UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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

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

Thursday, 4 June 1981.

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

BEFORE:

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

GEORGE A. FERGUSON, Ph.D., Member

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

APPEARANCES:

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

Appearing for the NRC Staff

- more -

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* * *

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* * *

I N D E X

WITNESSES:	DIRECT	VOIR DIRE	CROSS	BOARD	RECROSS	REDIRECT
Philip S. Justus) Robert E. Jackson) Robert H. Morris) Earl E. Brabb) Darrell G. Herd) James F. Devine) William L. Ellsworth) Raman Pachumani) [resumed])						
By Mr. Barlow [resumed]			1317			
By Mr. Cady			1422			
By Mr. Edgar			1428			

EXHIBITS:	FOR IDENTIFICATION	IN EVIDENCE
Licensee's Exhibits:		
No. 43	1427	
No. 44	1427(a)	1524
No. 45	1523	1524
No. 46	1523	1524

P R O C E E D I N G S

(9:00 a.m.)

JUDGE GROSSMAN: The seventh day of hearing in the General Electric show cause proceeding is now in session. We left off yesterday with Dr. Slemmons having completed his answer or discussion, and Dr. Brabb having indicated that he disagrees with some of that discussion. And I would like to comment now that I have noted that members of the panel are reluctant to allow their colleagues to say things that they don't agree with which they think impacts upon their professional integrity, and I think that is an admiral trait for experts to have, and we would encourage you not to sit and remain silent if your colleagues do offer an opinion that does not coincide with your own. We would like to have the full matter discussed from all points of view on the panel.

Mr. Swanson, do you have something?

MR. SWANSON: Yes. Two preliminary matters. One is dealing with the matter you just brought up. So that I don't forget it, I did want to indicate to the Board that we have copies of a signed letter from John Maxwell to William Ellsworth. We don't have sufficient copies to distribute to everyone, but if we could have this bound into the record everybody will

have access to it from the transcript of today's proceedings. It appears to be exactly the same letter, but with the signature at the bottom.

So at this time we would request the Board to have a signed copy of that letter, and in view of the fact that the document has been introduced into the record, that I would ask the Board to have a copy of the signed letter from John Maxwell to William Ellsworth bound into the record. I will provide a copy to the Reporter.

MR. EDGAR: No objection.

JUDGE FERGUSON: Mr. Swanson, I think that was done at my request, and I want to take this opportunity to thank you for doing that. Apparently the matter received more attention than I had intended it to receive.

My only concern, and I think you can understand this, is that documents, however they are introduced, if they are letters with names, it appears to me that signatures should be affixed. My concern is that this not be a matter of great weight, and if you would simply replace the unsigned letter with a signed letter, that would be perfectly all right with me. I do not want this to appear in the transcript as though it were a matter of great importance.

MR. SWANSON: I appreciate that. Preliminarily, let me pass the thanks on to Dr. Ellsworth since he is the one who obtained the copies that I do have. But of course the unsigned letter was distributed as a page in the transcript as issued thus far. I think short of trying to retrieve those, I think perhaps the only way we could get it into the record would now be to have it received and bound in as a page in today's transcript.

(Board conferring.)

JUDGE GROSSMAN: Mr. Swanson, now that you have brought the signed copy, and we are satisfied that it is authenticated, we will accept the unsigned copy appended to the prefiled testimony. I have forgotten whether that has already been admitted.

MR. SWANSON: Yes.

JUDGE GROSSMAN: It has. Then that will suffice.

MR. SWANSON: We do have the original, which I would propose then to just circulate among our counsel and the Board.

JUDGE GROSSMAN: We are satisfied that it is authentic. For whatever it is worth, the unsigned copy is part of that exhibit.

MR. SWANSON: Okay. Thank you.

JUDGE FERGUSON: Let me express my gratitude to you, Dr. Ellsworth, also, for whatever effort you put forward.

MR. SWANSON: Returning to the matter that the Board brought up as the last order of business yesterday, we did discuss the nature of the concern expressed by Dr. Brabb yesterday, and I think a brief history would be appropriate to set the matter in perspective.

The Staff's Safety Evaluation Report contained an analysis of data concerning offsets as a result of the San Fernando earthquake of 1971. It was based primarily on the data by the document by Barrows and others.

We asked for admissions from the other parties that the inclusions were correct. We received them, and we prepared a stipulation based on them.

Subsequent to that, we realized that another document was being prepared, that a review was being conducted which contained other data. It was a document which resulted in the publication of the document by Sharp which we distributed at the beginning of this hearing.

As a result of that preparation, I had requested that, despite the admissions we have, that the

other parties might feel more comfortable, as would we, if we added a caveat to the stipulation. It appears in stipulation (i). That caveat is that, we put in parenthesis, that "(The following items were based on data by Barrows, et al., 1973.)"

Inadvertently, that caveat was not applied to item (h). It should have been, because (h) was also based upon Barrows data.

The concern is that one might be able to draw somewhat different conclusions from Barrows than what one would from Sharp, based on the distribution of offsets across the zone, which was the zone of break in the San Fernando earthquake.

As a result of our further discussions, we have prepared and proposed an addition to stipulation (h). I have shown a copy of this to counsel for Intervenors and the Licensee, and have been told that they have no problem adding this caveat.

I would then propose to the Board to read a statement which adds on to stipulation (h), which I believe accurately reflects the concensus of the panel and accurately portrays the manner of describing the distribution of breakage across the San Fernando Zone.

At this time, then, with the Board's permission, I would like to propose this addition to stipulation (h).

JUDGE GROSSMAN: Mr. Barlow, I would like to hear from you on that. Do you agree to the matters there?

MR. BARLOW: To stipulating to what he wants done?

JUDGE GROSSMAN: Yes.

MR. BARLOW: Could I have a minute to review it again, please?

MR. SWANSON: By the way, I indicated that I had shown it to counsel for the parties, and I was indicating Mr. Cady.

JUDGE GROSSMAN: I understand that, but I really think that the matter ought to be resolved to everyone's satisfaction. We don't care to have something on the record that may be somewhat inaccurate just because counsel have agreed to it.

MR. EDGAR: Mr. Chairman, if I could make a suggestion, I don't think I have the problem that I wasn't aware that it was going to be a stipulation. I don't think I have a big problem on the statement. I would just as soon defer this, and we could talk about it among the three of us.

MR. SWANSON: Well, perhaps I should withdraw the offer of adding it as a stipulation; but I would like to read the statement as a statement on

behalf of the panel, and the Board could of course verify with the panel that that indeed reflects their viewpoint.

JUDGE GROSSMAN: That sounds like the wisest procedure.

MR. EDGAR: I just feel a little bit disabled from going through a statement which must necessarily have had a lot of expert input. They don't use their words carelessly, and I can review it, but only to a limited extent. Presumably it is true. I have no problem with that.

MR. SWANSON: I will withdraw the offer.

MR. EDGAR: The panel could make the statement, and then explain it, and that would probably be helpful.

MR. SWANSON: Okay. I will withdraw the offer of amending the stipulation, and I will then propose to read a statement which I am told accurately reflects the concensus of the panel.

Whereupon,

PHILIP S. JUSTUS,

ROBERT E. JACKSON,

ROBERT H. MORRIS,

EARL E. BRABB,

DARRELL G. HERD,

JAMES F. DEVINE,

WILLIAM L. ELLSWORTH,

and

RAMAN PICHUMANI

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

MR. SWANSON: "Our review" -- this being the review of the panel -- "of the Sharp (1981) report results in a refinement of the earlier observations based on our analysis of Bonilla (1973) which could be interpreted to indicate that displacement during the 1971 San Fernando event was distributed uniformly across a zone 200 meters wide. This new treatment in the Sharp paper indicates that the majority of the displacement probably took place over a narrower zone commonly 5 meters or less." That is the statement.

Perhaps I should ask the panel if there is anyone on the panel who disagrees with that statement?

WITNESS BRABB: Since I was the instigator of the difficulty, I will say that I agree both with the statement and with the counsel's characterization of my concern and the reason for the statement.

MR. SWANSON: May I take the silence from the other panel members to indicate that they also agree with the statement?

(Panel members nod in the affirmative.)

JUDGE GROSSMAN: It is the Board's understanding that all the panelists agree with the statement.

Mr. Barlow, you did ask a question, and you did have an answer from Dr. Slemmons, and you do have the opportunity to pursue the matter further if you want, now. Do you want to pursue that somewhab further?

MR. BARLOW: Yes, sir. And I would like to clarify that I am in no way questioning the stipulations which we agreed to. I am, rather, examining the discrepancies between the 1979 Staff SER input and the 1980 SER.

JUDGE GROSSMAN: Fine. With the basis of the testimony by Dr. Slemmons yesterday, and the new statement by the panel which the panel endorses, you may proceed further, Mr. Barlow.

Dr. Brabb?

WITNESS BRABB: Excuse me. Before we begin,

your Honor, I have a question about procedure that you had discussed just prior to this. I want to make sure that I understand it.

Most of the conversations that we have had among ourselves are to try and refresh our memories, or to clarify some information regarding a great mass of material that we are trying to remember. Therefore, it has been very helpful to us.

Did you mean by your comments to try and prevent this type of conversation among ourselves?

JUDGE GROSSMAN: Not at all, but we do not want you to sacrifice any part of your professional opinion in order to arrive at a concensus on the panel.

WITNESS BRABB: Thank you. To the best of my knowledge, that has certainly not been done with respect to myself, and I doubt with my colleagues.

JUDGE GROSSMAN: Thank you.

Dr. Barlow?

CROSS-EXAMINATION (resumed)

BY MR. BARLOW:

Q Dr. Brabb, at the close of the session yesterday we were discussing with Dr. Slemmons the characteristics of the San Fernando earthquake, and other concerns regarding surface faulting at the GETR site. You noted that you had a disagreement with the

way things had been presented, and I could not help but notice after that that you were the subject of a lot of discussion amongst your colleagues on the panel.

I would like to ask you to explain in detail what your disagreements were; and also to explain to me whether or not since 5:00 o'clock yesterday you were pressured by the NRC Staff, or anyone else on the panel, to bring your opinion into line with the current staff position in the 1980 SER.

A. (Witness Brabb) Mr. Barlow, there are several questions involved in that. Can we take them one at a time?

Q. Certainly. Could you tell me, first -- or could you describe for us the communications which resulted from your disagreement yesterday after the close of the session at 5:00 o'clock. Were you subjected to any sort of pressure by the NRC Staff, or by the USGS administrators regarding your disagreement?

MR. SULLIVAN: We have multiple questions, once again. I think if we want to get this matter clearly explained on the record, we are going to have to make sure that the answers are responsive to the questions.

JUDGE GROSSMAN: Dr. Brabb, could you handle the question in the form that it is in now?

WITNESS BRABB: I think I can handle at least part of the question in the form that it was asked, so I will try to respond to that. And perhaps if that is not satisfactory, he can follow up with an additional question.

JUDGE GROSSMAN: That is fine.

WITNESS BRABB: I will respond specifically to the question of whether I was pressured by NRC Staff or my administrators to modify my views.

My colleagues who are sitting at this table, and the other colleagues at Menlo Park, know that of all of the people in that Center who is least adaptable to pressure, I am one of them. And neither the people from the USGS administrative staff, nor the NRC people have been pressuring me or tempting me to change my opinion, or modify my testimony to conform to any overall views.

I am speaking here today from a conviction. My opinion will come out regardless of official positions on anyone's side.

BY MR. BARLOW:

Q. If I might explore this a little, my question was not whether you gave in to the pressure, but whether you were asked to bring your opinion or disagreement more into line or conformity with the position of the other

members of the panel.

A. (Witness Brabb) The answer to that is "no."

Q. Now, Dr. Brabb, could you explain to us in detail the form and details of disagreement which you had yesterday which you raised as a concern?

A. Well, I think counsel has described that in general terms. That is to say, I am -- was uneasy with the characterization of the displacement of San Fernando as being equally distributed over a wide zone, when the evidence from Dr. Sharp's report clearly indicates that it was concentrated in a fairly narrow zone.

The consequences of this of course are, in the one case, to indicate that the amount of movement that could be predicted from this would be relatively small and diffuse over a large area, in contrast to relatively large in a small area.

A. (Witness Jackson) Mr. Barlow, I would like to add just something to that. I agree with Dr. Brabb in his concern yesterday that it should be raised. We were very well aware in the issuance of the Staff SER that we were dealing with a data set which was not the best data set, because we were looking at vertical offsets.

In using the Bonilla paper of 1973 to represent a zone of movement, the purpose of putting

that aspect in the SER was to indicate to the parties in the proceeding, and I guess the Board in the end, that when you are using fault data in measurements of fault displacement which are represented often on figures as maximum, that you need a sensitivity to how those measurements are made.

Now as a geologist, we all know how they're made and we have an appreciation for them. Dr. Brabb was absolutely correct in the way the term was written. It could mislead to indicate a uniform distribution across this 200-meter zone.

The figure, Figure 4 of the paper, if read by a competent geologist, would be able to infer -- conclude by reading that that the dominant amount of displacement did take place in a narrower zone than the 200 meters.

So the way it is written was misleading, and I agree with the modification fully.

MR. EDGAR: Could I have one clarification? I think Dr. Jackson used a reference in your remarks to "Bonilla." Were you referring to Barrows?

WITNESS JACKSON: Please let me look it up. It is important.

(Pause.)

The report is entitled "Surface Faulting by

U.S. Geological Survey Staff." It has a number of authors -- it looks like approximately 10 -- the first author is Bonilla. They are referenced by a footnote. It is contained in a preliminary report published by the U.S. Geological Survey and NOAA, USGS Professional Paper 733. My reference is to page 63, Figure 4.

WITNESS JUSTUS: If I could add, there may be a little confusion about the use of -- or the reference, Bonilla '71, or Bonilla '73. There is another Bonilla paper that perhaps should also be referenced here. There is a similar statement in it.

It is a paper by M. G. Bonilla entitled "Trench Exposures Across Surface Fault Ruptures Associated with the San Fernando Earthquake." It is a 1973 document.

WITNESS JACKSON: We all agree on the panel, and it is important to note, that the Sharp data is a more direct measurement and was done more for the purposes of the kind of discussion we are having here with regard to GETR.

BY MR. BARLOW:

Q Dr. Brabb, if we can all agree that Sharp's report indicates that the displacement probably took place over a narrow zone commonly 5 meters or less, and we attempt to apply this new information to the GETR

site and the Verona Fault Zone, in your opinion is it possible that within the Verona Fault Zone that displacements on a single strand or branch of the Verona Fault Zone could cover a width of 5 meters or less?

A. (Witness Brabb) I haven't considered that possibility before. May I confer with my colleagues before I respond?

Q. Certainly.

(Witness panel confers.)

A. My colleagues say they don't understand the question. I have to admit that I don't really, either. So I wonder if you can use slightly different terminology to express the same thought for us?

Q. Perhaps I could simplify the question.

JUDGE GROSSMAN: Before you do that, we are having a little equipment problem, and we would like to take a five-minute break.

(Recess.)

end JWB #1

JUDGE GROSSMAN: Mr. Barlow, you were ready to ask a question. Proceed.

MR. BARLOW: Yes.

BY MR. BARLOW:

Q Dr. Brabb, my question is, could displacements during a single event on the Verona thrust fault zone occur within a width of five meters or less?

A (Witness Brabb) Yes.

Q Would this displacement in this narrow zone be associated with movement on a single strand of the Verona Fault zone?

A Would you repeat the question again, please?

Q Perhaps I should change the word from "would" to "could."

Could this displacement within a narrow zone be associated with movement on a single strand of the Verona Fault zone?

A Yes.

Q Okay. Thank you.

I would like to get into a discussion of the discrepancies between the 1979 SER input and the 1980 SER in the sections entitled "Current Staff Position."

A (Witness Jackson) Could I interrupt you for a minute? I would like to add to Mr. Brabb's last response. I need to make one measurement.

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Q Okay. Perhaps while you are calculating that, I could --

(Panel conferring.)

A (Witness Jackson) Fine. That's not what I thought it was.

Q I do have one more question along that line for Dr. Brabb.

Dr. Brabb, in the hypothetical situation that we are discussing, could the displacement within a narrow zone of five meters or less occur on a strand of the Verona Fault zone that could pass directly beneath the reactor?

A (Witness Brabb) Would you please state the question again?

Q Certainly.

Could the displacements during a single event on the Verona Fault zone, within a narrow width or zone of five meters or less, occur on a strand of the Verona Fault zone directly beneath the GETR reactor?

A I think the answer to that is theoretically yes. I don't think it has been established that there is a splay or strand of the Verona beneath the reactor, but that is certainly one of the possibilities.

Q Dr. Brabb, I would like to ask your opinion of the statement that is found in the 1979 SER input in the section entitled "Current Staff Position," page 8.

Do you have that document available?

A This is the 1980 SER?

Q No, the prior one, 1979.

A (Witness Jackson) Could you repeat the page numbers?

Q Page 8.

A (Witness Brabb) I don't have the reference.

JUDGE GROSSMAN: Excuse me. Let me just say we don't have that SER in evidence, Mr. Cady. Are you prepared to distribute copies to the Board and offer that document in evidence? I have a blank page for page 8 in the copy that is already admitted into evidence, and I think it would facilitate matters if you would offer your exhibit.

MR. CADY: Your Honor, because of logistic problems and the copies that I received yesterday, the copies were deformed in certain matters, and I'm getting copies done this morning, and they will be delivered this afternoon containing the '79 SER with the conclusions included.

JUDGE GROSSMAN: Okay. Thank you. Proceed, Mr. Barlow.

WITNESS BRABB: I believe I have it in my briefcase, if you wish to wait a minute or two. I will attempt to retrieve it.

MR. BARLOW: I can wait. Are there any copies

amongst the panel?

WITNESS BRABB: It's just been found. Page 8, is it?

BY MR. BARLOW:

Q Yes, page 8, number 4. Could you read the text of the section entitled No. 4 on page 8?

A (Witness Brabb) "Although future displacements have a higher likelihood of occurring along the existing fault breaks, rather than between them, this likelihood cannot be quantified for the Verona Fault zone based on current geologic observations. The concept and study of new faulting (initiation of new fault breaks) is in an infantile stage. Observations to date indicate the paths of surface displacement within and adjacent to fault zones during earthquake events are not predictable."

Q Thank you.

Based on your analysis of that statement, could you give us your opinion on whether or not a future surface displacement of the Verona Fault zone could surface directly beneath the position of the GETR reactor within that zone?

A The question is could? Is it theoretically possible?

Q Yes. Perhaps it would help if I preface that question with another question.

Is it possible in a thrust fault zone in California that a new surface displacement or surface break could occur anywhere within the fault zone?

A I think the answer to that is yes.

Q Okay.

A All faults are new at some time, and therefore certainly new breaks within the zone can be anticipated; at some time over a geologically long period of time, additional breaks may develop.

Q Okay. Within the Verona Fault zone, is it possible that a future surface displacement could occur directly beneath the site where the GETR reactor is located?

A I don't think there's any disagreement that that is indeed possible. The difficulty is trying to quantify that and how likely it is.

Q Within the statement which you just read from the 1979 SER, it reads:

> "The concept and study of new faulting (initiation of new fault breaks) is in an infantile stage, and observations to date indicate that paths of surface displacement within and adjacent to fault zones during earthquake events are not predictable."

Now, do you know whether or not since 1979, there has been a significant increase in the understanding

of this phenomenon or is this,the study of new faulting or initiation of new fault breaks, still in an infantile stage in 1981?

A I would like to defer to some of my colleagues. They may be in a better position to answer this question than I am. I don't know the answer to that.

Does anyone else want to struggle with it?

A (Witness Jackson) Since I wrote the sentence that's included in the SER, I'd say our data base has not increased in any significant way. I think as a result of some of the studies we have done for this site, and thinking that we have done, and discussions we have had among ourselves and the USGS, a lot more thought has gone into this problem in the past two years than previously. But I would still say the statement holds true.

A (Witness Devine) I'd like to add a comment on that, if I may.

JUDGE GROSSMAN: Certainly.

WITNESS DEVINE: I don't think we ought to confuse two different things here, and I believe there is, at least from what I hear in the questioning, and that is the study of how new faults are created or propagated is in an infantile stage. But that is a separate point from the fact that we have observed the occurrence of faulting in thousands and thousands of places, and consequently have

made previous statements about the likelihood of movement occurring on existing faults, rather than generating a new one.

Those are two separate thoughts, and one does not relate to the other one directly. So the fact that the study of how new ones are generated is in the infantile stage does not detract from what we have observed on existing faults.

BY MR. BARLOW:

Q I have a follow-up question for both Dr. Jackson and Dr. Devine regarding the latter part of the statement in the section that we are looking at, in which it says that:

> "Observations to date indicate that paths of surface displacements within and adjacent to fault zones during earthquake events are not predictable."

Dr. Jackson, can you tell me why that statement and this entire last sentence of this section from the 1979 SER was eliminated from the 1980 SER?

A (Witness Jackson) You have to ask my attorney that. But to amplify -- I'm not trying to be cute -- the reason why it was deleted is I think this statement was made prior to the probability studies being done, in terms of decision as to the likelihood of a new fault being

generated between the two existing faults. We wrestled with this problem with great difficulty both in '79 and '80. The major difference, the most significant difference was the probabilistic studies that were done, plus a consideration of relooking at the information we have had previously, taking a hard look at it.

Q Am I correct in my interpretation that in 1979, the Staff stated that the path of future displacements within a fault zone was not predictable, and yet in 1980, you have changed that position to accept the probability studies by the Licensee, and other probability studies which do predict the paths of future surface displacement within the Verona Fault zone?

A It's a very long question. I'm not sure I have all the elements of it. I don't think the probability studies in any way predict one way or the other what is going to happen. They give a frame of reference or a numerical frame of reference to assist a geologist in his judgment as to quantifying what that likelihood is.

Our advisors, in studying the probabilistic studies -- the basic conclusion, and they will have to confirm that in the panel that they're on -- was that because of uncertainties in the method, the probability calculations indicated that you didn't have to consider surface offset. It is such a low probability that you don't

have to factor in that as, let's say, a design basis event.

However, because of the uncertainties in the process of calculating it, in the judgment of all the geologic personnel who were involved in this site, I think there were seven geologists that we polled at one point in time, it was our judgment that surface faulting must be considered under this plant, and so the change there was not whether or not it would occur, but the likelihood of a certain amount occurring.

I caution against the use of prediction. Prediction is not a proper term to use. We are estimating -- prediction has a time-space relationship which we are not trying to accomplish here.

Q I'm having a hard time understanding the difference between the word in the 1979 SER, "predictable," and the use of probability studies to estimate a number and to predict or -- it's my understanding that the probability studies are predicting the future behavior of the Verona Fault zone.

Could you explain to me if you have a different understanding how the probability studies are not predicting future behavior in that fault zone?

A I'll defer to the probability experts as to how they use the term "predict." When I was making this

statement, it was based on solely geology and no consideration of probability estimates.

Q Can you attempt to explain to me why this part of the 1979 SER was left out of the 1980 version?

A It doesn't matter to me if it were in the '80 version. I did not know when we wrote the 1980 version we referenced the entire document. As far as I understand, this whole document was appended to it at that point in time.

JUDGE GROSSMAN: Dr. Jackson, are you saying that the sentence that we are discussing now would have been just as valid in the 1980 SER as it was in the 1979, and that you as a geologist have not changed your opinion with regard to that sentence?

WITNESS JACKSON: No. I think I -- let me answer it. I think there were two questions there.

I still endorse this statement with the caveat that when that statement was made, it was based on no consideration or knowledge of the probability, and I think I could explain that.

You know, as geologists, we think we know everything about what's going on out there, or we at least know our limitations. The interesting thing about dealing with the probability experts is they often tell us that some of the things we think are very important are really

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not very significant to the likelihood of something occurring or not occurring.

It's a painful experience for a geologist, because we earn our living doing this. But, on the serious side, it indicates to me, and it was, as we went through the process, somewhat of an education as to how significant some of the parameters we were looking at were.

