. FERGUSON: Well, that helps to clarify a point in my mind.

Now let's turn back if we possibly can to the panel. Dr. Reed, you gave us a chronology of events leading up to today, I suppose, and our review of this particular document. In that chronology you mentioned a recent publication of USGS within the past few days, I think you said. Is that the document that we are referring to, the Sharp document?

WITNESS REED: That is correct.

DR. FERGUSON: There is nothing further than the document we are now referring to, is that correct?

WITNESS REED: As far as I know, that is correct.

DR. FERGUSON: Okay. You also suggested that the analysis that you have undertaken perhaps could be relied upon more heavily than the Sharp interpretation because you used many more points. I think you said Sharp used on. 3 whereas you used 80 or more points, is that correct?

WITNESS REED: That's correct.

DR. FERGUSON: Is that negligence on Sharp's part or why did he not use more points, if more points would have been valuable?

WITNESS REED: I don't know the reason why he did not include the other points, other than this general feeling that was expressed by Dr. Brabb and possibly Sharp may also have believed it that in order to analyze the data you had to have net slip values. You could not work with the components. If you limit yourself only to net slip values, Sharp was stuck with his data, although he could also have used at least one other reference, and that was Kamb's data that gave net slip values, which were not -- I forget the number of points there, but there wasn't more than 10 or 12 additional ones he might have used.

DR. FERGUSON: Very good.

MR. EDGAR: May I ask a point of claricication here?

DR. FERGUSON: Sure.

MR. EDGAR: People -- in some of the discussions there has been a discussion or the use of the nomenclature

"the Sharp data" and "the Sharp paper", and it may be confusing in the record. There are two documents authored by a gentleman by the name of Robert Sharp. One is Reference 3 to our Appendix B, which is cite dat page B-11 of our Exhibit 1. That is what people have commonly referred to as the so-called Sharp paper That was an earlier, 1975 document. The Open File Report is a subsequent calculation based on the 1975 paper.

So it might be well for people to consider a convention to distinguish the two Sharp papers.

DR. FERGUSON: Thank you, Mr. Edgar. We have only one document that we are discussing today and that is the Open File Report 81-668.

Let me ask anyone on the panel to help me understand a little bit about the areas mentioned in this document that I just referred to. Help me understand these regions in terms of where we are at the present time, that is, where Livermore stands. I don't know where -- I haven't had a chance yet to review any documents that would help me understand where the Sylmar segment is or the Tujunga segment. Could you in just a few brief words -- or the Lakeview segment, for that matter -- relate those segments to any document that we have seen in our testimony thus far?

WITNESS HARDING: Judge Ferguson, if you have a copy of our testimony, which is Exhibit 1, I'll try to find a

photograph for you. I think maybe with reference to that we can point these out.

Figure 36, which is on page 63. If you look at the bottom photograph in Figure 36 you will see the name San Fernando F there -- that's for San Fernando Fault. The white dashed line which is right next to that name is the Tujunga segment. Now the fault makes sort of a right angle bend there toward the north and then again trends westerly out from that bend. That little westerly trend out from that bend is the Sylmar segment. The Mission Wells segment is a very short segment which really doesn't show up too well at this scale, but it would be on the end, on the westerly end of the Sylmar segment.

And then the eastern portion of that line which I pointed out was the Tujunga segment would then be the Lakeview segment. Have I covered them all?

DR. FERGUSON: Yes. Thank you, Mr. Harding. It was very helpful.

Let us turn to the Open File Report 81-668, page 8. There is a discussion there called Prediction of Future Fault Displacements. I'd like any member on the panel who would want to help me understand this to interpret the discussion at the bottom of that segment which begins -- and I quote -- "Although the rupture length, displacement and magnitude M 7.0 of the 1940 earthquake were larger than those

of the M 6.5 event of 1979, the ground surface broke along nearly identical traces in the fault segment common to both events. If we had attempted to predict the 1979 earthquake magnitude, rupture length, and maximum and average displacement of the 1940 event, we would have overestimated each. If the order of the events were reversed, however, the less than .4 meter average displacement and the .8 meter maximum movement of the 1979 earthquake would have seriously underestimated the more than 1.7 meter average and the 6 meter maximum slip of the 1940 shock. Underestimation would have been minimal if the maximum 1979 displacement were used to predict the average 1940 displacement."

Would someone on the panel interpret that statement?

WITNESS JAHNS: I think my own first comment would be in the way of a generalization. The real point that Bob Sharp is making here is to point out the inherent uncertainties of purely empirical correlations. He is saying that here we have a very unusual situation of two events on the same fault, the Imperial fault, with epicenters on different parts of the fault but with an actual overlap of the two rupture areas along the fault. So this represents an unusual opportunity to compare the parameters of two events with a lot of other things being equal because of the commonality of the occurrence.

He is simply saying that you can't very well use the parameters of one event to predict with complete satisfaction

the parameters of another event on the fault either preceding it or succeeding it. The somewhat confusing at first glance manner of presentation is I think nonetheless very interesting because he is attempting to point out what would have happened if one had used the 1940 event to predict the later one or the strongly contrasting result that you would have obtained if the later event had been used to predict let's say a future event exactly like the 1940 one.

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DR. FERGUSON: Dr. Jahns, that's the point I think I'm trying to make. We, of course, are attempting perhaps hopefully successfully to predict what the future will hold and I guess the point that I would really like to have the panel address is are we essentially allowing the possibility,as pointed out in the section of Sharp's p[illegible] exist.

That is to say -- or to occur. That is to say, we have looked at past events and we hope to predict future events. And presumably we have predicted that future events would be small. Because of the uncertainty that you have just mentioned, Dr. Jahns, is there the possibility that our predictions would be incorrect? The direction of our predictions.

WITNESS JAHNS: I think I would respond affirmatively if the approach were entirely empirical. This is, of course, the great weakness of a purely empirical approach in dealing with events of this sort. Specifically to this case in the Imperial Valley. The 1940 event was a truly unusual one in terms of the maximum surface displacement along the fault. Now, if one examines beyond the purely empirical relationships and begins to ask questions about the fault itself in terms of seismic moment, which in its turn reflects some certain fundamental parameters of a given earthquake and of the materials affected, then one can begin to understand why this happens to be a peculiar point on any kind of empirical plot.

Because, here's an area where the crust is very thin, so if one is examining the amount of energy released as expressed by the magnitude and the rupture area -- the rupture length in effect has to be pretty long in order to give an area with a shallow crust. In other words, the vertical dimension is short. In order to give an area appropriate to that kind of energy release, the length of the rupture has to be considerable and so does the amount of rupture. Because the seismic moment, whichis a fairly fundamental parameter, is the result of multiplying these factors.

So, if one is pretty small, the other has to be large. This is the kind of qualification that I would -- I try to keep in my own mind in comparing these empirical plots.

DR. FERGUSON: I want to thank the panel for coming back and helping us understand the significance or lack thereof of this particular document.

There is one question that I failed to ask each member of the panel yesterday after my cross examination and that question is this. Based on your judgment as an expert in the field in which you are, do you feel there is any factor that has not been considered that would endanger health and safety if this plant were permitted to restart? I'd like each of you to answer that question separately.

WITNESS REED: My answer to that question is no.

WITNESS JAHNS: That's mine as well.

WITNESS HARDING: I would have to concur.

DR. FERGUSON: I thank the panel very much for coming back. I have no further questions.

CHAIRMAN GROSSMAN: Mr. Barlow or Mr. Cady. Who ever wants to conduct this part of the cross examination. Do you need five minutes to prepare?

MR. CADY: Yes, Your Honor. Five minutes would be fine. Thank you very much.

CHAIRMAN GROSSMAN: We'll be recessed for five minutes. Off the record.

(A brief recess.)

CHAIRMAN GROSSMAN: On the record.

Will the panel members please take their seats?

MR. EDGAR: Mr. Chairman, I'd like to state for the record, we'd like to have Dr. Garrison Kost join the witness panel. We have previously identified him as a witness in connection with this piece of testimony to be available to answer --

CHAIRMAN GROSSMAN: I'm not sure we had our signals straight here. I had assumed -- Did you have cross examination of the prior panel on the matters that were discussed this morning?

MR. CADY: Cross examination for the geology offset panel?

CHAIRMAN GROSSMAN: On the matters that were on the

Board questions and the discussion this morning. You are certainly entitled to cross examine on that and I assume that everyone was preparing for that.

MR. CADY: No, Your Honor, we are perfectly satisfied with the examination performed by Judge Ferguson.

CHAIRMAN GROSSMAN: I see.

MR. CADY: Thank you.

CHAIRMAN GROSSMAN: Mr. Swanson and my remarks were directed towards all counsel here.

MR. SWANSON: I had understood that you were allowing us a chance to think about the remarks that were just made as opposed to the new panel. May I have just a moment?

CHAIRMAN GROSSMAN: And for that matter, Mr. Edgar, too, you're entitled to ask questions on redirect, too.

MR. EDGAR: Yes, sir.

MR. SWANSON: I really have one statement that I want to make and I guess just one question for Dr. Reed.

CROSS EXAMINATION

BY MR. SWANSON:

Q I just wanted him to explain what he meant by his methodology being more rigorous than Mr. Sharp's.

A (Witness Reed) What I meant by that comment was in regards to the ability to use all of the data that was available in Sharp's analysis in the open file report. He works only with his data points rather than everybodies data

points of the data that's available.

Now, in the analysis that we conducted in Appendix B, we were able to use not only Sharp's data, but other data -- Other data being not only net slip vectors, but also components.

Q And in using that other data, did you include points of zero amounts of offset in the analysis?

A That is correct, as did Sharp.

MR. SWANSON: That was all the questions I had. I did have one statement that I want to make, because there was a chance that this might not come up again and it's in reaction to Dr. Ferguson's question about the Imperial Valley event and I think that raises -- obvious in my mind, perhaps, because I'm sensitive -- a question as to why the staff did not also consider that to be relevant and why we didn't offer it, perhaps for that purpose.

The answer is simply to refer to the last sentence of that document, the Sharp Document, which states that it would be inappropriate, however, to extend the use of this factor -- again referring to the Imperial Valley data -- to other faults. Particularly faults that are not strike slip in character until further comparative studies are made after future fault displacements.

To my knowledge I believe the characteristic of the opposed -- the Verona Fault having thrust characteristics as opposed to strike slip characteristics is unchallenged in

this proceeding and it was on that assumption that it was -- falls into the category of faults other than strike slip that would render the Verona Fault to be not valid to comparison with the results that Mr. Sharp had on the Imperial Valley. It was for that reason that we also considered this not to be material -- or at least this aspect -- not even to be relevant to this proceeding and it was on that bases that we decided not to offer it into evidence.

CHAIRMAN GROSSMAN: Mr. Swanson, I'm sure you intend to ask the panel the appropriate questions in order for them to state on the record what you've just indicated, since, of course, you're not presenting evidence.

MR. SWANSON: Of course, our statements in the Staff testimony about the characteristics about the Verona Fault being thrust as opposed to strike slip, but it could be highly on direct.

CHAIRMAN GROSSMAN: What I'm directing my remarks to right now is the fact that the record will not be complete if you intend to rely on your statements as to why the Imperial Valley data are not appropriate for use in the Staff's expert testimony. You will need your witness to state that.

MR. SWANSON: I understand that. I was merely, I guess, following up on the question that Dr. Ferguson asked earlier as to why we are distributing it and I guess implicit in it is why we are not offering it and this last factor which

was separate from the analysis of San Fernando data came up in questioning and I just wanted to make the record clear, since this was not in evidence, why this additional part was also not considered to be relevant by the staff.

I did not mean it to be testimony.

CHAIRMAN GROSSMAN: Mr. Edgar?

MR. EDGAR: I have one question of clarification. Dr. Ferguson had asked several questions and Mr. Harding responded in defining the location of certain segments of the San Fernando Fault. There was also an element of Dr. Ferguson's question that I don't think was answered.

REDIRECT EXAMINATION

BY MR. EDGAR:

Q Where is San Fernando located in relation to Livermore and could you give us, Mr. Harding, by reference to your figures in your testimony, a location for the San Fernando Fault in relation to the Verona Fault and Livermore?

A (Witness Harding) I'm not sure that my figures cover the whole state, which you would have to do to show that comparison on one figure, but we're sitting here in Livermore in the Bay Area of Northern California. The Transverse Range is in Southern California some 400 air miles from here. Three to four hundred air miles from here, due south.

Q Where in relation to Los Angeles are the Transverse

Ranges?

A The Transverse Ranges are just north of Los Angeles. In fact, the eastern portion of them are the northern boundary of the eastern part of Los Angeles Basin. In San Fernando we have an intervening mountain range there which is the Santa Monica Mountain Range which separates the Los Angeles Basin from the San Fernando Valley and in that portion, the Transverse Ranges are the northern boundary of the San Fernando Valley.

MR. EDGAR: I have no further questions.

DR. FERGUSON: I have a short question for you, Mr. Edgar. Are you trying to establish the fact just then that the San Fernando range was some distance from the Verona Fault?

MR. EDGAR: I just wanted to be sure that the record wasn't confused on that point, were their analogies. I mean, they're being used in this proceeding and the technical analysis as analogies to one ancther, but no where thus far in the record, has anyone said where San Fernando is geographically and where Verona is geographically and how far about they were.

DR. FERGUSON: And that was what you were trying to establish, is it not?

DR. EDGAR: It wasn't a very profound point. It was one of clarification.

DR. FERGUSON: Thank you.

CHAIRMAN GROSSMAN: The panel is now excused. I would like all of you to realize that if you're recalled, we are not going to give you the oath again. You remain under oath. Thank you very much, gentlemen.

(Whereupon, the panel was excused.)

CHAIRMAN GROSSMAN: Dr. Kovach?

MR. EDGAR: Your Honor, I had mentioned earlier that we would like Dr. Garrison Kost to join the witness panel. We have identified him as available with this panel to answer questions in the area of the interface between seismology and the structural engineering. Some of the questioning yesterday bore on that interface and we'd like to have him join the panel to facilitate responses in those areas.

CHAIRMAN GROSSMAN: Dr. Kost, would you raise your right hand please?

Whereupon,

GARRISON KOST

having been first duly sworn, was called as a witness herein and was examined and testified as follows to join the panel which had previously been sworn:

CHAIRMAN GROSSMAN: Please be seated.

MR. CADY: Your Honor, to begin with, I would like to examine -- is it doctor or mister Kost?

WITNESS KOST: Doctor.

MR. CADY: I would like to examine Dr. Kost on his educational qualifications that would qualify him as an expert before turning the remainder of the examination over to Mr. Barlow.

CHAIRMAN GROSSMAN: Proceed.

CROSS EXAMINATION

BY MR. CADY:

Q Could you please give us a brief summary of your education background, because in the testimony that was submitted, there was no resume attached.

A (Witness Kost) My resume can be found in the back of Exhibit No. 22 which is the structural panel, Panel No. 3. You may want to refer to that.

Q To facilitate, so that I don't have to look through my desk, could you just give me a brief rundown of what -- your education?

A Yes, I have my bachelor's degree in civil engineering with emphasis on structures. I have my master's in engineering degree in structural engineering and also a doctorate from Stanford University in structural engineering. I'm a licensed civil and structural engineer in California.

Q Could you give us a summary of your experiences with seismology and in the area of seismology and seismicity, please?

A I've been involved in the design of structures to

withstand earthquakes for many years. For basically all of my professional career. And during that time, I have worked very closely with seismologists to develop engineering criteria for the use and design and evaluation of structures to withstand earthquakes.

MR. CADY: Thank you very much.

CHAIRMAN GROSSMAN: Mr. Barlow, proceed.

MR. BARLOW: Thank you.

CROSS EXAMINATION

BY MR. BARLOW:

Q I would like to begin with a few brief questions to Mr. Gilliland. Mr. Gilliland, has Dr. Bruce Bolt consulted for General Electric regarding the seismic hazards to the GETR?

A (Witness Gilliland) Dr. Bolt repaired a report with respect to the microseismicity in the region of the GETR.

Q Was that report the one published in March 1980 entitled, Seismicity of the Livermore Valley in Relation to the GE Vallecitos Plant?

A I believe so. I don't recall the exact title, but I believe it's correct.

Q Are you aware of a report prepared by Dr. Bolt regarding the January 1980 Livermore earthquake sequence which shook the Vallecitos Valley?

A I understand that Dr. Bolt has prepared the report and -- but I have not seen it nor have I read it.

Q Has General Electric presented a copy of that report in this proceeding?

A Presented a copy of that? No.

Q Why did General Electric decide to not present Dr. Bolt as an expert witness for the GE seismology panel?

A Let's see. I believe we answered that in an interrogatory. Let me refer to that for just a moment?

(Pause)

Our answer reads as follows: This is in answer to interrogatory fourteen, your interrogatory data 3-16-81 and our response of April 3. Drs. Bolt and Hansen were among those consultants who provided input to GE seismic and geologic investigation. Dr. Kovachs will use that input along with additional information identified in Licensee supplemental responses to Intervenors to present GE's overall position on seismic considerations and we believe that the roles of Drs. Bolt and Hansen in the GETR review was not great enough to warrant their appearances as witnesses.

Q Thank you.

Dr. Kovach, are you aware that Dr. Bruce Bolt as the chief seismologist at the University of California at Berkeley Seismology Laboratory has prepared a number of reports regarding seismic events in Northern California in the

region of interest regarding the GETR?

A (Witness Kovach) He's written many papers that I'm aware of, but -- Can you hear me?

Q Yes, I can. Go on.

A I'm not aware of what specific reports you're referring to.

Q Specifically, I was referring to a number of reports that he has issued that catalogue seismic events in the region.

A Are you talking about the routine catalogues that come out of the seismigraphic station?

Q Those are included, yes. Are you familiar with those?

A I'm familiar with those, yes, I see them from time to time.

Q I believe you said yesterday that you have reviewed Dr. Bolts report on the Livermore earthquakes of 1980?

A I have read that paper. Is that the Seismological Society Bulletin?

Q Have you discussed that earthquake sequence with Dr. Bolt in preparation for this hearing?

A No, I have not.

Q Have you discussed with Dr. Bolt his opinions of the phenomena or concept of seismic focusing?

A No, I have not.

Q Have you -- excuse me. Are you familiar with a report which includes a working hypothesis of Dr. Bolt on -- entitled Policies for Seismic Safety, published by the University of California at Berkeley Institute of Government Studies in 1979?

A I'm not personally acquainted with that document.

Q If I could read a couple of sentences from that report that are in regards to Dr. Bolts work in this region and ask you your opinion of those statements, if that's okay.

MR. EDGAR: Could we have the document made available to the witness so he can see the complete context of it?

CHAIRMAN GROSSMAN: Yes, certainly, counsel. Would you show that document to the witness?

(Pause)

BY MR. BARLOW:

Q Dr. Kovach, could you read the first two complete paragraphs on page two of this document, which would be the subject of my questioning.

CHAIRMAN GROSSMAN: Could you first let him have a chance to look at what the document is?

MR. BARLOW: Certainly.

(Pause)

WITNESS KOVACH: Would you give me a minute, please?

MR. BARLOW: Certainly.

CHAIRMAN GROSSMAN: And counsel, could you also

bring the document up here first so we can see before the answer is --

WITNESS KOVACH: Should I read it aloud first or circulate it?

MR. BARLOW: Certainly.