So I think it should be modified. I think this is a fair statement of the state of the art in the geosciences without consideration of probability, and I don't -- does that answer what you were saying?

JUDGE GROSSMAN: My understanding of what you have just said is that as far as your position goes, the sentence is still valid, and you haven't changed your opinion, but you are willing to defer to the probabilistic experts as to whether that sentence ought to be in there or not; is that a fair statement?

WITNESS JACKSON: I thin it's a fair statement, yes.

BY MR. BARLOW:

Q Dr. Jackson, if geologists and geoscience experts cannot predict the path of future surface displacements within a fault zone during earthquake events, can you explain to me how statisticians could do that? Do they have something -- do they know something that you don't?

A (Witness Jackson) I think they do know a tremendous amount that we don't know, and in fact, as I just explained earlier, they can look at the parameters and input assumptions with sensitivity studies as to how they impact those probability studies, and they certainly can make a vast contribution.

It may be a good time to comment on the use of probability. As Branch Chief of the Geosciences Branch, we have attempted to institute the use of probability in an extremely cautious way. Probability is a fantastic tool when used properly. The problem with probability and using it in the geosciences area is that it must be used with great caution and insight, and not used to totally displace the deterministic or more empirical type methods. The fear that we have in types of implementing is that people run off with the numbers and begin to use them as if they are cast in concrete, without the caveats that all probability experts attach to them.

So it's used more in a weighting context. It's used in assisting in making a judgment as to where you ought to be in terms of a distribution of -- a distribution of use on a particular topic.

I haven't been in a scientific discussion with this panel or any other geologic group in which there was not a range of opinion, and probabilistic methods are one way

to deal with that range of opinion.

A (Witness Devine) I'd like to make a comment on that, since my not saying anything implies that I agree with both the question and the answer.

I disagree with the implication of the question, and that is you indicate that geologists cannot predict ground breakage and statisticians can, and I disagree with that.

Dr. Jackson answered the previous question that statisticians do not predict ground breakage, and he did not repeat that in his answer, and I would like to have that included in the answer. They do not predict any more than we do. They do assess the probability of a phenomenon occurring, and that's what he's referring to.

A (Witness Jackson) I'd like to expand, if I could. A probability estimate does not rule out something happening.

Q Okay. In other words, even if the probability is one in a million, it still could happen?

A That's true. The probability that you will get run over by an automobile crossing Van Ness has a certain number, and hopefully that won't be exceeded, you know, by most of us.

(Laughter.)

Any of us. Or the sun rising. There is a certain

probability the sun won't rise on a given day. So it clearly should not be interpreted that that's a number that's being attached to when the next time something will occur. I just want to make sure they understand.

Q In your answer, you said there are a number of critical parameters. Would you agree that in the site-specific GETR situation, that one of the most critical parameters in terms of the validity of a probability study would be whether or not there is an existing shear beneath the reactor?

A The most accurate answer I can give is I'm not sure.

Q Okay.

A You asked me a similar question a day ago or day before yesterday in which I said I would defer to the probability panel. I do not know all of the -- I do not recall all of the aspects which are critical to the assumptions. You'd have to ask them, or Dr. Slemmons.

JUDGE GROSSMAN: Excuse me. I would like the views of Drs. Brabb and Herd with regard to that sentence, if you do have any view on that. And also, as a follow-up, whether you believe probabilistic studies would influence your position with regard to that sentence.

Dr. Brabb?

WITNESS BRABB: Yes. At this stage I think there

has been sufficient discussion in between that we don't have the question clearly in mind. Can we have a recasting of the question?

JUDGE GROSSMAN: Could you repeat that sentence that was in the 1979 report? That's what I would like to ask Dr. Brabb and Dr. Herd's views on.

MR. BARLOW: The sentence is:

"The concept and study of new faulting (iniatiation of new fault breaks) is in an infantile stage, and observations to date indicate that paths of surface displacement within and adjacent to fault zones during earthquake events are not predictable."

WITNESS BRABB: I thought that was then followed by another question having to do with the predictability of the fault occurring beneath the GETR, and it was that latter question that we were focusing on, and the one that I would like to have repeated.

MR. BARLOW: The question I asked Dr. Jackson in the most recent stage of questioning was whether or not he agreed that on a site-specific basis at that GETR site, that one of the critical parameters, geologic parameters, as input into the probabilistic studies, is whether or not there is an existing shear beneath the reactor site. Is that what you were --

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JUDGE GROSSMAN: Excuse me. I would prefer that we go back to the first question. I'm interested, Dr. Brabb and Dr. Herd, on your views with regard to that sentence in the SER as to whether you agree with that sentence, first of all.

WITNESS BRABB: I agree with the sentence in that how new faults are initiated is not well known on a world-wide basis.

Therefore, I think this statement is accurate, at least at that point. I am not sure I disagree with the second part of it, because there is an inference here that the situation is hopeless.

I think that in the site-specific case in GETR, for example, there are a number of geologic observations that would lead us to conclude that movement is more likely in certain specified spots than other spots, and therefore these future earthquake events in that sense are predictable, in that we think that most of the movement is likely to take place on the shears that are already seen in the trenches in the vicinity of GETR.

So I would agree with the first part, but not the second.

end2

JUDGE GROSSMAN: Dr. Herd?

WITNESS HERD: Thank you.

I concur that the study of the concept of new faulting is in an early stage in terms of the science, but I certainly would take a point of diversion from the last part of the sentence. As far as I know, neither Earl nor I had any input into the sentence.

If I may, there was an open-file report prepared by Manuel Bonilla in 1979 entitled "Historic Surface Faulting Map Patterns in Relation to Subsurface Faulting in Relation to Pre-existing Faults" which was presented in part of a conference on analysis of actual fault zones in bedrock.

In that, Manuel Bonilla talked about a number of instances where that there were -- I think they're something of the order of several hundred, more than a hundred surface faulting events that he has studied. And in that, as well as in personal discussions with Manuel Bonilla subsequently, I have gained the impression that most of all the displacements that have been seen historically have recurred on faults that have already existed in the material in which the rupture occurs, such that I get the impression and I see some strong data to support, that most of all the displacements that have been observed historically have occurred

in pre-existing fault zones.

So the idea that it is not predictable in the sense that it is not going to recur on an existing fault does not seem to be in agreement with the worldwide surface faulting data.

JUDGE GROSSMAN: Can either of you quantify the predictability? Or is that still in an infantile stage?

WITNESS HERD: Well, for myself I am not a statistician and I would prefer not to attempt such a thing. I think Dr. Slemmons, who was among our panel yesterday who is, like Manuel Bonilla, an authority on worldwide surface faulting data, would be the person best qualified and prepared to answer, or even attempt it -- and I am not sure that they would.

JUDGE GROSSMAN: Well, my question is to you as a geologist, whether you believe that that can be predicted by statisticians, or that you can quantify the probabilities; not whether you can do it yourself.

WITNESS BRABB: I believe it can be quantified, your Honor, and I think that the data suggests that the number would be very high. I would hesitate to say 90 percent as opposed to 70 percent, but the predictability would be fairly high.

Number one, yes, it would be possible to

quantify this. I believe that it has been done, and I believe that that number is fairly high.

BY MR. BARLOW:

Q. Dr. Herd, in your response to the Board you said that future surface displacements are most likely to occur within existing fault zones. Is that correct?

A. Have "apparently occurred" in existing fault zones, as far as I am aware from the historical record.

Q. Applying that to the GETR site, would we expect future surface displacements to occur within the Verona Fault Zone?

(Witnesses confer.)

A. My colleague has pointed out a point of nomenclature of "zone" versus "trace." Would you restate your question?

Q. Yes. Applying this to the GETR site, would we expect future surface displacements to occur within the Verona Fault Zone?

A. Well, I think it is logical that if there is a faulting event on the Verona Fault Zone it is going to occur in the Verona Fault Zone. The question is how it is distributed on certain breaks.

Q. Okay. Within the Verona Fault Zone there are a number of shears. I would like to ask you two

questions based on quotes from your deposition of March 25th, 1981, if you could turn to page 172 through 174.

(Pause.)

A. Once again, I would like a moment to look at the context of the statement that you wish me to refer to. Now on page 172, where would you wish me to look at?

Q. I am beginning on line 6 there, and continuing through line 16 on page 172, if you could take a moment to read it.

(Pause.)

A. Okay. I have had a moment to look at that. Your question?

Q. Thank you. On page 172, line 6, the question is: "Q. Are you aware of any lineaments that run through the reactor foundation?"

Your answer is: "A. I am unaware of any, but we did not make a systematic search on our part for those."

Does that mean that the USGS was not asked to make a systematic search for lineaments beneath the reactor foundation, or lineaments that might run through the reactor foundation?

A. No. Let's be clear in terms of the context of this question and my answer. I pursued an independent

investigation of the Livermore Valley which had no association with the Nuclear Regulatory Commission or General Electric. It was done strictly as an attempt to document faulting that was of recent age in the Livermore Valley area.

The question, as far as I understand it, is in reference to lineaments or aligned features in the topography which my colleagues, I believe it was from the Nuclear Regulatory Commission, had noticed in looking at some aerial photographs which led them to suggest that certain trenches at the GFTR site be extended, if I remember the context of the question correctly.

In the course of my investigation prior to that time, I had looked at aerial photographs of the site area and had not seen those sorts of features which my colleagues subsequently did. As a consequence, I felt that the question was one with reference to these features that they were talking about. I hadn't seen those, but I wasn't looking for the types of things that they were. What I was after was physical geomorphic evidence which would suggest a discontinuity that could be attributed to faulting.

Q Was that what you means by the statement that you "did not make a systematic search" --

A. Could I just interrupt you a second?

Q. Sure.

A. I was trying to provide a regional synthesis and understanding, not a detail. I was not out there to do a site geology of the GETR site.

Q. So by "systematic search," you meant a site-specific detailed study?

A. That's correct. I was not out to do a site-specific study. This was part of, and remains, a study that I did and am continuing to do over the entire Central Coast Ranges of California.

Q. Thank you.

A. (Witness Jackson) I would like to add to that, Mr. Barlow, if I might.

The context of the identification of potential lineas across the site was done -- I guess I might have been the first one to notice it; I'm not sure whether the USGS brought it to my attention, or vice versa -- but in doing so, it means sitting and looking at photographs, and these were I think RB-57 high-altitude photographs, and some lower altitude photographs, that we identified these features.

We sat and discussed these in several meetings, if I recall, as to wax pencil lines that I had drawn on that. Now I guess there are -- and I'm

not an expert in linear analysis, lineament analysis, but I imagine there are systematic ways of doing it.

The purpose here was to look and see if we could see anything, and we were just looking at the photographs. So in that regard, I did do a search of those photographs in the most immediate proximity to the GETR site.

Q Thank you.

Dr. Herd, on page 174 of your deposition starting with line 3, if I could quote your response, you said: "A. As far as we know, the GETR lies within that zone of faulting. In faults of such complexity, there are typically other breaks that comprise that zone. Since this lies between well-known identified breaks, there is good reason to suspect that we might have other faults. In other words, there is greater likelihood of other faults in that zone, as opposed to outside of the fault zone. So in the idea of speculation, there is, I believe, some legitimate geologic reason to be concerned that there might be other faults in that particular sector, as opposed to outside of it."

Can this be interpreted to mean that there could be faults within the Verona Fault Zone that would surface beneath the reactor itself?

MR. EDGAR: I would like to have the complete

quotation read into the record. The last sentence of Mr. Brabb's -- or Dr. Brabb's statement is missing. It provides context for that statement, why he was answering the question.

BY MR. BARLOW:

Q. The last sentence of the quote is: "If only just to qualify the word 'speculate,' it isn't wild speculation. There are some reasons to be concerned about the existence of other faults."

Can this be interpreted to mean that there could be faults within the Verona Fault Zone that would --

A. (Witness Herd) Just a second. We want to establish to whom the -- from whom the answer came.

Q. Oh. Is it possible it was not you?

A. Because Mr. Edgar indicated Dr. Brabb.

MR. EDGAR: I may have misspoke that. My concern was with the completeness of the quotation, not with -- I see the problem now.

WITNESS BRABB: I don't think it makes any difference. If it was Dr. Herd, I concur in the statement and would say, in response to the question: Yes, it is possible.

BY MR. BARLOW:

Q. Thank you.

A. (Witness Jackson) Dr. Barlow -- Mr. Barlow, before I leave this line of questioning, I had glanced back at Mr. Vestlie's testimony to refresh my memory relating to probability of faulting with an existing shear present. And from Section B of his testimony, he states that -- it is in the SER, Appendix Section B. I don't know whether I should read the whole paragraph, but I guess I will:

"Based on sensitivity analyses and the alternative model, the probability of a surface offset occurring beneath the reactor building has been shown to lie between 1×10^{-6} per year and 1×10^{-5} per year (to order of magnitude precision). The highest surface rupture offset probabilities calculated were 1×10^{-4} per year and corresponded to assuming values for parameters at the conservative end of their range." He goes on to state that "an undiscovered shear under the reactor building could give a surface rupture probability of 1×10^{-4} per year if T star equals 8000 years is used as the period in which no offsets have been observed. Values of T star greater than 40,000 years would again give probabilities less than 1×10^{-5} per year."

So he's -- in reading this, it appears to me that he has said you can modify your probabilities based

on your observations or certain assumptions about whether or not a surface -- an undiscovered shear exists or doesn't exist.

Q Dr. Jackson, would you --

JUDGE GROSSMAN: Excuse me. If there was more than one undiscovered shear, would that also lower the -- excuse me, or raise the probability even more?

A WITNESS JACKSON: I have no idea. You would have to ask Dr. Vestlie that.

BY MR. BARLOW:

Q Dr. Jackson, would you agree that the probability experts depend on geologic input as to parameters and assumptions that they use?

A (Witness Jackson) That is my general understanding, yes, to some degree.

Q In the quote that you quoted there from Dr. Vestlie, if I could have you reread the sentence which includes "if T star was 8000 years"? Could you read that sentence?

A "An undiscovered shear under the reactor building could also give a surface rupture probability of 1×10^{-4} per year if T star equal 8000 years is used as the period in which no offsets have been observed. Values of T star greater than 40,000 years would again give probabilities less than 1×10^{-5} per year."

I didn't define "T star." I assume it is in here somewhere.

Q. The clause, "if T star equals 8000 years for the period during which no surface offsets occurred," is that one of the geologic input parameters or assumptions that is used in a probability study?

A. On page 7 of the same section, Dr. Vestlie indicates that T star is the age of soil beneath the reactor building. In the column above that on page 7, he indicates a series of T stars of 40,000, 62,000, 128,000, 160,000, 195,000. I prefer to delete what he did with these and leave those to him.

They are geologic parameters, obviously.

Q. Right. This geologic parameter characterized in that quote that you sited of 8000 years since last surface offset, am I correct in understanding that characterization of that?

A. I didn't --

Q. Could you read that phrase again about 8000 years, just that part?

A. "T star is the age of soil beneath the reactor building".

Q. The part that has the words "8000 years" in it?

A. "An undiscovered shear under the reactor

building could also give a surface rupture probability of 1 x 10^{-4} per year if T star equals 8000 years is used as the period in which no offsets have been observed."

Q The "period in which" --

A. And it goes on: "Values of T star greater than 40,000 years would again give probabilities less than . x 10^{-5}."

Q Okay. The phrase that I am interested in pursuing with Dr. Brabb and Dr. Herd are the words "period in which no offsets have been observed".

Dr. Brabb, would you agree that there have been no offsets in the Verona Fault Zone for the past 8000 years?

A. (Witness Brabb) No.

Q Could you characterize for us your opinion, or a range of numbers during which you believe the most recent offsets occurred in the Verona Fault Zone?

A. Our best estimate -- and this is based largely on the work of Dr. Herd, not myself -- is that we believe that movement could have occurred between, or since 2- to 4000 years ago, 2000 to 4000 years ago.

Q Either Dr. Brabb or Dr. Herd, is it possible that offset occurred 1500 years ago, or less than 2000 years ago? I saw some discrepancy in the documents I

read. Could you explain the discrepancy between 1500 years, 2000 years, and 4000 years in these estimates?

A. (Witness Herd) I don't believe 1500 is the number that the U.S. Geological Survey staff has provided you. I believe that number may come from Dr. Slemmons' article. I think the impact is the same. It is still less. We have, I believe, concurred as a group that the displacement is less than 4- to 2000 radiocarbon years.

A. (Witness Jackson) Dr. Slemmons -- we discussed this yesterday -- Dr. Slemmons felt he had taken that number from the USGS report, and he indicated yesterday he was in error.

I would point out, however, in the Safety Evaluation Report of 1980 on page 10 we indicated: "Based on these" -- let me go back one sentence, if I might.

"Dr. David B. Slemmons, an NRC consultant on fault evaluation indicates in his letter report attached as Appendix E to this report that he would place an error band for fault displacement in the soil between approximately 1500 to 2000 years to 4000 years before present for Trench B-1."

Now based on these recommendations, we conclude that offset of the youngest soil horizon could have

occurred within about the last 2000 years.

I would like to go on to add a comment that that is at the -- more closer to the ground surface than the reactor base would be. If I have the opportunity, I notice in responding to an earlier question, responding to a probability discussion, I was reading over a section of the Safety Evaluation Report on page 15 which may be misleading, and I wanted to make sure it was correct. Again, it is the May '80 study in which we are discussing the bottom paragraph, beginning with "deciding."

The sentence says: "Deciding the proper surface offset design basis for a facility within a fault zone by using the proposed probabilistic methods is not favored by any of the geological personnel involved in the review of this site."

That is an incorrect statement. It should -- I think it would be, "Deciding" should better be written inserting "the sole use of" between "using" and "the". I will read the sentence as I think it would better represent the way we are approaching this.

"Deciding the proper surface offset design basis for a facility within a fault zone by the sole use of the proposed probabilistic methods is not favored by any of the geologic personnel involved in the review

of this site."

In fact, based on our discussion, we favor the use of this with great caution.

JUDGE FOREMAN: I would like to ask a quick question -- and I don't know whether you can answer this or not, Dr. Jackson. Within the sensitivity of Dr. Vestlie's probabilistic analysis, is the method sensitive enough to distinguish differences between 2000 years and 8000 years? Or is that too small a difference to influence the 10^{-4} probability number?

(Witnesses confer.)

WITNESS JACKSON: I think it -- my understanding is that it is of -- let me answer it this way: It may be sensitive enough to assess it. I don't know the answer, and I think it would be better directed to Mr. Vestlie or Dr. Slemmons.

JUDGE FOREMAN: I suspected that.

BY MR. BARLOW:

Q Dr. Jackson, you just pointed out a revision to the May 1980 SER, and I am wondering if this is another modification of the Staff's position similar to what we have between the 1979 SER and the 1980 SER?

A (Witness Jackson) I don't understand the question.

Q Okay, I will rephrase it to put it in context.

The Staff issued in 1979 an SER input on geological and seismic design bases for the GETR. Then in 1980, the Staff issued another SER on these design bases which has been characterized as a "drastic reversal of position."

Now you are further modifying the May 1980 report to change what I consider an important part of it, which is: You quoted a sentence that the geological personnel involved in the review of this site did not favor the use of proposed probabilistic methods for establishing proper surface offset in the design basis.

Now you are saying -- or are you now further modifying that and changing that position?

JUDGE GROSSMAN: Dr. Brabb I think would like to respond to what has been done, and then I think Mr. Jackson can respond to the question.

WITNESS BRABB: No, sir. What I would like to do is ask that we take a break at this time. It has been a long time, and it sounds like a long, complicated question, and I would request a break at this time.

JUDGE GROSSMAN: All right. Let's take 10 minutes until 10:30.

(Recess.)

end JWB #3

JUDGE GROSSMAN: I've lost track of whether Mr. Barlow was going to ask a question or Dr. Jackson was going to answer one.

WITNESS JACKSON: I had a question before, and I'd like to answer it. I think I do recall it.

The air conditioning is turned on. Can you hear me?

JUDGE GROSSMAN: No. I thought there was some seismic event.

(Laughter.)

WITNESS JACKSON: To answer the question most directly, Mr. Barlow, the comment I was making is not a change in the Safety Evaluation Report. That requires some review by management and issuance by Mr. Denton or others.

What I was trying to point out is a clarification on that particular statement.

A second item, if I might add, is that earlier I had mentioned -- it relates to the topic area you are changing, changing conclusions. I may have inferred that it was a legal determination. It's not. From the time we issued the May -- the September '79 Safety Evaluation Report, and the May '80, we did a lot of detailed work. We thought about the determinations we had made previously. We went through either two or three Advisory Committee on Reactor Safeguard -- Advisory Committee on Reactor

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Safeguard meetings in which new information was provided, so that all contributed to our modification of conclusions.

I may not have taken every word from one and look at it in detail as to whether I changed that word, one on one between the two SERS. I think we did indicate where there were significant changes by an asterisk in the conclusions.

JUDGE GROSSMAN: Excuse me. In the sentence on page 15, in which you indicate that you may now have changed your position from when it was written --

WITNESS JACKSON: That's not true. When that was written, it was my opinion that the sole use -- it has to be obvious because we then went along to use probabilistic probability and weighing it in our judgment as to the amount of offset that ought to be specified as a design basis offset under the plant.

JUDGE GROSSMAN: Well, in any event, your position now is that you had intended that -- what you now change it to at the time it was written; nevertheless you spoke for a number of other people there, and I wanted to find out who these other geological personnel were that you referred to in the sentence.

WITNESS JACKSON: There were -- well, Dr. Herd, Dr. Brabb, Mr. Morris, Mr. Slemmons and Dr. Justus. So we could ask everyone what their opinion is.

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JUDGE GROSSMAN: Well, I intend to. I wanted to find out first whether you included the USGS in there, and apparently you did.

WITNESS JACKSON: This came from a discussion that took place at the one of the Advisory Committee meetings, where we were essentially being asked questions on the applicability of probability.

JUDGE GROSSMAN: And I did want to find out from Drs. Brabb and Herd whether they agreed with the original sentence which is the one on page 15, with the beginning paragraph there, the first sentence of that.

WITNESS BRABB: Deciding the proper surface offset?

JUDGE GROSSMAN: Yes.

WITNESS BRABB: May we have a moment just to study the information?

(Panel conferring.)

WITNESS JACKSON: While they look at that, I would comment, Dr. Slemmons was also involved in those considerations, and he should be asked later on.

JUDGE GROSSMAN: I hope,Drs. Brabb and Herd, you are referring to the original sentence and not to what it was changed to.

(Panel conferring.)

WITNESS BRABB: I don't think that's a fair

representation of my position. I think that it is helpful to have probabilistic determinations made. My concern was some of the probabilistic information was over the assumptions that they had made; the geologic assumptions were, I felt, unreasonable, that they did not include the possible expected events that we could foresee, and therefore I have checked into some of the earlier probabilistic studies.

I have not, in fact, looked at the later ones to make certain that all of my earlier concerns were met, but I have a general impression that the later probabilistic studies were more in tune with the possible geologic parameters. But I did not agree with the original --

(Panel conferring.)

WITNESS BRABB: I do favor using probabilistic methods as an assist to try and understand the expected probability of an event. My concern is that some of the probabilistic studies that were originally made, and geologic parameters as their assumptions, I felt were unreasonable.

JUDGE GROSSMAN: This sentence does, of course, refer to particular probabilistic methods that were used, and I take it you had reservations about those particular methods?

WITNESS BRABB: Yes, sir.

JUDGE GROSSMAN: Okay. Dr. Herd, do you agree

fully with the position that Dr. Brabb has stated now?

Rather than put you on the spot that way, if you have anything to add with regard to your own position, could you please do that, to the extent it might not have been fully explained by Dr. Brabb?

WITNESS HERD: Well, fortunately, in all truth I can say I can't remember the discussion that apparently went into the preparation of the statement. I do recall discussions that preceded the release of this or the discussion of this document. I guess this would have been in the spring of 1979 and other times about discussions on what would be the amount of displacement in the Verona Fault zone that we observed, and discussions about how they could be applied.

But, as I had said earlier, I am not a statistician and I tremble at the thought of trying to come up with some sort of a probabilistic assessment, and clearly would have had no input in terms of trying to calculate one, because it's not my specialty.

JUDGE GROSSMAN: Mr. Devine?

WITNESS DEVINE: Thank you.

I was involved, with Dr. Jackson, in helping prepare that thought, and my concern is the way this sentence was modified to say "by the sole use of," I agree with fully. The way it's written. I agree with Dr.

Brabb. The way it was originally written, that is an incorrect representation of my thought, the way it is written. But the way it's modified by "the sole use of," I agree with fully.

WITNESS MORRIS: I agree with the conclusions as stated by Dr. Brabb.

I'd also like to point out that Dr. Herd, due to other schedule conflicts, did not attend one of the ACRS meetings, and I believe it was at that meeting that this subject was discussed.

WITNESS JUSTUS: I had some input into this, as well, and as stated originally concerning whether we favored this method or not, I agree with that. I did not favor this method over deterministic methods. That's the intent, I think, or the context of "favored" here.

Similarly, I don't favor the sole use of probabilistic methods for deciding design values.

JUDGE GROSSMAN: The problem here, of course, is that the way it's been changed, it really doesn't mean very much and, of course, anyone can endorse that, but anyone -- I find it hard to believe that anyone could disagree with that statement, and the only statement that really takes a firm position is the one that was in there, and I want to have your views as to whether you stand by it, and apparently you have modifications of what it said to begin with, and

that's all on the record.

You may proceed.

MR. BARLOW: Thank you.

BY MR. BARLOW:

Q Dr. Jackson, in the 1980 version of the Staff SER, No. 8, on page 6, it reads:

"Combined loads caused by fault offset at the surface and vibratory ground motion must be considered to act simultaneously, because there is no reasonable way to conservatively forecast the location of rupture initiation, the mode of rupture propagation, and the potential source area for radiated seismic energy, or the sequence of possible interaction among the Calaveras, Verona and the Las Placitas Faults."

Well, let me continue one sentence more:

"In the view of the above, there is insufficient evidence to support the proposition that strong ground motion and surface fault displacement will be separated in time."

A (Witness Jackson) Why don't you read the rest of the conclusion? Or I will.

Q Okay, sir.

"Although we recognize that the entire

one meter of displacement noted in 5 above will probably not occur co-seismically during the time of the earthquake, we are unable to quantify what portion of that earthquake will occur during strong shaking. We recommend further consideration of this aspect by the structural engineers and their consultants, depending upon the critical significance of this observation to the structural evaluation."

Now, here it seems that we are entering one of the areas where there is an interface between the geologists, the geosciences branch of the Staff, and their consultants from the USGS and elsewhere, and the structural engineers and the probability experts, and it becomes a rather critical factor when the engineers and the probability people deperd on certain geologic or seismic inputs, assumptions or parameters which are to be provided by your group and your consultants and the USGS.

Can you explain to me the wording in this quote:

". . .because there is no reasonable way to conservatively forecast the location of rupture initiation," et cetera?

If there is no reasonable way to conservatively forecast all of these geologic and seismic parameters in the

interaction of the Calaveras, Verona and Las Placitas Faults on a site-specific basis, how can you give -- or what level of confidence would you ascribe to the geologic assumptions and input that you give to the structural engineers and probability experts? Doesn't that weaken their arguments?

A (Witness Jackson) You have asked about five different questions. I'm not sure how to respond. I'd ask you to break it up into individual questions.

Q Okay. Do you agree, as I assume you do, that as it says in the SER, there is no reasonable way to conservatively forecast these geologic and seismic parameters?

A That's not what that says.. Is that the question? The question is no.

Q Can you explain to us what it is that there is no reasonable way to conservatively forecast?

A The statement here is essentially -- I think you may be misreading it. The statement here goes toward the question of the initiation of rupture, which is at the place of the location of the hypocenter of the earthquake. This doesn't relate to the displacement or rupture initiation of the ground surface.

The intent of this is to say -- the question we're talking about here is when does the ground shaking

wave caused by the earthquake hit the plant, as compared to when the actual surface rupture intersects the plant? The two move in different velocities. The ground wave vs. the actual movement of the rupture plane itself. Because you don't know exactly where the hypocenter of the earthquake is going to be, it is then difficult to calculate the time it takes for the earthquake ground wave, ground motion to reach the plant, as compared to the rupture reaching the plant, the actual movement of the plant. And the context of this sentence, in conclusion, was toward that end. And the way you handle it, or the engineers handle it, is by my telling them that they should treat them both at the same time, because you can't have it worse, but with recognition that there are other options where further work could be done if it becomes a very critical parameter.

And we, as a matter of interest -- I work in the Geosciences Branch. There are four branches in that, four branches under the same assistant director, which combine the disciplines, which are called Assistant Director for Structures & Components. We interact very closely with the structural engineers. We communicate to make sure that they are not misusing what we are giving them as input, as best we can.

Q Based on your explanation, I would like to refer you to the 1979 SER input, and specifically the cover letter

ar4-11

from Harold Denton and Edson Case to General Electric, if I could quote for you.

A Is this the September 27, 1979?

Q Yes, that is one cover letter, and then there is another cover letter of transmittal which is a memorandum for Chairman Hendrie, Commissioners Gilinsky, Kennedy, Bradford and Ahearne from Harold Denton.

A I don't have that.

Q Is there a copy available to the panel?

A No.

Q Could I quote from it?

A I would like to read it in its total context.

(Panel reading document.)

I have read both the letters.

Q I would like to read the letter from Harold Denton to the Commissioners. It says:

"The Staff has concluded that a surface offset of 2-1/2 meters could occur beneath the GETR, and therefore should be a design basis. Because this is greater than the 1 meter offset proposed by General Electric as a design basis, the Staff does not intend to continue its review of the GETR as currently designed and analyzed by GE."

And on the further -- did you, on that quote that

I read for you, did you participate in the Staff's conclusions that went into the memo from Harold Denton to the Commissioners?

A I don't participate. I'll interpret "participate" meaning involvement in some way. We wrote the Safety Evaluation Report which I'm sure was the basis for that letter.

Q Okay.

A The 1979 version.

Q On September 27, 1979, Harold Denton and Edson Case wrote a letter to General Electric which I believe you have before you. The last sentence on the first page of the letter reads:

"Furthermore" -- wait. Perhaps I should read the context. The paragraph reads:

> "The Staff's conclusion that a surface offset of 2-1/2 meters could occur beneath the GETR is in excess of the 1 meter surface offset to which the modified GETR facility has been analyzed by your staff. Therefore, we do not intend to continue our review of the GETR as currently analyzed. This includes the further Staff evaluation necessary to define the maximum vibratory ground motion in terms of effective acceleration and appropriate

response spectra.

"Furthermore, while you" -- meaning General Electric -- "may propose to analyze the GETR using the seismic and geologic design bases in the enclosed report, we are not aware of any structure which has been analyzed or built for this type of seismic load, and it is our current view that an analytical argument cannot be formulated which would conclusively support the ability of a structure such as the GETR to withstand a 2-1/2 meter surface offset."

Now, Dr. Jackson, would you agree that the difference between 1 meter and 2-1/2 meters as a seismic design basis for the GETR is rather critical when you discuss the input from the geosciences branch to the structural engineers for the various parties?

A It may be, yes. It may be.

Q Were you involved in the formulation of Harold Denton's statement in which he says:

"We are not aware of any structure which has been analyzed or built for this type of seismic loading, and it is our current view that an analytical argument cannot be formulated which would conclusively support the ability of a structure such as the

GETR to withstand a 2-1/2 meter surface offset."

A No, I was not.

Q Were you aware --

A Excuse me. I was not, but this is a cover transmittal letter which transmitted, I believe, the SER, and to that extent I was involved in many discussions that led up to that through the management chain.

Q I assume that Harold Denton and Edson Case are near the top of the chain of command at the NRC, and I assume that they would have discussed this with structural engineers within the NRC?

A Yes, that's correct.

Q Would you agree --

A I'm sorry, I shouldn't say that. I do not know.

Q Let me ask you this again. Would you agree that the difference between 1 meter and 2-1/2 meters of surface offset at the GETR site is a critical geological parameter?

MR. SWANSON: The question was already asked and responded to. I might make a suggestion. We seem to be trying to draw structural conclusions out of this panel, and I think we want to keep in mind that we do have a structural panel which follows which will, of course, be able to determine whether in their viewpoint it is critical.

JUDGE GROSSMAN: I believe the questions are

directed toward learning the input of this panel. Is that correct, Mr. Barlow?

MR. BARLOW: Yes, sir.

MR. SWANSON: But they're asking for conclusions of the significance of their input, and that is the problem. But the question has already been asked and answered. I don't see the need to keep hammering away at the same question.

MR. BARLOW: I can go on, your Honor, without pursuing the matter.

BY MR. BARLOW:

Q Dr. Jackson, in the 1979 version of the SER, the Staff states on page 8, Section 5, the Staff states there are -- the Staff states:

"2-1/2 meters of reverse oblique net slip could occur beneath the reactor along a fault plane which could vary in dip from about 10 to 60 degrees, provides a conservative description of surface displacement on the Verona Fault zone during a single event."

And then in the 1980 version of the SER, on page 5, Section 5, the Staff makes what I might characterize as a rather drastic reversal or change in position to reading -- have you found it?

"One meter of reverse oblique net slip

along a fault plane which could vary in dip from about 10 to 45 degrees, provides an appropriate description of surface displacement which could occur in the Verona Fault zone strand splay beneath the reactor during a single event."

end 4

Now I would really like to understand how the Staff arrived at their change in position from 2.5 meters as a conservative description of surface displacement to 1 meter providing an appropriate description of surface displacement.

Can you define the words "conservative" and "appropriate"?

A. (Witness Jackson) I don't evidently attach the significance to it that result in the question. I think the terms could be interchanged.

Q. Well, which is more conservative? And which is more appropriate?

A. They both have the same intent, that they be used for the design basis for fault movement at the plant.

Q. Why did you say in 1979 that 2.5 meters was "conservative," and then in 1980 you said that 1 meter was "appropriate"?

A. Well, I'm -- we put it together a year apart, I guess, is one reason, but there are other reasons. I think the terminology is such that "which could occur on a Verona Fault strand splay directly beneath the plant during a single event" -- "beneath the reactor during a single event". The other one is far less specific. It says: "provides a conservative description

of surface displacement on a Verona Fault Zone during a single earthquake event." I'm changing it from "splay" -- from the "zone" to the "splay."

That is one change. There are many reasons why the change in that conclusion came about, and they result to --

(Witnesses confer.)

Q Dr. Jackson, are you aware --

JUDGE GROSSMAN: Excuse me. There were two parts to that question, and I understood you to answer the first part as saying that "conservative" and "appropriate" could be used in the same sense there, that there really wasn't a change between "conservative" and "appropriate"; that the change was between the 2.5 meters and the 1 meter. Is that the substance of what you said?

WITNESS JACKSON: Well, in addition one specified on "the zone," which is the whole width of the zone anywhere within it; and the other was directed specifically at a splay under the plant.

JUDGE GROSSMAN: Okay. In other words, there were differences in the figures, but not -- but as far as the use of the words "appropriate" or "conservative," you belieeve, I take it, all the way through I've seen that you believe "appropriate" is

conservative.

WITNESS JACKSON: Yes.

JUDGE GROSSMAN: Okay, so we can drop that issue. It is just a question of what changed, or what may have been different between the 2.5 meters and the 1 meter.

WITNESS JACKSON: I don't want to mislead the Board. There is no intent to say that we did not change our position. There was a change in position.

BY MR. BARLOW:

Q Dr. Jackson, can you explain the Staff's change of position in light of the quote which I quoted before from the letter of September 27th, 1979, from Harold Denton and Edson Case to GE in which the Director of the Office of Nuclear Reactor Regulation of the NRC stated that: "We are not aware of any structure which has been analyzed or built for this type of seismic loading, and it is our current view that an analytical argument cannot be formulated which would conclusively support the ability of a structure such as the GETR to withstand a 2.5 meter surface offset."

A (Witness Jackson) That is clearly a structural question. I don't know how to respond to it. Structural engineers would have to be asked that question. Mine was an input.

Q The geologic input of whether to go for a 1 meter or a 2.5 meter surface displacement beneath the reactor appears to be the critical parameter in a decision by structural engineers.

MR. SULLIVAN: Objection. We are getting testimony from the interrogator.

JUDGE GROSSMAN: Let's see if we can clarify the situation. If I understand it, the panel that is sitting here now doesn't want to take a position on whether or not there could be a design for 2.5 meters, and they have indicated that they really didn't have any input in that.

They are agreeing that they had input into the question of whether there ought to be a plan for the 2.5 meters or 1 meter. I don't mean a "plan," but whether that ought to be taken into account. And they had input into that.

Is that correct, Dr. Jackson?

WITNESS JACKSON: That's correct. And the question is not that clear, because as our work was going on, the structural engineering work -- the further structural engineering work was going on. So I don't know how significant it is one way or the other to them.

JUDGE GROSSMAN: But my understanding is that what is objected to on the part of Mr. Swanson is

questions as to whether or not the structure could withstand any kind of displacement directed towards this panel. But I take it he does not object to your questioning the panel on the input of either 1 meter or 2.5 meters. And Dr. Jackson and the others would be able to respond to those questions.

Is that correct, Mr. Swanson?

MR. SWANSON: That is true, but I think if we are going to cast positions we need a clarification. My objection was that I think Mr. Barlow's questioning asked for an analysis of the criticality of 1 versus 2.5. And I think the implication was that he was calling for a structural position as to its significance.

Now there is at least one member of the panel, Dr. Pichumani, who would have a different perspective than a purely geology or seismology point of view. I don't think the question was clearly into focus, though. My objection really was that the way it was phrased I think it called for a structural response, and this panel had already indicated that it is not qualified nor is it prepared to testify, nor does it have direct testimony with which to be cross-examined on on structural matters.

JUDGE GROSSMAN: If I understand that discourse, Mr. Swanson objects to your requesting the

panel to comment on whether 2.5 meters versus 1 meter is critical, because it is only critical in the structural sense, if it is critical, and that is not a proper question for the panel.

Is that the basis of your objection?

MR. SWANSON: That is correct.

JUDGE GROSSMAN: And he has no objection, as I understand it, to your questioning on the input itself of 2.5 meters versus 1 meter. And I assume your next questions are directed to that, and I think you ought to proceed to that area.

BY MR. BARLOW:

Q Mr. Devine --

JUDGE GROSSMAN: With the possible exception of Dr. Pichumani. Can he respond to whether that was a critical input?

MR. SWANSON: He is not prepared to testify as to whether or not 1 or 2.5 meters is critical from a structural review analysis. He is prepared to testify, as indicated in his prefiled testimony, as to the soil interaction, the soil characteristics, as to whether or not one would actually experience that type of event 1.5 or 2.5 meter offset beneath the reactor is an input into the conservatism of the Staff position. But Dr. Pichumani is not a structural engineer.

JUDGE GROSSMAN: Dr. Barlow, you may proceed along the lines we have indicated.

BY MR. BARLOW:

Q. Mr. Devine -- excuse me, Dr. Devine --

A. (Witness Devine) Let's get that clear once and for all, please. It is "Mister" Devine. My colleagues here are doctors; I am not.

Q. Mr. Devine, was the USGS involved in the change of position between 1979 and 1980 from recommending a seismic design basis at the GETR site from a 2.5 meter surface offset to a 1 meter offset?

A. I guess it would depend on how you define "involved." We certainly had many discussions with the NRC relative to amounts of displacement on these faults. But we did not directly participate in the preparation of those words or that position.

A. (Witness Jackson) I would like to expand on that for one moment. The USGS, as Mr. Devine has indicated, was heavily involved in discussions on it. We asked them if the new information from one time to the other changed their conclusions; they indicated it had not changed, and you can read that in their cover letters.

Q. Dr. Morris, in your testimony, do you state, "However, we have contended throughout the proceedings

that 1 meter of surface offset is not a conservative estimate of the total amount of offset that could occur along the Verona Fault."

A. (Witness Morris) I am also a "Mister."

Q. Mister Morris, excuse me.

A. You're reading from --

Q. Your testimony.

A. My testimony.

Q. The question was: "Mr. Morris, what are the results of the USGS geological review?" It is question 11 in your testimony.

(Pause.)

A. All right. Proceed.

Q. Did I correctly quote your testimony?

A. Well, I would like you to read it again, if you would, please.

Q. Okay. The question is: Do you agree with the statement that you made in your testimony, "However, we have contended throughout the proceedings that 1 meter of surface offset is not a conservative estimate of the total amount of offset that could occur along the Verona fault"?

A. That's correct, and I answered that way yesterday, I believe.

Q. Can you explain to me the role of the USGS

in the decision by the U.S. NRC to change their position in their SER between 1979 and 1980 from 2.5 meters of offset to 1 meter of offset?

A. That's a complicated question.

MR. SULLIVAN: May I -- I don't want to object, but I think we need to clarify what the question has to be. He obviously cannot testify as to how it was used. He can testify as to his understanding of how it was used.

MR. BARLOW: That's okay with me.

JUDGE GROSSMAN: That's fine. Why don't you testify, then, with regard to your understanding.

WITNESS MORRIS: We were asked by the NRC to evaluate the Licensee's position that 1 meter of offset on the Verona Fault was a conservative estimate of potential offset.

Our conclusion, after reviewing that in light of our knowledge on the San Fernando earthquake, the amount of offset at various locations in the San Fernando Fault Zone, that 1 meter of offset was not a conservative estimate of anticipated movement.

WITNESS BRABB: May I add to that? In addition to the information that Dr. Morris -- or Mr. Morris --

(Laughter.)

WITNESS BRABB: -- that Mr. Morris contributed, we also of course considered the geologic information in the GETR site itself.

WITNESS JACKSON: I might add, Mr. Barlow, for the Board's consideration, I have never worked on a site in seven years at NRC in which there has been as close a liaison between the NRC Staff and the USGS.

BY MR. BARLOW:

Q. Well, Dr. Jackson, could you explain to us why there is a disagreement between the USGS position and the NRC position?

A. (Witness Jackson) There is no disagreement.

Q. How can the NRC position be that 1 meter of offset is appropriate or conservative, when the USGS contends throughout the proceedings that 1 meter of offset is not a conservative estimate?

A. There are other elements to the Safety Evaluation Report, substantially other elements. And it is our job, and my job as a resulator, to implement those other elements into a safety evaluation report to reach a conclusion.

Q. Is one of those elements the statement by Harold Denton that "no structure can be modified for 2.5 meters of offset"?

(Pause.)

A. That's a structural question. I haven't --

JUDGE GROSSMAN: Before we leave the topic, if I understand your answer, Dr. Jackson, you're saying that perhaps the USGS and the NRC agree in the final conclusion. But we are discussing right now the specifics of the 2.5 meter and the 1 meter. And it is my understanding from what has been said that the USGS and the NRC do disagree on that.

Now if I am wrong, I have missed something, and perhaps other people have missed something, and that is what we would like your clarification on.

WITNESS JACKSON: I'm sorry if I misled. Let me try to clarify it.

There is a difference between a "conclusion" in a safety evaluation report and inputs to that safety evaluation report. The amount of offset -- we are trying to specify a specific amount of offset that will occur under the plant on a given splay of the Verona Fault that should be used as a design basis for surface faulting.

The USGS has been commenting and providing advice on movement across the zone. We have not asked them -- and I will explain that in a minute -- to specify what they think is the proper design basis to be used on surface offset under the plant.

What we did do, however, was we implemented that with their input -- I say "we," meaning the NRC Staff. Also, at the advice of the ACRS and other individuals in the NRC, we moved forward with a number of other studies, the probabilistic studies being the primary one, and Dr. Slemmons relocking at information also. And that led to the modified conclusion. It was additional studies and modifications, and the probabilistic aspect was a significant portion of that.

The USGS did not review the probabilistic studies that were contracted to Livermore Laboratories, and I don't think we have ever tried to contend that somewhere on the Verona Fault Zone that 2.5 meters could not occur. In the geosciences area, we can't -- we just are not in a position to preclude things from happening. We're trying to get down to determining a design basis for surface offset for this plant.

Does that clarify the roles of the agencies?

JUDGE GROSSMAN: Well, it doesn't --

WITNESS JACKSON: I did want to expand on one point. Based on our mutual agreement with the USGS, and I think Mr. Devine will comment on this extensively, it has been historically the role of the USGS not to take positions particularly on design basis events. It has been the NRC's role. And that is based on

interagency agreement of mutual interest -- of mutual agreement between the two agencies. There are good reasons for that.

One is that, in my opinion, there are very good reasons for that. The role of the USGS is to serve as an independent review party and is very important in this to shed as much information as possible on a particular topic.

The decision-making role must remain with the NRC. Now that could lead to accusations of bias, if you like; but on the other hand, the USGS wants to maintain this independence as an objective organization. And I think that is important.

I would like Mr. Devine to comment, if he feels so.

WITNESS DEVINE: I would agree strongly with the last part you said.

There are two comments in your last discussion, though, that I would like to clarify for the record. One is, in attempting to describe our position, Dr. Jackson may have misled the Board on what we have said about where the displacement could occur.

As you recall from Yesterday's discussion on the T-1 Trench, our geologists feel confident that 1 meter has been exceeded on single strands of the Verona.

Dr. Jackson said, the fault "zone," and I don't want to get into that same distribution problem that we had at the close of business yesterday. We do maintain that it could occur on one strand.

Secondly, to set the record straight on the historical aspect about design-basis events. NRC terminology and AEC-NRC terminology has evolved over many years. At one time, the advice that we did provide to NRC was called "design-basis events." And some historical letters from both the USGS and NOAA, N-O-A-A, would use that terminology. So I mention that because that's not in conflict with what Dr. Jackson just described as our relationship, but the wording would make it appear that way in the old documents.

WITNESS JACKSON: I might comment a little further on that. Prior to 1973 essentially, the NRC did not have a Geology Review Section where there was one geologist at the NRC, and the USGS served almost the total review function on the geosciences area.

Since that time, we have a branch of about 16 to 20, depending on the hiring, professionals in the geosciences who now can, like Dr. Justus, do an independent review of the USGS, and in some cases an equal review of the information.

So we feel competent to be able to come to

independent conclusions in factoring information that the USGS also may not have in front of it, or have an opportunity to review. These individuals who have dedicated so much effort to this particular review, their main mission and role at the USGS is not to review nuclear sites. Their role is regional mapping. And they have dedicated a tremendous amount of personal effort to this particular review.

JUDGE GROSSMAN: Dr. Jackson, I understand the distinction you've made between "design basis," which is something that the NRC is concerned with, and "geologic estimates" which is something that the U.S. Geological Survey is interested.

Now one of the items that you have asked them to explore with regard to your coming up with a design basis is the amount of offset. And my understand-- that could be expected on the Verona Fault, the maximum amount of offset.

My understanding is that on that particular item, the USGS has consistently said that it is possible to have a 2.5 meter offset on a Verona strand.

Is that correct?

WITNESS DEVINE: I'm sorry, sir. That is not the correct characterization. We have not said 2.5 meters. I think we have said --

JUDGE GROSSMAN: Five feet to seven feet.

WITNESS DEVINE: -- that 1 meter is not conservative; and we have said in testimony it could exceed 1 meter. But we have not said 2.5 meters. That is not our number.

WITNESS JACKSON: I agree with that. That's correct. Your characterization is correct, of the information and differences between those and the USGS.

JUDGE GROSSMAN: Right. Okay. Let me qualify what I've said. You haven't said 2.5 meters, but you have suggested that there may have been offsets up in the range of 5 feet to 7 feet, which is approximately what the 2.5 meters would come to, or at least 2 meters.