(Pause)

MR. CADY: Your Honor, I've been assured by Mr. Barlow that in the other statements that of this nature so that we don't the round robin review of the document. There will be no more instances in his examination along these lines.

CHAIRMAN GROSSMAN: Thank you.

I believe there is a question pending to Dr. Kovach. Is that correct, Mr. Barlow, or hadn't you formulated a question, yet?

MR. BARLOW: I wanted --

CHAIRMAN GROSSMAN: Well, whether or not you had, please formulate a question now.

BY MR. BARLOW:

Q Dr. Kovach, could you read outloud the second full paragraph on page two of that document?

A (Witness Kovach) Yes. This document is entitled, Policies for Seismic Safety, Elements of the State Governmental Program and the author, apparently, is Stanley Scott from the Institute of Governmental Studies, University of California, 1979. I am reading two paragraphs on page two.

It says:

"Finally boundaries between major continental plates are known to pass through California and these plates are moving with respect to one another. Plate tectonics is, in fact, the most convincing and currently accepted explanation for a majority of earthquakes wherever they occur. Plate movements in California are demonstrated by such evidence as the northwesterly drift approximately three inches per year of the Farralon Islands about 30 miles west of San Francisco with respect to Mt. Diablo, 30 miles east of San Francisco.

"The resulting strain building up in the intervening formations would have to be relieved by slippages that will almost certainly occur along one or more of the major active faults traversing the San Francisco Bay Area. Given the long 73 year interval since the most recent great earthquake in Northern California, the amount of slippage that must occur will be sufficient to produce one or more great earthquakes."

Second paragraph:

"Relying on such evidence, the University of California seismologist, Bruce A. Bolt, recently put forth the working hypothesis that a great earthquake is likely to strike somewhere in California within the next ten years and probably will effect some major urban areas. Bolt estimates the likelihood of this happening within ten years as higher

than 50 percent. Moreover, he points out that the probability of such an earthquake occurring within a specified period increases progressively as more time elapses since the last great earthquake."

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Q Dr. Kovach, do you have any disagreements with the statements that you read?

A No, I have no particular disagreements .

Q Do you therefore agree with Dr. Bolt's working hypothesis that a great earthquake is likely to strike somewhere in California within the next ten years?

A I'm not sure that I would specifically agree with the time period of ten years, but I would agree with the statement that a great earthquake is likely to occur sometime in the future.

Q Would you define the word great in the term great earthquake? Are you familiar with Dr. Bolt's?

A As I recall Dr. Richter rdefined a great earthquake as an earthquake being greater in magnitude than 7.5.

Q Greater than 7.5?

A 7.5 or greater.

Q Thank you.

Regarding the evidence that Dr. Bolt's working hypothesis is based upon -- is this in regards to the concept of seismic cycles and seismic recurrence intervals?

A Well, I'm not intimately aware of how he formulated that decision so I'm not able to respond to that.

MR. EDGAR: Mr. Chairman, I have a suggestion here.

If the line of inquiry is directed toward the Bolt paper, Dr. Jahns is a co-author and he may be helpful.

CHAIRMAN GROSSMAN: Would you like to have Dr. Jahns join the panel, Mr. Barlow for responding to these questions?

MR. BARLOW: Yes, sir.

CHAIRMAN GROSSMAN: Thank you Dr. Jahns.

MR. EDGAR: I'm advised that I may have mis-stated it, but the two gentlemen have worked together and the subscribe to the same theories.

It may be worthy of asking a preliminary question in that line.

BY MR. BARLOW:

Q Dr. Jahns, are you aware of the evidence and concepts upon which Dr. Bolt has based his working hypothesis regarding an iminent earthquake in California?

A (Witness Jahns) Yes. To some extent, at least.

Q Does it involve the concepts of seismic cycles and seismic recurrence intervals?

A Indirectly yes. This sort of prediction that was quoted here is based on a combination of the notion of plate movement on the historic record and to some extent

on at least one of the two factors you mentioned.

And, it was one of those situations which Dr. Bolt had been thinking about this from one standpoint and I from another and we put our heads together and came up with the suggestion that was quoted, although, I believe there is a slight mis-quotation.

Our parameter was an earthquake of magnitude 7.0 or greater.

Q 7.0 or greater.

A And according to some classifications, that range would include all of the great range and part of the major range.

Q Dr. Jahns, did you attend the meeting of the Seismological Society of America at the University of California, Berkeley in 1981?

A Yes, I did.

Q Are you familiar with the presentations given there by Dr. Darryl Herd and Dr. Earl Brabb of the U.S. Geological Survey regarding seismic cycles?

A Familiar to the extent that I listened to the latter part of it.

Q Do you agree that there was a seismic cycle in Northern California from 1836 to 1905 leading up to the 1906 San Francisco earthquake in which there were a number of earthquakes of magnitude 5.5 to 7.5 along the San Andreas

fault and it's major branches in the Bay Area?

A Yes, I think this is a matter of record and Dr. Herd and Dr. Brabb made a very interesting compilation of the record and pointed out what the record shows, and fair enough.

Q Do you also agree that the 1906 earthquakes were followed by 50 years of seismic quiescence in the Bay Area from 1907 to 1956?

A In a relative sense, that's what the record seems to show.

Q Do you agree that beginning in 1957 with the Daly City earthquake of the magnitude of 5.5 or the magnitude of 5.5 range that a seismic cycle of earthquakes of 5.5 to 6.0 began to recur from 1955 to 1980?

A I frankly have no opinion on that because when one reaches the stage of converting data into some king of cyclical interpretation, then I want to do a great deal more thinking about it, than I have, about that particular question. It's a very interesting notion.

Q Do you agree that during the period of 1955 to 1980, the Bay Area did experience a number of earthquakes in the magnitude range 5.5 to 6.0?

A Yes.

Q Is part of the working hypothesis that you and

Dr. Bolt have presented, based on these data points from seismic history in Northern California?

A No, not really because we were concerned with a different and more limited aspect of the problem.

We began by sorting out earthquakes of magnitude 7.0 or greater, Richter scale.

And the other beginning points in addition to the historic record, was our best appraisal of the general behavior of the two principle plates involved in this part of the world and a sort of review of the evidence that very strongly suggests that this behavior has been in a gross sense fairly uniform for the last 4-½ or 5 million years, geologic time.

So, it was really those two things that we put together and clearly, this did not lead us into the kinds of considerations that Dr. Herd and Dr. Brabb were involved with because we so reduced our data base with the magnitude range we selected that it didn't have much significance in terms of trying to recognize any cyclical trends within the data.

Q Does the working hypothesis that you and Dr. Bolt have developed depend more on the rate of movement between the two plates, the North American plate and pacific plate and the build up of stress along the plate boundary?

A Yes, and it goes like this. In brief, you begin

with a basic notion of the two plates with a more or less uniform rate of motion so that in effect is a given.

Then you look at the historic record and the first thing it tells you is that there is an average recurrence interval of magnitude 7.0 or 7.0+ earthquakes through the last 150 years, of 15 years.

And it also tells you right away that this is an arithmetic number that doesn't mean all that much because the actual recurrence between two events has been as short as two years and as long as 34.

But we were impressed not only by this variation, but by the fact it's crowding on to 34 years, 29 as I recall now, since the last magnitude 7.0 or 7.0+ in California and if we do go another decade, California shall have set a new record for historic time in terms of this relative quiescence.

This is a sort of simple tool approach, but is one that I think is basically sound and we jointly considered at that time, it justifiable to suggest that somewhere in California, via an extension of this record and the context of plate motion, there was at least a 50/50 chance and I think it's probably greater than that, of an earthquake, magnitude 7.0 or larger within the next decade. Eight years now, I guess.

Q Is it possible that the earthquake which you were

discussing that may occur in the future could occur on the Calaveras fault?

A Yes, that's possible.

Q Could that earthquake occur in the region of the Calaveras fault near the GETR site?

A Yes, that's also possible.

Q If this earthquake could occur during the next ten years, is it also possible that this earthquake could occur at any time on the Calaveras fault?

A That's also possible, and your questions have an interesting sequence, because the probability associated with that is steadily decreasing, in the order of the questions that you proposed.

Q Dr. Jahns, are you familiar with the concept of seismic gaps?

A I can't claim familiarity with it and I'm familiar to the extent of having read about it in the published literature.

Q Dr. Kovach, are you familiar with seismic gaps?

A (Witness Kovach) Yes, It's my understanding that a seismic gap is a substantial region of a plate boundary that has not had a large earthquake within a time frame of 30 to 100 years and it's one hypothesis that the big earthquakes tend to occur along those portions

of the plate boundary which have not experienced a major earthquake in historic time met by the definition of the seismic gap.

Q Dr. Kovach, have you reviewed reports prepared for General Electric by Dr. Bolt and Dr. Richter regarding the historic earthquakes along the Calaveras fault?

A I'm not familiar with that specific -- you said Dr. Bolt and Dr. Richter?

Q Yes, sir.

Dr. Richter's report -- well, Dr. Bolt's report we've already referred to from March, 1980. Dr. Richter's--

A The microseismicity -- yes, I have read that report and I believe I have read Dr. Richter's -- I'm not sure what your --

Q Dr. Richter's report is dated December 9, 1977 and entitled, "Potential Earthquakes on the Calaveras Fault", GETR, Vallecitos, California.

MR. EDGAR: Dr. Kovach, would you like to see a copy of the document? Would it refresh your recollection?

WITNESS KOVACH: I have not read this report.

BY MR. BARLOW:

Q Dr. Jahns, have you read this report?

A (Witness Jahns) Yes, I believe so.

Q Dr. Jahns, are you familiar with the historic earthquakes along the Calaveras fault zone? Referred to as

the Dublin or San Ramon earthquake of 1861, I believe it's July 3, 1861 and the Mara Island earthquake of 1898?

A I'm aware of the record, but I can't claim familiarity with it.

Q Have you done any sort of study of either of those two earthquakes?

A No, I have not.

Q Dr. Kovach, have you done any sort of analysis of either of those two earthquakes?

A (Witness Kovach) No, I could make one general comment about the 1861 earthquake which you are referring to. Not a great deal is known about it and it's presumed to have had it's epicenter near Dublin and the 1898 Mara Island, one had damage in Vallejo and estimated magnitude was 6.0 and that's as much as I know about it.

Q Would you associate either or both of those earthquakes with the Calaveras fault zone?

A That's the presumed epicenter but as I say, they're so very old and it's -- not that much is known about them.

Q But it is generally presumed that they are associated with the Calaveras fault zones?

A Yes, to the best of my knowledge.

Q Would you estimate recognizing that there are not precise instrumental records of these quakes, but would

you estimate that those are the two largest earthquakes in the past, during the historic record along the Calaveras fault zone?

A Yes, I would presume so.

Q Would you agree that the so-called Dublin earthquake of July 3, 1861 was accompanied by surface rupture?

A As I say, I have no specific knowledge of that earthquake other than what I read in one book. There was presumed to be five miles of rupture on surface associated with that earthquake.

Q Was that earthquake well, let me put it this way.

CHAIRMAN GROSSMAN: Excuse me, Dr. Jahns, do you agree with that too, sir?

WITNESS JAHNS: Yes, that's what the record shows to the extent that I'm aware of it.

BY MR. BARLOW:

Q Is it true, Dr. Jahns, that both the epicenters of the Mara Island earthquake and the Dublic earthquake were to the North of the GETR site along the Calaveras fault zone?

A Yes, I believe so.

Q Is it also true that during the 20th century, much of the seismic activity along the Calaveras fault zone has been to the South of the GETR site closer to the

junction to the San Andreas fault?

A Certainly there's been activity. I couldn't say how much relative.

Q Is it possible, either Dr. Jahns or Dr. Kovach, I would like to hear both of your opinions on this, that the area along the Calaveras fault zone just opposite the GETR is in a stated seismic gap?

A (Witness Kovach) Well, I think that's not perhaps the correct way to phrase it. I think if you're asking me whether there's been a lack of seismic activity in the historical record along that segment on the Calaveras fault, yes, but I'm not sure that I would agree that that's a fair statement of a seismic gap, because we're focusing on a single fault rather than on the major plate boundary itself which takes all of the faults in that into consideration.

Q Dr. Kovach, in your definition of seismic gap you said it is an area along the active plate boundary which has not had a major earthquake within the past 30 to 100 years, is that correct?

A Yes.

Q I've seem some definitions that limit that to 30 years and Dr. Jahns in your analysis that you presented earlier, you said that thirty-four years or 29 years was a long time to not have a magnitude 7.0 along the San

Andreas plate boundary is that correct?

A (Witness Jahns) It's a long time not to have a magnitude 7.0 or greater earthquake somewhere in Southern California.

Q Associated with the plate boundary?

A Ah, not necessarily.

Because, what may have been California's greatest historic earthquake was on an intra-plate fault. The Sierra Nevada fault in Owens Valley.

Q Well, I think I've exhausted this line of questionning and I'd like to change subjects.

Dr. Kovach, would it be possible for you at this time to present the results of the analysis regarding the question that was presented to you yesterday?

A (Witness Kovach) Well, yes.

Basically on the -- would you want to maybe for the record rephrase -- I'll tell you what I did answer and what I can give you are the values of the accelerations based on the data set which I examined for 3 size events. Is that what you -- at the distance of the Calaveras fault is what I did.

Q Yes, sir, if you could specifically define the distance and the magnitude that you used?

A The distance I used was 3.5 kilometers and the magnitudes which I examined were 5.5, 6.5 and 7.5.

CHAIRMAN GROSSMAN: Dr. Kovach, could you summarize basically what you were doing and what the results are? So that, it makes a complete record for anyone who would read it at this point? Rather than just pinpoint answers to what was said yesterday.

Just pretend that we're in one of your classes.

(Laughter)

WITNESS KOVACH: Okay, what I did was examine the data from the Coyote Lake earthquake and the Imperial Valley earthquake of 1979 which are the most complete data set we have available for describing the near field behavior of ground acceleration as we approach the vicinity of the fault.

Now, I established a functional form by doing a non-linear regression analysis of these data sets.

Now, assuming this was the appropriate functional relationship I then used this relation to extrapolate to a higher magnitude, ie., a magnitude of 7.0 on a 7.5 earthquake and I tested it against the albeit limited data which are available for distances for less than 100 kilometers in the magnitude range of greater than seven and basically there are data from 7 earthquakes, so it's not an overwhelming data set by all means in that magnitude range.

And, the predicted values were in very good agree-

ment with magnitude range and from that I used that to estimate what the accelerations would be.

Now, to be specific, then, about this one set of values, for a magnitude 5.5, my analysis would give 0.2G. For a magnitude 6.5, 0.39G and for amagnitude 7.5, 0.74G.

BY MR. BARLOW:

Q Dr. Kovach, are these G values effective horizontal values or instrumental horizontal values?

A (Witness Kovach) These would be instrumental horizontal.

Q Did you calculate instrumental vertical acceleration values?

A No, I did not do a similar analysis for verticals. I did examine the vertical accelerations as I mentioned yesterday for the Imperial Valley and tried to understand the anomalusly high values and I excluded that in my analysis in coming up with my estimate of what the reasonable vertical acceleration would be.

That was the extent of my analysis.

Q Do you mean that in your testimony when you were discussing vertical accelerations, that you excluded the high verticals from the Imperial Valley data set?

A Yes, that's correct.

Q Can you justify that exclusion of that data?

A Well, the reasons for this which I did mention yesterday were, I felt convinced that the reasons for these high vertical acceleration in the Imperial Valley were due to a local site condition through the fact that there's a very strong velocity contrast near the surface which preferentially refracts the compressional waive upwards and gives it a bigger amplitude and as I pointed out, the comparison of the velocity structures in the Imperial Valley and Livermore Valley, I mean, they're completely different and I was of the opinion and am still of the opinion that using those values in the Livermore Valley is not appropriate.

Q Can you, back to the calculations that you did perform and that you just presented, are these results -- would the results which you presented in terms of G values, for these three different magnitudes be changed if the distance of 3.5 kilometers would be changed?

A Presumably there would be some slight changes, yes, but in the near field if we can examine the curve here, you notice that the peak horizontal acceleration as you approach the fault is flattening and so then I don't think that it's a major change if you want to change the distance.

MR. EDGAR: What figure are you referring to in

that respect?

WITNESS KOVACH: Figure 6 in my testimony on page 19.

Just to clarify it for the record, this is a plot of peak horizontal acceleration in G on the vertical axis versus the closest distance of the fault and it's a log/log scale because of the range of the numbers so --

CHAIRMAN GROSSMAN: What page is that?

WITNESS KOVACH: Page 19. Exhibit 21.

MR. BARLOW: Are we waiting for an explanation to Mr. Edgar's question?

Do you have a question pending?

MR. EDGAR: No. I just wanted to help clarify it.

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BY BARLOW:

Q Dr. Kovach, there seems to be a disagreement about the distance between the Calaveras fault zone and the GETR reactor. I realize that in some of General Electric's presentations they have given the value of distance for 3.5 kilometers which is the value which you used, but in other presentations by GE the distance was listed as 3.1 kilometers. In presentations by the NRC Staff and the USGS in the SER's, the distance was listed alternatively as 2.3 kilometers and approximately 2 kilometers. And I would like to ask you --

MR. EDGAR: I would like to object to the form of the question in that it assumes facts not in evidence.

CHAIRMAN GROSSMAN: Well, does counsel disagree with the statement made as to --

MR. EDGAR: I have no idea whether those statements are true, Your Honor. If I had a specific reference, but that doesn't constitute evidence. It is fair to ask the witness his opinion of what the distance is.

CHAIRMAN GROSSMAN: Well, it's fair first to clarify what has been submitted by the parties. Mr. Swanson, could you clarify the situation for us? Were there different distances used in various submittals?

MR. SWANSON: If he is referring to the 2.3 kilometer figure in the Staff's document, that apparently is a typographical error. It is mentioned several times as being

approximately in the neighborhood of 3 kilometers. It should be 2 - 3, not 2.3.

CHAIRMAN GROSSMAN: Two to three kilometers?

MR. SWANSON: That's correct.

MR. EDGAR: Mr. Chairman, the other point is in the record, in response to questioning by Mr. Swanson the other day and some by Mr. Cady, Mr. Harding answered questions on the distance and calculated approximately 2 miles, which corresponds to a range of 3.5 kilometers.

MR. BARLOW: If I may rephrase the question and pose a hypothetical question to the witness.

CHAIRMAN GROSSMAN: Yes.

BY MR. BARLOW:

Q Dr. Kovach, if you were calculating a .G value for horizontal instrumental accelerations for a magnitude 7.5 earthquake and instead of using the 3.5 kilometer distance which you used you used a 2 kilometer distance, do you think that the G value would be larger than the value which you calculated for 3.5 kilometers?

A (Witness Kovach) Yes, I believe it would be slightly larger.

Q Do you have an easy way of calculating what it would be? I mean, would you be able to do that calculation today?

A Yes. I could probably do that today, but not here at the table in two minutes.

Q Perhaps over the lunch break, if you could calculate the horizontal instrumental accelerations for a magnitude 7.5 quake at a distance of 2 kilometers, I would appreciate it. Okay. Going on to further questions, if you took the instrumental data set from the Coyote Lake earthquake in 1979 on the Calaveras fault and the Imperial Valley earthquake of 1979 on the Imperial fault and calculated the vertical accelerations versus distance, would you be able to extrapolate for magnitudes 6.5 and 7.5 and arrive at an estimate of the vertical accelerations at distances of 3.5 kilometers and in the distance range of 2 kilometers to 3.5 kilometers?