WITNESS DEVINE: In that context, yes. But I am disturbed on the use of words. We have said "could occur," and you described it as "expected." And as I recall in our NRC criteria, there's a big difference. It is "could occur."

JUDGE GROSSMAN: Oh, I'm sorry. I apologize for that. I didn't mean to say "expected," but "could occur."

Now Mr. Barlow is asking questions, and I really am not sure of what he is driving at, but I would certainly like to hear why it is with the input that the USGS has had with regard to 1 meter not being

an appropriate amount that might be offset on a Verona strand, the NRC has in its latest SER adopted the 1-meter position.

Now I don't know if that's what Mr. Barlow's questions are directed to, but I would like to hear why that has been done.

MR. SULLIVAN: Could I just add a statement? We need to be very careful I think in terms of words and maybe some defining is necessary. The Board just defined the USGS position and used the word "appropriate." It is more than a semantic difference that they used the term "conservative. So I don't want positions again to be miscast by use of words, which may not appear as significant to someone other than members of the panel.

JUDGE GROSSMAN: If I use the word "appropriate" with regard to the USGS, I would again say I erred because of course they are not in a position to make a judgment as to what is appropriate or not. However, I believe the word "conservative" with regard to them would not be inappropriate, or would it, as to a parameter?

WITNESS DEVINE: No, I would consider -- that is the word.

JUDGE GROSSMAN: Okay. Well, the USGS then has stated that 1 meter would not be conservative.

Nevertheless, in the design basis the NRC has utilized 1 meter as both appropriate and conservative. And I would like an explanation of why you have used a figure that the USGS does not believe to be conservative.

MR. BARLOW: Your Honor, could I interrupt --

WITNESS JACKSON: I would like to answer the question, if I could.

JUDGE GROSSMAN: Did you have an objection, or an observation on that? I'll allow counsel --

MR. BARLOW: I had no objection, but I would again note that we had a question which was not really answered, which was asking the Staff to define their use of the words "conservative" and "appropriate" in the sections about 2.5 meters and 1 meter. And I think that your question points out the criticalness of the Staff's definition of the words "appropriate" and "conservative" in those contexts.

JUDGE GROSSMAN: Well, could you answer my question, first, and then we will allow Mr. Barlow's question, if there is no objection.

WITNESS JACKSON: It is a very difficult question to answer, and in my view it is the main reason we are here in this hearing. I think that's obvious to everyone.

There are many factors that go into a decision

on a particular facility, and this one is difficult. The USGS was one major and principal input into our decision. Between the issuance of the Safety Evaluation Report in 1979, and the one in May of '80, a great deal of thought and additional work went into the review, both by the USGS znd by other parties.

My answer is essentially the Safety Evaluation Report and its appendices, all of the appendices -- Dr. Slemmons, Dr. Vestlie, the whole probability group included in that, and some other thinking that we had done on the data that we had used to reach the conclusions we had in the earlier estimates. I will try to summarize briefly some of these, but not go into detail.

Number one is the probabilistic analysis indicates that we do not need to use the maximum that we might observe for a given fault or fault set under the plant. That is my understanding of the probability analysis. Any specifics on that should come from the probability panel.

The second thing is: The comparison of the San Fernando Fault Zone which was the principal basis for the position that we took in 1979, we assumed the analogy one on one. That means that we transposed San Fernando to Verona and assumed that they were

essentially equivalent, with no credit given particularly for the conservative assumption. That conservative assumption being that there are a number of elements which indicate that that comparison is conservative; that making a one-on-one transferrence should not be done without some consideration that these zones are somewhat different.

The second thing was that another principal component in that safety evaluation was the treatment of the worldwide data on the amount of displacement versus fault length and magnitude. This is the data that was compiled essentially by Dr. Slemmons and using that information resulted in an estimate on the Verona Fault Zone in using that information. And I even have some of my early plots that we used, or graphical displays of those. We never recognized that those plots were actually plots of the maximum observed displacement observed during that event as compared to fault length.

end JWB #5

In other words, the regression line was drawn through the maxima of those worldwide data, not an average observed offset, but always the maxima. So using a factor of regression line to the maxima of worldwide data.

Another item is that the dominant style of displacement in the trenches examined appeared to result in displacements on the order of one meter. Recognizing trench T-1 is in disagreement with that, some of this information is reasonably new, but I think we knew there were potential exceedances for quite a while, and when we did write that SER.

The other thing is that we really believe that there is a low likelihood the fault does exist under the reactor. At least one equivalent to those where we have observed the large amounts of displacement. The faults observed in the trenches, being B-1, B-2, B-3, T-1 and T-2.

Again, we didn't rule out the possibility of it. I think -- and I discussed that yesterday in my testimony, some of the bases for that.

We further discussed this with Dr. Slemmons, and I think the critical decision or difficulty in making this is whether or not we are specifying parameters that must be at the upper bound -- excuse me, that's a mischaracterization. We are dealing with distributions of possible occurrences of events. As Dr. Justus indicated in his submittal

introductory statements, we at each one of t ose distributions have gone toward the conservative side, which means at least beyond what we think is a mean representation for the most part, and we have added cumulative conservatisms by adding them -- by assuming all of these occurred at the same time. The magnitude on the Calaveras being 7.5, the magnitude on the Verona being 6.5, and that type of consideration.

So the question is, must you take that element which could occur -- and I don't think we have ever ruled out that somewhere on the Verona Fault zone in total, that the 2-1/2 meters could occur. It's based on the probability that that full 2-1/2 meters will occur between the primary existing shears under the reactor and sustain a full movement of the maximum that was observed at the San Fernando Fault, and that's essentially a summary of what it is.

It's obviously far more complicated than that.

JUDGE GROSSMAN: Dr. Herd, did you have any comments to make with regard to the USGS position on what Dr. Jackson was talking about?

WITNESS HERD: No, I believe that Mr. Devine handled our position exceedingly well.

WITNESS JACKSON: Could we take a five-minute break?

JUDGE GROSSMAN: Certainly.

WITNESS JACKSON: Thank you.

(Recess.)

WITNESS JACKSON: I wanted to add to my last response, if I might. I'm not sure we left the Board with a clear impression of what we are doing.

There are two elements to my response. I think one may help clarify it. It's on page 10 of Dr. Justus' and our testimony. It's in answer to question 8, it's line 4, where it begins:

> "We agree with the observations that the possibility exists that offsets larger than 1 meter could occur at some time in the future. As stated extensively in the SER, however, it is our best professional judgment, based on considerations discussed in the SER that the occurrence of greater than 1 meter of surface offset directly under the reactor is unlikely -- is very unlikely. This is essentially a qualitative judgment which can be made based on geological observations which are in turn strongly supported by the various probability analyses which were also applied to the problem. When taken in total consideration, the input parameters specified for this site assumed a set of conservative steps."

I think the main difference is this: We are not

precluding that offsets greater than 1 meter cannot occur, but we are specifying that for the purposes of design of this facility, that the 1 meter is indeed appropriate, because of its sufficient -- it's essentially unlikely to occur, and we have required it, anyway. On a splay directly beneath the plant. Which is a very important distinction.

For instance, we do not have a splay that we know goes directly under the plant. Dr. Brabb and Dr. Herd have discussed this extensively in the excavation photos about the possibility, and that, but we do not know for certain that that's the case.

JUDGE GROSSMAN: Mr. Devine?

WITNESS DEVINE: Maybe it's not my position to speak, but I think Dr. Jackson used a double negative in his sentence which came out meaning the opposite of what he meant.

JUDGE GROSSMAN: I noted that. You said could not occur instead of saying that we agree that an offset greater than 1 meter could occur, and we took it in that sense.

Excuse me. I have one question. One item that you mentioned that you relied upon in arriving at the 1 meter figure was also the study made by the consultants ESA, to GE, regarding the trench. I'm not talking about the probabilistic studies. And that is a matter that has

been disputed by the USGS, and I would be interested in knowing how critical that reliance was.

WITNESS JACKSON: It is an extremely difficult question. In putting that in here, we reviewed all of the basic information provided by GE and their consultants, in terms of reliance -- we put it in here because it is one input to the package. They are a party, they have professional staffs, they have interpretations which they have drawn.

As a person who has the responsibility for evaluating everything, this obviously factored into our consideration. It would be extremely difficult for me to pick out elements or pieces that I favored -- I shouldn't say, it was done as a group, with Dr. Justus. I'm doing him a disservice -- we factored in many elements of GE's testimony and weighed, I must admit, in total context, we have weighed very heavily the recommendations of the USGS on the geologic aspects than anyone else, and we have almost put complete reliance on them.

WITNESS DEVINE: Your Honor, may I ask a question?

JUDGE GROSSMAN: Yes, sir.

WITNESS DEVINE: You made a reference to a disagreement between GE's consultants and the USGS, and I don't understand what that is.

JUDGE GROSSMAN: Perhaps the word "disagreement" was too strong.

WITNESS DEVINE: I'm not sure what --

JUDGE GROSSMAN: But the subject matter related to whether there was only two or three feet offset in trench, I believe, T-1, or whether that was in reality somewhere around five to seven feet.

WITNESS JACKSON: We are aware of trench T-1 information and the use of the U.S. Geological Survey on this in principle, and in specific for a long time, and yet there were refinements that came about as a result of our discussions during this hearing, obviously. But the basic information that the USGS had discussed with us before is about the same.

So it was factored in.

JUDGE GROSSMAN: Mr. Devine, you still look somewhat unhappy, and I just want to make sure that I haven't misstated anything that you think ought to be corrected. You are the expert; I'm not.

WITNESS DEVINE: As I recall the context of the question, I had the impression that you were describing the trench that would reflect the horizons beneath the reactor, and in that case I don't believe we have a disagreement with the Applicant.

The disagreement, as Dr. Jackson described, is

in T-1, which does not describe horizons directly under the reactor, and so the disagreement, I thought, was out of context with what I heard Dr. Jackson describing as the important input to this 1 meter beneath the reactor.

WITNESS JACKSON: Mr. Chairman, it's obvious that there are scientific disagreements on interpretation of many things on this particular site. I have disagreements to some degree with USGS on certain observations; they have them with me; I have them with Earth Sciences, and vice versa. It's the nature of our science.

JUDGE GROSSMAN: I hadn't been aware that anyone had discovered any offset in the horizons underneath the reactor.

WITNESS DEVINE: That's fine. That's what I thought we were giving the impression that we disagreed on. We do not disagree on that point.

JUDGE GROSSMAN: Mr. Barlow?

BY MR. BARLOW:

Q Dr. Jackson, we often hear in this discussion the use of the words "conservative" and "appropriate." Two times this week you have defined in your testimony or in answer to cross-examination a definition of the word conservative as "cannot get worse".

Can I interpret that to mean that one of your definitions of conservative is that a conservative value

is one that cannot be exceeded?

MR. SWANSON: Can I get a reference, please, to the context of those two definitions?

MR. CADY: In the very first day of Dr. Jackson's testimony, in response to a question concerned with conservatism, he said -- and it is a close paraphrase, but it is pretty near to a quote -- he said that it just can't get worse, and he did refer to it today again in passing as "cannot get worse" in another context.

MR. SWANSON: I just want to make sure that we don't take as a supposition that the sentence or the definitions mentioned were without any necessary foundation to put him in context.

JUDGE GROSSMAN: Mr. Edgar?

MR. EDGAR: Judge Grossman, I just think it's important that the witnesses understand -- I can excuse Mr. Barlow's -- the manner in which he phrases the question from time to time, perhaps, for a lack of prior training. But the witnesses should understand that they don't have to accept his characterizations.

The question itself, if you struck out all of the prior characterization, the question itself is permissible, it seems to me, and if the witnesses understand that and they explicitly ignore the characterizations, there's nothing wrong with it.

But it's taking us a lot of time because of the characterizations.

JUDGE GROSSMAN: Well, it seems to me that I can recall that discussion, and it related to the assumption of co-seismicity, and that in fact that word was used in that general sense. And I don't think in view of that, that there isn't any apparent misstatement, that Mr. Barlow can lead the witness on cross-examination, and he is, of course, an expert witness and has shown that he can handle himself on this. So I will permit the question and let Dr. Jackson fully explain that.

WITNESS JACKSON: As I understand the question, I do disagree slightly with the characterization, but I don't think it's important to my response. That is -- I do not have what I would call a specific definition of what is conservative.

The way I view things is that each item that you must make a decision on in a step-like sequence is a distribution of possibilities, and if you view that as essentially a bell-shaped curve, with tails of the distribution both on the low end and the high end, with some meaning about which the bell-shaped curve exists, that conservative should be clearly on the high side of the mean, as best we can; could be out as far as the extreme end of the tail of the distribution, as was done

in terms of the co-seismic definition.

In other words, there is some probability, if I use that as an example, there is some probability, extremely low probability, that the two will occur simultaneously. Let's say that's zero. So that's the lowest tail of the distribution.

There is some probability that they will occur -- part of the offset will occur during the strong ground shaking. Let's say that is more sort of the mean or the peak of the bell shape of the distribution. And then there is the far end of the tail which would say that they both must occur simultaneously. In other words, a probability of one.

Now I'm not a probabilistic expert, and I'm not trying -- I guess I'm discussing this in a quasi-statistical way, and I don't want to offend the statisticians that are in the audience. I am using this as a mental process that we go through. It's a decision process.

Now we tried to implement this decision process throughout this proceeding, and we tried to be toward the tail of the distribution. Now that -- you can -- as I mentioned earlier, on some of the parameters we are discussing, I don't think any of us, as expert geologists or seismologists could sit here and preclude almost anything in the earth sciences, and I mean preclude, that it's not

theoretically possible, and we only can work with that information that we have.

Dr. Brabb has used that term very many times, and just as in this science, decisions must be made, in this science, like all others, for this facility, and that's the best definition I can. I cannot give you a quantitative definition. It would vary.

BY MR. BARLOW:

Q Several times in this discussion I have heard you mention that the NRC cannot preclude the possibility that 2-1/2 meters of offset would occur at the GETR site. Is that correct?

A That's correct.

Q In that context -- well, first of all, let me -- in that context, how do you distinguish between conservative and appropriate when you change -- when the Staff changes its position from the 1979 position of 1 meter of -- excuse me -- the 2-1/2 meters of offset was a conservative description, and in 1983 change the position to 1 meter of offset as an appropriate description? How --

A (Witness Jackson) I answered that question earlier.

MR. SWANSON: I also would object to the characterization of "how do you distinguish" when Dr. Jackson earlier said he would use them interchangeably.

BY MR. BARLOW:

Q Let's try it from a different angle.

Are you aware, Dr. Jackson, of a stipulation which reads:

"The assumption that the San Fernando and Verona Fault zones are comparable is a conservative assumption."

It's designated as No. E.

A (Witness Jackson) Yes.

Q Do you agree that Dr. Slemmons and Dr. Herd and other people have characterized -- and also the reports which Dr. Justus discussed from Dr. Sharp and Dr. Bonilla and others -- have mentioned that in the San Fernando earthquake, there were offsets of 2-1/2 meters?

A Yes. I'd like to comment, however. The term "conservative" there is used not in light of the amounts of offset, but in terms of transference of one piece of tectonism in California to another area of tectonism in California.

Q Okay. Do you also agree that it has been agreed to by several scientists on this panel that the offset in the San Fernando earthquake included a 2-1/2 meter within a narrow zone of five meters or less?

A I was one of those scientists.

Q Would it therefore be a conservative description

of surface displacement on the Verona Fault zone during a single earthquake event that 2-1/2 meters of offset could occur?

A I don't believe so. It would be conservative. It would be further out on the tail of the distribution of possibilities, and the decision must be made in light of the transference of the tectonism of -- I use that term in a broad sense -- of San Fernando area to the Verona area.

Q Dr. Jackson, do you agree that --

JUDGE GROSSMAN: Excuse me. Excluding all other inputs into the determination, if you just had the San Fernando data and the data you have with regard, or the assumptions you have made with regard to the length of the Verona Fault, would the use of the data of 2.5 meter displacement with regard to the San Fernando event be conservative, if you were to project that to the possible offset on the Verona Fault?

WITNESS JACKSON: It would be conservative if it were on the Verona Fault zone, and the difference comes as to putting it under the plant itself on a splay which is under the plant. The San Fernando is a very complicated fault zone. It is one of the only good observations we have of such a fault behavior.

JUDGE GROSSMAN: I'm sorry, I think we lost the negative there as we had before. I believe you intended

to say, and maybe I'm wrong, that it would not be conservative with regard to the Verona Fault zone, but it would be conservative with regard to projecting it under the plant?

WITNESS JACKSON: That's right, yes.

WITNESS DEVINE: May I ask a question, your Honor?

JUDGE GROSSMAN: Certainly.

WITNESS DEVINE: In your characterization of what Dr. Jackson should assume, you said if you had only the San Fernando data. I assume he was not to throw out all the other information we have on fault displacements that's stored in the judgment process? I get concerned when I assume you mean only San Fernando.

WITNESS JACKSON: I would like to clarify. Taking the maximum displacement that occurred in San Fernando and transferring it to the Verona Fault zone, and then saying that 2-1/2 meters of offset can occur on the Verona Fault zone is indeed conservative.

end 6

JUDGE GROSSMAN: I understand that you're saying that with regard to all the data, and my question was very specific. I referred only to the data relating to the fault length in the San Fernando Fault Zone and the Verona Fault length. And I believe you answered it, and I don't want to venture into any muddier waters now.

BY MR. BARLOW:

Q. Dr. Jackson, would a reasonable definition of the term "conservative" in this context be a definition of a value that is the maximum observed data in nature?

A. (Witness Jackson) I wouldn't accept that as a definition, no, not in licensing.

Q. You would not accept the maximum observed data as conservative?

A. As you asked it, if I am correct and heard you correct, if I would accept that as my definition?

Q. Yes.

A. For purposes of licensing and licensing decisions, I don't think that that is an appropriate -- I personally don't think that's an appropriate description of "conservative."

Q. Do you mean that --

A. For when you're discussing a single element.

Q Well, then, would you mean that it would be more conservative to characterize a conservative value as one which cannot exceeded, wehther or not it is the maximum observed or not?

A I guess I don't understand the question.

Q Would it be conservative to -- Would a conservative value in a seismic design basis be one which cannot be exceeded, rather than one which is the maximum observed?

A "Cannot be exceeded"? I just don't know how to answer it. If it's something that can't be exceeded, then it has to be conservative I would say. It has to be on the far end of a tail of a distribution of something.

Q Okay. Given that I have characterized two definitions of "conservative values in seismic design bases," are either of these definitions ones which you use when you describe a value as conservative?

A Sometimes. The latter part I would say "no."

JUDGE GROSSMAN: I think it is about time for the luncheon break, and I would like to ask if you have any idea, Mr. Barlow, as to how long your cross-examination is going to take.

(Pause.)

JUDGE GROSSMAN: Mr. Barlow, we are not going

to hold you to that. We would just like a general idea, but you will be permitted to give your full cross-examination.

MR. CADY: Right. Mr. Barlow has no further questions, but I expect to take about five minutes upon return, unless you would like me to go onward now. I am going to consult with Mr. Barlow over lunch, and I expect that I will go between five and fifteen minutes.

MR. BARLOW: Your Honor, if I might explain the reason that I have no further questions, it is because I have to go and catch a plar to San Diego to work on some other things. I was supposed to leave yesterday, but I stayed for the morning session.

(Board conferring.)

JUDGE GROSSMAN: Mr. Barlow, why don't you proceed in the questioning now.

MR. BARLOW: Okay. Thank you, your Honor.

BY MR. BARLOW:

Q. Mr. Devine, in the analyses of reactor sites in California and in the work which the USGS does for and with the NRC Staff, are there discussions about the definitions of the words "conservative" and "appropriate"?

A. (Witness Devine) Yes, I think there are.

Q. Do you recall in the context of the GETR

review discussions about the uses of the words "conservative" and "appropriate" in regards to the value 1 meter and 2.5 meters of offset?

(Witnesses conferring.)

A. Mr. Barlow, I'm certain in all the discussions we've had with NRC concerning fault offsets in the GETR area "appropriate" and "conservative" and "appropriately conservative" have been used in hundreds of ways, and were involved in many, many portions of conversations.

Q. Mr. Morris, in your testimony when you characterized -- when you make the statement, "that 1 meter of surface offset is not a conservative estimate of the total amount of offset that could occur along the Verona Fault", is your use of the word conservative -- could you define your use of the word "conservative" in your testimony?

A. (Witness Morris) I used it in the context that that value could be exceeded.

Q. In other words, your definition of the word "conservative" is a value that cannot be exceeded?

A. No. I said it "could" be exceeded.

Q. Okay.

A. In the context of the sentence referred to.

JUDGE GROSSMAN: Well, could you indicate

what you use the word "conservative" to mean in that context?

WITNESS MORRIS: Well, if 1 meter was the expressed dislocation on the Verona Fault, I think we would admit that there is a possibility that a greater dimension could result from a given earthquake.

WITNESS DEVINE: I would like to elaborate, if I may, since I used those words myself, and reviewed those of Mr. Morris.

In this sentence, realize what we're trying to say is that we're trying to estimate the total amount that could occur. And in our judgment, 1 meter is not a conservative estimate of that. That means the likelihood of 1 meter being exceeded as the total amount of offset that could occur is reasonably high. Therefore, we would not characterize it as conservative.

BY MR. BARLOW:

Q. I would like to ask the USGS panel if they were ever asked to characterize the potential surface offset at the GETR site for the purpose of seismic design bases in the site review.

A. (Witness Devine) Not to my knowledge.

A. (Witness Morris) No.

A. (Witness Brabb) No.

A. (Witness Jackson) Mr. Barlow, I would suggest

that it would be a better question if it were qualified to say: on the conservatism of the offset, the design specification on the splay under the plant. They clearly have made many recommendations on the Verona Fault "zone" that you used.

A. (Witness Devine) Yes, but not for design purposes.

Q. I do not want to qualify my question, Dr. Jackson.

A. (Witness Devine) And my answer stands: Not for design purposes.

Q. Mr. Devine, you stated earlier this week that there is an annual grant of $1.3 million per year which is given by the NRC to the USGS for studies of geology and seismology in regards to reactor siting; and that there is a priority ranking process in which priority lists are generated.

Then Mr. Morris further elaborated that this research is not site specific research and is not for -- is restricted to not providing basic data; and furthermore, is not oriented or programmed to do detailed, site-specific studies.

Can you explain why site-specific studies are not done, and why specific research and data is not part of the priority ranking process in the way that

this money is used by the USGS?

A. I can answer that last question, but there are a variety of errors in your characterizations that preceded the question. If you allow me to disregard your original statement, I will answer just your last question.

Q. Okay.

MR. CADY: Could you please clarify what you would feel to be an adequate representation of what was discussed before --

MR. BARLOW: Of what you're answering.

MR. CADY: Right. Before you answer, could you please characterize, or correct any deficiencies that you found in Mr. Barlow's question before you answer the question?

WITNESS DEVINE: Well, that will take awhile, because he characterized a variety of things that I didn't agree with. I would have to have them read through.

To begin with, I didn't say the $1.3 million, and on it goes. So there were a variety of errors in his characterization which I can't handle.

BY MR. BARLOW:

Q. Perhaps I could simplify and restate the question. Could I simplify and restate the question for

you?

A. (Witness Devine) Certainly.

Q. Can you explain for us why the USGS does not consider it a priority to do site-specific research at reactor sites with money that is given to them by the NRC?

A. Yes, I can. The amount of money it takes to do a detailed site investigation for one site would be far in excess of the amount of money we receive from NRC by a large amount. So consequently, it would be a very inefficient use of the money and our expertise to concentrate it on one small postage-stamped sized piece of terrain relative to what we can do for NRC by doing regional and process type research that can be applied throughout the nuclear licensing process.

Q. Okay, within that context, then, if you are doing regional studies for the NRC in regards to reactor siting, has the USGS ever made any recommendations or regional studies for the NRC in regards to siting reactors along the San Andreas Fault system or its branches in California?

A. You said "recommendations or regional studies," I'm not sure what that means. Obviously we've not made recommendations to do studies site reactors along the San Andreas, so I guess I don't understand your

question.

Q Okay. I'm sorry. I understood you to say today and earlier that the USGS uses this money for regional studies, rather than site-specific studies. My question is: Has the USGS done a regional study along the San Andreas Fault system and its branches in California in terms of making recommendations to or studies for the NRC in regards to reactor siting in California.

A Again, there seems to be a variety of things asked there. I'll try to answer it, but there are several parts in that question that require answers in different ways.

For example, we have in the past received money from NRC to install and operate seismographs in and around the San Andreas Fault system. So, yes, we have done that, if that's part of your question.

We have made investigations of various faults. Some of the money may have been provided from NRC for some of the support of the fault investigations. Specifically, I would have to go to the manuals and records to find out which is which.

So we have made fault investigations. We've made earthquake moniroting of the San Andreas system. Some of the money has come from the NRC to do that.

Q Dr. Brabb, independently of the GETR site review, you are involved in the Earthquake Hazards' Reduction Program of the USGS? Is that correct?

A (Witness Brabb) Yes.

Q Within the Earthquake Hazards' Reduction Program, is there any sort of study of critical facilities that might endanger the public if they were damaged during an earthquake?

A (Witness Devine) Let me try to answer that, if I may. I think there are assessments made of -- there are several reports published by USGS which has assessments made of damage that could occur to critical facilities such as schools and hospitals and fire stations in overall seismic risk studies that are done by USGS, if that answers your question?

Q Dr. Brabb, in your opinion having worked in the San Francisco Bay Area in the Earthquake Hazards' Reduction Program, would it be an earthquake hazards' reduction to not operate nuclear reactors on top of earthquake faults?

A I'm sorry. There are several negatives in there that I don't understand.

MR. SULLIVAN: Could I have the question repeated, please, before the response?

JUDGE GROSSMAN: Mr. Barlow, let me find out

where we're going right now. You don't have very much time here. Is this the last line of questioning you have?

MR. BARLOW: Yes, your Honor, it is.

MR. SULLIVAN: I would like to have the question repeated, please.

THE REPORTER: "Q Dr. Brabb, in your opinion having worked in the San Francisco Bay Area in the Earthquake Hazards' Reduction Program, would it be an earthquake hazards' reduction to not operate nuclear reactors on top of earthquake faults?"

WITNESS JACKSON: Mr. Barlow, if I could ask for a clarification which I think may make for a better question, there is a distinction, or at least many of us use a distinction between "hazard" versus "risk," "hazard" being the identification of a potential of something happening, the "risk" being the impact of that on a facility or a population.

Now I assume by the characterization you have made that you are really discussing "risk. "Hazard" is a separate element of that risk.

Did you mean "risk"?

JUDGE GROSSMAN: Well, who was the question directed to?

MR. BARLOW: To Dr. Brabb.

JUDGE GROSSMAN: Can you answer the question?

MR. SULLIVAN: Then I will object --

WITNESS BRABB: No, sir, I cannot. I would like to try and respond, but I don't understand the question.

BY MR. BARLOW:

Q Can you tell me what it is you do not understand about the question?

A (Witness Brabb) If I knew that, I could probably answer it.

(Laughter.)

JUDGE GROSSMAN: Would you like that question repeated again, or would that not help?

WITNESS BRABB: No, sir. Maybe I can struggle and try.

MR. SULLIVAN: Mr. Chairman, I did register an objection. I think implicit in there is at least one assumption. That is, that there is some design involved. These witnesses have already testified they do not get involved in the design of reactors. They are not asked to offer design values. The question assumes that, among many other things, they have a knowledge of design in order to make an assessment of whether or not there is a reduction of hazards by operating a plant on or off of faults in the Bay Area.

I think we have enough basis without going further to conclude that this witness is not qualified to answer a very general question on reduction of risks of operating a nuclear power plant.

JUDGE GROSSMAN: Mr. Cady?

MR. CADY: Your Honor, if Mr. Brabb is on this Earthquake Hazards' Reduction panel, he probably is qualified to answer the question as posed by Mr. Barlow, regardless of the objection of the Staff.

MR. EDGAR: May I add something to this? I support Mr. Swanson's objection, and I would add one other point. I think it is terribly unfair to ask a witness to answer a question that he does not understand.

If the purpose here is to bring out the truth, I would not think that they'd want to have a transcript page that shows a question that they don't understand, and then a stab at the answer, and then be held accountable in the future for the match between the question and answer. I think particularly dealing with scientists, they do demand some precision, and it is only fair to require that they have a question that they can understand.

JUDGE GROSSMAN: With regard to Mr. Swanson's objection, on hearing the question I don't find double negatives, so I assume that objection -- I believe your

first objection before rereading the question had to do with double negatives. That is not so?

MR. SULLIVAN Well, I didn't make an objection before I asked for the question to be reread the first time. I just wanted to hear it again before I considered my objection.

The objection is as to the ability of this witness to respond to the question that we already have established a basis in the record to indicate that he is not in fact qualified to answer the ultimate question that was posed to him.

JUDGE GROSSMAN: Well, the second objection, if I understood it, had something to do with the design basis, which I didn't hear in the question either. Was that part of the question?

MR. SULLIVAN: No, it was implicit in that. He didn't ask him a geology question. He asked him a question about reduction of -- as I understand it, about hazards from operating a nuclear power plant.

JUDGE GROSSMAN: And my understanding is that you are, sir, on a panel of hazards' reduction?

WITNESS BRABB: No, sir, that is not correct.

JUDGE GROSSMAN: Well, maybe that is a problem, then.

(Laughter.)

JUDGE GROSSMAN: Is that outside your field of expertise?

WITNESS BRABB: I am, as part of my geologic duties, I am responsible for preparing geologic information that may relate to earthquake hazards' reduction. There is no official panel of which I am a member that makes decisions about how this information is used. Therefore, I am prepared to say that I do prepare maps that are used for a variety of people, including, one would hope, the Nuclear Regulatory Commission, in terms of different kinds of geologic hazards.

To the extent that people use those maps to reduce hazards, then this is fine.

JUDGE GROSSMAN: Do you make recommendations as part of your formulation of maps and other data for the NRC?

WITNESS BRABB: Not regulations with respect to the operation of a reactor, which I think was the context of the question. We would make no recommendations in our maps as relates to the operation of a reactor.

JUDGE GROSSMAN: Has that sufficiently clarified it so that you could rephrase the question?

MR. BARLOW: If I could lay a foundation with a preliminary question?

JUDGE GROSSMAN: Proceed.

BY MR. BARLOW:

Q Dr. Brabb, what is your position in the Earthquake Hazards' Reduction Program?

A (Witness Brabb) I am the coordinator for Earthquake Hazards' Reduction studies in the San Francisco Bay Region.

Q In that program, do you come up with recommendations about fault zones where critical facilities should not be located?

A No.

Q Okay.

JUDGE GROSSMAN: It is about time for our luncheon break.

MR. BARLOW: That concludes my cross-examination, your Honor. I would like to just take this opportunity to thank you and the Board for your patience and your assistance in my cross-examination, and I would like to thank this panel for their patience and their explanations.

JUDGE GROSSMAN: Thahk you. We will adjourn until 2:00 o'clock.

(Whereupon, at 12:25 p.m., the hearing was recessed, to reconvene at 2:00 p.m., this same day.)

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AFTERNOON SESSION

(2:00 p.m.)

Whereupon,

PHILIP S. JUSTUS,

ROBERT E. JACKSON,

ROBERT H. MORRIS,

EARL E. BRABB,

DARRELL G. HERD,

WILLIAM L. ELLSWORTH,

RAMAN PICHUMANI and

JAMES DEVINE

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

JUDGE GROSSMAN: The afternoon session is begun.

Mr. Cady, did you have questions to ask?

MR. CADY: Just a very short series, your Honor.

CROSS-EXAMINATION (Continued)

BY MR. CADY:

Q Gentlemen of the panel, I am assuming that you are familiar with the process known as the scientific method. Could any one of you please describe for the Board and myself exactly what procedure is used to conform to the scientific method?

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I realize I am going back to the early stages of your education. This is a primary tool used by you in analyzing any given problem.

Could you please just elaborate as to what this encompassed from step A through -- I believe it is a five-step process. Could you please go through the process for us, please, any one of you, if you are familiar with it?

A (Witness Brabb) I'm sorry, but the applause in the next room drowned out the last portion of your question. It looks like we are going to have a problem throughout the day, so we need to maybe have a repeat of the last half. I got that you wanted us to define the scientific method, but beyond that, I'm not sure what your question is.

Q That's just fine. Could any one of you please describe what the purpose of the scientific method is, and what the various steps are in that method?

JUDGE GROSSMAN: Excuse me. Mr. Swanson?

MR. SWANSON: The noise is enough to bother me over here, and it must be very disturbing to the panel. I wonder, before we get into this, can the members of the panel -- are they significantly distracted by the noise?

(Panel members nodding affirmatively.)

MR. SWANSON: I was wondering if maybe by a little bit of shifting, we could put the witness panel over

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here this afternoon.

JUDGE GROSSMAN: Let's take a recess and see if there is anything that the hotel can do about that. So why don't we take a five-minute recess.

(Recess.)

WITNESS JUSTUS: Mr. Cady, in response to your question of defining the scientific method, I would like to ask for a clarification for starters, for this reason:

Defining the scientific method is a little bit like defining a game plan. You've got to know the game, the rules, the objectives, the personnel involved, and so forth. It would help to know if this is a championship game or not, so to speak.

In other words, when you are asking for a definition of the scientific method now, are you asking this in the context of this -- of the science and methods used in this proceeding or in a broad sense, such as would be discussed in a science classroom?

BY MR. CADY:

Q In this proceeding.

A (Witness Justus) All right. The aspects -- the scientific method in this proceeding involves the formulation of a problem or set of objectives. Beyond that is a series of formulations of approaches. It is sometimes possible to establish a set of multiple working hypotheses

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or multiple working prejudices with which one starts to develop a series of reasons for pursuing a particular approach.

Now in this proceeding, the science is both pure and applied, and the methods vary with each application.

In the pure sense, you might say that the method -- a scientific method is established to establish the veracity of previously generated material or existing information, or to find new information.

I think I am perhaps rambling or slowing a little here because the objectives of the definition aren't clear. There is a difference between perhaps a pure scientific method definition and operational definition. Perhaps just with those thoughts, I ought to pass the mike to my colleagues who might be more specific.

Q If anyone could help out Dr. Justus, please do.

A (Witness Brabb) I don't think he needs any help, but I might have a slightly different perspective. For me, the scientific method does start with a problem, focus on a problem, the gathering of information that might relate to the problem, the classification and analysis of this information, the formulation of some hypotheses about how the problem might be solved, and then a testing of those hypotheses to see whether or not they are valid.

ar8-5

Q Do you mean in the context of testing, they arrived at hypotheses, given the information available?

A Yes.

Q I'm directing this question to the members of the panel.

Is it possible to formulate different hypotheses and arrive at different conclusions, given the same set of data?

A Of course.

MR. CADY: I have no further questions.

JUDGE GROSSMAN: Mr. Edgar?

MR. EDGAR: Yes. Before I begin, I would like to read off a list of documents I will be using in cross-examination, so everybody will have them before them.

The first is Licensee's Exhibit 2, Figure --

WITNESS JACKSON: We can't hear you. You'll really have to speak up.

MR. EDGAR: Licensee's Exhibit 2, Figure B-1. That is the so-called Phase 1 geologic investigation. And Figure B-1, we were talking about the other day, or the panel was talking about. It's the log of the T-1 trench.

The next exhibit will be Licensee's Exhibit 6, which is the so-called Phase 2 geologic investigation, and the figure of importance there is Figure B-2.

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The next exhibit is Staff Exhibit 3; then Staff Exhibit 4, which I believe we have in blow-up form on the other side of the room.

The next one is Staff Exhibit 1-B, in particular the USGS report appended to Exhibit 1-B, which is -- and within that report, Figure 13, which is a series of plates, showing soil -- or rather, sediment formation and soil formation processes.

I will then, of course, refer to the Staff testimony and the deposition of March 25th, '81, and then I have two more documents that I'll pass out copies to everyone.

The first of these documents is a memorandum or letter on the letterhead of the California Division of Mines & Geology stationery, signed by Perry Y. Amimoto, District Geologist, dated October 28th, 1977, and entitled Vallecitos Nuclear Facility. I would like to have that marked for identification as Licensee's Exhibit 43.

(The document referred to was marked Licensee's Exhibit No. 43 for identification.)

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MR. EDGAR: The next document is a document, U.S. Nuclear Regulatory Commission, memorandum for William P. Gammill -- that's G-a-m-m-i-l-l, Assistant Director for ST, DSE, from R. B. Hoffman and R. A. Jackson, and

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dated October 31, 1977. I would like that marked for identification as Licensee's Exhibit 44.

(The document referred to was marked Licensee's Exhibit No. 44 for identification.)

MR. EDGAR: And then one final document for reference is attached to, as an appendix, to the so-called 1977 Staff SER, a report from the California Division of Mines & Geology on their review of the Vallecitos geology. I will make a brief reference to that. The 1979 SER.

JUDGE GROSSMAN: I thought you said first 1977, and so could you tell us which one it is?

MR. EDGAR: Okay. It is the 1979 SER. Let me be more specific. Attached to a letter dated September 27th, 1979 from Mr. Denton, Director -- or Dr. Denton, Director, Office of Nuclear Reactor Regulation, addressed to R. W. Darmitzel, Manager, Radiation Process & Product Section, GE, is the so-called 1977 Staff -- 1979 Staff SER.

As one attachment to that SER, one finds the California Division of Mines & Geology report.

WITNESS JACKSON: Is that Appendix D?

MR. EDGAR: I struggled with that.

WITNESS MORRIS: Appendix D, August 16th?

MR. EDGAR: That's correct, Appendix D, and it's

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transmitted by a cover letter dated August 16th, 1979. If anybody on the panel cannot hear me, please let me know.

BY MR. EDGAR:

Q A couple of points of clarification first. Early this morning there was questioning that may have confused or overlapped two separate concepts. There was discussion of Dr. Vesely's testimony on probability analysis and questions about the definition of the term t*.

Secondly, there was questioning concerning the ages of offsets in the trenches.

First, am I correct in understanding that t* represents the age of soils beneath the reactor?

A (Witness Jackson) Yes, that's what Dr. Vesely defines it as in his document.

Q Now am I also correct that the two to four thousand year figure referred to this morning by the geology panel represents the Staff's or the panel's opinion as to the age of offsets on existing shears?

A (Witness Morris) Yes, that's correct.

Q Mr. Devine, there were questions addressed to you --

JUDGE GROSS: Excuse me. Dr. Brabb?

WITNESS BRABB: I have a question, Mr. Edgar. Was Mr. Vesely using the term soil in the engineering sense, which is much different concept of soil than the two to

ar8-9

four thousand foot reference in the upper layer in the trenches? I wasn't clear in my mind whether that was not only a different age, but a completely different use of the term soil.

BY MR. EDGAR:

Q All right. Fine. Thank you.

Mr. Devine, you had testified, and I'll paraphrase it -- feel free to change the paraphrase -- that the likelihood of one meter being exceeded is reasonably high, in your opinion.

I take it that answer was in the context that the likelihood of one meter being exceeded somewhere along the Verona Fault zone is reasonably high. Am I correct?

MR. SWANSON: Excuse me. Could we have the wording read back? I'm not quite sure that I would agree that that's the way Mr. Devine answered it.

MR. EDGAR: Well, he's free --

MR. SWANSON: Okay.

MR. EDGAR: What I'd like him to do is to clarify the context of his answer, and he's perfectly free to restate it any way he wishes, but I want to be sure I understand the context.

WITNESS DEVINE: Yes, I understand your question.

In my use of the expression "reasonably high," it was relative to the sentence in Mr. Morris' testimony

ar8-10

on page 5 which was quoted:

"However, we have contended throughout the proceeding that one meter of surface offset is not a conservative estimate of the total amount of offset that could occur along the Verona Fault."

It was in that context that I said reasonably high.

BY MR. EDGAR:

Q Could I draw your attention to the Staff Exhibit 1-B, Appendix A, pages 25 to 32, Figure 13?

A (Witness Herd) Would you please repeat again? What did you say? Which exhibit?

JUDGE GROSSMAN: Could you identify which exhibit Staff 1-B is?

MR. EDGAR: I'm sorry. Staff Exhibit 1-B is the May 1980 SER, and the document I am interested in discussing is the Survey's Appendix A or the geological report, and Figure 13 is the set of plates that discusses sediment and soil formation.

BY MR. EDGAR:

Q If I could direct your attention to Figure 11.

A (Witness Herd) No. 11, Figure 13?

Q I'm sorry, Plate 11, Figure 13.

JUDGE FOREMAN: Do you really mean Appendix A in

ar8-11

there, or Appendix B?

MR. EDGAR: Appendix B. I stand corrected.

BY MR. EDGAR:

Q Dr. Herd, or whomever else on the panel may wish to address the question, could you, starting with the top -- well, with the section of sediments and soils above the shear, to the right of the shear, starting at the bottom, soils or sediments, could you please identify each layer or each horizon as to its age and characteristics, starting from the bottom up?

A (Witness Herd) Okay. Starting in the block on the right side, that is the upper plate, the lower unit, which has a circle with dipping lines drawn into it, is to represent the Livermore gravels, which are believed to have an age at the GETR site in excess of 300,000 years, probably on the order to 2 to 4 million years.

Immediately above it, which is the sharp contact between the Livermore gravels and the next unit above it is an unconformity which has cut and truncated the Livermore gravels, representing a period of erosion some time between -- well before 130,000 years ago, but presumably less than 300,000 years, by analogy to the age of materials at the GETR site.

Atop of that --

Q Is th- that little triangle, when you talk about

the nonconformity? Is that that little sliver at the top, the Livermore gravels?

A It would be the boundary immediately above the Livermore gravel, everywhere that you see it diagrammed. The little vestige of the uncolored material just immediately in the triangular wedge above the fault, below what is Xeroxed in a very dark color, is unweathered material in the unit which is deposited on top of this unconformity. So there is a material layer, colluvial and alluvial in origin, which extends upwards to the top of the very top edge of that black-shaded part of this horizon. Everything in that black-shaded horizon down to the top of that unconformity is this colluvial unit deposited some time still before 130,000 years ago.

The black-colored material -- pattern, rather, which is red in the original Xerox, is to represent the soil development in the B horizon, the so-called paleo soil, to represent soil development that occurred between 70,000 to 130,000 years, by Dr. Shlemon's investigation.

Immediately above it is a lighter shaded horizon. The boundary between the two patterns and shading is another unconformity which represents a surface of erosion locally referred to several times in this discussion as the so-called stone line. This is the surface of erosion which was active presumably during the period -- near the end of

the last coalesciation, when the climate was wetter, stripping and truncating the buried soil -- the soil below it, which is now buried.

On top of that buried soil, and that stone line, has been deposited a colluvial-alluvial unit in the last 17 to 20,000 or so years, which comprises the material above that to near surface.

Into that material is developed the modern soil profile, modern solum, as indicated, I think, in Roy Shlemon's profile which consists of at the lowest part.-- the lower shaded pattern represents the B horizon of the modern soil.

Immediately above it is an unshaded portion which is the white area, which is to be the A-2 horizon, the A-E horizon also by another designation, and then immediately above it is the A-1 horizon, which is developed into the material which is that portion of the soil immediately beneath the surface.

Q I would like to establish a convention so that we don't have to waste a lot of time with terminology. I will refer to that white area, that strip up near the top, as the A-2 horizon, if that's acceptable.

A I understand that.

Q And then below that, in the dark shaded area, if you wouldn't mind, I'd like to call that the paleo soil. I

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have developed some habit of doing that. And I think that's it for basics.

Oh, and then I'll use the term stone line as a shorthand to identify that surface that one would associate with erosion.

Now, am I correct that if one were to examine the horizons which exist on the left-hand side of the shear, that these horizons would correspond similarly in age, except they are offset in space?

A That's correct, except for just one minor correction. At the immediate top left on the lower plate of the fault, you will notice that there is a thickening in the uppermost shaded horizon there. That represents the increase and enlargement of the A-1 horizon on the down-thrown block because of local erosion along the surface, and accumulation of some material locally on the lower plate since that last episode of faulting.

Q And you would associate that thickening with -- if you compare plate 10 with plate 11, prior to the time at which the plate 11 profile developed, there was a scarp as shown in Figure 10?

A That's correct.

Q And the thickening can be associated with erosion of that scarp and other -- and what else, I suppose?

A Well, there are two processes underway. There

ar8-15

is erosional beveling of the upper block that's underway, and material deposition on the toe of the fault scarp, with accompanying simultaneous soil development into that material.

Q All right. And I will want to get into that. I will ask, after the next series, some questions on the distinction between sediments and soils, if you will allow that oversimplification, but I'd like to get that straight.

A As long as we understand for my purposes that I consider soil the alteration as opposed to just unconsolidated material.

Q Fine. Fine.

If I could draw your attention to Licensee's Exhibit 6, Figure B-2, which is a photograph of the B-2 trench.

end 8

BY MR. EDGAR:

Q Is it fair to say that Figure B-2 represents a typical cross-section of the horizons that characterize the GETR site geological investigations.

A In the area around the GETR, that is true, although that is not the same, necessarily for the soils immediately in the T-1 trench that I recall.

Q All right. With that qualification, simply, just to state it simply, this is more or less the classical horizon that one would see on the GETR site.

A More or less, I think that would be a fair statement.

JUDGE FOREMAN: We are having trouble finding some of these--

MR. EDGAR: It is in the Phase II Report, which is Licensee's Exhibit No. 6. It is at Page B-7. There is a picture of a man standing in the trench.

BY MR. EDGAR:

Q Now, what I would like to do is have you relate the horizon shown in the trench photograph to the horizon that we discussed in regard to Figure 11.

A Okay, fine. Just a second. I think it would be also useful to have the trench log --

JUDGE FOREMAN: You are going to have to help us on that one. The trench log of B-2 is -- Could you give us a page

reference?

THE WITNESS (Mr. Herd): Okay, fine, excuse me a second. I had found it, and an additional document that I had, and I will find the original. It is in Appendix A -- Let me find the page.

Okay, fine, it would be Figure A-9. It might be useful also to have at hand. Tha is page A-21 in Appendix A of the Earth Science Associates Phase II Report, February, 1979.

BY MR. EDGAR:

Q. And that is on Page A-21?

A. That is correct, I just wanted to have it in hand.

Q. I understand.

A. Now, with reference to Figure 11 -- to Photo 11, excuse me. With reference to My Figure 13, No. 11, do you want me to compare what we see in this trench to that figure, correct?

Q. Yes.

A. Just going down?

Q. Yes.

A. Well, if you don't mind, I prefer to go from top to bottom, rather than from bottom to top.

Q. No problem.

A. In the very top portion of the trench is the very dark black-grey horizon, immediately below the surface that is the A-1 horizon, and continuing below that is both a lighter

whiter-colored area, which is both the A-2 and the B horizon, according to the trench log of that area. Immediately below that, is an abrupt sharp contact, which separates that whiter colored horizon which is both the A-2 and B horizon of the modern solum, from the truncated surface atop of the material which is the next older soli horizon developed into colluvial-alluvial units below it. That red horizon there is then the red-colored B-2 horizon of the burried paleosol.

Q That is the rust-colored wedge that you see in the photograph?

A That is correct. That is the rust-colored area and te sharp demarcation line, the top of it, the surface of erosion, is the stoneline. The surface of erosion more or less 17,000 to 20,000 years ago. Below that is the material which you can see gravelly sand and clay, and that is in part material which is alluvial-colluvial material deposited atop of the Livermore gravel, and this material because of the fact that the soil is at least of the order of 70,000 to 130,000 years in age, the material has to predate the age of the soil development, and then some place below, on the lower part of the trench, and I am not sure without looking carefully on the whole of the trench B-2 log, would be the Livermore gravel.