A In principle I could do that. But it would, you know, involve some fairly substantial calculations. It is not something I can do overnight.

Q Would it be possible to do that by sometime next week?

A No.

Q How long would it take you to do such a calculation?

A My problem is I am going to Europe here very shortly and I just can't get involved in any heavy computations.

Q Do you have available to you the data set of the vertical accelerations from those two earthquakes?

A Yes, I would have them available.

Q If you are going to be leaving for Europe, is there anyone else that could make that calculation for the Licensee?

MR. EDGAR: Mr. Chairman, I would like to interpose an objection. I don't know where the line is leading. I really question whether the Licensee is under an obligation to perform calculations in support of the Intervenor's case.

CHAIRMAN GROSSMAN: I think this is going to require some consultation among the Board members. Let's clarify, though, for the record what you are requesting here. Is this -- were you requesting a computation based on the vertical accelerations of the Imperial Valley event now as projected to the distances from the GETR to the Calaveras fault?

MR. BARLOW: Yes, sir. That is correct in terms of the Imperial Valley earthquake and the Calaveras fault; however, I also requested an analysis of the Coyote Lake earthquake on the Calaveras fault which was a 5.7 magnitude and a magnitude 6.5 on the Verona fault.

CHAIRMAN GROSSMAN: Mr. Edgar?

MR. EDGAR: Our concern here is the fact that in Dr. Kovach's testimony, if you look at Figures 6 and 7, he has taken a data set, performed a regression analysis to develop a function that he then tests against the higher magnitude data set. So he can run a number for you if you pick the magnitude and pick the distance. But the question now presented is will you take another data set for us and run a new regression analysis. I am convinced that that's not our responsibility unless there is some strong showing

that the information is critical and that it can be obtained from no other source.

DR. FERGUSON: Mr. Barlow, I think the point has been raised. Could you help us focus on just where you are going with this line of questioning, what do you hope to establish?

MR. BARLOW: Okay, sir. I would like to establish some estimate by the Licensee's witnesses and next week we hope to do the same with the NRC Staff witnesses from the USGS of an estimate for the GETR site of ground accelerations both vertical and horizontal for earthquakes of magnitude 7.5 on the Calaveras fault and 6.5 on the Verona fault at distances that are estimated at various values. So that's why there is a range in the distance values. But do you understand?

DR. FERGUSON: I understand what you just said. Can you make that calculation? The reason I ask that question is are you asking the Staff or will you ask the Staff and the Licensee to do it to compare it against your calculation? Just why are you asking them to do it?

MR. BARLOW: We have not made those calculations. We are asking them to because these are the critical distances and magnitudes that are being discussed and the ground accelerations are very critical in analyzing the seismic design criteria for the GETR.

DR. FERGUSON: What do you think the calculations will show? Do you feel that -- do you have any indication that these calculations, if performed, will reveal anything that cannot be revealed otherwise?

MR. BARLOW: Well, the primary concern is that in the Imperial Valley earthquake, as I understand it and as Dr. Kovach said yesterday, the peak vertical acceleration was a 1.74 G and it was at a certain distance from the Imperial Valley fault. There also were peak vertical accelerations in the Coyote Lake quake on the Calaveras fault which are of concern to us. We would like to see those data sets applied to the GETR site calculations for seismic design criteria because we agree with Dr. Kovach that these two earthquakes are two of the best instrumented earthquakes in history and that the data sets from these two earthquakes are very good as pointed out in Dr. Kovach's testimony in Figure 6.

But these calculations have not been performed, apparently, in the testimony presented by the Licensee.

CHAIRMAN GROSSMAN: I believe the issue has been joined as to whether those earthquakes are appropriate, that is, the vertical accelerations from those earthquakes are appropriate figures to be utilized with regard to the GETR site. Now perhaps the Staff and Licensee want to stipulate that if they are appropriate then there is no way that a GETR can operate or -- I don't know. But the point is that is an

issue and maybe there is an easier way to resolve it.

MR. EDGAR: Let me suggest this: if the question is what is Dr. Kovach's calculation for 2 kilometers distance, if you are talking about distance and any variations in magnitude, those numbers are readily inferrable from Figures 6 and 7 of his testimony. The Intervenors can pick off the data points. That can be scaled simply. We have testimony that the vertical accelerations are not valid data points in Imperial Valley. We believe that that's the case. We haven't heard any testimony to the contrary.

Now we are being asked to go through the complete data set and redo the regression analysis. I don't see that that's our obligation.

CHAIRMAN GROSSMAN: I take it the vertical accelerations would not be in any proportion to the horizontal accelerations.

MR. EDGAR: To be fair there, Dr. Kovach has explained that he has done his regression analysis in the horizontal and, based on his analysis of the earthquake records, he recommends two-thirds of verticals for the horizontal case.

CHAIRMAN GROSSMAN: Well now if let's say in the Imperial Valley it were three-halves rather than two-thirds is it an appropriate -- if the conditions were the same in the Imperial Valley or analogous, would that three-halves be

an appropriate figure for vertical versus horizontal at the GETR site? Maybe that's an easier way of doing it. I don't know. Is there any simple proportion that can be used, either utilizing the horizontal figures on the assumption that the data from the Imperial Valley event and the other event mentioned are appropriate for use at the GETR site? Can you answer that, Dr. Kovach?

WITNESS KOVACH: I'm not sure I'd know how to do that because the site conditions are so uncomparable, as I mentioned. I'm not sure that I could come up with a magic scaling number that would be appropriate for the GETR site.

MR. SWANSON: Mr. Chairman, since Mr. Barlow indicated he will also ask the USGS to do that next week, perhaps I should indicate that we also would object and the basis would be that -- of course this is premature -- but that there is insufficient foundation to show the relevance of doing such a calculation. The argument will be, and it will be based on the testimony, presumably, which will be in the record at that time, that the data points that he wants to use cannot be transferred in a meaningful way to the GETR site because of anamolous situations. We will introduce testimony, if need be, to that point.

CHAIRMAN GROSSMAN: Let me ask you, did you perform the calculations anyway with regard to projecting the figures from Imperial Valley to the GETR site, whether you intend to

use them or not? When I say you I mean your Staff.

MR. SWANSON: I'm not sure what the answer to the question is, if they can or not. They have not been done and the reason would be because the data points I think that Mr. Barlow refers to in his testimony are not deemed to be relevant to the GETR site because of the anamolous conditions that existed that created that. So that calculation has not been done and we would strongly object to performing it, should Mr. Barlow then request it.

DR. FERGUSON: Mr. Barlow, I want to make it very clear if I possibly can to you that the Board is very interested in all of the things that you have and all of the things that you are trying to bring out. It is, however, difficult for us to understand the motivations that you have for asking for work to be performed unless you tell us what that motivation is. The motivation cannot be simply that you want to see the numbers. There must be a stronger basis than that.

MR. BARLOW: Are you asking me to explain that?

DR. FERGUSON: Can you share that basis with us?

MR. BARLOW: Yes, sir. At the GETR site I believe it has been agreed by the parties that the Calaveras fault is near the site; the distance is a matter of disagreement, whether it is 2 or 3 or 3.5 kilometers. But it has also been agreed that the Calaveras fault could generate an earthquake of magnitude 7.5. And in both the testimony by the Licensee

and the testimony by the NRC Staff there have been estimates of the seismic design criteria both for vertical accelerations and for horizontal accelerations. Now these estimates for the vertical acceleration estimates are based on a formula of two-thirds -- the verticals are two-thirds of the horizontals. And yet in the data sets of the two most relevant earthquakes which -- that statement of most relevancy comes from the USGS and the Licensee's witness and in the testimony -- the data sets on the vertical accelerations exceed two-thirds of the horizontal accelerations and they also exceed the seismic design criteria that have been recommended both by the Licensee and by the NRC Staff.

We are concerned that in the written testimony by the Licensee and by the Staff there are no calculations for vertical accelerations based on the data set from these two relevant earthquakes.

DR. FERGUSON: When you use the word "relevant earthquakes" are you implying that the earthquakes on the Calaveras fault are similar to the earthquakes on the other fault that we have been talking about?

MR. BARLOW: The two earthquakes that I am referring to are the Coyote Lake earthquake of 1979 on the Calaveras fault and the Imperial Valley earthquake of 1979 on the Imperial fault. As I examined Dr. Kovach yesterday, the Imperial fault is a branch of the San Andreas fault and the

Calaveras fault is also a branch of the San Andreas fault.

DR. FERGUSON: Is that the connection between the two that makes in your mind the calculation relevant, that they are both branches of the San Andreas?

MR. BARLOW: Yes, sir. They are both strike slip faults along the plate boundary of similar length and similar potential magnitude. There are other points of relevancy that could be brought out under cross examination regarding the soil characteristics and the distances from the fault plane to the data set that were recorded.

CHAIRMAN GROSSMAN: We'll take a ten-minute recess now.

(A brief recess.)

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CHAIRMAN GROSSMAN: On the record.

We left off the discussion with Mr. Edgar not having had a chance to have his say.

MR. EDGAR: I'd like to make one basic point first and that is that's there's been a little bit of confusion here when people talk about the Imperial Valley data set. When we're talking about the high vertical accleration, we're talking about a set. We're talking about a point. A data point at station six. Now, in order to do what Mr. Cady suggests, which is to go back through all the vertical data, we would have to do a regression analysis on that data. We would have to get computer access and we have no guarantee that we could do this in a timely manner.

The fact is that the same thing applies to the Staff. The U.S.G.S does not accept the anomalous data point of Imperial Valley at station six. It is addressed in our testimony at pages 22 through 23. We don't think that the effort will be of any value to the record, either. We're talking about re running a set of data that will give you the same result in the end because our experts and the Staff's experts, don't accept the anomalous data point.

We think that if it is an important point, than there is no reason why Mr. Barlow can't undertake his own calculations.

CHAIRMAN GROSSMAN: Mr. Edgar, you're not asking

us to decide the case from the bench here, are you?

MR. EDGAR: No, sir. I'm asking one more suggestion perhaps and that is there is no reason why the Board can't inquire further of these witnesses as to the signficance or meaning of rerunning the data sets and ask the same of the Staff and perhaps make the decision based on more specific facts.

CHAIRMAN GROSSMAN: But aren't you asking us to decide at this point that the Imperial Valley event and Coyote event have no relevance as far as the vertical acclerations and thereby decide the case here?

MR. EDGAR: No, sir. What we're asking you to consider is whether or not we have an obligation to rerun a data set when our experts have testified without any contradiction that the anomalous data point should not be considered and we're perfectly willing to withstand the test of cross examination on that issue.

CHAIRMAN GROSSMAN: Mr. Swanson, do you have something to add?

MR. SWANSON: I was just going to add that I also would ask that a decision on the relevance -- whether or not a bases has been established -- be made, at least, to whether or not G.S. should do this calculation until after we have had an opportunity to hear the response of our witness on this point, who will be the assistant director of the United

States Geological Survey and he will be thoroughly prepared to respond to questions about this anomalous point. I might add, though, that Coyote Lake event being brought up -- I'm not sure that a similar thorough response would be available. This is the first time that this has been brought up as an evidence of -- as a need, perhaps, for further analysis in response to interrogatories and in testimony. The only events that were mentioned is support of the accleration for the design value by the Intervenors with the San Fernando and Imperial Valley events.

And on those events, of course, we will be thoroughly prepared. But, my bottom line is that I would ask that as a Board's decision as to whether or not a bases has been established for inquiring further into an analysis, they make that decision after they have had an opportunity to hear the response of the assistant director of the United States Geological Survey.

CHAIRMAN GROSSMAN: But, both of you do agree that what we want is a prima facea showing, not a dispositive showing in which the Board can decide right now from the bench that we're going to conclusively determine that we're going to throw those events out as far as determining vertical acceleration.

MR. EDGAR: No, we're not asking you to pre-judge before all the evidence is in. What I'm suggesting is that

I think we've got the presumptions reversed. The evidence is not in yet. There has been no prima facea showing which would place an obligation on us to generate data that may well be meaningless.

CHAIRMAN GROSSMAN: Well, at least not sufficient in your view.

MR. EDGAR: That's correct.

CHAIRMAN GROSSMAN: Did you want to add something, Mr. Barlow?

MR. BARLOW: Yes, sir. I would like to just say one thing. We are not talking about the use of only one data point, as Mr. Edgar was saying. There are other data points. There are several data points of vertical acceleration within the data sets from those two earthquakes in which the vertical acceleration exceeds the horizontal accleration and therefore, we're not limiting this discussion to the one point which they claim in anomalous.

CHAIRMAN GROSSMAN: Well, Mr. Barlow, I hope that you can bring all of these matters out in cross examination. Basically, the Board has decided not to decide the question right now as to whether we ought to compell the computations, but to allow you more time to connect up that particular need to the testimony that's been given and so we'll allow you to proceed along those lines, now.

MR. BARLOW: Okay, thank you.

BY MR. BARLOW:

A Dr. Kovach, in your testimony on page 19, figure 6, you have calculated peak horizontal accelerations from two earthquake data sets, Imperial Valley and Coyote Lake in 1979. In the following figure 7 on the next page, you have calculated peak horizontal acclerations from several other earthquakes.

Could you explain to me why, in your testimony, you did not calculate similar graphs for peak vertical accelerations from these or any other earthquakes in regards to the seismic design critera for the GETR reactor?

A (Witness Kovach) I did a tentative study of -- very early in my work of the GETR site looking at the vertical accleration data from the Imperial Valley and I did a preliminary regression analysis excluding several of the anomalous points which was very clear when one -- from the geology and also a careful examination of the strong motion accelogram itself, that they were clearly anomalous and I was convinced at the time I did that analysis that two-thirds was an appropriate value -- two-thirds of the horizontal.

Q Yesterday, I asked you if you were familiar with vertical acceleration data from the Coyote Lake earthquake and I believe your response was no. Is that correct?

A I did not look at any vertical component data for the Coyote Lake earthquake.

Q So, you don't know if the vertical data there exceeded the horizontal?

A I have no first hand knowledge and I don't have the data in front of me.

Q I also asked you if you knew whether or not in the Imperial Valley data set, whether there was more than one vertical acceleration data point in which the vertical acceleration exceeded the horizontal accleration and I believe your response was that you were not familiar with that data set to answer that question. Is that correct?

A Yes, I believe that's correct.

Q Have you not examined the vertical data set from the Imperial Valley earthquake other than the largest peak accleration on the vertical scale?

A I did mention that I have looked at some of the vertical data and I did a preliminary analysis which I just mentioned.

Q Can you recall from that preliminary analysis whether or not there are more than one data points in which the vertical accleration at Imperial Valley exceeded the horizontal acceleration?

A I can't remember the detailed specifics. I believe there were several where the verticals were comparable to the horizontals, but I could not give you a number from memory.

Q I asked you if you knew of any other earthquakes

besides these two earthquakes in which the verticals exceeded the horizontals and I believe your answer was that you only knew of one, the Gazli earthquake of 1976. Can you recall any other earthquakes besides these three in which verticals exceed the horizontals?

A No, not from memory, I can't.

Q Do you have access to data sets from earthquakes which might provide an opinion for you or not?

A Possibly.

Q Would it be possible for you to do a review of those data sets to see if you can find another earthquake in which verticals exceed horizontals?

A Would it be possible? Is that what you asked me?

Q Yes, sir.

A Anything is possible.

Q Would it be possible with your -- When are you leaving for Europe?

A I don't have the time to do any detail, you know, technical analysis in the time that's left for me here, so I can't respond to that in a very positive way.

Q When are you leaving for Europe, sir? Are you going to Europe?

A Yes, shortly.

Q When is that?

A Well, it depends a little bit on the way these

hearings are going. I'm trying to time my departure to be here to be constructive. So, roughly the middle of next week, I would like to go.

Q In the data set which you used in your figure 7 on page 20 of your testimony, you have several earthquakes that you have examined. Do you agree that the bulk of the data in that figure is at a distance of 30 kilometers or more from the recording site?

A Yes.

Q What -- How would you characterize your level of confidence in your methodology of extrapolating to closer distances using data sets that are at that distance?

A Well, if I were to exclude the -- My level of confidence was somewhat higher than it would be if I would not have had these two earthquake, mainly, the Gazli and the Tabas, which are the only two earthquakes which we have strong ground motion in the near field in the magnitude range greater than 7. If we were to exclude those two data points and attempt to do a prediction, I would have much less confidence in the results.

Q On that page, you say that in the last full sentence on that page 20 of your testimony -- you say, thus mean horizontal peak acclerations ranging from .57G to .74G are appropriate for a magnitude 7 to 7.5 earthquake on the nearby Calaveras Fault.

Now, here you're using mean values. If you used peak values rather than mean values, would you come up with a number larger than .74G?

A I did use peak value. I used the mean of the peak value. I don't quite understand --

Q The mean of the peak values. If you used the maximum peak value, would you come up with a larger number than .74G?

A Yes, that's obvious.

Q Would that number be based on the Gazli earthquake and the Tabas earthquake which provide the data set at a distance of three kilometers?

A Would you rephrase the question?

Q In your figure 7, you have data points from the Gazli earthquake and the Tabas earthquake at a distance of three kilometers. If you were to calculate the maximum peak horizontal acceleration rather than the mean value, would the maximum value be based on those data points?

A The values that are shown here are the peak values.

Q Yes, sir, so would the maximum value value be --

A That would be the maximum value.

Q Could you tell us what those values are from those two earthquake?

A Would you give me a minute to consult my notebook?

Q Yes.

A If you had dividers you could scale it off, but I'll look it up, if I can find it.

Q While you're looking it up, could you also find the peak vertical accelerations from those two earthquakes?

(Pause)

MR. EDGAR: Would this be a good time to break for lunch?

CHAIRMAN GROSSMAN: After the answer would be a good time, I would think

MR. EDGAR: Yes.

WITNESS KOVACH: For the Gazli earthquake, the peak values were .75 and .67 and the vertical was 1.3. For the Tabas earthquake the vertical malfunctioned and I'm not sure that I could put my finger on here, but it looks like it's .8G, approximately.

BY MR. BARLOW:

Q The horizontal peak is .8G?

A Yes.

Q So, in the Gazli earthquake --

CHAIRMAN GROSSMAN: I'm sorry and what was the vertical?

WITNESS KOVACH: The vertical didn't operate.

CHAIRMAN GROSSMAN: Oh.

WITNESS KOVACH: During the Tabas.

BY MR. BARLOW:

Q So, in the Gazli earthquake you had a .75G horizontal peak and a 1.3G vertical peak?

A That's correct.

Q Would you agree that the vertical exceeded the horizontal at Gazli?

A That's what was there. Yes, I agree with that.

Q Was that vertical data point at a distance of three kilometers?

A Excuse me?

Q Was that vertical data point at a distance of three kilometers from the recording site?

A I think the Gazli is a little bit larger. I think it's roughly three and a half to four kilometers.

Q Would you agree that that is comparable to the distance from the Calaveras Fault to the GETR site?

A Yes.

CHAIRMAN GROSSMAN: We're going to break for lunch now. So, why don't we come back at 1:30 p.m.

Off the record.

(Whereupon, at 12:00 noon, the hearing was recessed until 1:30 p.m., this same day, Friday, May 29, 1981.)

AFTERNOON SESSION

1:30 p.m.

CHAIRMAN GROSSMAN: On the record.

The hearing is now in session.

Mr. Barlow, would you proceed?

CROSS-EXAMINATION (Resumed)

BY MR. BARLOW:

Q Dr. Kovach, are you familiar with the works of the USGS scientists Drs. Boore, Joyner and Porcella regarding ground accelerations?

A (Witness Kovach) Yes, I presume you're referring to the most recent report of theirs?

Q Well, actually, I believe there is a series of three reports on USGS professional circular number 672 and then USGS professional paper number 795 and then the March, 1981 which they presented as I said in March.

Are you familiar with all three of those papers?

A Yes.

Q Have you analyzed the ground accelerations versus distance and magnitude relationships which they have presented in those papers?

A I've checked my analyses against their recent one, yes.

Q How does your analysis and method compare to

the analysis and method that they used?

Or, excuse me, how do your results compare with their results?

A Their results in the near field are roughly 20% higher than mine.

Q 20% higher, is that for the mean or for all levels?

A I would presume it's for the mean.

Q Can you explain why their values exceed your values by 20%?

A Well, their analysis is a matter of debate in the seismological community but basically they made the assumption that peak ground acceleration does not saturate with an increase in earthquake magnitude and I don't believe that that's the correct hypothesis.

Q Can you explain why you support the hypothesis that peak ground accelerations saturate at certain magnitudes?

A Well, if one looks at the fact that the local magnitude scale which is based on high frequency components tends to saturate for the higher magnitudes and number 2, the fact that the data from the near field for Coyote Lake and the require a curvature correction which comes out to be magnitude dependent.

Q Could you explain how the hypothesis works that

hypothesizes that magnitude saturates at certain magnitudes, by that I mean, can you explain at what magnitude level you hypothesize that accelerations saturate?

A I would only be guessing, since we don't have any acceleration data in the near field for earthquakes greater than 7.7 which is the Tabas, but I would imagine it's in that range between 7.5 and 8.0.

Q I'm sorry, between 7.5 and 8.0?

A 8.0, yes.

Q So, below magnitude 7.5 you would expect a correlation between magnitude and acceleration?

A Well, there is a slight dependence in my analysis on earthquake magnitude, yes.

Q And yet in the near field of less than 10 kilometers distance from the recording site, what data sets exist other than the Imperial Valley data set that provides any data for that range between magnitude 6.5 and magnitude 7.5?

A Well, all of the data that are available are on my figure.

Q That's all the data available on a worldwide basis?

A That's all that I am aware of. On page 20.

Q Are those figures 6 & 7 in your testimony?

A Figure 7 on page 20.

Q Figure 7 on page 20. Is all of the data that is available at less than 10 kilometers in the range magnitude 6.5 to 7.5?

A That's all the data I'm aware of.

Q Therefore, is it necessary for the purposes of predicting ground accelerations for seismic design criteria at a site like the GETR to extrapolate from the available data?

Do you use extrapolations in your methods?

A I use the functional form which I mentioned earlier and tested it against this data which is not really an extrapolation. It's just putting in a higher magnitude into the functional dependents that I use when I'm testing it here against the observed data.

Q Can you tell me the magnitude of the Imperial Valley earthquake please?

A I believe it's 6.6.

Q And the peak horizontal instrumental value is .8G, is that correct?

A I believe that's correct.

Q If you had -- excuse me. Could you tell me the distance that that .8G was recorded from the fault?

A Would you repeat the question please?

Q In the Imperial Valley earthquake, the peak horizontal instrumental data point was .8G. What was the

distance from the fault that that was recorded at?

A I believe 3 kilometers.

Q 3 kilometers.

If you had an earthquake near the GETR site, on the Verona fault of magnitude 6.5 that was directly beneath the reactor or on the Verona fault zone, whether or not it was directly beneath the reactor itself, can you extrapolate using the Imperial Valley data and come up with a predicted value for the peak instrumental horizontal value for acceleration?

A I believe I did that in my testimony. Yes, I did that.

Q What value did you extrapolate?

(Pause)

A Well, I believe that an appropriate magnitude for an event postulated along the Verona fault was in the range of 5.5 to 6.1 and my testimony on page 22, I estimated .4G as an appropriate value.

Q .4G. Was that for zero distance?

A Between 0 and one kilometer.

Q Between 0 and one kilometer.

And yet you used a magnitude range of 5.5 to 6.0.

If you used a magnitude 6.5 on the Verona fault, could you estimate the peak instrumental horizontal accelera-

tion for that?

A Yes, I could.

Q Have you done that?

A What?

Q Have you done that calculation?

A No, I have not done that calculation.

Q Were you ever asked to do that calculation?

A I was not asked to do that calculation.

Q Mr. Gilliland, could you explain to me why General Electric did not ask Dr. Kovach to calculate the ground accelerations for a magnitude 6.5 earthquake on the Verona fault?

A (Witness Gilliland) We asked Dr. Kovach to help us with respect to the seismological matters at the site and in his evaluation, he concluded that the values of 5.5 to 6.1 were appropriate values to use.

We rely on his expert capability and therefore left it at that.

Q Mr. Gilliland, if the NRC staff and the USGS Geological Survey decided that an appropriate magnitude for the Verona fault is a magnitude 6.5, would you ask Dr. Kovach to calculate the peak horizontal instrumental accelerations for a magnitude 6.5 on the Verona fault?

A Well, I think it depends a little bit on the nature of how the request read we had received.

We have talked about that and my understanding is that the value wouldn't be terribly different so we haven't spent a lot of time with respect to that point so if they were to make a point of that, we would consider it given what they said.

Q Dr. Kovach, if you were to calculate the peak instrumental horizontal acceleration on the Verona fault for a magnitude 6.5 earthquake at 0 distance, would the value be higher or would you expect that value to be higher than the .4G that you estimated for your former calculations?

A I wouldn't want to speculate on that without doing the calculation.

Q In doing that calculation, would you use the data set from the Imperial Valley earthquake?

A I would perhaps, you know, consider that to be part of it, yes.

I would certainly look at the data.

Q And, you noted that in the Imperial Valley quake at a distance of 3 kilometers, the peak instrumental horizontal value was .8G which is double the value that you predicted using your calculations, is that correct?

A If you look at the graph on figure 6, you'll see that there are roughly -- several data points at three kilometers and you've taken the maximum one of .8 and

asking me if I would use that as the sole basis for estimating the value and the answer to that is no, I would not.

I would attempt to look at all of the data and come up with the best assessment at that distance for that magnitude range that I could.

Q Am I correct in understanding you to say that you would choose to use the mean value rather than the peak value?

A I would use the mean of the peak values observed.

Q Dr. Kovach, if you were to analyze an earthquake of the magnitude of 6.5 on the Verona fault at zero distance from the GETR, to calculate the peak vertical instrumental accelerations, do you have any idea what value you would come up with?

A (Witness Kovach) No.

Q Did you calculate the peak vertical accelerations based on a data set or did you just use the 2/3rds of horizontal formula to arrive at your estimate of vertical accelerations?

A I did not do that calculation.

Q You did not calculate the peak vertical accelerations at the site for a magnitude 6.5 on the Verona Fault?

A That's correct.

Q Mr. Gilliland, why did General Electric not ask

Dr. Kovach to calculate the peak vertical acceleration for a magnitude 6.0 to 6.5 earthquake on the Verona fault zone?

A (Witness Gilliland) Yes, let's see. Did I understand correctly -- is that the question you asked before? It seems the same question.

Q I was talking about --

A So what's the difference?

Q I was talking about horizontal accelerations in previously asked questions.

A Horizontal? Well, again, we were relying on Dr. Kovach's evaluation of the data and what he has stated with respect to the vertical, we rely upon.

Q Why did you not ask him to calculate the vertical values for the magnitude range which he used in the 5.5 to the 6.0 range?

A Well, let's see.

My impression from what he has said here today and what he has told us, in previous conversations is that his assessment produced values of vertical accelerations which we have subsequently used in our evaluations of structure and based on that, we didn't see any point with proceeding with the point you were raising.

Q Were you aware that Dr. Kovach's assessment's of vertical accelerations were based on the formula that

vertical is 2/3rd's of horizontal? Rather than an analysis of the data set?

A My impression is that he evaluated the information that he had, that he did not start arbitrarily with 2/3rds

Q I'm sorry, I didn't understand that. Could you repeat that answer?

A I said, my impression is that he arrived at that conclusion having looked at the data set. He did not start with the notion that the vertical would be 2/3rd's of the horizontal.

He reached that conclusion having looked at the data.

Q Did you realize that he was excluding certain data in his calculations?

A Certain -- what are you referring to?

Q The Imperial Valley vertical accelerations, the Gazli earthquake acceleration, vertical accelerations and the Coyote Lake vertical accelerations?

A I'm sorry, I can't hear you. You're not speaking very clearly.

Q Oh, I'm sorry. Did you realize that Dr. Kovach was excluding from his analysis the vertical data sets from the three earthquakes, the Coyote Lake quake, the Imperial Valley quake and the Gazli quake?

A I'm aware that the -- there is at least one data

point in the Imperial Valley which was anomalusly high for reasons which have been subsequently described by a number of people including Dr. Kovach here today.

And, for that reason, should not have been included in the data set.

It is my understanding that similarly for Gazli the high vertical is due to conditions similar to the one that gave rise to the value at Imperial Valley and therefore appropriately is not included.

What we're trying to do, I believe is to establish proper design values for the GETR site and simply to say that the values that one gets from instrument accelerations at a place like Imperial Valley which is in a different geologic setting and for a different character of fault is not a proper thing to do. It is restricted, and so you must rely on the experience and judgement of persons who have evaluated this data so that it is possible to arrive at the proper values for GETR.

I think through -- to simply jump into it and say the Imperial Valley has a value and therefore the GETR should have the same one is not proper.

It is not consistent.

Q Mr. Gilliland, did General Electric employ as consultants geologists or seismologist to estimate seismic design criteria prior to the construction of

the GETR?

A Let's see.

I would have to do some looking. My recollection is that in the course of the design effort, the GETR and the records -- this is an old historic situation, that in the course of the design that there were persons employed to evaluate seismological matters with respect to its design.

Q Were those persons geologists or seismologists?

A Well, I'm having trouble recalling the names so I would have more difficulty recalling their specialty, so I guess my best answer to you at this point is I don't know.

Q I recall during the discovery process a statement which you may be familiar with that in regards to the excavation photographs of the hypothetical fault and the foundation of the GETR, that G.E. had employed certain individuals of the Parsons company who looked at the possible faults and foundations but that none of those persons were geologists or seismologists, is that correct?

A I think that the response, well, the information with regard to that particular point is that there were personnel assigned to the construction of the GETR who observed the excavations but while they were not trained geologists but at the same time had experience in excavation

for buildings felt in subsequent questionning that they would observe any major structural phenomena that might have been there.

I think that's verified by the examination that has been subsequently made of construction photographs that were taken and have been evaluated.

Q Dr. Kovach, if an earthquake of magnitude 6.5 occurred on the Verona fault zone, directly beneath the Reactor, would you expect the fact that it's at zero distance to cause higher ground accelerations than we observed in the data sets at distances larger than zero?

A (Witness Kovach) Excuse me, larger than what?

Q Larger than zero?

A I really wouldn't want to speculate because when you're pushing in the curve into zero distance, it becomes almost horizontal and I, you know, couldn't answer that off the top of my head.

Q Is that based on the saturation theory?

A No, that's just based on the functional formula I was using.

Q In your?

A In my analysis.

Q In your analysis.

Have you looked at the March 1981 report by Drs. Boore, Joyner and Porcella from the U.S.G.S in regards

to a magnitude of 6.5 earthquake at zero distance for the peak horizontal instrumental value?

A Well, I'm aware that they have done that calculation, but I can't recall what the result is.

Q Do you have that report in front of you?

A I may have it in my brief case but I think it's in front of me, just give me a minute here.

(Pause)

I presume you're referring to Open File Report 81-365?

Q That's correct.

A By Joyner, Boore and Porcella?

Q Yes.

Did you -- could you look at their peak instrumental horizontal chart?

(Pause)

Could you look at a magnitude earthquake 6.5 at zero distance and tell us what the peak horizontal value is?

(Pause)

A No, no, I can't do that because you don't go to zero on log paper but you want me to read the value off the end of the graph?

Q I'm sorry, I don't understand. Could you repeat your answer?

A Well, what's plotted here is the logarithm of the distance versus --

Q Do they have a chart that compares distance to horizontal accelerations?

A Yes, that's what I'm pointing to here.

Q It does not have a zero distance on here?

A Well, it doesn't, you can't go to zero on log paper.

Q Can you go to less than 1 kilometer?

A Yes, we can go to less than 1 kilometer.

Q Can you tell us what the value is?

A Well, I would estimate, it looks like half of G. 0.5G.

Q While you're looking at that could you look at a magnitude 7.5 earthquake at a distance of 3.0 kilometers? And, tell us what the value is?

A Well, it's hard to read on this, but it's approximately -- they're estimating 1G will be exceeded 50% of the time.

Q One G would be exceeded 50% of the time?

A That's correct.

Q And on the 84th percentile chart, is there one of those?

A I can't read it on the graph. Oh, excuse me, I'm still looking at the wrong --

(Pause)

I guess the 84% curve here, exceedent's probability?

Q Yes, sir.

A I don't know, it's something in excess of 1G, I can't read it on the graph here.

Q In excess of 1G?

(Pause)

Dr. Kovach, are you familiar with the data set from the Milindi Ranch quake in Bear Valley in 1972?

A (Witness Kovach) No, I'm not.

CHAIRMAN GROSSMAN: By the way, before we go further, were those values horizontal or vertical accelerations that you just gave?

WITNESS KOVACH: They were just horizontal accelerations.

CHAIRMAN GROSSMAN: Thank you.

BY MR. BARLOW:

Q Dr. Kovach, you said that you had examined Dr. Bolt's paper on the Livermore Earthquake of 1980, is that correct?

A (Witness Kovach) I've read the paper, yes.

Q Do you agree that the earthquake of January 24, 1980, was a magnitude 5.5?

A Well, I have no reason to disagree with Dr. Bolt's

determination of magnitude.

Q And yesterday when we were looking at this in regards to your testimony on page 14, where you say that there is roughly 31.5 times as much energy released for each full-step increase in earthquake magnitude and I ask you to compare the energy released during a 5.5 earthquake with the energy released during a magnitude 7.5 earthquake and you said that the larger earthquake would release approximately 900 to 1000 times as much energy as a smaller earthquake?

A That's correct.

Q Now, in examining the earthquake of January 24, 1980, the Livermore earthquake, I asked you whether or not you were familiar with the concept that that earthquake exhibited seismic focusing and you said yes, is that correct?

A (Witness Kovach) Well, I read the statement that Mr. Bolt said there seemed to be some evidence for focusing on that earthquake.

Q Do you think that the reason -- do you think that Dr. Bolt's statement is based on an analysis of the distance from the epicenter to a point approximately 15 kilometers south at the Livermore lab site where there was extensive structural damage?

A I don't know the answer to that question.

Q Are you aware that there was extensive structural damage at the Livermore nuclear laboratory during that earthquake?

A I'm not familiar enough with any of the damage to make a knowledgeable statement about that.

Q In a hypothetical situation, if you had a magnitude 5.5 earthquake and 3 kilometers from the epicenter you had trailer parks and dikes on saturated mud, would you expect damage to the --

MR. EDGAR: Object to the hypothetical on the grounds that there's no foundation in the record.

CHAIRMAN GROSSMAN: I assume you're going to connect that up later, is that correct, Mr. Barlow?

MR. BARLOW: Yes, sir.

CHAIRMAN GROSSMAN: You may proceed.

BY MR. BARLOW:

Q Would you expect strong ground shaking at a distance of three kilometers from the epicenter of the magnitude 5.5 earthquake that might damage a saturated dike or a trailer -- a house trailer?

A I'm not a structural engineer, perhaps Dr. Kost can.

Q Dr. Kost, could you answer that question?

A I didn't understand the last three or four words that you used, could you please repeat them?

Q Yes, sir.

If there were an earthquake whose epicenter were three kilometers from a trailer court with house trailers and also three kilometers from a water saturated dike, in the delta of the San Joaquin river valley --

A With a water saturated dike?

Q Yes. Would you expect some sort of damage to either the trailer court or the dike during that earthquake? At that distance and at that magnitude, 5.5?

A I think that it's quite possible that there would be damage to trailer courts. It's well known that such

structures have not behaved well in earthquakes. They tend to be supported on concrete blocks and are in fact just sitting there perched on very flimsy supported structures and I think as a result of this experience it has been learned that that's an undesirable form of anchorage and as a result, we're beginning to design anchorages for trailer courts to mitigate and eliminate such damage.

Q Dr. Kost, are you aware or or have you analyzed any of the structural damages during the Livermore earthquake of 1980?

A No. I have not.

Q Dr. Kovach, focusing on the subject of seismic focusing, have you analyzed a report from the earthquake engineering research institute dated December, 1978, entitled engineering features of the Santa Barbara earthquake of August 13, 1978?

A (Witness Kovach) I've read the report, I've not analyzed the report.

Q Dr. Kost, have you read that report?

A (Witness Kost) Yes, I read it.

Q Dr. Kovach, having read that report, and looking at the characteristics of the Santa Barbara earthquake, --

A (Witness Kovach) Excuse me, which earthquake?

Q The Santa Barbara earthquake of August 13, 1978 -- would you expect to find higher ground accelerations in the town of Goleta or Isla Vista at a greater distance from the epicenter than you would expect to find in the city of Santa Barbara at a closer distance to the epicenter?

A That's quite possible.

Q If there were higher ground accelerations, on the University of California campus at a greater distance from the epicenter than downtown Santa Barbara, would you attribute those higher accelerations to the phenomena of seismic focusing?

A That's one possible explanation.

Q And have you looked at the after-shock and main shock epicenter locations of that quake?

A No, I have not.

Q Would you be of the opinion that the seismic rupture propagated in the direction of Isla Vista rather than the direction of Santa Barbara?

A Well, from what I read in the report, it's the analysis, the preliminary analysis suggested that there was a rupture that went up/dip up the plane so I accept that as a base value.

Q Dr. Kovach, if you had a hypothetical earthquake on the Calaveras fault zone, which was a few kilometers to the North of the GETR site near the town of Dublin, the

epicenter, it's near Dublin, and the rupture was propagating to the South, do you agree that it would be possible to have seismic focusing in the direction of the GETR site?

A (Witness Kovach) That's possible, yes.

Q Do you agree that the seismic focusing in this hypothetical situation could cause higher ground accelerations in the direction of rupture propagation?

A That's a point of possibility. I mean, seismic focusing is very clear to be much more pronounced in ground displacement and ground velocity. However, a more correct statement would be that you might expect it to have effect on the peak accelerations, but in some cases, it's clear that that does not happen.

Certainly if one looks at some of the data from the Imperial Valley, some of the Mexican stations that were behind the rupture front, they had comparable amplitude to -- in the accelerations to those stations that were in front of the rupture, so there it's not at all definitely conclusive that focusing had that much of a pronounced effect on the accelerations.

The reasons for this are, that there can be local variations in the direction of rupture propagation, there can be scattering and lateral refractions, and all of these tend to reduce the effect of the high frequency components of acceleration.

So, I'm not disputing the fact with you that yes, focusing can have an effect on ground acceleration but I'm saying that sometimes it may not.

Q Would the degree of seismic focusing depend on local seismic fault geometry as one of the parameters?