Q Now, this rust-colored wedge, which was -- and the wedge and the two sections of material that can be associated with it, are red in color.

Could you explain the mechanics of how that dstinctive red color develops?

A. Certainly.

The B-2 horizon of this burried soil is interpreted by Dr. Schleman to represent soil development which was quite intense, and which was accomplished during the last interglacial -- 70 to 130,000 years ago, either due to a process of just sufficient time of soil development, or due to intensified soil development, perhaps because of more favorable climate, warmer, whatever. The soil has matured to a degree such that the B horizon, which is the horizon in which clay and minerals accumulate, has become quite prominent. And because of iron alteration, and accumulation, you have a very intense red color to it. So the red color is from the iron and the blacky character, which you see in the face of the outcropping, is due to the clay accumualtion in it.

Q. And this clay accumulat on can be generally -- or can be correlated with generally impermeable character of this soil, is that correct, impermeable?

A. Ues, locally.

Q. Would you draw the distinction, if there is one?

A. Excuse me, I think I know where we are headed with the questions, but go ahead. The material, as far as I know, does not, in this particular area, necessarily cause a perched water-table, or anything of that sort. A perched water-table

would require the ground water move across the surface of it. As far as I know, we have no local evidence that there is perching of water above the surface. There isn't any local accumulation of iron, hardpan, or the like on the top of it. We see in the soil above it, the A-2 and B horizon directly on it, suggesting that there is groundwater perculation down to the top, and I don't know of any evidence that we have seen in any of the trenches to indicate that there is lateral migration along the top of the surface. Had the A-2 horizon been immediately on the surface here, or -- and not separated by a B horizon, we would have had good evidence that there might have been local leaching, but that it is not.

Q. Would you explain the process by which the A-2 horizon develops?

A. Fine.

The A-2 horizon is a peculiar type of horizon in soils. It can form in a variety of ways. The A-2 horizon is one which is characterized by elluviation or removal or leachable ions and materials, such that you have it effectively bleached, giving it a white color, characteristically. An A-2 horizon is formed in some conditions by actually chelating of leachable ions, and the like, such that they are surrounded by plant resins and the like, such that the types of soil and -- excuse me, types of vegetation cause these horizons to form, because they permit the leaching out -- preferential leaching out of

these ions and materials. Sometimes this is quite spectacular, for example, in podzols, this A-2 horizon is quite prominent. In fact, podzol, these white soils that you find in forests, gets it name from a Russian word which means ash soil, because of its white color. And also, sometimes, these A-2 horizons can form by concentration of groundwater and movement along a surface, just like what we just mentioned.

Q And what is your best judgment, if you will, as to the origins of the A-2 horizon as we see it, in for example the B-2 trench?

A Last night I went back and checked on the soil conservation literature, just to be sure of this answer:

The soils that we see here were originally mapped as part of the Positas series of soils, and these bore and older classification name, called planesols, such that soils developed in the alluvial materials of this general character are typified by a A-1 horizon, the dark black-grey unit on top, the A-2, the white, ashen layer in between, and the B horizon below it.

I don't believe that there has been a successful identification made yet, in California, for the origin of these soils, but they are widespread, for example, in the Livermore Valley, and they are certainly not unique to situations where there is groundwater perching of the like that are found in regional areas. For example, I had a number

of trenches -- I had at least three trenches open of my own in the Livermore Valley, in 1975, and we saw these very types of soils elsewhere. They appear to form, I suspect, probably as a consequence of vegetation and acidity in the ground.

Q. Dr. Herd, I guess I would like to try to define not only the mechanism of how they form, but where they form, where do you associate them in depth and time?

A. Could I just refer a moment to the Soil Conservation description of the soils?

Q. Sure.

A. I think that might provide a very good way to describe it to you.

I broke my glasses on Monday, and trying to hold them on during the course of this meeting has been a task.

Q. We will bear with you.

A. Okay, fine. I am going to refer to Soil Survey of Alameda Area, California, which was published by the U. S. Department of Agriculture Soil Conservation Service; this would be Series 1961, No. 41, and there is a description on Page 67 of these planesols, and perhaps if I just read, both for my benefit and yours, we can just answer that question for you. This is beginning under the section "Planesols":

"In the Alameda area, the Cotati, Positas, and San Ysidro soils are Classified as planesols. Planesols are an intrazonal group of soils, with an alluviated surface horizon

-- that would be the A-2 horizon -- underlaid by a B horizon, more strongly alluviated." In other words, where there is more concentration of the material that is the B horizon. --"cemented or compacted than that of associated normal soils. They develop upon a nearly level upland surface, under grass or forest vegetation, in a humid or sub-humid climate. The San Ysidro and Positas soils lack models in the A-2 and B-2-T horizon are better drained than the Cotati. They also have some properties thatresemble those of the strongly developed non-calcic-ground soils..." Well, it continues on with another definition.

Then, if I may, I will also then go on to the next point, to talk about soil formation here:

JUDGE GROSSMAN: We will take five minutes here.

(A short recess was taken.)

JUDGE GROSSMAN: Dr. Herd, I believe you were in the middle of your presentation.

WITNESS HERD: Thank you for the moment of time to fix the glasses, as well as to find the part of the reference that I was interested in. Thank you.

I would like to continue to read, just a further discussion of the soil formation, particularly of this A-2 horizon, and in particular, I will continue to read out of this Alameda County Soil Survey volume, and in this particular instance, I will read under the section entitled: Major

Processes of Soil Formation, beginning with the second paragraph on Page 64:

"As drainage in soils improves, the excessive amounts of salts are leached. The sodium clays, which are easily dispersed, are puddled around together. Some of the colloids also move downward from the surface layer, and accumulate in the layer beneath. After a long period, most of the fine material has accumulated in the B horizon, and mostly silt and sand are left in the A horizon. Weathering of minerals in the B horizon also contributes to its high content of clay. If leaching continues for a long time, the A horizon of the soil finally becomes acid. B horizon becomes very slightly acid to strongly alkaline, and has well developed collumnar structure. This horizon underlies a distinct leached A-2 horizon. The soil generally has hog-wallow relief. Apparently the A horizon has risen, and the B horizon is exposed in many places. Locally, the shallow depressions are called "slick spots."

So to summarize, as I understand the soils process of formation in Livermore Valley, and in general, a soil profile develops first, as basically just in a pristine alluvial-colluvial material. First you have the accumulation of organic material at the surface, because of vegetation, rootlets, and the like, which gives it a grey cast. Because of the organic -- Because of the action of rainwater on to the soil, and given time,

material and chemical weathering proceed to work lower in the profile. The A horizon begins to increase in size, and there is a downward movement and an accumulation of material below it, and a new-forming horizon, the B horizon. As this process continues to develop, then intermediate between the darkened A-1 horizon and the B-2 horizon is this intermediate alluviated A-2 horizon. These three horizons, then, as time passes, continue to enlarge in size and grow together, deepening downward. At some point in the future, they reach a stability, when the horizons don't seem to increase relatively as quickly in size and depth, but tend to intensify in their development in colors, many times.

For example, the B horizon in modern soil, below the A-2 horizon, is light colored. This contrasts sharply with the B-2 horizon in the burried soil, which is quite red and very well developed.

BY MR. EDGAR:

Q. You indicated that there were two alternative processes that one can associate with development of the A-2 horizon.

Is that correct?

A. That is correct.

Q. And there--

A. Among others, but those -- we talked about two, right.

A. Right.

Q. And is it possible that the A-2 horizon finds its location where it does, because of the impermeable clay in the so-called burried paleosol?

A. In some areas, yes, this is true, although I again call your attention to my comment I made earlier, if I understand the logging of the trenches before, by Roy, and in our examinatin as well. There has always been an intermediate B horizon above the stoneline, but below the A-2, which would indicate that the A-2 here isn't controlled by groundwater percolation immediately on it, because there is clearly no apparent groundwater stagnation. Water must be moving downward in the profile to allow for material that would be moving out of the A-2 to accumulate below it in the B-2.

Q. Now, one more point of reference:

If you could turn back to Figure 13, Plate 11, in Appendix B of the May SER, Staff Exhibit No. 1-B, and likewise, turn to the trench photograph in Figure B-2 of Licensee's Exhibit No. 6.

A. Right.

Q. What I would like you to correlate is the red -- distinct red clay wedge or triangle that you see in the B-2 trench, associated with the burried paleosol unit.

Could you correlate that with the corresponding unit in the Figure 13, Plate 11 cross-section?

A. Diagramatically that is the attempt, although there is not one-to-one full correspondence, okay?

So you want me to do it, or...?

Q. Yes, could you? Would you?

A. Okay, fine.

In the photograph on the right, on my right; that would be Figure B-2 -- If we could, we will just sort of look at the area where the fault has caused the offset of the burried soil, and we can work backwards in the soil profile, because that may help you understand its relationship to this diagram.

Among the gentlemen, who is not Mr. Harding -- I can't remember who it is -- there is the bright red burried soil, the B-2-T-B. Above that is the whiter-colored material, which is the A-2 and B horizons of the modern solum, and that grades upwards into a greyer zone, which is the A-1 horizon of the modern soil. But notice that as you continue in a verical direction, there is an immediate abrubt change in color back to the red. Now, what is happening here is that the burried soil that has been thrust out and crossed over a top of the modern soil, such that you have old soil juxtaposed upon modern soil, giving this reverse order relationship which seems to be apparent here.

Similarly, in the diagram that I had in Figure 11, you can see that same sort of relationship, if you look to the

area in the lower plate that is below the fault to the left. Coming up from the bottom in this diagram, just like on the right side there was the Livermore gravels, a surface of Alluvium, then the very dark-colored horizon, that is that burried B horizon, another surface of erosion which separates it from the B horizon of the modern soil, which is lightly toned, the whitish A-2 horizon, and then immediately above it, the fault plane, with some gravel, which you can see, if you look at the photograph, as well, and then back into the burried B horizon of the soil, which has been thrust out across the other one.

The thing that I would note is that in my diagram, the A-2 horizon has been enlarged for emphasis here, as opposed to the one on the right, interms of the real world. That is the picture over here of B-2. The A-2 and B are much thinner in actual profile development. They have been enhanced in my cartoon, to help show them, and help screen them.

Q That is just for the purpose of illustration.

A That is correct.

Q Okay.

Now, a couple of basic questions, to establish some time-space reference points:

If you could turn to Plate 9 of Figure 13?

A Okay.

Q Now, am I correct in understanding that the shear

stops at the stoneline?

A. Yes, unfortuante, in terms of the xeroxing, but it does indeed.

Q. And that would be below the point -- or the lighter colored layer, which is labeled as the A-E or A-2 horizon, on this diagram.

A. Well, it would be even more than that. It would actually be below the B horizon. Remember the B horizon is in the modern solum, in the material which rests directly atop of the stoneline. So it is at the base of the B hroizon, where the stoneline is located, at which point the edge of the surface of the fault is truncated.

Q. Now. if one proceeds down from that point where the fault stops, if you will, along the plane of the fault, one sees an offset of the burried paleosol, is that correct?

A. Yes, that is correct. It is digramatically handled there, and there is some thinning of it as well.

Q. All right.

If one looks at this diagram, what conclusions can one draw, assuming that this was one saw, what conclusion would one draw as to the age of last movement in this system, along the fault?

A. Okay, from this relationship, one would conclude that the age of last fault movement must predate that stoneline--

Q. Which I understand is--

A. --is -- well, by inference, we are saying it is 17 to 20 thousand years, more or less at the end of the last glaciation. Roy and I have differences as to what timeframe that may actually represent, but for purposes of our discussion, I am satisfied with 17 to 20 thousand years.

Q. Okay, now if one were in the field and saw this in a trench -- This is a hypothetical -- what surfaces would one employe in order to make the offset measurements here, if one were seeking to determine the amount of offset?

A. Well, because of erosion coming into the problem here, it become difficult to -- You cannot use the top of the soil, because it has been stripped off, and at best, you would look for other horizons which would help you determine the amount of apparent offset. One certain way would be to look for an erosionaly unconformity, which would separate -- or some sort of material boundary, which would be continuous on either side, and try and measure on that offset, or -- Well, that would really be the best.

Q. But one would go, I take it, to the bottom contact of the burried paleosol, is that--

A. In this instance, not, because the problems that exist there, in terms of using the bottom of the burried paleosol is that you really don't know what was the actual full amount of development in the soil. It might have had some sort of irregular lower boundary. It would give you an

approximation, but not necessarily an exact determination.

Q But if you wanted to approximate the situation here, you would measure the offset at the lower paleosol contact.

A At the base of the development of the lower soil.

It is a technical question, which is bothersome to me, but I feel I should make it: Soil profile horizons are quite -- usually, most distinct at their upper boundary, and become gradational to their lower boundary. So as a consequence, when you are looking at the lower part of a B horizon, it is a very, very obscure gradational boundary, rather than something abrupt and linear and sharp. So it becomes very difficult to pick it out. So that is why I would be very, very hesitant to use the lower part of the burried soil to try to measure the offset.

It is a technical question.

Q All right, but presumably, one could approximate it.

A Crudely, perhaps.

Q Okay. And one can also associate that approximate movement with an interval of time, in this particular model, I assume.

A Yes, if you had -- Well, you mean you would have a relative chronology.

Q All right.

A Not an absolute, necessarily.

Q Assuming that I measured the quantity "X" at the

base of the paleosol, recognizing your qualification on accuracy, but assuming that I measured the quantity "X" then I could conclude that I have moved "X" feet in 17 to 20 thousand years, based on this particular profile.

A. No, no, no. I think you have just used numbers incorrectly.

Q. All right.

A. What you would have concluded is that based on this diagram the movement is older than 17 to 20 thousand years, and then that the movement is bracketed between that -- As an upper limit, it is 17 to 20 thousand years. The lower limit would be the age of formation of the burried soil, whatever that may be.

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Q And can I associate any of that movement with the lower soil contact, the Livermore gravels, if I wanted to bracket the time interval in which this happened?

A Well, in a crude sense, yes. I'm just trying to be more specific. So let's just make a reference so we can have a more comprehensible discussion. For example, here in this trench, assuming that this buried soil is the soil developed 70,000 to 130,000 years in age, then we would say that the movement that we see represented here is younger than the age of that soil and it was younger than 70,000 to 130,000 years, the offset is younger than that, but older than 17,000 to 20,000 years.

Q Okay. Fine. Now if in the same diagram let's assume that I looked at Plate 9 and hypothetically I changed one fact and that is that I observe no offset at the base of the varied paleosol, okay, that my shear runs through the Livermore gravel and stops at the base of the varied paleosol. What conclusion would I then draw about the age of the last movement? To an approximation.

A Okay, let's repeat it again. That you see no evident displacement along the bottom of it, yet the break continues into that material.

Q No, I'm sorry. I didn't give you the question correctly. I have a shear in the Livermore gravels extending upward, but I stop the shear at the base of the varied paleosol

contact and it does not continue into the paleosol.

A Okay, we are talking about one of the previous illustrations in this figure?

Q Yes. I think Plate 5.

A All right.

Q And I just want to establish what conclusion would I draw then about the age of the last movement.

A From Plate 5 relationships described, that is, where we have an evident fault offsetting the Livermore gravels, a surface of erosion which has truncated the fault and the surrounding Livermore gravels and overlain by an apparently unfolded alluvial - colluvial unit in which this soil is developed and this is to be the varied soil? Okay. Then we would say that at this locality the faulting has to be younger than Livermore gravel, which would be, as I say, perhaps on the order of 300,000 years to 2 to 4 million years in age, but older than the age of the development of the soil horizon, which in this case would be 70,000 to 130,000 years.

Q Thank you. Okay. Now let's go back to Figure 11 and if you look at -- I'm sorry. Figure 13, Plate 11. I keep confusing it -- now in that diagram we see the shear extending through the Livermore gravels, through the varied paleosol, and my copy is indistinct, but I assume the shear passes all the way to the top surface of the A-2 horizon. Is that correct?

A That is correct.

Q Okay.

A And into the A-1 horizon.

Q All the way into the A-1?

A Yes.

Q Okay. And the reason for that is, if you go back to Plate 10 you see the shear offset all horizons and then you eroded it back down.

A That's correct.

Q Okay. Now if I have the Figure 11 model and I've got my offset A-2 horizon, what is the age of the last movement?

A The age of the last movement is in essence one which is quite recent in time, by my understanding of here. It's at least as -- it can be no older than the age of formation of the modern soil profile that includes most prominantly the A-2 and the underlying B horizon. And in the scenario of events which I have just described to you, that to me represents a profile that is forming under the modern environment and so, as a consequence, it is one that has been maturing over the last thousands of years and then this displacement has been one that postdates that quite recently and then there has just been some minor modification. We don't see any relocation of profile development. So that is where I came up with the conclusion that the age of last offset is quite recent in the trenches.

Q And that's where we have used the 2,000 to 4,000 year interval?

A Well, let's be specific in terms of where the 2,000 to 4,000 years come from. Roy, Dr. Schlemon, sampled materials for radiocarbon dating from trenches, I believe B-2 was one of them, and dated particular horizons. It is from these soil ages that we have indication of the relative ages of these horizons. They appear to have radiocarbon ages of 2,000 to 4,000 years. Now Roy has preferred to say that an age calibration must be made to make them look older. He thinks that there is modern root contamination in these ages. But we see no -- I personally don't see any reason to require this 50 percent increase in contamination to do that.

Q Well, all I wanted to get at was if you have the A-2 offset then I take it it is your view that one can associate that offset with movements within the last 2,000 to 4,000 years. Am I correct or not?

A Well, here in this case because of those C-14 ages that we had that suggestion, right. I think it is very recent. That is correct.

Q And am I using the correct time interval that you would propose?

A Yes. But I -- go ahead. Excuse me.

Q If you want to add anything, go ahead.

A No, I just wanted for the completeness of the record

that Roy had interpreted an age of something, I believe of 8,000 years for the A-2 horizon.

Q All right. Now let's assume now that I have this model here in Plate 11 and if I look back to the sequence of events here, I have started all the way back on Figure 4, where I had a fault in the Livermore gravel, and then I go to Figure 6 and the Livermore gravel is offset and the paleosol is thrust up. Then you have erosion, an age of colluvium and alluvium. In 10 we get another offset.

A That is correct.

Q So I take it this model that is depicted in Figure 13 contemplates two episodes of movement within its -- I take it the model reflects or illustrates two episodes of movement.

A Since the formation -- since the deposition of the material into which has formed this buried soil.

Q Okay. Now --

A At least two. This model contemplates two. Right.

Q Right. I'm not talking about the trench. I'm talking about this particular model and what physical events you are illustrating.

A Right.

Q Now we go back to Plate 11 and we can -- is it correct that we could measure an offset by reference to the upper surface of the lefthand A-2 horizon and the upper surface of the righthand A-2 horizon?

A That's correct. The upper surface would be the better of the surfaces, correct.

Q On each contact?

A Right. Assuming that there hasn't been much alteration of the materials. The better measurement is the erosion of the surface below at the stone line.

Q Okay.

A Because that doesn't travel in time. It is always constant. In other words, if I may just make a point, because soil development proceeds through time these horizon boundaries are also moving through time, as opposed to an actual physical stratographic contact which is secure in the stratigraphy of the earth. In other words, it remains motionless while the horizon boundaries proceed to move downwards. Given time, for example, the B horizon in the modern soil may actually develop through that stone line. It hasn't yet.

Q But as of the present time, the stone line is the more physically discrete and stable point for taking a measurement.

A That's correct.

Q All right. Now assuming I did that, then if I measured 'y' feet then I would be able to conclude that I had moved 'y' feet in a period of how many years?

A You would conclude that the 'y' offset has occurred since the age of the formation of the paleosol -- excuse me.

Let me try it one more time. You would conclude that the displacement has occurred certainly since 17,000 to 20,000 years and, more appropriately, since the actual formation of the soils which it displaces.

Q Now if I also went in the same diagram and I went to the lower contact of the buried paleosol and made a measurement of that offset, okay, which I will call 'z' feet. I am looking at the boundary of the lower contact of the buried paleosol in the Livermore gravel here. What conclusions would I draw if I measured 'z' feet as to the time period in which 'z' feet of offset accumulated?

A Let me be sure I've got the 'z' and 'y' correct.

Q Okay.

A Okay.

Q 'Y' is the stone line.

A Right.

Q 'X' is the A-2 --

A Let me just annotate this down.

Q Sure.

A Okay, 'y' is the amount of offset of the stone line?

Q Correct. And 'x' --

A Just a second. (Pause) Okay.

Q 'X' is the amount of offset of the A-2.

A Okay.

Q And 'z' is the amount of offset of the lower surface

of the buried -- is the amount of offset of the buried paleosol measured at the lower contact.

A Okay.

Q Now if I took my measurement of 'z' along the line of the fault, what conclusions could I draw about the time period in which a displacement 'z' had accumulated along the fault as measured at the lower paleosol contact?

A Okay, 'z' would be a cumulative number which would show -- well, in this case 'y' and 'x' would be presumably equivalent because we are saying that the -- well, not equivalent. More or less in terms of the relative degree of amount of offset. They may be slightly different. And then 'z' would be the actual component let's say of 'y' plus the amount of movement that occurred in the interval between the stone line formation and since the development of the buried soil. So there is at least two offsets represented in 'z' measurement.

Q Okay. So when you take 'z' you've got two offsets and the time period that we can associate with that is -- is a time period between 17,000 and 20,000 years and 70,000 to 130,000 years?

A No.

Q Okay. Correct me on that.

A Okay. 'Z' would represent a displacement amount which would be younger than 70,000 to 130,000 years and

slightly older -- certainly prehistoric, but of the order of the age of the soil which it offset, which we have been talking about 2,000 to 4,000 years. So that is the range of that offset. In crudest form, 2,000 to 4,000 years on top and at the lower boundary since 70,000 to 130,000 years.

Q Okay. Now I don't want anybody doing mathematics on the witness stand, but you are talking about a range in the neighborhood of 68,000 years to 126,000 years, I assume.

A If I'm not allowed to do the math, I guess that sounds crudely right.

Q I don't want to have anybody do the math on the witness stand but that's a ballpark --

A I appreciate that.

Q For practical purposes, we are talking about a time period in the range of 60,000 to 120,000 years.

A Tens of thousands of years.

Q Okay. Now could we take a look at Staff Exhibit 4? There is a good blown-up copy of it over here.

A Okay. That's the one on the wall that we have used. Shall I put it up on the easel? Do you wish me to stand and with reference to it?

Q I just want to mark a couple of points so we get a frame of reference for discussion here. If you wouldn't mind doing that.

A No, I would be happy to.

(Pause)

MR. EDGAR: Now we've got it labelled and we are working with Exhibit 4.

BY MR. EDGAR:

Q If you can, and I am interested in your views, what is the -- we have this dotted line at the top which I will identify as consisting of the wavy line between Point 10, 2, 3, 4, 5, 6, and 7. It is a dashed line.

A (Witness Herd) Correct.

Q Okay. What age would you associate with that line?

A If by analogy elsewhere to the other trenches, this might well we -- and we have talked about it before -- the probable stone line, the surface of erosion, perhaps, of the order of 17,000 to 20,000 years.

Q Now I'd also like to identify one more reference point here. You have now marked at my request two lines at the bottom of the diagram which are dashed lines. You have marked them X, X, an upper line and a lower line, both marked X. Is that the lower contact of the so-called buried paleosol?

A Okay. There is an important distinction to be made here that I think we had better stop on.

Q All right.

A Okay. I'm not sure that we can make an equivalence one to one now of this buried paleosol with the ones that we have been talking about elsewhere. It is my recollection that

this buried soil had a very black, thick massive character to it, one which is totally unlike the character of the very bright red buried soil that we saw elsewhere. The point was asked are we really sure it is even a buried paleosol. We are not sure because, as far as I am aware, the trench log doesn't really give us that clear definition of that to call it a buried soil. In fact, there are no soil horizons, as best I can see, identified in this log. We are inferring and trying to reconstruct.

From my recollection, I seem to recall that this unit above the fault to the right, for example, here above that contact where this hatched pattern appears, was a black massive dark clay. I think that's right. I'm trying to recall that. I'm not sure, though.

JUDGE GROSSMAN: To keep the record straight, when you say the question has been asked, that was asked by someone on the panel, I guess Dr. Brabb.

WITNESS HERD: Right. He was wanting to make sure that I was trying to remember what I had seen in that trench and I think that is a good question to jog my mind. But the question I did want to direct myself to was the certainty of equivalence of these soils in age. I don't know if they are the same. The soil that we saw here, if it was this black organic rich material, could well have been a boggy-like soil that had formed in a depression of some sort and it could have

formed in an interval totally unlike the interval represented elsewhere in that buried B-2 T soil, the one that has an age of 70,000 to 130,000 years. If the stone line is correct, we know that in terms of age chronology it has to be at least 17,000 to 20,000 years, but how much older than that, I don't know.

JUDGE GROSSMAN: Dr. Brabb, could you comment?

WITNESS BRABB: Yes. I would just like to state for the record that I, too, am not convinced that that horizon is due to the paleosol and I certainly would share Dr. Herd's reluctance to make it equal to the other paleosol.

WITNESS HERD: Yeah, that is an important point because age equivalence, age of soil development, was so unlike what we saw elsewhere, if I recall correctly, that it is really necessary to have some sort of indepedent radiometric control to be sure.

BY MR. EDGAR:

Q But the only -- correct me if I am wrong, but I did gain the impression that you thought there was at least one benchmark here and that would be the so-called stone line.

A (Witness Herd) Yeah. Right. I think that that is a possible fair equivalence, although nowhere in all the course of the investigation has it been demonstrably proved that erosion surface is everywhere time synchronous, that is, of the order of 17,000 to 20,000 years. It has been a working

assumption that has been made. It would be nice to have some radiocarbon ages to prove that. Roy had envisioned a -- Roy Schlemon -- had envisioned a series of events which he felt would result in the formation of the stone line and he felt that this would require a wetter interval and since in our own discussions that we recognize that the last glaciation was a wetter time than it is today, he felt that this period of landscape instability would be of the order of at least of the end of the last glaciation, of 17,000 or 20,000 or perhaps as young as 10,000 years, from my vantage point.

But to be sure that that is everywhere time synchronous and represented here as well is not clear.

WITNESS BRABB: Can I ask a question, Mr. Edgar? Was your question whether or not we were fairly confident that there was one identifiable horizon in this trench, namely, the stone line, but that you didn't mean to necessarily imply a correlation with the other trenches in terms of the identification of that as the same unit, which was the problem Dr. Herd was discussing? Do you see our difficulty?

MR. EDGAR: Well, why don't you explain it, because I'm getting confused.

WITNESS BRABB: I thought your question was related to the identification of the line on that chart --

MR. EDGAR: It was.

WITNESS BRABB: 6-7, 10-3, and at least the 4-5 line.

MR. EDGAR: Right.

WITNESS BRABB: And Dr. Herd then digressed because he had a hangup with the word "stone line" and the possible implication that this stone line is the same as the stone line in the other trenches. There is both an identification problem, no. 1, is it a stone line, and the answer to that is yes in this trench; the second part of the problem is a correlation problem, is the stone line in this trench the same as the stone line in the other trench and therefore does the model that Dr. Herd has work in both places.

MR. EDGAR: And then I suppose there is a corollary question.

BY MR. EDGAR:

Q And that is whether the soils -- in your view, I am asking -- whether the soils in Trench T-1 correlate with that in the other trenches.

A (Witness Herd) Certainly, and that is a very important question in terms of the -- if this is the buried soil beneath, the actual age of it. Because if it is, as I recall, a black, organic, boggy-type soil, that is the setting in which I envision the interpretation of it, the soil appeared to me, if it was that, to be one formed in a poorly drained condition such that it could have formed in wet times.

Well, we have looked at the B-2 T horizon, the red one, to suggest perhaps interglacial times when it is warmer

and dryer and longer periods of soil formation. There is nothing to preclude this other soil, if that's indeed the case what it was, to have formed in the last glaciation, between 70,000 and 20,000 to 17,000 years ago. In other words, totally out of phase in time and perhaps much younger than the soil which we have elsewhere called 70,000 to 130,000 buried paleosol.

Q And you are suggesting the possibility that but I assume that there is no positive evidence to indicate that.

A Certainly there is not, but there is every reason to suspect that it probably is not time synchronous because of the fact that it doesn't display the same sorts of degrees of soil development.

Q And if it were asynchronous it could be off in either direction, I assume.

A If it's asynchronous?

Q You were talking about time synchronous. Perhaps what we are suggesting is that your inability to correlate with the other trenches suggests that the times could be off in either direction.

A Well, I would tend to think it is more likely to the former, to the front, to the more recent past, than to make it much older. Because remember, we are talking about an apparent surface of erosion in part along this contact and the older that you make this unit below it, the more material

you are going to have to erode because this is that surface of erosion, the only one that separates the most recent alluvial-colluvial deposition from the material below it. And we don't see a vestige of a buried red soil or something like that in here, if that's what I remember correctly. So I am of the impression then that it is probably at the very least at the same age of the other one elsewhere or younger than that, and probably younger.

WITNESS BRABB: If I may make a comment just at this stage. I realize this is a long and difficult and complex explanation, but it does relate to one of the reservations that we stated in our report, and that is that we felt that there were not sufficient number of ages developed in the dating of these deposits that gave us a degree of confidence that we would like to see in the interpretation. And this is one of the examples of that, where it would have been helpful to have it.

MR. EDGAR: All right.

BY MR. EDGAR:

Q Having established at least a point of reference for discussion, do you agree that it is reasonable to place more weight on direct measurement rather than interpretations or reconstructions when dealing with the question of offset?

A (Witness Herd) Certainly.

Q And would you agree that the purpose of the T-1

Trench was to define the absence of presence of faulting and not the size of offset?

A As far as I know, that was the purpose of almost all of the trenches at the GETR site.

Q But at the time you and other geologists were out in the trenches is it fair to say that no one had the size of offset in his mind as a primary issue?

A For me in my recollection of the issues that were underway that were important to look at this trench were first and certainly foremost was the existence or absence of faulting; secondly, the style and nature of faulting; and third and most importantly as it related to the issue of safety issues since the NRC Staff had asked us to go out and look, was the question of recency of age of last offset. And it was to those ends that we did cursory looks and made the brief examination that we did.

Q I see. And so the last point that you mentioned in your answer had to do with the age of the offset, is that correct?

A As I recall it, we were to go -- we were trying to look in on this first glance for me to see what was the probable age of last offset, was it quaternary, in the last 2 million years, more specifically, was it holocene, less than 10,000 years, since these had real meaning and importance to the NRC staff in terms of age categorization of recency in

activity of faults.

Q Okay. Now is it true that the five foot offset interpretation which you explained yesterday is not a direct measurement?

A That's correct. We're inferring it from the trench log. Let me make a brief comment in terms of the time that I visited that trench. When I visited the trench I believe it was on a Saturday or a weekend of some sort. We came in, we walked through the trench and we were taken as well, I believe, up to Trench T-2 on that same day. In the trench, the walls hadn't been completely cleaned, I don't think, and logging hadn't really begun and in fact we were -- I remember picking at the walls with a rock hammer or something, a shovel end, trying to look at and trying to map out some of the soil horizon contacts. It is in that stage that I recall that we saw an evident offset of this white ashen unit, which I believe at the time was worried to be a colleachy horizon. Then in subsequent trenches we have seen elsewhere which has confirmed repeatedly that it appears to be this simple A-2 horizon of this planesol.

Q If you are more comfortable sitting down, you are welcome to.

A I am more comfortable standing up.

Q Okay. Either way.

A This is my operating mode. Thank you.

Q Am I correct in understanding that the only direct measurement in the [illegible]-1 trench was approximately a 15-foot displacement measured at the lower contact which we have labeled on this drawing X-X?

A The measurement that you are referencing is one made by Mr. Morris and I am not sure exactly where it was. He made that measurement. I am not sure.

WITNESS MORRIS: To the best of my recollection, that was the location but it was not an absolute measurement using a yardstick or ruler of some kind. This was an estimate of dimension in the trench.

BY MR. EDGAR:

Q And do you agree that GE's consultants actually measured that offset at that lower contact or do you know?

A (Witness Herd) I do not know that.

Q Okay.

A It is reported in one of the tables in Mr. Harding's testimony, but I don't remember seeing it mentioned in any of the other discussions before.

WITNESS MORRIS: May I add to my response on the former question? Realizing that we did not have the logs with us when we looked at this trench, I would have to go back in my mind and say all right, this is the unit I think I saw and that is the offset. So in that respect, what you have marked as X is what I recall seeing in the field.

MR. EDGAR: Fine. I recognize the time interval affects memories.

JUDGE FOREMAN: Excuse me. I have one question relevant to that. Clarify for me, in view of the discussion about the ambiguity of the nature of that X layer, is it fairly certain that the discontinuity of X, the discontinuity of the shear on the right, represents a discontinuity from the X layer on the left? Is it certain that those are the two same boundaries?

WITNESS HERD: I think from the logging of the trench that would be the inference in terms of that, looking at the --

JUDGE FOREMAN: In view of your ambiguity about the nature of the soil and so forth I was wondering whether you were ambiguous about whether that was truly an offset.

WITNESS HERD: I certainly recall a most dramatic offset in the lower portion of the trench which was much larger than that which I was seeing in the modern soil, the A-2 horizon. So I seem to recall a whiter, colleachy-like material in the lower part of the trench which was juxtaposed against this black, organic, rich soil. That seems to be the crude recollection that I have. And that offset seemed to be enormous when we were looking at it.

But unfortunately, I didn't have an opportunity to ever of course have this trench log with me in the trench to check the relationships and my understanding of what they

represent, because the trench log came out well after the trench was closed.

WITNESS JACKSON: I would like to add just a brief response. Dr. Herd has properly characterized the need and the intent of the examination of the trenches that was made on that day. It was with the focus of mind as to whether there was probable or capable faulting around such that you might need to take licensing action. I do recall difficulty in correlating units across the fault zone, identifying one unit equivalent to something on the opposite side of the fault. And it is because -- I'm not sure the Board can really appreciate it -- trenches go through different stages of aging. We chip them off, they change a little bit in nature as they are chipped off and as they dry out. So you can see different things based on this.

So I think you have to be a little cautious in correlating across the fault one unit to the other. I don't have any reason to doubt what's in the trench logs, but I think you have to be cautious on that point.

WITNESS HERD: Can I make two comments?

JUDGE FOREMAN: Yes.

WITNESS HERD: I want to amplify the comment that Dr. Jackson has made. This A-2 horizon is a difficult beast to see in the field. It is one that shows up best after several days of drying out of the soil. It seems to intensify.

Because it is bleached, when it is wet it doesn't stand out quite evidently in the wall of the trench. So that that picture that you see here in Figure B-2 that we have talked about before, that in white-collared A-2 horizon, on the first minutes of having the walls of the trench opened and the first days, you generally don't see it. It has to mature and dry out before it becomes clear.

Similarly, I recall in this trench that part of that A-2 horizon was quite prominent and then, as you would pick at it, you would literally make it disappear because it would get into the fresh part of the material, the wetter part, and the white collar would just no longer be visible. So that you couldn't really pick out that evident contact anymore where that A-2 was. So it was very difficult to try and follow the A-2 because, as you tried to clean the wall or the face to follow it you destroyed the evidence for it.

Secondly, I want to just make a point, this enormous offset that I was saying. It was enormous to me because I didn't realize how much offset that we would probably see. But it was something of the order of 10, 20 feet, in my recollection. Certainly of the order that Mr. Morris has reported.

BY MR. EDGAR:

Q Okay, and what time period? Let's ask the hypothetical question. Let's assume that the X - X represents the lower paleosol contact. Then what time period would you

associate with the occurrence of the total cumulative offset of let's say 15 feet?

A (Witness Herd) Assuming that the cumulative measured offset of that contact, assuming it is the base of a buried soil, 70,000 to 130,000 years old, would be an offset which would be younger -- that has occurred since that time, up to the episode of the last displacement and including the episode of the last displacement. So in essence, the last 70,000 to 130,000 years.

Q Okay. So for practical purposes, we say that the 15 feet of movement occurred over a period which is approximately 70,000 to 130,000 years, is that correct?

A Assuming that soil were that age.

Q Yes. And we are assuming. The hypothetical I gave you is that the line X - X on Staff Exhibit 4 is the lower paleosol contact.

A That's correct. Okay. I understand what you are talking about.

Q Now you indicated that the A-2 horizon is difficult to see in the field.

A It is not difficult generally after the walls of the trench have been cleaned and they have been allowed to mature and dry out. Then it becomes sometimes the most evident thing that you see in the walls of the trench as you enter it.

Q Okay. Let me try to place a time line, if you will.

Am I correct in understanding that the visit to the T-1 trench by the NRC Staff and USGS Team occurred on October 22, 1977?

WITNESS JACKSON: I'd like to make one comment. There was a trip the week before that, the Saturday before that. I remember these trips quite well because I asked to go on them at 3 o'clock on Friday afternoon in both cases. So there was one one week before that. We examined the trench. It was a bucket, back-hoe type trench which had effectively smeared out the clays alongside of the trench and had made it not easy to see those features. So we came back, we asked that they be cleaned off better and allowed to dry out and come back a week later. That was the 22nd.

BY MR. EDGAR:

Q The second inspection was the 22nd, am I correct?

A (Witness Jackson) The second inspection was the 22nd.

Q Okay.

A (Witness Herd) Before the show cause order was released. That's my recollection in terms of time chronology.

WITNESS JACKSON: Yes. I wanted to add one thing. Drs. Herd and Brabb did not make that visit the week before. It was just Dr. Morris and I. And others.

BY MR. EDGAR:

Q The five foot offset interpretation that you have developed is a reconstruction of the offset based on the

trench logs and not a direct measurement, is that correct?

A (Witness Herd) That is correct.

Q Now the original interpretation or reconstruction which you performed -- and by that I mean the one explained in the March 25, 1981, deposition -- was done at sometime between August,1979, and April, 1980, is that correct?

A That's correct.

Q And am I correct in understanding that the exact date of the interpretation cannot be defined more closely than that from present memory?

A That seems right, because it was performed at the time when we started to put together the April, 1980 -- that would come out in April, 1980, our summary of faults at the GETR site.

Q Okay. So at some time prior to the April, 1980, report, which is Appendix B to Staff Exhibit 1(b), the reconstruction or interpretation was developed?

A That's correct.

Q And it is your recollection that it was developed for the purpose of inclusion in Appendix B.

A Expressly to try and determine the amount of offset that we could infer on the different faults. We looked at, just for the record, we looked at all of the trench logs -- when I say "we", Earl and I -- to try and discern where we could find offsets horizons that we thought we could measure

with at least some degree of confidence We couldn't in Trench T-2, nor could we in Trench A, and H, of course, as well.

Q In A and H there was a difficulty, I assume, of taking a direct measurement.

A Okay. But H had already been measured in the field and reported in the Earth Science Associates report. And A, we felt that there was so much complexity and ambiguity in terms of the proper interpretation that we just felt unable to correctly attempt anything there at all.

Q All right. Now if you have any trouble with the definitions on the trenches, please tell me, but I usually refer to in the Phase II investigation, the trench that was essentially running up north -- well, it was placed north, immediately to the north and east of the reactor, is the B-1 - B-3 trench. Is that a fair description?

A No. B-1 and B-3 are separate trenches, as I recall. B-3 is to the south of the GETR and B-1 is to the north. B-1 was extended to become B-2 on the end. Or they were dug separately and finally joined. I can't remember.

Q I just want to set the convention so that when we are talking about B-1 what are we going to describe.

A Okay. There is a figure in my, in our report -- my apologies, Earl.

WITNESS BRABB: Mr. Edgar, if you could kind of

indicate where we are going in the discussion we might be able to find the figure that would be most appropriate. But certainly Figure 1 in c --

WITNESS HERD: April, 1980, report might suffice. There is perhaps even a better log diagram in the Earth Sciences Associates report of February, 1979. Do any of you know where it is in that -- I think it is in the front. Can I look just a second?

BY MR. EDGAR:

Q Let's take a look at Figure 1.

A (Witness Herd) Okay.

Q The B-1 is located to the north and to the west of the reactor, I take it.

A That's correct.

Q And the Trench B-3 is located somewhat to the east and south of the reactor.

A Correct.

///

BY MR. EDGAR:

Q And then trench T-1 is located to the east, farther east than B-3.

A Correct, nearly at Highway 84.

Q Right, and together trench T-1 and trench B-3 and trench B-1 intersect the -- trace the fault at the hill front.

A That is correct.

Q And, am I correct that trenches B-1 and B-3 were dug between August, 1978 and December, 1978?

WITNESS BRABB: We don't have that information, but that is the general tyimeframe of my recollection, but I would not like to specific, with respect to month.

WITNESS HERD: It sounds right. It certainly going to be before February of '79, and after February of '78.

BY MR. EDGAR:

Q Okay.

And am I correct that the B-1 and B-3 trenches were on the same trend as the shear exposed in trench T-1?

A As best we know, they were dug at the foot of the hill front.

Q Is it correct that the modern soil was offset two feet in the B-1 trench?

A That is the measurement that is reported and which we believe to be correct.

Q And that was indeed a direct measurement, am I

correct?

A. It is a measurement that Roy has reported in his diagrams as being a direct measurement.

Q. And you have no reason to questionits accuracy?

A. No, because I was there with the trench log in hand, and feel that is an accurate representation.

JUDGE GROSSMAN: Is this a convenient time for a ten-minute break?

MR. EDGAR: Yes.

(A short recess was taken.)

JUDGE GROSSMAN: On the record.

Excuse me, Dr. Foreman has a question.

JUDGE FOREMAN: I have -- I guess it is a question. It is probably more a comment.

In looking at Appendix B of your report, on lower case numeral two, ii, on your Item No. 4, you list the various thrust movements, and you list three feet of thrust movement on the B-2 fault.

WITNESS HERD: Correct.

JUDGE FOREMAN: It seemed to me they were talking about a two-foot movement, just as they ended up.

WITNESS HERD: I guess I have lost the context of the -- Two feet would be for B-1...

BY MR. EDGAR:

Q. Let us be sure we get it correctly:

You have a table on Page 22 of Appendix B.

A. Yes, we show B-2 as having three of offset of the stoneline albic horizon, in other words, the A-E or A-2 horizon, and two feet for the B-1.

Can I help you reconstruct the reference otherwise?

BY JUDGE FOREMAN:

Q. No, It seems to me that the difference between two and three in this context might be of significance.

A. Certainly.

Q. I just wanted to see if my memory was correct, and I was correct in that.

A. Mr. Swanson asked a question of me at break as well, and I just wish to make sure that I didn't miscast something:

I said that in the course of preparation of our April, 1980 document that Dr. Brabb and I had reviewed trench logs for all the trenches, in an attempt to try and reconstruct the amount of offset of the last tens of thousands of years. So we locked at every trench log that we found available, to try and look at this reconstruction. That doesn't mean that we had all the trench logs in when we visited all of the trenches.

We have stated before that we didn't have trench logs when we visited trenches T-1 and T-2.

Is that clear now?

BY MR. EDGAR:

Q. Now if one wereto take into account all of the direct

measurements taken in the GETR trenches of both Phase I and Phase II, for offsets of the modern soil and burried paleosol, is it true that more than 20 direct measurements of offsets were taken?

A. Is this the number that is reported in Dr. Schleman's measurements?

Q. Well, we can give you and exact number, by references and tables, if that would -- or why don't we do this: I can direct you to a table in Mr. Harding's testimony, and you can verify the number later, at a break or some time.

A. Okay, that is fair enough.

Q. Okay, so what I am really interested in determining is that there were -- Were there more than 20 direct measurements of offsets taken in the trenches?

JUDGE GROSSMAN: Well, I think the witness has indicated that he can't answer now. If there is something that you want to show him that will refresh his recollection, when you come back...

MR. EDGAR: I think we will save time if we do it at a break, or at some period other than during the questioning.

I will just proceed--

WITNESS HERD: There were a number of measurements.

MR. EDGAR: Exactly, he knows there were a lot of measurements, but he doesn't want to have a precise number, until he checks it.

JUDGE GROSSMAN: You won't save time if you do it during the break today, because we are not going to have any more, other than a final break.

MR. EDGAR. I understand.

JUDGE GROSSMAN: Unless you request one.

BY MR. EDGAR:

Q Is it true that there was no surface expression of the offset at trench T-1?

A That is correct. "Surface offset," in other words, a step in topography?

Q Right.

A To the best of my recollection, no.

Q All right.

A Nor on any of the others.

Q Putting aside trench T-1, is it true that in the trenches dug during the Phase II investigation that there is no direct measurement of any offsetin the modern soil, in excess of three feet?

A That can't be assured, unfortunately, because trench H, which would apply to that, which has a minimum of 1.5 of offset, had its surface bulldozed off of it, so we don't know how much offset was there. That is a minimum.

Q Now, but there is no reliable positive evidence as to indicate that the offset in trench H exceeded three feet, is that correct?

A. I have no way to be able to preclude that. I can't remember.

Q. I understand you can't preclude it, but I am asking you: Is there any reliable positive evidence to demonstrate an offset greater than three feet?

A. When the top of the horizon is trimmed off, I don't know how to answer that, except that it is just a minimum number, and to try and guess at the upper boundary is pointless as far as I am concerned.

Q. Well, then what you are saying then is then there is no direct measurement in trench H of an offset greater than three feet, is that correct?

A. The apparent minimum offset in trench H is 1.5 feet. It is that succint?

Q. Right, and you cannot cite specific physical evidence which would demonstrate that the offset is greater than three feet, is that correct?

A. Nor evidence to preclude it.

Q. Understood.

So that, repeating my question, putting aside trench T-1 and trench H, is it true that in the trenches dug during the Phase II investigation, that there is no direct measurement of any offset in the modern soil in excess of three feet?

A. To the best of my recollection, that is correct.

Q Turing to Staff Exhibit No. 4, yesterday, as you explained the most recent interpretation, based on the new information made available by the photographs in Staff Exhibit No. 5 and 5-A -- Have I got that correct?

A. Yes, 5-A and 5-B.

Q Exhibits 5-A and 5-B, okay, in light of that, you developed an additional interpretation, and during the course of the explanation, you made the remark that the -- "We now think the step is real, based on the photographs."

Could you explain what is meant by that, if I have correctly characterized it?

A. Okay, to briefly summarize: In the calculation of the offset that we reported in our April, 1980 volume, which we explained under deposition, because of our uncertainty in terms of the logging of the trench, particularly in the area of the fault, knowing that we had observed an offset of the soil, we had interpreted an offset of about five feet, by extrapolating six seven to point nine, and then measuring the apparent offset of that surface from ten three, which we concluded to be originally once continuous, and measured that offset nine three distance, which, as we have talked about before, is about five feet, but as much as seven feet.

However, when we had done this, we then required then, of course, that we were extrapolating a surface accross this step, ignoring it, in that log.

Q Okay the step is defined by six, five, four.

A Correct. Thank you, I appreciate your getting the nomeclature here, six, five, four.

By extrapolating across that, we were overlooking it and incorporating it into our interpretation. With the semi-discovery, for me, of the photographs, and looking at them, it would appear that the step that exists between surface four five and six seven was real, this intermediate bench, such that that was essential to incorporate and concern ourselves with in the calculation of apparent offsets.

WITNESS BRABB: I would just like to expand very briefly on this:

I think Dr. Herd is using the expression "is real" in the way that we go about analyzing the information. We have to weigh the information that is provided to us. Inasmuch as the trench logs are not the trenches themselves, we have to evaluate the information to try and determine what we feel is real, accurate and correct, versus that that may be a mistake, or mislogged, or misinterpreted. So in that sense, we felt that the photographs added documentation that we were comfortable with that that step exists.