A Well, the focusing depends on several parameters. It depends on the velocity of the propagation of the rupture, it depends on the shear waive velocity of the material and it depends on the azimuths in which the way your stations are relative to which way the ruptures went.

Q Is fault geometry one of the parameters?

A Would you define what you mean to me as fault geometry?

Q Would you expect the occurrence and characteristics of seismic focusing to be effected by the parameter of fault geometry?

A I'm unclear as to what you mean by fault geometry. Do you mean which way the fault is aligned?

Q No, I mean if there is a complex geological situation such as we have around the GETR site and to be more specific, if you had the Calaveras fault zone paralleled by the Pleasanton fault zone and the Verona fault zone and you had an earthquake whose epicenter is near Dublin, with the rupture propagating to the South, how would that rupture propagation interact with the fault

geometry of the Calaveras fault, the Pleasanton fault and the Verona fault?

A Could I ask Dr. Jahns to answer that question?

A (Witness Jahns) I must confess to begin with, I share Dr. Kovach's difficulty in understanding precisely what you mean by fault geometry. Do you mean the attitude of the fault surface or it's straightness or just what?

Q I mean the strike of the fault geometry as mapped on the surface.

A Whether it's straight or not? Simple or branched? Joining or rejoining?

Q The geometry as mapped on the surface.

A Then your question allows for a very wide latitude of answers because so much depends on whether it is a simple break, whether it's straight or curved -- or whether it's a series of an anastomosing break, something of that sort.

Insofar as the Verona structure is concerned, we know it can't be simple because there's more than one break.

And, they're sub-parallel to be sure but they're not planar. They show curvature of various kinds. The Calaveras doubtless is even more complicated, comprising of very large numbers of breaks that branch and join.

Now, if you could indicate what you would like me

to do with this I'll see what I can do.

Q Considering the complexity of the fault geometry with these faults, if you had a magnitude 7.5 earthquake on the Calaveras fault zone, whose epicenter was near the town of Dublin, in a similar location of the earthquake of 1861, according to the records that we have, and the rupture from this earthquake propagated to the south along the Calaveras fault zone, would it be possible for this rupture to break along different strands of the Calaveras fault?

A That's possible, yes.

Q Would it be possible for the earthquake to rupture a new strand parallel or adjacent to the Calaveras fault zone?

A It's also possible.

Q In otherwords, it could break new ground?

A The fact that there is more than one break on an existing fault zone indicates that of course, that's happened at least once during geologic time.

So it is possible, yes.

Q Could that rupture happen to the East of the Calaveras fault zone?

Or, whatever hypothetical distance?

A I would say yes.

Q Therefore, could this hypothetical rupture occur

close to the GETR site than the mapped trace of the Calaveras fault zone?

A Yes, that's within the realm of possibilities.

Q Would you expect a new rupture such as this to occur along a zone of weakness of an older fault that might be mapped in the area that is rupturing?

A Such as the Verona?

Q No, sir, not the Verona.

A If there were an existing zone of weakness, the geometry of which was appropriate for ready transmission of the rupture from the Calaveras to a new trend, then this certainly would be a possibility.

Q Looking at Intervenor's Exhibit No. 1, could you -- do you have that available?

(Pause)

A I believe I have it here somewhere.

Q I'm sorry, looking at Intervenor's Exhibit No. 2.

(Pause)

No, the next one which is the ESA Map. That's number one you have there. Well, looking at number one first, on Darrell Herd's map in Intervenor's Exhibit No. 1, would it be possible for an earthquake of magnitude 7.5 on the Calaveras fault to rupture along the fault which is mapped by Darrell Herd between the Calaveras and the GETR?

A Well, first, so that you don't place too much weight in the response I might make, I don't know personally of the nature or even the existence of this fault.

Nonetheless, I would say that it is possible, albeing extremely improbable for rupture to occur along such a trend, whether or not the fault is there now.

Q Okay, thank you.

Dr. Jahns, are you familiar with the Pleasanton fault zone?

A I know of it and I can't claim familiarity with it.

Q Have you examined any maps which show--?

A I have looked at maps which show it depicted, yes.

Q In your opinion, would it be possible for a large earthquake on the Calaveras fault zone to rupture along the Pleasanton fault zone?

A I would not expect that.

One can't rule it out completely as a possibility. Instances are known throughout recorded seismologic history world-wide. A major rupture occurring on one fault and then either at essentially the same time or shortly later, what amounts to sympathetic rupture occurring on another fault nearby but not necessarily connected. That happened in 1952 in connection with the Arvin-Tehachapi and the Bakersfield earthquakes. That might be roughly

comparable to the hypothetical case you pose, in that there are two faults sub-parallel, some considerable distance apart but in the same region and not apparently connected with seismic events close enough in time so that one could make a reasonable supposition that somehow the later one represented the change in stress/strain relationships associated with the earlier one.

Q Therefore, do you agree that a subsidiary or a lower order fault can experience sympathetic surface faulting during the earthquake on a nearby major fault?

A As a possibility, yes.

I think I should add that almost all of these things that we've been talking about since I think your first question in this series deal with the -- just the extreme limits of the probability scale.

But, they are possible.

Q Dr. Jahns, could you describe for us the characteristics of the 1952 White Wolf fault earthquake near Bakersfield including a description of the sympathetic faulting that occurred and the major aftershock that occurred near Bakersfield?

A Well, the White Wolf fault is a break that has general East/West trend and represents North/South crustal shortening. So, it is a thrust, and the average dip on the order of 45°. It's one of the faults that some

investigators group as a transverse range structure. Others consider it as a sort of outlying feature North of the transverse range's proper.

But it certainly has transverse range's affinities in the terms of it's attitude and general movement since.

In 1952, there was a major shock, magnitude 7.7 on that fault with both dip/slip and horizontal slip components of movement so it was a left oblique thrust slip and each of thise slips, was -- each of these components was on the order of two feet, three feet about maximum for any of the components.

A really big earthquake with surface rupture of course, and with a highly asymetric epicenter relative to the total length of the fault -- the epicenter lay at almost the extreme West end of the fault and I should add here to place this in proper perspective the White Wolf fault is essentially parallel to the Garlock fault and specifically it's trend although East/West in general is East/Northeast and it extends in that structurally interesting block of ground that lies north and east of the intersection of the San Andreas fault and the Garlock fault in the Big Ben region.

Geographically it's located South, Southeast and East of Bakersfield at the South end of the San Joaquin Valley.

It's one of those faults so far as we can determine with the data available that's capable of one of the larger kinds of earthquakes that occur in California off the San Andreas fault and probably with a very long recurrence, average recurrence interval, especially relative to those along the San Andreas itself.

Now, the fault along which the subsequent several months later Bakersfield earthquake occurred, is sub-parallel to the White Wolf, north of it, in the general vicinity of Bakersfield and not exposed at the surface.

Presumably it is because the earthquake that occurred after the ARvin-Tehachapi was great enough to have caused surface rupture but I'm speaking now of a geologic sense.

If you look at the fault prior to that earthquake, does it show scarps and features of that sort and the answer generally is no and that's largely the expression of the youthfulness of the materials in that part of the San Joaquin Valley floor.

That earthquake occurred -- I don't recall that exact length of intervening time, but it was many weeks later, long enough so that it became a fruitful area of discussion among seismologists as to whether it was an aftershock or a separate sympathetic break or something

not associated at all. Does that respond to your request for a description?

Q Yes, definitely, I have a couple of follow-up questions on that earthquake. Could you tell us the magnitude of the aftershock or the secondary shock?

A I don't recall it's exact magnitude but it was as usual for after shocks and at least an order less than the 7.7 Arvin-Tehachapi.

Q And it was a different fault than the main shock?

A Yes, that's right.

Q Do you know the distance between the secondary fault and the main fault?

A Well, that's your term secondary fault.

Maybe it was, I don't think anyone knows for sure.

There was a major difference in energy release, of course and it was at least a factor of 50 and perhaps a good deal more.

Q Do you know the approximate distance between the epicenter of the main shock and the epicenter of the secondary shock or aftershock?

A Well, as I recall the epicenter of the subsequent shock lay both North and East of that for the principle shock which was extreme West on the White Wolf

fault.

Q Do you know the approximate distance between those two epicenters?

A No, that I don't.

Q Do you know an approximate length of the White Wolf fault?

A No, not without being able to scale it off on the map.

It extends from the vicinity of Wheeler Ridge and Grapevine where it probably is buried beneath the pleato and some other shallow thrust features there in that very complex area to points corresponding to the southern projection of the Kern Canyon fault at the south end of the Sierra Nevada.

It's a distance of a good many 10's of miles.

Q Do you have any idea of the length of the fault that the aftershock occurred on?

A No, I don't think anyone does really. There is enough information on aftershocks of that earthquake to provide some notion of a rupture area, but how far beyond that, East and West, the fault extends, no one really knows because Westward it's part of the Valley that has not been explored for oil so there's very little sub-surface control at the right depths.

Q Did the aftershock cause surface rupture?

A I beg your pardon?

Q Did the--was the aftershock accompanied by surface rupture?

A Not to my knowledge.

Q Did the aftershock cause more damage than the main shock?

A As I recall in dollar terms, yes, because it affected a great many more people. The principle factor was demographic.

Q Do you agree that in general large earthquakes along the San Andreas fault boundary on strike/slip faults in California can be accompanied by significant aftershocks that can cause damage?

A I'm sorry, I didn't catch all the words in the last part of your question. Would you mind repeating that?

Q Certainly. Do you agree that large earthquakes on strike/slip faults in the San Andreas fault boundary, plate boundary, excuse me, be accompanied by aftershocks that can cause significant damage?

A Yes.

Q Can such aftershocks be located at a distance of several kilometers from the main fault and the first epicenter?

A Well, without attempting to be picky or to split hairs, if you're willing to include in this category

sympathetic failure on adjacent faults, things of that sort, yes, that's possible.

I would prefer to think of an aftershock as something representing continuance of the same rupture that caused the original shock.

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Q Would you characterize the aftershocks as being adjustments of stress in the crust?

A That's the view of most, I believe.

Q To be site specific at the GETR site, is it possible that a magnitude 7 to 7.5 earthquake on the Calaveras fault would be accompanied by a significant aftershock?

A Where?

Q Within a radius of several kilometers from the fault.

A On the Calaveras fault?

Q Either on the Calaveras fault or adjacent to it or on nearby faults or zones of weakness.

A Okay. Just so I understand, our nomenclature remains slightly different.

Q If you want to correct my nomenclature --

A No, no. No correction is necessary, just so we are clear. Some sympathetic shock, I take it.

Q Yes, sir.

A Yes. I think this is possible and probably with the same order of probability as a rupture in hitherto unbroken ground. I am very reluctant on bases other than those we have heard here in this room to relate some sort of failure on the Verona fault directly to failure on the Calaveras because their respective geometries and movement senses are so fundamentally different.

Q Are you aware --

A It's still possible.

Q Okay. Are you aware that during the January, 1980, Livermore earthquake on the Greenville fault that there was sympathetic surface rupture on the Las Positas fault?

A That's what I've been told. And I have been told by others whose opinions I also respect that it is very doubtful that that is primary rupture. So evidently it is not in scientific senses an open and shut case. But it is something that is certainly another possibility.

Q If it were not primary rupture would it be secondary or sympathetic surface faulting? Is that how you would characterize it?

A I should say if it were not primary rupture the most likely thing would be a surface expression of fling or heave of soft unconsolidated materials. These things are very widespread and, incidentally, are the -- that problem is the principal reason for doubt as to how much surface displacement there was on the old events on the Hayward and Calaveras fault. It is very hard to determine from the descriptions of laymen however carefully made they might be whether one is dealing with primary surface faulting or some form of heave or fling of relatively soft material, which is a quite different thing.

Q Considering the nature of the soil characteristics

at the GETR site, could you have this phenomena of heave or fling on the GETR site during a major earthquake on the Calaveras fault?

A I would not expect much of that at the site, no.

Q What is that based on?

A The cover of really soft, unconsolidated material is very thin. That sort of thing happens most typically in domains where there is a considerable thickness, many tens or a few hundreds of feet of very soft material and it is in those circumtances you typically get the jello in the bowl phenomenon where you rattle a bowland the jello really moves.

Q Is that jello bowl phenomenon generally associated with valleys?

A Very commonly because they are the logical places for the accumulation of soft, unconsolidated, very weak materials.

Q Considering the GETR site and the proximity of the Calaveras fault, do you believe that it is possible during the past few million years of activity on the Calaveras fault that at some time in geologic history the Calaveras fault was rupturing further east than its present location?

A At a given latitude, you mean?

Q Yes. At any latitude.

A I think that's possible, if one couples that with a depth. The fault may not be vertical. If it were dipping

say steeply east then there could well be rupture east of its present trace a little bit. This kind of thing is well known where there are clusters of well-controlled epicenters along the San Andreas and the Sargent and Hayward faults.

Q Is that a possible explanation for the observations by the USGS that epicenters along the Calaveras fault are typically east of the fault?

A That's one possible explanation. Another one equally -- maybe even more generally applicable -- is difference of materials on either side of the fault so that the paths of -- the travel paths can be different and be characterized by different velocities. So that the epicentral locations could be modified on the scales that you are talking about.

Q Would that be related to the fact that between the Calaveras fault and the Hayward fault there is the Sinole Ridge which is a crustal block that is tilted up between the two fault zones? Is that what you meant by the difference in characteristics of the soils on the two sides of the Calaveras fault?

A Yes. Certainly this is a belt in California geology where there is juxtaposition commonly along steeply dipping faults of different materials of different geologic ages and in many instances violently different physical properties and hence differences in elastic properties or properties relative to the behavior of elastic waves.

Q Considering your responses to the last few questions, do you agree that it is possible that an earthquake of magnitude 7 to 7.5 on the Calaveras fault could be epicentered between the Calaveras fault and the GETR site?

A With an accurately located epicenter?

Q I'm not sure what you mean by that.

A Well, all epicenters are plus or minus something in terms of location. So in your hypothetical instance you want one known to be in terms of l. ation.

Q I realize that there are errors of location, but with the typical errors of location.

A Okay. No, I would not expect that.

Q Is it theoretically possible?

A I would think that would be possible only in connection with what amounts to a newly developed fault which is possible, but way out there at the very end of the line of probabilities. Because here one has in the Calaveras a very well defined zone of breakage and to depart substantially from that zone of breakage with a brand new rupture in previously unbroken rock is simply not something one would expect.

Q Okay. Thank you, Dr. Jahns. Dr. Kovach, returning to you and the discussion of seismic focussing, I would like to ask you a question. Do you agree that in thrust faulting there can be seismic focussing in thrust faulting in a vertical or vertically dipping angle?

A (Witness Kovach) Yes, I agree it is possible.

Q Do you think that such focussing during thrust faulting could cause higher ground accelerations than would occur without the focussing?

A That's also possible and, as I explained before, it is also not possible.

Q If there were a thrust faulting event on the Verona fault beneath the GETR reactor could that thrust faulting event be accompanied by seismic focusing?

A It's possible.

Q Dr. Kovach, have you reviewed a report by Dr. Darrell Herd of the USGS dated 1977 entitled Geologic Map of the Las Positas, Greenville and Verona Faults, Open File Report 77-689?

A No, I have not read that report.

Q Have you ever seen the epicenters of earthquakes in the Livermore Valley chart which accompanies this report? You may have seen it separately from the report, since you do seismicity studies. Would you review this document, please? Dr. Jahns, would you also review the document?

A (Witness Kovach) I have not seen this before.

Q You have never seen that page from Dr. Herd's report?

(Pause)

MR. BARLOW: Could we have this page marked Intervenor's Exhibit No. 4?

(The document was marked for identification as Intervenor Exhibit No. 4.)

MR. SWANSON: Could we have just a couple of minutes while we get our copy of this document? I assume there is going to be some questioning on this.

MR. CADY: Your Honor, may we have a five-minute break so that the witnesses can review the document and give the staff the time to find it?

CHAIRMAN GROSSMAN: Fine. We'll take a five-minute break.

(A brief recess)

CHAIRMAN GROSSMAN: On the record. Mr. Barlow, you may proceed.

BY MR. BARLOW:

Q Dr. Jahns and Dr. Kovach, have you had a chance to review the document, Intervenor Exhibit No. 4?

A (Witness Kovach) I have looked at the map, yes.

Q I believe you said that you had never reviewed this document in the past, is that correct?

A I have not seen this map before and I have not seen the report and I don't know in what context this map fits into the report.

Q If I might briefly describe the context in the report --

MR. EDGAR: We'd like to have the report speak for itself. Could you -- why don't we go ahead and have you describe it and see if that will shortcut it.

MR. BARLOW: Okay. This report accompanied Dr. Darrell Herd's original map in 1977 of the Calaveras, Verona and Las Positas faults. The report and the map were published I believe in September, 1977, the month prior to the shutdown of the GETR reactor. They discussed the epicenter map, which is Exhibit No. 4, is in a section entitled Tectonic Implications of Faulting in the Area -- I believe that is a paraphrase of that.

BY MR. BARLOW:

Q The epicenters which you see on Exhibit No. 4, would you agree that those are associated with faulting in the region of the GETR site?

A (Witness Kovach) Not necessarily. As I say, I don't know how this map was prepared. It looks superficially to me like it's a -- the points are laid out in a grid so it is like a computer plot. I don't know what the uncertainties here are in any of the locations. So it would be presumptuous of me to try to associate any of these epicenters with faults on the basis of this map. I might add that there is a more up to date one in my testimony which Prof. Bolt prepared which has many more epicenters and much larger data base than apparently is used in this --

Q I realize that, Dr. Kovach, and I agree with you that there is a larger number of epicenters on Dr. Bolt's map. But I wanted to focus for the purposes of this period on this series of epicenters which occurred during a certain time period in the 1940's in the Livermore Valley. If you were to -- first let me ask you a general background question. Can you describe for me the errors of uncertainty in epicenter location and how they have changed since 1940 in this region?

A Well, it is my belief that prior to 1969, before the USGS installed a very dense network of stations, that most of the epicentral locations in this area are based on the readings from I believe two stations, Berkeley and Mt. Hamilton, which -- and they are based on using S-T time. So I would say prior to 1969 the uncertainty in the epicentral locations are certainly much larger than they were after 1969.

Q Could you quantify the range in kilometers in the errors of uncertainty prior to 1969?

A Well, I can't be overly specific since I, you know, didn't review the locations here. But I would say thatthe recent ones after 1969 are believed to be accurate to within a kilometer or so. I believe the ones prior to 1969 could be in error by several kilometers or so.

Q Several kilometers, is that the range of uncertainty?

A That's my estimate.

Q If you applied that range of uncertainty in epicentral

location to these epicenters before you in Exhibit 4, is it possible that the epicenters of those earthquakes could be beneath the GETR site?

A I don't know what the scale is here on the map. So I can't really answer that.

Q I'm sorry. I don't know either. It's not on there. Okay. I just have a couple more questions. Dr. Jahns --

A Well, let me just answer. Yes, it is possible that some of these epicenters could be close to the GETR site; however, because of the uncertainty they could also be further away.

Q Okay. Dr. Gilliland -- Mr. Gilliland -- could you explain to me why General Electric did not ask Dr. Kovach and Dr. Jahns to review Dr. Darrell Herd's original 1977 report which came out a month before the shutdown of the reactor?

A (Witness Gilliland) I think at the time that that report was issued the principal interest had to do with the proposed faults that were shown on that map and the attention was focused on that particular thing. Subsequent in the investigation and I suppose at least a couple of years after that the point of microseismicity of the site was raised and it was at that juncture that we asked Dr. Bolt and he in turn Dr. Hansen to review the microseismicity of the site and it was at that point that they collected, well, they really already had in their computer the collection of epicenters

which are shown in this Exhibit 21, Figure 1. So that study was done and it is -- I think that is the sequence of events. It seems to me there was no oversight, particularly. The focus of interest at the time that map was issued was not this particular feature.

Q Mr. Gilliland, would you agree that it would be helpful in this proceeding if your consultant seismologist, Dr. Bolt, were made available for cross examination?

A At this point I don't see any need. At least I haven't seen a need to this juncture. I haven't seen it demonstrated. Perhaps you have reasons that I am not aware of. Furthermore, the work that was done by Drs. Bolt and Hansen is reflected in this report and I have not noted questions which were specifically in that direction that could not be answered by Dr. Kovach. So I see no need.

Q Well, it appears to be difficult to rely on other people's review of Dr. Bolt's work when he did original research on the site, on the seismicity at the GETR site. Did General Electric --

MR. EDGAR: Is that a question or --

MR. BARLOW: I have a question.

MR. EDGAR: I'd like to object on the ground that that's an argumentative statement, not a question.

MR. CADY: Your Honor, I believe that these questions should be directed to the Board as to whether or not we should

be allowed to have Prof. Bolt here to give testimony as to his reports and as to his input into GE's testimony.

CHAIRMAN GROSSMAN: If you are referring to specific inputs that Dr. Bolt had which the witnesses cannot respond to, I believe you ought to point those instances out to the Board. But I am not familiar from what has been testified to that there were gaps that are attributed to Dr. Bolt's having gathered the information. If you want to be specific, we will entertain your request.

MR. BARLOW: Okay. I would like to point out two documents that are authored by Dr. Bolt that are very relevant in this proceeding. One has been mentioned quite often.

CHAIRMAN GROSSMAN: These are documents that have been offered by GE that were authored by Dr. Bolt?

MR. BARLOW: The first one was offered by General Electric; the other one was not entered into this proceeding, but was authored by Dr. Bolt. The first one, which was distributed to all parties, is dated March, 1980, entitled Seismicity of the Livermore Valley in Relation to the GETR. The second one is dated February 2, 1980, entitled The Greenville Earthquake Sequence of January 1980, by Dr. Bolt, Dr. McEvilly and Dr. Erhammer of the Seismographic Station at the University of California, Berkeley.

In addition to this, there have been other references to other reports by Dr. Bolt in the testimony by these witnesses

and several times when I asked Dr. Kovach about these reports by Dr. Bolt he had not discussed either the reports or the earthquakes that we were discussing with Dr. Bolt in cases where Dr. Bolt was the primary author of the reports on these earthquakes.

CHAIRMAN GROSSMAN: Well, the problem is that you are the one who raised the reports by Dr. Bolt and not the Licensee's reliance on Dr. Bolt that made those reports appear relevant to the case. Now the usual procedure is for you if you believe the reports to be relevant to get them during discovery and then to request that the author, Dr. Bolt, be available. Now that's something you didn't do and now we are up to the hearing and while it is true the witnesses have testified to those reports, it was in response to your questions and, as a matter of fact, if I can recall, the sum of their testimony is that they have never reviewed the reports or analyzed them, which may be another question but it doesn't justify your being permitted to bring in Dr. Bolt at this point.

Now if you can show some special need, which I don't believe has been shown, for Dr. Bolt the Board might listen a little more sympathetically. But all that we have on the record so far is that there have been those reports and the witnesses are not familiar with them.

MR. BARLOW: Would it be possible for us to demon-

strate a specific need for Dr. Bolt early next week, on Monday? If we could document and distribute copies of reports at that time?

CHAIRMAN GROSSMAN: I'm not sure that that would supply what we need in order to determine that the reports are significant. Now I understand you are going to be testifying either as an admitted expert or as making an offer of proof. Now if as a result of that testimony it appears that the matters are significant and are things that the Board must have, it may be at that time the Board will decide to request Dr. Bolt's appearance. But those are hypotheticals.

What we are saying is that as of now I don't think any kind of demonstration has been made that would require Dr. Bolt's presence.

MR. BARLOW: I understand fully. I was only asking if we could make that demonstration later than today.

CHAIRMAN GROSSMAN: Certainly the Board will reconsider on the basis of what goes further in the proceeding.

MR. BARLOW: Thank you.

BY MR. BARLOW:

Q I have just one more question and then I will be completed. Dr. Jahns, is it -- do you agree that in California there have been faults which were previously considered inactive that have ruptured during earthquakes?

A (Witness Jahns) No, I don't believe I would agree

with that right off the top of my head, so to speak.

Q Specifically, in the San Fernando earthquake of 1971 was that fault previously considered active and capable of such an earthquake?

A Relative to your question, in that specific context I would most definitely disagree. For this reason: contrary to published and unpublished reports immediately following that earthquake, something on the order of 98 percent of the total surface trace of active faulting was known and on maps prior to the event. Not only that, the maps were not published, but they were available, readily available to the public for use, largely under the aegis -- compiled under the aegis of the Metropolitan Water District of Southern California. These compilations were made in connection with the water distribution system of MWD that is still under construction, the so-called Foothills Feeder System.

Nearly every one of those faults was definitely judged as active and certainly the evidence was overwhelmingly in favor of that. So I think if one were seeking an example of a fault that was assumed not to be active and proved to be, that would be one of the world's worst. Now one of the world's best would be the White Wolf that we have talked about already, in that there were two schools of thought relative to that one. According to one school this was not an active fault, on the basis of subdued topographic expression

and a number of other factors. According to the other school of thought, it was an active fault in a region where recurrence intervals on the average were very great. And that of course turned out to be the more correct point of view.

Q And that's the fault that in 1952 generated a magnitude 7.7 earthquake?

A That's correct.

Q Thank you, Dr. Jahns.

MR. BARLOW: I have completed my cross examination.

CHAIRMAN GROSSMAN: Mr. Swanson?

CROSS EXAMINATION

BY MR. SWANSON:

Q Just one question for Dr. Jahns and then one for Dr. Kovach. Twice, Dr. Jahns, you mentioned a probability -- I think first was as to the probability of sympathetic response of an event and later I think you were talking about new breakage and you said the probability of certain events was towards the extreme probability, and I just want to make sure the record is clear when you meant toward the extreme you did mean towards the lower end of probability, meaning a trending toward a zero probability, is that correct?

A (Witness Jahns) Yes, that certainly is correct.

Q Okay. I just wanted the record to be clear on that point. Dr. Kovach, on pages 10 through 13 of your testimony you comment on a report by Ellsworth and Marks. That would

be I assume the Ellsworth and Marks report entitled Seismicity of the Livermore Valley, California, Region, 1969-1979, is that correct?

A (Witness Kovach) I believe that is correct, yes.

MR. SWANSON: For the information of the Board, that is an attachment to the Staff's October, 1980 Safety Evaluation Report. I'm sorry. May 23, 1980.

BY MR. SWANSON:

Q You refer there to a figure showing focal mechanism solutions, the points of which you also reproduce on your Figure 4 contained on your page 10, is that correct?

A (Witness Kovach) Yes.

Q Is it your contention that points V and VI are claimed by Maxwell -- excuse me -- by Ellsworth and Marks to be associated with the Verona fault?

A Well, I believe that is what Ellsworth and Marks stated in the report, yes.

Q As well as Points III and IV, is that correct? Those you are indicating are also claimed by Ellsworth and Marks to be associated with the Verona fault?

A Well, they were claimed by them, but if you look at Figure 5, the Event III in the vertical section is way below the projection of the hypothetical dipping line at 45 degrees there, so I don't believe it is likely that that can be claimed to be associated with the Verona fault, not in the

spatial sense.

Q But it is your contention I guess as stated on page 11 of your testimony that those two points, V and VI, have -- can be associated with the Livermore Fault Zone rather than the postulated Verona fault? Is that correct or are you just referring to Point VI?

A Point VI, as I said, could be attributed to the Livermore Fault Zone. And that leaves one focal mechanism in that V that could possibly be associated with the postulated Verona fault. However, in that subsequent letter which I refer to from Bill Ellsworth to Prof. Maxwell, he has now revised his analysis of that event and finds that it is compatible with both strike slip or thrust faulting and so it weakens the case for saying that that proves --

Q Looking at your Figure 4, am I inferring correctly that you sketch in the postulated Verona -- excuse me -- Livermore fault zone as being a northwest trending fault?

A Did I sketch that in? Is that your question?

Q Pardon me?

A Did I sketch that in?

Q Is it your contention, as represented in that figure, that the Livermore fault zone, postulate Livermore fault zone, is a northwest trending fault? Is that correct?

A Yes.

Q And it is your testimony, then, that Point VI on

that figure is compatible with that northwest trending fault zone?

A It's possible.

Q Does it appear to you as though they are trending in the same direction?

A Which is trending the same direction? I'm confused.

Q I'm referring now solely to Point VI of the fault plane solution.

A Well, there is some discrepancy in the strike there; however, there is some uncertainty in the focal plane solution that could be rotated 10 degrees either way. So I don't know how bad the discrepancy really would be.

Q But it is your contention that those two points, V and VI, are incompatible with a thrust fault associated with the sketch of the Verona fault?

A Well, no. My contention is that those two -- those V and VI -- well, particularly V, can be either strike slip or thrust faulting and so, depending on which solution you favor, yes, it is compatible with thrust faulting or no, it is not compatible with thrust faulting, if you accept that the strike slip solution is the correct solution.

Q Point V is apparently not unequivocally a thrust event, but wouldn't you indicate that it is certainly a predominant characteristic?

A That's the subsequent conclusion that was reached by

Ellsworth himself.

Q And yourself, did you say?

A By Ellsworth himself.

Q Himself. I see.

MR. SWANSON: We have no further questions.

CHAIRMAN GROSSMAN: I have a few questions. It appears to me from some of the responses you made that there seemed to be a lack of interest in the Livermore Earthquake of 1980 and no interest at all in two reports by Dr. Bolt on the Livermore earthquake. Could you tell me why it was of such little interest to you, Mr. Gilliland?

WITNESS GILLILAND: Well, the Livermore earthquake was observed at the GETR site, but the levels were very low or relatively low, and well within and in fact on the very low side of the values that were being used for the evaluation of the structures and those that had been under consideration. And so it was not felt necessary to elaborately examine the Livermore earthquake in order to have it add to the body of knowledge that was being used for the evaluation of the GETR facilities because it didn't appear that it would -- that it was any kind of a bounding or limiting or at the limit kind of an earthquake. So that for that reason it was not actively pursued.

Furthermore, that earthquake occurred at a time when -- and the data are still coming from the evaluations

of that earthquake. I don't know that any of our consultants' views have changed. I think everyone seemed to believe that that didn't represent any kind of limiting phenomena, therefore it wasn't necessary to evaluate, that it would fall within the frame of what we had already done.

CHAIRMAN GROSSMAN: Well, was it also your opinion that there couldn't have been any significant data that could be derived from those earthquakes, aside from the fact that they may not be limiting data?

WITNESS GILLILAND: Well, I guess the proper thing to say is no, we didn't believe there would be significant new information derived by carefully examining the earthquake. In the course of these evaluations we had already studied a number of earthquakes, especially the Imperial Valley in relation to near field phenomena with its significantly increased amount of measurement data. And I think it was our collective opinion that it wouldn't be necessary, that it would not add consequentially to the body of information that we were using.

CHAIRMAN GROSSMAN: Does everyone on the panel agree with that? Dr. Jahns?

WITNESS JAHNS: Yes.

WITNESS KOST: I could add something to that. As Mr. Gilliland stated, the recordings were made on the second floor of the reactor building for that earthquake and I don't

recall the exact number, but I believe that the maximum acceleration in the horizontal direction from that earthquake was 5 percent G or less, which is quite a bit below the criteria for which the facility is being evaluated. So if you have 5 percent G on the structure, the free field acceleration will then be somewhat less than that, so it is an even smaller value. So it puts it down in a very small range of significant earthquakes.

CHAIRMAN GROSSMAN: Dr. Kovach, I believe that or at least my notes show that you had indicated that although you had determined a G value for 5.5 to 6 magnitude earthquake you could, if necessary, calculate a G value adjusting that to a 6.5 magnitude earthquake, and then in response to a later question indicated that you couldn't tell without making the calculations whether the G value would be greater at 6.5. Is that the substance of your testimony on that point?

WITNESS KOVACH: I don't recall that I testified that way.

CHAIRMAN GROSSMAN: Well, now why don't you just give us a direct answer. If you were to increase a magnitude from 5.5 to 6 to 6.5 you would necessarily get a higher G value, would you not, if all the other conditions remain the same?

WITNESS KOVACH: Yes, that is correct.

CHAIRMAN GROSSMAN: Now you also in response to

questions indicated that you had excluded some data for making your calculations on vertical acceleration. But I am not quite sure that you indicated what data you did include in making your calculations.

WITNESS KOVACH: The data which I did include in my -- in a preliminary analysis was data from the Imperial Valley earthquake of 1979, excluding two anomalously high points in the data base.

CHAIRMAN GROSSMAN: Oh, in other words, you did use data from the Imperial Valley event but you excluded some of the points for that event, is that correct?

WITNESS KOVACH: Yes, that is correct.

CHAIRMAN GROSSMAN: Did you use any other events other than the Imperial Valley?

WITNESS KOVACH: No, I did not, not for the vertical component acceleration.

CHAIRMAN GROSSMAN: Mr. Gilliland, it was my understanding that the Staff had based its current position on a maximum 6.5 magnitude quake. Is that your understanding, too, sir?

WITNESS GILLILAND: Yes. That is what is stated is the SER.

CHAIRMAN GROSSMAN: But I understood from the testimony this morning that your consultants only determined ground accelerations on the basis of 5.5 to 6.0. Was that

also indicated today?

WITNESS GILLILAND: It seems to me that the values are slightly in excess of 6, but not 6.5. I think that is what was said today.

CHAIRMAN GROSSMAN: I believe at one point it was stated to be 5.5 to 6.1, was that the correct --

WITNESS GILLILAND: I think that is correct.

CHAIRMAN GROSSMAN: Well, didn't you think it would be important to determine what the values would be for the magnitude earthquake that the staff was basing its position on?

WITNESS GILLILAND: Well, let's see. I may have a little difficulty following the trail of exactly what we did in that regard. We looked at the Imperial Valley and the Coyote Lake earthquakes and then, as earlier described by Dr. Kovach, made some predictions on the basis of the curve fits that he did to the Imperial Valley data. That which is shown in the testimony does not show how we arrived at the IV value, but it was in the process of this fitting to the data that I guess we didn't quite get to the 6.5. Perhaps we ought to talk about that for a minute before I respond further. Would that be all right?

CHAIRMAN GROSSMAN: That is fine if you want to consult each other on that.

(Pause while the witnesses confer.)

CHAIRMAN GROSSMAN: Mr. Gilliland?

WITNESS GILLILAND: If I can get this to you straight from what I have just been told -- what we have just discussed. We, as I said, as we looked at this we were looking at the value that the staff had indicated, 6 to 6.5, and Dr. Kovach did some evaluations that indicated that the values would likely be between 5.5 and 6.1. So far as that is concerned, that was in the neighborhood of the 6 to 6.5; however, we had previously adopted the values of acceleration effective acceleration for purposes of structural evaluation that had been proposed by the Staff and so felt it unnecessary to go into the elaboration of that conversion ourselves. So having adopted their value already, we didn't feel it necessary to proceed with that.

CHAIRMAN GROSSMAN: But it is possible that if you made your own calculations on the 6.5 magnitude that your figures for acceleration would exceed the Staff's figures for acceleration on their 6.5 magnitude projection, is that correct?

WITNESS GILLILAND: It's possible, but my recollection of the evaluations that we were doing would be that that wouldnot be the case. It seems to me that we were starting with a value of acceleration that was lower than that started with by the Staff and I would not expect us to arrive at a higher value by making that calculation. We felt that had we

done it it would be within the limit of what the Staff had calculated.

CHAIRMAN GROSSMAN: But you didn't actually make a calculation to determine that?

WITNESS GILLILAND: No, we didn't actually do that

CHAIRMAN GROSSMAN: Going back to the vertical acceleration data, Dr. Kovach, in which you eliminated two of the data points, is that the calculation that you used to arrive at the two-thirds projection of vertical as opposed to horizontal?

WITNESS KOVACH: Yes, that is correct.

(Pause)

DR. FOREMAN: I guess I am attempting to follow the cross examination. There are some generalities and thoughts that occurred to me. So I would ask, am I right in concluding that aftershock possibilities were considered and that the calculation you came out with was much more conservative than any aftershock possibilities, any effects of aftershock that might occur?

WITNESS JAHNS: Relative, that is, to an event on the Calaveras fault?

DR. FOREMAN: Yes.

WITNESS JAHNS: Yes. Normally the aftershock can be reckoned as at maximum not greater than one whole point on the magnitude scale below the original shock.

DR. FOREMAN: So therefore they were considered but not necessarily dealt with in your analysis?

WITNESS JAHNS: That is correct, so far as I am aware.

DR. FOREMAN: Okay. And then, to go on down the line, the same applies for -- excuse me?

WITNESS KOST: Can I add something about aftershocks that -- to state that they were dealt with in the structural analyses. So we have evaluated the structure's ability to withstand aftershocks as well as main shocks.

DR. FOREMAN: Yes. I recall reading that. And does the same kind of thinking apply to seismic focusing? I guess indeed you did consider seismic focusing, but the kinds of events that could happen or the results that could happen were far less than the numbers that you arrived at for the design basis?

WITNESS JAHNS: Yes, I would agree with that.

DR. FOREMAN: Okay. I am just verifying my impressions. And the same applies for sympathetic faulting?

WITNESS JAHNS: Yes.

DR. FOREMAN: And then with respect to the thinking about the seismic gap, the implication that this area was due for a large earthquake, if indeed rhythmicity does occur, you considered that possibility and that such an event would fall within the calculations or the effects that you had --

for which you had calculated for?

WITNESS JAHNS: Yes. The seismic gap notion undoubtedly has some merit but is very difficult to apply in specific instances in the absence of much more knowledge than we now have relative to the differences in behavior along a given fault. In the case of the Calaveras, for example, there is a very substantial fraction of the current activity expressed as creep, aseismic creep. That makes a whale of a difference in terms of its influence on the seismic gap notion. Because if a given segment of a fault is in effect not very effectively accumulating strain energy then it is creating in a sense its own seismic gap in temporal terms. So that the seismic gap notion is nothing that can be very easily and readily applied.

Perhaps Dr. Kovach has something addition to say to that.

WITNESS KOVACH: Well, the only thing I would perhaps add for your information, I feel very strongly that a magnitude 7.5 earthquake on the Calaveras is what we would call a maximum credible earthquake. In order to have a magnitude 7.5 we would have to have roughly a rupture length along the fault of 100 miles or so and that is roughly the mapped length of the Calaveras fault. So we would have to rupture the entire Calaveras fault to end up with a magnitude 7.5. My personal opinion is that it is more probable to be a

magnitude 7.0 where we are talking about rupture lengths of the order of 40 kilometers or so. And indeed, this checks very well with the 1868 earthquake on the Hayward fault, which is the adjacent fault to the Calaveras which had a magnitude of 7 estimated and a rupture length of 40 kilometers. So in my opinion, the maximum credible is 7.5 and the most probable is 7.0.

DR. FOREMAN: Thank you.

DR. FERGUSON: I'd like to ask a few questions. In your testimony, Dr. Kovach, on page 8, there is a Figure 3 there, titled Mechanism for Faulting. Now I saw or could discover no reference to that figure. But I surmise that it was included to help us to understand the symbols on Figure 4. Is that interpretation corre

WITNESS KOVACH: Yes, that is correct.

DR. FERGUSON: So reference to Figure 3 was just an oversight?

WITNESS KOVACH: An oversight, I guess, on my part, yes.

DR. FERGUSON: Looking at Figure 4, then, which appears on page 10 of your testimony, I note in that figure a heavy line, for lack of a better description I will call it a serrated line. It is near the bottom of the figure. It represents a fault, I presume. Have you identified that line? The line that I call a serrated line --

WITNESS KOVACH: The serrated line?

DR. FERGUSON: Yes.

WITNESS KOVACH: This figure was taken from the Ellsworth and Marks report and that line is presumed to represent the Verona fault.

DR. FERGUSON: Yes. It does represent the Verona fault, is that correct?

WITNESS KOVACH: It is meant to represent the position of the Verona fault.

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DR. FERGUSON: Thank you.

Let me just clarify one think in my mind. I have seen the Verona Fault in the material that has been presented thus far, described as the Verona Fault or the postulated Verona Fault. Clarify that? Is it a fault or is it a postulated fault? Does the fault exist? I guess that's the testimony I would like to have.

WITNESS KOVACH: The purpose of my seismological testimony is to try to point out that if one were looking solely on the bases of the seismic evidence to say that the Verona Fault were there, the data do not support that as a definitive conclusion. Therefore, I use the word postulated fault.

DR. FERGUSON: So, in the minds of seismologists, the fault is not definite, is that correct?

WITNESS KOVACH: That's correct.

DR. FERGSUON: In the minds of geologists, Dr. Jahns.

WITNESS JAHNS: It depends on which geologists you are comunicating with.

DR. FERGUSON: Let's deal with the one at hand.

WITNESS JAHNS: In mind mind, there is no Verona Fault.

DR. FERGUSON: In turning back to Figure 4 and looking at that postulated fault. I'd ask you to keep that in front of you and also if you -- or your counsel can provide it

for you, Exhibit No. 1 of Licensee -- Exhibit No. 1 of the Licensee and turn to page 25.

The purpose of this line of questioning, Dr. Kovach is just to try to understand the nature of that fault as best I can. We're now on page 25 of Licensee's Exhibit No. 1 and we also have your testimony in front of us, Dr. Kovach.

Looking at Figure 4 and Figure 4 of your testimony and Figure 16 of Licensee's Exhibit No. 1, am I to interpret the line that is mapped in your Figure 4 to be the total line in Figure 16 of Licensee's Exhibit No. 1, commencing at the far upper end of the solid line on which are the words, Verona Fault, going down the southerly -- the southeasterly direction, I guess, and including the doted portion of Verona Fault that vears eastwardly toward the Las Positas Fault.

I'm just trying to show a correlation between those two figures. The specific question is, is the line shown in Figure 4 of your testimony the total line shown in Figure 16 of Licensee's Exhibit No. 1?

WITNESS KOVACH: I do not specifically answer that or I do not specifically know if it is. The line that was in the Ellsworth and Marks Report was just meant to depict the approximate position for seimological mapping purposes where the relative position of the Verona Fault is to these epicenters shown here. I would have to defer to perhaps Dick Harding, if you don't mind, as to whether this figure

shown here is comparable to this one, because there may be some form of artistic license in there that I'm not aware about.

DR. FERGUSON: I believe that Mr. Harding was the person who testified to this and I would assume -- Well, maybe I should not assume what he would say. The thing that I'm only trying to establish is the fact that in the testimony just above Figure 16, there is an argument that in fact that dotted line does not represent a portion of the fault and when we look at your testimony, we see the line seems to be definite.

Now, I see Mr. Harding approaching the microphone quietly. Would you like to enlighten us, Mr. Harding?

WITNESS HARDING: Nobody asked me to, but I assume the question was coming up.

I believe that the hatchered line which is shown in Figure 4 on page 10 of this panelist's testimony, is -- includes this dashed portion. In other words, the easterly bend of the Verona Fault.

DR. FERGUSON: That would be my assumption, too, from the way it's drawn. But is the argument just above Figure 16 on page 25 of the Exhibit, an argument that that in fact, can not be the case?

WITNESS HARDING: That's my argument, yes.

DR. FERGUSON: So, based on that, there is some error in figure 4?

WITNESS HARDING: I believe that figure four was taken from somebody elses report, not mine.

DR. FERGUSON: Which you do not agree with, of course?

WITNESS HARDING: No, I don't agree with figure 4.

DR. FERGUSON: Do you agree with it --

WITNESS KOVACH: I have no opinion about Figure 4, I just used it as a cartoon for reference here in terms of the approximate location of these focal mechanisms to the presumed position of where the fault is. I would not construe this -- my Figure 4 to be an accurate geologic map by any -- and I'm sure that Ellsworth and Marks, wouldn't either.

They would not presume that this is an accurate geologic map.

DR. FERGUSON: Dr. Jahns, are you in agreement with Mr. Harding's view of these two figures?

WITNESS JAHNS: Yes, sir, I am.

DR. FERGUSON: Thank you.

Let me direct, now, your attention to some conclusions that you reached on page 13 of your testimony, Dr. Kovach. On page 13 of your testimony you say, in part, in summary a review of the available seismic evidence supports the following conclusions. And you give three and I will read only one. The one that I am interested in and that's conclusion number three which is found on page 14.

The conclusion is this: The theoretical assignment

of earthquake foci to a postulated fault is not independent evidence for the postulated Verona Fault. The Verona Fault can only be assumed to be active if at all for reasons apart from the available seismological evidence. And I'll stop there.

I just had a little difficult. Perhaps, it's semantics. Are you intimating in that second sentence, which I'll read again. And the sentence is this: The Verona Fault can only be assumed to be active, if at all, for reasons apart from available seimological evidence. Does that statement suggest that there are reasons to believe that the Verona Fault is active or are you saying that if the Verona Fault is active, it must be for some other reason that you do not know of?

WITNESS KOVACH: The second thing that you stated.

DR. FERGUSON: Then it was semantics?

WITNESS KOVACH: Yes.

DR. FERGUSON: Thank you, I have no further questions.

CHAIRMAN GROSSMAN: Before we get to your re-redirect, Mr. Edgar, I have a few up on what Dr. Foreman asked.

Dr. Jahns, did you mean to indicate that you took into account and calculated the effects of seismic focusing or rather that you made a determination that it was unnecessary to consider the case of seismic focusing?

WITNESS JAHNS: You're addressing me, sir?

CHAIRMAN GROSSMAN: Yes.

WITNESS JAHNS: No, actually neither. I have not made any effort quantitatively to appraise the effects of seismic focusing.

CHAIRMAN GROSSMAN: Does the same hold true with regard to sympathetic faulting?

WITNESS JAHNS: So far as I'm concerned, like seismic focusing, it's a qualitative concept that really winds up as a matter of judgment in terms of its input relative to the problems here. So, nothing is strictly quantitative.

CHAIRMAN GROSSMAN: You just made your determination that it doesn't apply and that's as far as you went.

WITNESS JAHNS: About all that one can do, really, is examine the historic record world wide to get the largest possible data base and simply ask the questions, in how many instances can one make a reasonable case for the occurence of sympathetic faulting. The number is very small.

CHAIRMAN GROSSMAN: I'm not challenging your conclusion, I'm just asking what was done. I wanted to get that straight. That's fine. That fully explains it.

And I take it, the same thing holds true with regard to seismic gap. That it just was not considered by you to be relevant to the situation we had here and after that initial determination, nothing further was done with that?

WITNESS JAHNS: Quite so. There were too many complicating factors for the Calevaras Fault.

CHAIRMAN GROSSMAN: Thank you.

Mr. Edgar.

REDIRECT EXAMINATION

BY MR. EDGAR:

Q A couple of points of clarification. First of all, Dr. Kovach, you had indicated in response to questions by Mr. Barlow, that you had undertaken some review of the data for the Santa Barbara, Mammath Lakes and Livermore earthquakes. Is that correct?

A (Witness Kovach) Yes, that is correct.

Q Did that review affect any of your conclusions as to the appropriate seismic design bases for GETR?

A No, it did not. I did examine the data and all of the data for less than six kilometers to the fault. The mean value from all of those earthquakes is about three-tens of the G and so my conclusion was that it would not effect the design criteria.

Q Now, during responses to Mr. Barlow's questions, you mentioned that there was an observed 1.33G vertical acceleration to the Gasli earthquake. Is that correct?

A Yes, that's correct.

Q Could you explain the bases for that acceleration value and whether you consider it to be valid?

A In the case of the Gazli earthquake, there -- the velocity structure, again, is quite anomalous compared to the Livermore Valley case. There's a very deep low velocity sedimentary layer present beneath the station and the net effect of that -- There are actually two effects that you get in the sedimentary layer. One is a very strong P to horizontally polarized to what we call SV conversion and there -- it contributes to ringing, which probably amplifies the vertical component of motion.

I have prepared a graph here, which I think should be presented, which is a plot of depth in kilometers verses the E wave velocity in kilometers per second for the three areas that we were discussing. Namely the Imperial Valley, the Gazli area and the Livermore region which clearly points out the differences in the velocity structures. I think that this could be of some use to the people.

DR. EDGE: Would counsel and the Board like to see that?

MR. CADY: Yes, please.

CHAIRMAN GROSSMAN: We'll take a look at it.

(Pause)

WITNESS KOVACH: What's plotted here --

DR. FOREMAN: You're on the record. Do you want to explain it on the record?

WITNESS KOVACH: What's plotted on this figure is a

plot of the compression or P wave velocity in kilometers per second along the X axis verses the depth in kilometer. The point to be made here --

DR. FOREMAN: The depth of what in kilometers?

WITNESS KOVACH: The depth from the surface.

DR. FOREMAN: Of the wave or the depth of the shoot.

WITNESS KOVACH: Well, just the thickness of the sediments and the rocks,which the earthquake would progogate up to the surface.

And the point to be made here in the Imperial Valley there is a very strong gradiant in the upper part which causes this upward refraction of the P wave motion. In the Gazli, there is a very deep low velocity sediment layer and then a huge jump to a much higher velocity and this causes very strong P to SV type of conversion and it causes a ringing in the sedimentary section and has a tendency to amplify vertical component of the strong ground motion --

MR. CADY: Excuse me. Could you explain what the P to SV relationship is? You're talking to people who are not quite schooled in this type of activity.

WITNESS KOVACH: Well, a P wave or a compressional wave is defined as a wave in which it's motion is alternate compressions and rarafactions or push-pull in the direction of propogation. In other words, if the wave originates where I am sitting and comes towards you and passes beneath your

feet, you would feel vibratory motion in the direction of propogation. Now, a shear wave on the other hand, the direction of vibration is always transverse to the direction of propogation. NOw, since it is transverse, you can also split that into two components called the horizontally polarized SH, which would be the one vibrating in the horizontal plane and an SV wave which is polarized in the vertical direction, but oscillating to righ angles of the directional propogation.

So, you can get a conversion of a P wave motion to an SV type of motion. It's a demonstrated seismological fact. You can not get a conversion, though, from a P to an SH type of motion. It has to have the vertical component.

CHAIRMAN GROSSMAN: I notice this document.you handed us has numbers to it and apparently is based on some quantitative data. Is that correct, sir?

WITNESS KOVACH: Yes.

CHAIRMAN GROSSMAN: Where is this underlying data that you have to support this document? First tell us what it is?

WITNESS KOVACH: What the data are?

CHAIRMAN GROSSMAN: Yes. What is the nature of the data that you used inorder to compile that document?

WITNESS KOVACH: The data from the Livermore Valley earthquake were taken from the Ellsworth and Marks Open File Report and this is the crustal model, which is assumed to be

the appropriate one for the Livermore Valley and it's based on calibrations of some explosion shots and refraction experiments and the time term analysis of the seismic waves.

The data from the Gasli earthquake are in the published literature and their also based on seismic refraction experiments that were done by the Russians in the epicentral area and likewise the refraction data for the velocity data for the Imperial Valley are probably based on data from my thesis which I did in 1962. Since I did the first seismic refraction experiments in the Imperial Valley.

CHAIRMAN GROSSMAN: I had understood that you drafted this during the luncheon break, but was this something that you had had that had been done some time ago?

WITNESS KOVACH: The data had been available, yes.

DR. FOREMAN: But when did you put them altother, he's asking? Just now?

WITNESS KOVACH: I just made the graph last night, because the point came up about the vertical accelerations and I want to have this as an additional piece of evidence to help explain the reasons for these high vertical accelerations.

CHAIRMAN GROSSMAN: Is there anything pending, Mr. Edgar.

MR. EDGAR: I would like to have that, if I may, marked for identification as Exhibit No. 42.

(Pause)

(The document referred to was marked for identification as Licensee's Exhibit No. 42.)

MR. CADY: Your Honor, if I may? We have no objection to its being marked as an exhibit, but I believe Intervenor and Staff would like to review the underlying data that was used in the preparation of this diagram.

MR. EDGAR: No problem. I just want it marked for the sake of an organized record for the moment.

BY MR. EDGAR:

Q Dr. Jahns, you were questioned in regard to the so-called working hypothesis that you and Dr. Bolt developed. As I understand that hypothesis, there is some notion that there will be a great earthquake in California at some point in the near future. Is that correct?

A (Witness Jahns) Qualitatively, yes. To be more specific, a magnitude 7 or larger earthquake.

Q Does that refer to California as a whole, or is the theory based on specific fault areas or locations?

A Somewhere in California. California as a whole.

Q Taking this hypothesis into account, does this give you any reasons to believe that one could exceed a 7 to 7.5 event on Calaveras?

A As a possibility without attached timing? Yes,

it certainly is a possibility.

Q Do you believe it's likely?

A I think it's likely for some time in the future, because it's an active fault.

Q Do you believe that it's likely that in some point in the future that there will be an event of greater than 7.5 on Calaveras?

A No, I do not. Not greater than 7.5. I quite agree with Dr. Kovachs that 7.5 is a reasonable value for the maximum credible event and that's plus or minus nothing. 7.5 is the maximum.

Q Dr. Kovach, in connection with your testimony, you were asked a series of questions by Mr. Barlow concerning the U.S.G.S. Open File Report by Joyner et. al. and that is Open File Report No. 81-365. Could you explain the basic nature of the analysis in that Open File Report?

A (Witness Kovach) Well, the basic analysis was to take a set of data and subject it to a statistical analysis with not allowing for the fact that magnitude saturation was probably the appropriate thing that happened and it came up in the analyzed and obtained a set of curves.

Q At what magnitude levels were the data upon which they did this statistical analysis?

A At what magnitude levels? Could I refer to the document?

Q Sure.

(Pause)

A There magnitude range which they considered apparently ranged from 5.0 to 7.7.

Q Have you -- Does that Open File Report give you any reason for changing your conclusions concerning the seismic design bases?

A No, it does not.

Q This is addressed to Drs. Jahns and Kovach, both. Let me address it to Dr. Kovach? Do you have any reason to believe that one can expect due to an event on the Verona Fault the phenomena of seismic focusing at the GETR site?

A (Witness Kovach) I -- As I stated earlier, I believe that's certainly a possibility that could happen.

Q Do you believe that it's likely?

A That would be only a prediction on my part. Yes, I think it's likely.

Q Would that have any opinion or would that have any influence on your opinion as to the appropriate values for instrument acceleration?

A No, because the data base which I used if focusing were present, it's already present in the data base. And so there is no need to take data base and make a separate allowance for focusing since it's already in the data base.

Q Dr. Jahns, you were asked some questions concerning

the possibility of branching or new branches from the Calaveras Fault, In your opinion, is it likely that a new branch could form on the Calaveras Fault which could extend on to the GETR site?

A (Witness Jahns) I would say extremely unlikely. Even if the Verona Fault were joined as a simple branch and even if the Verona Fault were a near vertical feature like the Calaveras, the chances of a new joined being formed in that fashion would be extremely small. All one has to do to test this is examine the number of breaks on a fault like the Calaveras at a given latitude and consider that in terms of say the calculated number of earthquake events through a million years of geologic time and this comes out to an extremely low probability.

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Q Dr. Kost, during much of the questionning, the issue was presented as to vertical versus horizontal acceleration as measured on instruments.

From a standpoint of structural response, what is the significance of the vertical versus the horizontal components?

A (Witness Kost) I think that can be answered as follows. The numbers that have been discussed today have been ratios of the vertical to horizontal on the order ot 2/3rds which, by the way is very consistent with standard practice in the earthquake engineering field and ranging perhaps to ratios of 1 or similar numbers.

What we're concerned with here is very high frequency motions in vertical directions due to these close in earthquakes that have been the main topic of discussion today. These high frequency motions attempt to in effect to be averaged or filtered as the seismic waives pass across the large base of the structure such as the GETR reactor building as opposed to a very small seismograph station.

The effect of this averaging is to produce a much smaller effective acceleration in the vertical direction, than the numbers that have been talked about today so the ratio in effect as it influences in response to the structure is indeed much less than one times the

horizontal or 2/3rds the horizontal for that matter. I think it's also worthwhile in discussing vertical motions to note that structures are inherently very strong in the vertical direction.

They are designed for primarily, for vertical motions, or vertical loads. I'm speaking here of typical office buildings.

When you have the structure such as the GETR reactor building and similar buildings which have a very heavy massive wall, the vertical strength of these such structures is very very high. The design configuration is ususally based on considerations other than just supporting the vertical weight of the structure and they need a certain amount of shielding perhaps or other considerations.

As a result, the stresses in the various structural members in these massive walls tend to be very low in the vertical direction. The net effect is that the vertical accelerations from earthquakes have very little influence on the conclusions regarding the adequacy of the structure.

MR. EDGAR: I have no further questions.

CHAIRMAN GROSSMAN: Dr. Kovach, just a clarification. You indicated that you took into account seismic focusing in your data base. Did you mean to say that

you took it into account specifically or you just assumed that there is just as much chance that there was seismic focusing in the data you used as there would be in a postulated event in the GETR vicinity?

WITNESS KOVACH: Yes, the answer is to the second part of your question.

CHAIRMAN GROSSMAN: The latter part.

But it may be that there wasn't any seismic focusing in the data base and there might be a GETR and vice versa that might be seismic focusing in your data base and there may not be at GETR. Those possibilities are there too, is that correct?

WITNESS KOVACH: Yes.

DR. FOREMAN: I have a question.

What order of change might a seismic focusing introduce in a seismic event? Would they increase an ordinary event? A factor of 1-½, 2, an order of magnitude? What sort of things are we talking about?

WITNESS KOVACH: It would be very hard to generalize or give you an answer because it depends on many parameters such as how fast the rupture is and what's the shear waive velocity but I would say off the top of my head, it would certainly not be more than 20%.

MR. CADY: Your Honor, I would like to ask some questions of this panel after I have had time to review

the data base that Dr. Kovach used to prepare that last figure, relative to the excessive vertical accelerations at the Gazli site, the Livermore, the Imperial Valley sites of the earthquakes and I would specifically like to direct questions as to the validity of the data and as a result, the computations to Dr. Kovach and also, if necessary, have Mr. Harding present to discuss the underlying soil structures of those areas.

CHAIRMAN GROSSMAN: Well, my understanding is that the exhibit that has been marked for identification is not going to be offered and definitely if it is offered, there will be opportunity for impeachment, but nevertheless there has been some testimony that's been given that's been based on that document and I think you will have an opportunity to ask some more questions with regard to the testimony, if not to the document itself which is not apparently going to be offered.

Is there any objection to that, Mr. Edgar?

MR. EDGAR: No objection to that.

CHAIRMAN GROSSMAN: I believe that concludes the business of the panel and I would like to thank you for appearing here. Before we call a recess I'd like Counsel to approach the bench and we can decide on the procedures.

MR. CADY: To the panel members, thank you very

much.

MR. EDGAR: Could I ask, I had made an offer of Exhibit 21 and I don't think the board made a ruling so I would like to have that admitted into evidence if I may.

CHAIRMAN GROSSMAN: I take it Exhibit 21 is a testimony?

MR. EDGAR: That's correct.

CHAIRMAN GROSSMAN: Any objection, Mr. Cady?

MR. CADY: No objections.

CHAIRMAN GROSSMAN: Admitted.

(The document referred to, having been previously marked for identification as Licensee's Exhibit No. 21, was received in evidence.)

CHAIRMAN GROSSMAN: We will take a ten minute recess.

(Whereupon a ten minute recess was taken.)

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CHAIRMAN GROSSMAN: On the record.

The hearing is now in session. I believe our next witness is Dr. Brillinger?

MR. CADY: That is correct. Let me introduce him to you. This is Dr. David Brillinger, Chairman of the statistics deparment, University of California at Berkeley.

David, I would like to introduce you to the Board members. Judge Grossman in the center. On your left, Judge Foreman and the gentleman on the right is Judge Ferguson and I would like to now turn the examination over to Mr. Edgar.

CHAIRMAN GROSSMAN: Dr. Brillinger, could you stand please and raise your right hand?

Whereupon

DAVID ROSS BRILLINGER

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

MR. EDGAR: Do we have an exhibit number for Dr. Brillinger's testimony? Can we assign one, so that we can have a shorthand --

MR. CADY: Let's make that Intervenor's Exhibit No. 5.

(The document referred to was marked for identification as Intervenor's Exhibit No. 5.)

CROSS EXAMINATION

BY MR. EDGAR:

Q Dr. Brillinger, I will refer to your written testimony, what your caption, comments re; the Vallecitos Nuclear Reactor/Slash GETR as Intervenor's Exhibit No. 5 or Exhibit No.5. So, if you'll bear with us, that's a simple convention and might save some words.

A couple of initial points of clarification. In regard to the preamble of your testimony, I see a statement which I will quote. I was requested by an aid to Congressman R. Dellums to review certain materials related to the siting of nuclear reactors at Vallecitos, California. Following that, it says, this material was and five documents are listed.

Am I to understand that these are the only documents which you have reviewed in connection with the GETR proceedings.

A No. To the date of April 19th, that's true, but I received quite a few other documents since then which I have read through.

Q YOu have undertaken a review of other documents in addition to these?

A Yes. I haven't had the time to go into the depth I did with these documents, here, but I read through.

Q Now, based upon the review of additional -- of the additional materials are there any specific corrections or additions that you would make to your testimony?

A I would say my opinion of the additional material is quite similar to my opinion of these five documents.

CHAIRMAN GROSSMAN: Wait, can I interrupt for a second here. I'm not sure we got your full name and address down on the record. Did we, Ms. Reporter? I don't recall.

Would you give it to her?

WITNESS BRILLINGER: It's David Ross Brillinger, statistics department, University of California, Berkeley, California 94720.

CHAIRMAN GROSSMAN: I'm sorry, Mr. Edgar.

BY MR. EDGAR:

Q Now, in regard to your professional experience, am I correct in understanding that you have no educational training or professional experience in the field of nuclear engineering?

A That's correct.

Q And likewise in the field of geology?

A That's correct.

Q And seismology?

A I've co-hosted courses in seismology, so I've instructed courses in seismology. I'm not sure how you view that to being educated.

Q Well, do you consider yourself to be an expert in the field of seismology?

A No, but I do consider myself to be a professional

amateur in a sense.

Q Could you define the elements of the term professional amateur, as you use it?

A Perhaps, I could describe a statistician and then convert into that. A statistician is an individual who applies mathematics and certain parts of mathematics especially to problems of science in the broad range and during the course of his career, if he is a mainstream statistician, he'll find himself working with subject matter from various substantive fields.

The field I have worked in mostly would be seismology. I've worked a great deal in neurophysiology, also.

My thesis advisor once said that a statistician couldn't consult with a chemist unless he became a chemist, so I would say that there are areas of seismology I know as well as the seismologist do, because I've worked in those. There are many areas that I have never worked in.

Q You used the term mainstream statistician.

A I meant as opposed to a theoretical statistician.

Q So, that would be an applied statistician?

A A mainstream and especially an applied statistician.

Q Now, in regard to -- You do not have any expertise in structural engineering, I would assume?

A No, I don't.

Q About how much time did you -- have you spent

reviewing the documents that formed the bases for your testimony?

A Several hundred hours, I would say.

Q Have you done or performed any independent calculations or probability analysis in regard to the probability of surface offset beneath the GETR?

A No, I haven't.

Q If I could call your attention to page one of your testimony under the last sentence, you have the phrase included, the end result of the analysis, the single number probability constitutes too brutal a summary of the situation.

Now, what type of results would you prefer, rather than a single numbered probability? Is the implication there that you would prefer to see a range of results?

A Yes, of different qualitative character. Somehow there seemed a great deal of concern of demonstrating that a certain probability was less than ten to the minus six and that is something of interest, but there are many aspects. A single number only has so much information in it. It's a very complicated situation.

Q So, you would prefer to see a range and perhaps a parametric analysis that give one a broader sense of the meaning rather than the single number?

A Whole succession of numbers devoted to different aspects of the problem.

Q You indicated that a single number probability, ten to the minus six, has meaning in and of itself. What meaning would you assign to that?

A Excuse me.

Q Am I correct that you do attribute some significance to the probability, ten to the minus six per year?

A Yes, but it depends totally on the context in which it is introduced.

Q Let's pick the context of the probability of the surface offset beneath the GETR. What in your mind is the significance of that value, ten to the minus six, per year.

A In that case, the specific number doesn't mean too much to me. It is very small.

Q But it has no meaning other than an intrinsic meaning, I assume?

A I'm not sure if you're trying to get at what one means by probability within the foundations of the probability.

Q We're not understanding each other. I'm just -- Your statement was that there may be some meaning that one would attribute to the number ten to the minus six per year and I'm trying to ask you what significance you would ascribe to that value.

A This sentence, I was directing myself to the fact that a lot of energy went into producing a single number. This afternoon, I felt the same thing was happening to

producing a single acceleration value and a single number is a very restricted entity. One needs a great deal more of context in other numbers of other quantities to be able to proceed to consider the problem.

Q Do you believe that you have reviewed every probability analysis that GE has performed for these proceedings and submitted to the NRC?

A I don't believe that, but I don't know. There are several other in the documents that I have received since April 19 stating -- addressing the same problem, but in a different fashion, slightly.

Q And so you're not -- You can neither claim nor disclaim whether you have reviewed the complete body of analysis accomplished by GE?

A I'm certain that I have not reviewed the complete body of analysis.

Q Have you reviewed any analysis of the probability of the surface offset beneath the GETR conducted by any person other than GE?

A There was an analysis conducted by Terra Corporation that I saw.

Q Have you reviewed that?

A Yes, I looked through that.

(Pause)

Q On page two, the first full paragraph, you make the

statement in the second sentence and I quote, or in the third sentence and I quote, A cursory review of the literature yields conflicting physical values for many of those employed.

Can you give me a cross reference to your, in other portions of your testimony where the statement as to the conflicting physical values is found? Where you might expand upon that.

A Where I might have expanded in these pages?

Q Yes.

A I had in mind things like the discussion that took place earlier this afternoon like the values of G.

Various formulas were made use of in the course of the probability calculations that had values in them and when I looked through the literature, I was finding various opinions on the values of those parameters.

Q I was trying to get just a clarification. When you talk about conflicting physical values, that brings to my mind something like a geological parameter.

A I mean like the maximum acceleration. That that's the sort of value that one might expect that an earthquake a certain distance from a certain fault of a certain character.

Q What I'm trying to get at, if you could assist me a little bit, is that you indicate that you undertook a

cursory review of the literature and what I'm trying to do is develop a list of the physical values where you believe there were conflicts between the physical values in the literature and I couldn't find them in the later portions of your testimony. Perhaps, you could help me.

A Well, if we went through the various reports one by one, that may be the best way to do that, if you'd like to do it.

Q Okay, we'll hold that for the moment, but I'm looking within the body --

A The general procedure I followed was I read the report and then I would refer to some value that had been observed or some curve that had been computed. I would go lock at the references and related references and I would find quite a wide ranch of value. Some values were double what different people were quoting quite different values.

Q Down in the next paragraph on page two, you, in the last sentence, the statement appears that quote, the implications of deliberately building in bias parens conservatism, question mark, close parens, comma, need to be investigated, unquote.

What do you mean by that? Could you expand upon that?

A Yes, various values are propogated through the studies and the approach seemed to be rather than to try to

find the best value, the best answer to whatever the question was. At each stage, one would take the most conservative value and propogate that value forward and the usual statistical procedure would try to produce a best estimate and then to attach a range of error around that. Sc, it was as if at each stage you were biasing your estimate and propogating the biased values through. That's not a traditional procedure and there might be some unexpected implications to proceeding in that fashion.

Q Is the concern -- I'm trying to develop some sense of what the concern is. Is the concern that if one keeps building in this conservatism then one doesn't have a clear definition of the limits of error of the analysis?

A It's not clear just what the answer means. THe final a swer that one has come up with. The normal statistical procedure is to try to produce the best estimate that one can of whatever value one is interested in and in this case, and this is what I think is one of the defects that I see in these studies -- it's been very much compartmentalized. ONe tries to solve this problem and then one fees a single number, say, a maximum acceleration into another compartment and then one feeds something from that compartment and probably some single number into a later compartment and biased in a conservative direction, values are propogated through rather than best estimates.

Q In the abstract, wouldn't you agree that compounding conservative assumptions or values, as the case may be, is an approach which tends to produce worse results than one would normally expect?

A I think normally, but I wouldn't be surprised but there were some logical counter examples one could construct.

Q Can you conceive of any in the context of the GETR safety analysis?

A Well, perhaps something has been made so thick, it then effects in a negative way some other aspect of the structure.

Q Can you point to any specific examples of --

A No, I can't. This just struck as a non traditional manner to estimate parameters.

Q Do I take it the comment is not made in the particular sense, but it is more of a question that one might pose from the standpoint of approach or methodology to problem solving?

A Yes.

(Pause)

Q In the third full paragraph -- the third paragraph on page two, you have the statement and I quote, a full risk study should be carried out in the approach of such a study assessed in detail. You also indicate that the study should include among other things, all reactors at the site. Do you

know how many reactors there are at the site?

A No, but I was baffled, because it talked about probabilities per reactor year and if there is only one there, I didn't think they would do that.

Q Well, if there were just one at the site, would it make any difference? In other words, wouldn't per reactor year be a proper convention?

A I was surprised by the terminology per reactor year.

Q So the sense of that comment was a question born out of the convention per reactor year?

A Yes, to me it made me think there must be a second reactor there. At the point in which I had these documents, I didn't have a detailed description of what was actually at the facility.

Q If you'll turn to page three of your testimony, the first full paragraph. You include the statement and I'll quote it. Suppose a coin ten times and a tail comes up each time, then the question, are you then willing to proceed on the bases that the probability of a head, the next flip, is one over the quantity n plus two, question mark.

Let me ask you in another context, the same question. I take it your point is that -- Let's just assume that n is a large number and specifically if we assume that n is a hundred and twenty-eight thousand, what then would you say is the probability of a head on the next flip?

A I would have to -- So, you're saying the coin has been flipped a hundred and twenty-eight thousand times and has come up tail every single one of those times?

Q That's correct.

A I would say it is somewhere near one of it being a tail. To have a precise answer, one over n plus two is just scientifically deceptive.

Q What I'm trying to get at is the notion that you indicate that LaPlaces rule of succession has fallen into discredit. Is that correct?

A That's true.

Q What I'm trying to do is put a little sense of context on that and let's suppose that we have the expression one over n plus two and n is a very large number and I'll choose 128,000 for that number. When n is a large number, can I proceed with some confidence that the probability of a head on the next flip is quite small?

A Well, you'll have to define your terms. It could be a perfectly fair coin and come up a tail 128,000 times. If you ask me to work up the probability of that happening, I can and if you ask me to work up the confidence of various values coming up, I can.

Q Let me put it another way, then. You say that we could have a very fair coin and it's flipped 128,000 times and you get a tail everytime, but if you're going to use your

own money in this process, wouldn't you question the fairness of the flipper?

A Yes, because a very rare event would have had to happen for that to take place. This is something one could work out a specific probability for. I'm objecting to a single specific number being there rather than the value being propogated as a random variable.

Q I'm missing the point, because it seems to me that your criticism goes to the use of the method in general of that employed in the document that you've critized. What I'm suggesting or what I'm asking to you or of you is if one has an historical record that is quite long and is consistent, then is it going to make any difference if your historical record is long, wouldn't you expect that the probability of a head on the next flip is quite low?

A It would depend totally on the context. If you were to phrase that, do I think, because we're getting farther and farther away from the 1905 San Francisco earthquake the chances of another one are getting smaller and smaller, I certainly would.

Q Let me put it in more pointed terms then. Let's suppose that I have two shears. One on each side of the reactor building. Let's further assume that I know for 128,000 years, I have had movement along the shears further that for 128,000 years I have had no movement off the

shears, between the shears. Then does it matter what method I'm using? Is it highly likely that I won't have movement off the shears in the future?

A See, it's just total opinion. I could answer that in some subject fashion, but it's not being produced by any scientific methodology, my answer to you.

Q What is your opinion, then?

A In my submission here, I asked why in the world are there two shears, then? If it's just going to break along existing shears, there should only be one shear there.

Q We'll go on to that, but would you then feel that there is no scientific way of taking the information of movement on the shears for 128,000 years and no movement between the shears for 128,000 years and developing an estimation of the probability of movement under the reactor foundation. Is that your position?

A I forget which way you way you phrased the question. I believe it is possible to evaluate a probability of that character by collecting information at other locations, by looking at micro studies in the laboratory. Things of that sort. I think when one doesn't have many observations, one wants to look for similar situations and make use of the data from those situations. One doesn't want to invoke a Bayesian argument.

Q When you talk about -- I'd like to pick up on one

point in the latter part of your sentence or your answer, if you will. I believe you said when one doesn't have many observations. Am I correct?

A Yes.

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Q Would you consider a record of movement on the shear for 128,000 years, and a record of movement with no movement between the shears for 128,000 a small record?

A (Witness Brillinger) Yes, I would.

Q And it is sufficiently small that you believe that it cannot be used for the purpose of developing a probabalistic analysis?

A I think one can draw some conclusions but they would have to have suitable qualifications and I think the conclusions would almost be the statement you made.

Further condensation isn't really helpful.

Q I'm sorry, I didn't hear the last remark.

A Further condensation down to a single probability is not really helpful.

One can make the remark for 128,000 years, etc., etc., whatever you said.

Q And that's as far as you would go with it?

A I think I mean, if I were involved in such a study, I would be looking very hard for other related information and not leave it at that.

Q And you would just take that information at face value and ascribe no significance to it other than as a historical fact?

A Yes, at this point, yes.

Q All right. Now, I'm almost a layman. I'm close.

I really should call myself a layman. Would you give me some sense of what this arguement is about the Bayesian analysis school of thought versus the classical. Would you put that into some kind of nutshell? It seems to be a big controversy.

A Yes, there are several sorts of Bayesians.

The classical arguement in statistics would proceed by -- there would be some experiment of interest and it would seem reasonable to describe outcomes of that experiment in terms of probabilities.

There would be certain unknown constants in those probabilities in a variety of circumstances. When we have a population, the constant might be the mean of the whole population.

In an experiment that can measure the force of gravity, the parameter would be the true force of gravity.

Bayesians proceed by taking that constant, that parameter and viewing it in turn as having a random character to it. Subjective Bayesians would ascribe to probabilities by using their own subjective knowledge and experience and that is the most debatable area of Bayesian statistics because it means that the estimates that one ends up with have the persons biases built into them in a very specific fashion. There is continual argue-

ment over estimates produced in that fashion.

There is another sort of Bayesian arguement, empirical bayes, where in point of fact, the parameter can really be viewed as a random variable and for example, this business of this $\frac{1}{n+2}$, for the probability of a head, suppose what was the case that there was a box that say had 10,000 coins in it. One of them had probability. One in 10,000 of turning up tail, another probability 2 in 10,000, another probability 3 in 10,000 all the way through. The last one had the probability of 9 in 10,000 let's say. The way the experiement proceeded was you reached into a box and picked out a coin and you didn't know which one it was and you flipped it and you found yourself with tails n-times. Then the classical statistician would agree that $\frac{1}{n+2}$ is a sensible estimate of the probability of a head coming up, but do you see that I constructed an experiment in which this parameter value, this constant going along with the coin had in fact been generated by a previous experiment.

Q I'll perhaps oversimplify so you be careful not to take me at my word, but as I understand it, the Bayesian approach is one that involves the process of judgement in arriving at --

A The subjective Bayesian approach.

Q Yes. I mean, no one's free from sin altogether but

he seems to be about the worst, is that --?

A To the classical statisticians.

If he is a bright enough person, then his answers are going to be all right, but one doesn't know when one has a bright person and that's the bad part.

Q And the thing that's distinguishing here is the application of the judgement factor, I take it?

A Yes.

Q Now, getting back to the other thought that we had discussed very preliminarily which is the implication of building in a conservative bias throughout each junction or each juncture of ones analysis, what would you think of a Bayesian statistican who made conservative assumptions as his means of implementing subjective judgement?

A You've simply described him. I would just take him as that, a conservative Bayesian statistican.

Q Well, would that -- why would you tend to have a lack of confidence in his results in that case?

A The difficulty comes in that, well, the difficulty for a statistician comes in that most statisticians would view their role as being objective role, as indicating what conclusions can in fact be drawn from given data sets in given experiments and by bringing the subjective information in a specific fashion, that fights against the

natural role of the statistician. As long as he states what he's done, well, that's quite fair and proper.

MR. EDGAR: Mr. Chairman, I am not going to finish this afternoon. To be fair I really think I've got a full hour to go.

I'm willing to do whatever the parties are willing to do or the Board.

CHAIRMAN GROSSMAN: I think this is an appropriate time to recess until Monday morning at 9:30 in San Francisco.

(Whereupon, the hearing was adjourned at 4:53 p.m., to reconvene at 9:30 a.m., Monday, June 1st, 1981, in San Francisco, California.)

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This is to certify that the attached proceedings before the

Nuclear Regulatory Commission

in the matter of:

Date of Proceeding: May 29, 1981

Docket Number: 50-70-SC

Place of Proceeding: Livermore, California

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

Ruth Portune

Official Reporter (Typed)

Ruth Portune

Official Reporter (Signature)