WITNESS HERD: Thank you.

BY MR. EDGAR:

Q And there is a corollary to that, I assume, and that is when the original interpretation was done -- I assume you

did not recall, at that time, that there was a step or bench at six, five, four.

WITNESS BRABB: Not exactly correct.

We did recall that the ashen horizon was offset, so that in that sense, there had to be a step somewhere. What we couldn't be certain of is where that is in relation to that diagram.

WITNESS HERD: Right. In other words, this step that I have been talking about figuratively is one which occurs between the A-2 horizon, and we observed an offset, so that there would have been a step somewhere in that trench, and it was a question of where it was, and in the calculation of that apparent offset that we went to it.

BY MR. EDGAR:

Q Well, it is a question of whether there are two steps, really, isn't that the issue?

A That is correct, now.

Q Yes, and I take it that your recollection vague, at the time you did the reconstruction, and did not recall seeing two steps.

Is that fair?

A Well, I think it is more accurate to say that when we visited the trench, we were at that stage, for me, just looking at it to get a crude impression of what was there. I don't remember making a thorough examination of the entire

trench, an looking specifically at that one particular point to learn it. So I don't feel that I made an exhaustive interpretation of it, or have any idea of what all is there.

Dr. Brabb, would you like to add?

WITNESS BRABB: No, only that there was a possibility that the step was from between line six, seven and ten, three. That is what we couldn't recollect.

MR. EDGAR: Okay.

WITNESS JACKSON: That is my recollection, also, in the trench, that we were there, we knew there was a step, I didn't recall the two steps in the--

MR. EDGAR: Okay.

WITNESS BRABB: That is all I am trying to get at.

WITNESS HERD: But again, I point out it wasn't logged, and we were pecking at the wall, trying to follow the A-2, and it was disappearing, and I didn't have any confidence that I had any real ahndle of the stratigraphy in the trench, and we didn't have the log in hand.

BY MR. EDGAR:

Q. And the difficulty you had, when you embarked on the reconstruction, was because you didn't have the log, at that point, you had no frame of reference to ve_fy.

Is that the point?

A. That is correct.

Q. Now, you also indicated yesterday -- correct my

paraphrase, if it is not correct -- but you indicated that the second step, which is defined by seven, six, five--

A. Got it.

Q. Okay, that you would associate that with false movement, because that observation would be consistent with the pattern of behavior exhibited in the other trenches.

Am I correct?

A. (Witness Brabb) No, I don't think so.

Q. Well, could you explain it then?

A. I think we are talking about the line five, six, are we not, rather than five, six, seven?

Yesterday, I raised the possibility that line five, six could also be interpreted as a fault, and that that might explain the step in the surface four, five, six, seven.

Q. I guess -- Let me try to make my question a little more explicit, and that may help:

Is it possible that the step which is defined by seven, six, five is simply topographic relief?

A. (Witness Herd) Well, if it is topographic relief, I propose there were two interpretations, that it was a surface of erosion, which would have been ancient relief that is now burried, or that it is a once continuous surface that is offset.

Q. Right. And what I am suggesting, or what I am trying to explore is that it is possible that it is topographic

relief, but what observation leads to the conclusion that it is more probably associated with offset? That is what I want to try to encapsulate.

A. Okay, well, what makes it more probable to be offset?

For me, it was the A-2 horizon, and the remembered offset in that horizon.

Okay, I guess I should explain that more, or not?

Q. Could you elaborate on that?

A. Sure.

We are talking about a contact which I said between point six, seven, four, five, and apparently ten, three appeared to be the stoneline, and above it a white or ashen area, which we thought to be the A-2 horizon. For me, it was not only the steps in the stoneline, but also in the A-2 horizon, plus this remembered offset of actually looking at seeing the A-2 physically offset that made me believe faulting was the preferable interpretation of this trench log.

The A-2 horizon, as we have been talking about in previous questions, is this horizon that forms parallel to the surface, below the A-1 horizon, and it, in the exposures that we had seen in the trenches in the GETR area, has regional continuity and parallelism with the surface. And in this trench, by inference and documentation, what we have with the photographs in some of the annotations of the log, not only does the stoneline make an abrupt drop, elevationally, but

there is an accompanying drop similarly in the A-2 horizon, that the increase in depth below the surface of the A-2, from latitude six, seven -- excuse me, longitude six, seven to the area four, five, is an increase in depth of the A-2 horizon by nearly, what, almost a hundred percent? It would appear to be of that order. And then again, between four, five and ten, three we are increasing it another almost 40 or 50 percnet again.

Now the increases that these abrupt -- Excuse me, these increases in profile depth are abrupt. They occur within only a foot or two distance laterally, and these abrupt steps are -- and increases in profile development are unlike things that we had seen elsewhere, where the soil stratigraphy was undisturbed by faulting.

What we are requiring, as far as I understand it, on ten, -- in the areaof ten, three is to have the A-2 being well below the surface here. I don't know the scale. I would look to be on the order of --

Does anyone have a ruler?

/ / /

tp5

WITNESS HERD: Over in the area around 10 - 3, the A-2 horizon is nearly 3 to 4 feet below the surface and so we are saying, then, there is an A-2 which is covered by a very thick A-1 horizon. But by the time we get over here, let's say over in 6-7, the A-1 horizon is very thin and the A-2 now can be no more than less than just a single foot below the surface. So we are saying that there is an increase of almost two or three feet of depth of that A-2 horizon immediately as we pass from Point 6 to 5 and then again immediately as we pass from 4 to 3.

These abrupt increases in the depth of the A-2 and the overlying A-1 increase accompanying it are atypical of the soil profile development which we see in the Livermore Valley area, to the best of my recollection and experience. And the steps when we have seen these before, these abrupt increases have been associated with faulting as so classically displayed in this photograph here in B-2.

BY MR. EDGAR:

Q Yeah, but I thought you told me that these soil horizons, you are not in a position to correlate these soil horizons with the other trenches.

A (Witness Herd) I'm not having to make --

Q And you can't have consistency both ways here.

A Okay, but I can do witn the modern soil, can't I, because the modern soil is the planesol, the posita soil which

we have seen elsewhere, the ones that I have looked at in the trenches. Regardless of its particular age locally, it is one that develops in a same uniform fashion, as best of my understanding. That is, that it forms by downward percolation of material. It is a weathering horizon. And by comparison to regional relationships and looking at the trenches locally, not more than -- what? -- several thousands of feet away in Trench B-1, B-2, the same sort of relationship is associated with faulting and then we've got it here and that's what we are interpreting it with.

Excuse me. Dr. Brabb has something.

WITNESS BRABB: I believe our correlation difficulty, Mr. Edgar, was with two older horizons, the stone line and the paleosol.

WITNESS HERD: Yes.

WITNESS BRABB: These are not the points in issue here.

WITNESS HERD: Yeah, that is correct. We want to be sure on the stone line, the surface of erosion, because that may not be everywhere time synchronous. And certainly that buried soil beneath it, if that is what it is, is the problem of what does it fit in timewise.

BY MR. EDGAR:

Q Did anyone or can anyone testify that they saw a shear from Point 12 to Point 5?

A (Witness Herd) I certainly cannot do that. I think Bob, in your memo or something like that --

WITNESS JACKSON: I remember an awful lot of shears in that trench, but I don't know that I could identify that line on that log as one that I did see.

BY MR. EDGAR:

Q Okay. I'm not talking about the line on the log. I'm talking about the extension of the line from Point 12 to Point 5.

A (Witness Jackson) Oh, I misunderstood you. I guess if you go from 12 to deeper in the trench or to the right, there was one shear that we spent some time looking at and trying to extrapolate up toward the surface. I recall searching for that and not being able to see anything definitive that I felt could contribute to this type of offset; however, and this is a very important point, the material in this area is very blocky and very, if I remember correctly, bleached, very hard to trace through that particular area. So I think it was somewhat ambiguous. I think I indicated that --

WITNESS HERD: Did you think it was bleached or was it the black material, the black, blocky clay?

WITNESS JACKSON: Oh, it was a grayish material.

BY MR. EDGAR:

Q Did it have distinct columnar blocks in it?

A (Witness Jackson) Yes, it did.

Q And did it have the same soil structure as the typical buried paleosol with the exception of color?

A I am not at all a soils engineer and soils stratigrapher and I don't recall.

WITNESS HERD: I recall that it did not. I remember a different type of ped structure.

BY MR. EDGAR:

Q Did anybody on the panel see a shear extending from Point 5 to Point 11?

A (Witness Herd) When I visited the trench in that area of the trench I believe it was covered with spoil and colluvium and there was no attempt on my part made to try and clean it because it was being cleaned at the time is my recollection.

WITNESS BRABB: I'd like to state for the record that I could see -- I don't recollect any faulting between 5 and 12 or 5 and 11, but that it could be there.

MR. EDGAR: Okay. That's fair.

WITNESS HERD: I would agree with that.

JUDGE FOREMAN: Why do you say it could be there?

WITNESS BRABB: For the reason Dr. Herd just mentioned. There was a difficulty in seeing the features clearly in the trench. Also, we were in there for a different purpose at that time. It was mainly to establish that there was young faulting that might have implications for the reactor. We did

assure ourselves at thattime what there was indeed young faulting, that is to say, the modern soils were offset. But I cannot be specific about where that fault is located with respect to this trench log. That's my difficulty. Because we did not have this trench log when we went into the trench.

JUDGE GROSSMAN: You were only interested in determining whether the faulting fit the definition of a capable fault for the NRC standards at that time?

WITNESS HERD: The three points that we were looking at were, one, was there or was there not faulting; two, dip; and three, age.

JUDGE GROSSMAN: It's the third point that I am asking about. That was the only one that you were interested in as far as time goes.

WITNESS JACKSON. If I recall correctly, it was a reasonable interpretation of recent or age, and we were not trying to be definitive in terms of the age of the last movement, as to whether it was 35,000 years or less. It was was there reason, good reason to conclude that there was a high possibility of its being young in age.

WITNESS DEVINE: If I may add something. At that stage I'm not sure either that Drs. Herd or Brabb knew what the NRC definition of capable faulting was. They were looking for young faulting, as he has been describing it, in their own terms of young faulting. It was at a later time when NRC

definitions began to be applied to their analysis of what they had seen.

WITNESS HERD: Sorry for the interruptions.

MR. EDGAR: No problem. I'm used to it.

BY MR. EDGAR:

Q Does anyone on the panel remember seeing two shears in close proximity that broke and extended up into the A-2 horizon?

A (Witness Herd) It's a parallel answer to what you just asked. Again, we would have to, from my point, we would have to repeat that the examination was cursory. I recall one, that there was colluvium and material in the walls of the trench. I didn't feel that there had been a chance to even accurately look at it, so I can't say that there wasn't. I certainly observed one. There may have been more. I knew that I certainly saw, as Dr. Jackson has pointed out, a cluster, a number of parallel faults in the trench, certainly in the lower part.

Q But that was in the lower part.

A That's what I'm trying to say. Right.

Q Now, do the photographs which were 5(a) and 5(b), Staff Exhibits 5(a) and 5(b), clearly show a fault extending from Point 12 to Point 5 to Point 11?

A Clearly show it?

Q Yes.

A No. But I'm not sure exactly what they show, other than to help us be sure that there was an apparent A-2 in the area of 4-5. It is with Bob Jackson's marking on the line above the area of that shore in Photo 5(a) and 5(b) that we got the impression that must have been the area of 6-7.

Q Okay.

WITNESS JACKSON: Mr. Edgar, I remember vividly the discussions we had about the extension of this to the surface because it was one of the critical elements of whether or not we should issue a show cause order. And I think it was inconclusive at that point in time. In the later memo that I wrote or helped to write after returning from the trip we noted a number of items. I think there was a topic of discussion it was difficult to see and there was not a consensus -- I think there was a general consensus among the trench on the 22nd that it wasn't definitive one way or the other.

MR. EDGAR: Okay.

WITNESS HERD: I don't agree with that.

WITNESS JACKSON: Definitive one way or the other did we see a shear running through there?

WITNESS HERD: Oh, well in the protector area. The point is that we have recorded before that we saw and observed offset in the surface soil. We have reported that in our February -- excuse me, in our report of '79, I guess it is.

WITNESS BRABB: And I would add further to that that

my recollection of those trench meetings is that we were all agreed, including the consultants, that the surface soil was offset. Therefore, at a later time when we received the trench log and those soils are not shown as offset, it was a surprise to me.

WITNESS HERD. Agreed. To me as well.

BY MR. EDGAR:

Q Now, looking at the shear which -- the extension of which is the line from 12 to --

A (Witness Herd) Excuse me. I was not listening.

Q Okay. Let's look at Licensee's Exhibit 2, Figure B-1, which is the trench log of T-1.

A Okay. I have it in hand. I will sit down this time.

Q Do you have that figure B-1?

A I have it now. It's in the rear of the volume. It's a figure that folds out, unpaginated.

Q Now I'd like to try to get this oriented. The step at 7-6-5, or the step defined by the broken line 7-6-5 is at Station 130, is that --

A That's correct.

Q Okay. Now, between Station 130 and 140 one can see a shear, the extension of which is line 12-5-11.

A Yes.

Q Okay. Now is there any evidence in the log to show that there is an offset of the blocky clay or I think it's

labeled -- yeah, it's a blocky clay soil unit at the lower contact.

A You mean silty clay?

Q Well, let's start with that, yeah. Let's take them both. Let's take G first.

A No, there is no indicated offset there, according to the trench log.

Q All right. And there is no indicated continuation of the shear past Point 12 on Staff Exhibit 4.

A That's true.

Q Does anybody recall from observations in the trenches that the soil units H, G were offset at the lower contact? At the extension of Line 11-5-12, that is the shear above the main shear there.

A (Witness Jackson) I'm afraid I lost the track of the beginning of your question.

Q All right. Fine. What I'm asking is let's assume we take Line 11-5-12, project it through the shear shown in the trench log, and project it down right to the bottom of the soil contact. Does anyone recall any offset on that lower soil contact which could be associated with the shear defined by the extension of 11-5-12?

A (Witness Herd) Not to be argumentative, but I am troubled when you keep using the word "soil". You are using it as a graphic generalization, not necessarily that's what

was there. And as I believe I said yesterday, that was one of the troubles with entertaining this hypothesis in terms of extending it below. But given the nature of the material that it is in and the like and the great numbers of faults below, I am bothered that there isn't any documented offset, although I am equally not prepared to say that there wasn't because I didn't have an opportunity to check it and, given the density of faulting and the nature of the materials in which the fault occurs, I am not sure that that would have necessarily been recognized readily.

WITNESS BRABB: I'll answer the question for myself. I have no recollection of a shear in that area, but I will point out that in terms of the trench log it indicates that in one case we are talking about silty clay and the other case we are talking about silty clay clay. These units, if you will, are very nearly identical and therefore it may be a little misleading to show the dashed line along there as something that represents a stratographic unit comparable to what we were looking at in the other trenches.

WITNESS HERD: And if I may just for point of reference, if you look over in the area of just below Station 160, there is a contact interpreted into the area where this fault is shown as going and it sort of just dies out over in the area of 147. And that contact there is mapped as a contact very subtle gradational over a 1.5 to 1 inch, such that I get

the inference from here that recognition of stratographic horizons were difficult at best and almost impossible to follow laterally, certainly as evidenced there. So that I am not bothered, if this contact couldn't be followed over into the area of the fault near 135 I similarly am ready to imagine that it would have been very difficult to envision these contacts as well here.

BY MR. EDGAR:

Q Okay. But let me ask a hypothetical question then. Let's suppose that what we are seeing in this shear which is defined by line 11-5-12 is thrust faulting and let's also assume that we see no offset of the lower unit. If that is true, is it physically impossible for that fault to extend up the line 12-5-11?

A (Witness Herd) It's not physically impossible for it to extend. You can have a simple break in the soil that can continue all the way to the surface. But the point would be that you are trying to make, I believe, is that there would be no apparent displacement on that fault.

Q That's right. There would be no offset. Is that right?

A That would be the inference if the contacts are there.

Q Right. And in the absence of evidence that those contacts were offset, then one has some difficulty projecting

the line upward from 12 to 5 to 11, is that a fair statement?

A (Witness Brabb) We have admitted, Mr. Edgar, that we have some problems in that area and we are speculating. We tried the best we can to provide the information that we have at our recollection. We have tried best to indicate some of the difficulties that we have with the theory that we have for the origin of the offset of the steps. As we pointed out, there are some missing gaps, and this is one of them.

Q I appreciate your candor. I was simply trying to get this said another way.

A How else can we say it?

Q I'm not trying to rub anyone's nose in this. This is just a matter of fact that needs to be brought out.

A (Witness Herd) If I may just for a moment, not to complicate things further, but I think it might. Dr. Ellsworth reports that in the course of my discussion of Trench T-1 apparently Dr. Slemmons thought that there might be an alternative interpretation, certainly one with faulting, which would not require fault offset in the area of Step 5-6, but certainly fault offset along 8-3, in that area. It's a third hypothesis we haven't heard about.

WITNESS JACKSON: I talked to Dr. Slemmons about that briefly and I think he was referring to -- and you could ask him when he returns -- he did not go in this trench and I think he was just looking at this log with limited understanding of

it and saying this was a possibility.

BY MR. EDGAR:

Q Dr. Jackson, I handed out a copy of your memo to is it Dr. Gamel or Mr. Gamel?

A (Witness Jackson) He deserves a doctorate, but he is Mr.

Q Okay. Mr. Gamel. Dated October 31, '77, and that is marked for identification as Licencee's Exhibit 44. The part I am interested in is the following, it is in the first full paragraph, and I quote: "An apparent six inch offset of a faint colleachy rich zone approximately one foot below the present ground surface was also noted. No clear evidence of a fault can presently be traced to this offset, although the nature of soil at this location might mask such evidence." Is it a fair inference here -- and I recognize that memories are not always intact with the passage of time -- but is it a fair inference that the reference in the memorandum is to what area on Staff Exhibit 4?

A My best recollection is that it is 6-5-4.

Q Okay. Now if the colleachy --

A If I might add, the reason I remember that discussion was the reason I mentioned yesterday, that we had a lengthy discussion about -- and I don't know who it was with -- about grazing on one side of the fence line versus the other side of the fence line as causing this change in elevation. That's

why I focused on it after this much time. It was related to the photographs, 5-A and 5-B, I think is where that statement was being made, to the best of my recollection.

WITNESS HERD: The question -- and I'm curious of this -- the six-inch offset, are you inferring it to be one that was apparent net slip, if it were in the sense of movement along a fault plane, or just vertical offset?

MR. EDGAR: Yeah. I guess that's a legitimate question.

WITNESS JACKSON: No. The sense of what I meant there -- it's a long time -- the sense of what I believe or was trying to intend there was the difference in elevation between surface 4-5 and 6-7, the change in elevation.

WITNESS BRABB: Then I would like to point out, at least if the trench log is correct, that it looks like the approximate order of magnitude of that is somewhere around two feet.

WITNESS JACKSON: That's the vertical distance from 5 to 6.

MR. EDGAR: Yes. Dr. Herd can estimate it there on the scale.

WITNESS BRABB: Also, that we attempted on the photographs, Staff Exhibit 5(a) and 5(b), to try and estimate what that step would be using the width of the trench shoring as a scale. We're not sure what the trench shore, how wide it

is. We used 8 inches. Is this a reasonable length? Maybe Mr. Harding can help us out. Do you recall how wide those trench shores are?

MR. HARDING: I believe it's probably closer to 5 or 6.

WITNESS BRABB: Five or six inches. That would then increase our measurements to something on the order of what we see on the trench log.

WITNESS HERD: It's two feet, more or less, in terms of the step.

MR. EDGAR: On a rough scale.

WITNESS HERD: On a rough scale.

BY MR. EDGAR:

Q Okay. Dr. Jackson, the offset that you were talking about is one that you are at least mentally you think was vertical.

A (Witness Jackson) All I'm talking about is the distance in elevation between 4-5 and 6-7. I'm sure that that is what we were talking about. To the best of my knowledge, that's what that referenced.

Q Okay. Now could we turn to Staff Exhibit 1(b), Appendix B, and back to Figure 13.

A (Witness Herd) That's the cartoon?

Q Yes.

A Okay.

Q Now if you go from Figure 10 -- excuse me -- Plate 10 to Plate 11 of Figure 13, you see first an offset of a young soil horizon, of the modern soil horizon, the A-2, and a scarp there on the ground and then you go over to Plate 11 and the scarp is eroded off and there you are. The thing that I would like to know is that first if indeed one were to have an offset in the modern soil isn't it true that in order to have erosion of the paleosol layer one must have exposed that paleosol layer to the atmosphere? In other words, one cannot erode unless one is exposed.

A Okay. You just jumped two things which I'm not sure I follow. Let's go to the point --

Q Let me break it down. Let me just try this.

A All right.

Q Start with Plate 10.

A Right. I'm looking at it.

Q We have offset the young soil.

A Right.

Q And there is a distinctive wedge represented by the paleosol in Plate 10.

A Right.

Q Then we flip over to Plate 11 and we see a scarp there which has now been -- we don't see a scarp there, it has been eroded off.

A Right.

Q But there is no erosion of the buried paleosol because it is in fact buried.

A That's correct.

Q Okay. Now if we now translate this to Staff Exhibit 4, how can you explain the absence of a distinctive wedge if the offset -- where is the wedge? We have drawn a lot of lines, but we don't have a wedge here.

A Okay. We noted yesterday as well and I noted yesterday in my discussion that that is an important point. I would repeat the caveats and the concerns that I expressed before. There is really an important point to get across here. There would have been really only one particular horizon that would have been visible in all of the material above, given time to dry out. That would have been the A-2 horizon. The rest of it would have been -- appeared to be just the simple A-1 horizon, a dark, greyish-brown material. The A-2 from the A-1 would have appeared totally indistinguishable if it were fresh in terms of cleaning off the wall.

In other words, you could have an A-2 here along the bottom, you know, stepping across right here, and if I had gone in within a few minutes and cleaned off the face, the material throughout the whole length of the top of the trench would have appeared identical, even though there would have been two physically mappable soil horizons in there, the A-2 and the A-1. Given time, a few days to dry out and the like,

the A-2 would have been clearer. What I am concerned on is that I have no clear assurance here that that A-2 horizon, which would have been the critical way to have seen in part some of these contacts, would have been recognized in part.

I agree that there should have been a more defined wedge out of some of this other blocking material, but in looking at the photograph, 5-B and the like, the context there seemed very, very subtle and obscure. I agree it is troubling that it isn't there, but I'm equally troubled and know that it is difficult to see some of these contacts as well.

Q What concerns me as a laymen is that we are drawing lines here that are forming wedges and triangles and the only way you can back off the wedge or triangle is to have erosion and you can't have erosion unless you've got that surface -- this wedge -- exposed to the atmosphere during the period of erosion. You can't erode buried soil. That's my common sense interpretation. Am I way off base?

A No, no. I agree with your concerns and, as I say, I tried to express those yesterday. However, if you just look at the gross topography in the whole view of Trench T-1 log, it is quite prominent that the horizon is a linear until you come to these abrupt two steps right in the area of the fault. That certainly suggests something happened there. I know that the consultants have -- particularly meaning Harding and others -- have said that doesn't preclude faulting in the

past, it just altered it. But given the nature of the materials, the time it was logged, the clear difference in observation that we have on the fact that I recall that the soil was offset yet don't see any documentation of it nere, the fact that Roy didn't log any of the soils in here, we have no assurance to preclude that it's not been offset. I remember that it is offset. I am trying now to reconstruct that offset from this. I recognize that these contacts are difficult to envision because they are not mapped as such.

WITNESS BRABB: Let me try and state it in my words as best I can. This log does not show soils unit. Soils units are not mapped on the log. We are trying our best to reconstruct what the soil units are in there from the information provided in the log of the material character plus the photographs that we have seen. There is nothing from that information, though, to preclude the extension of the A-2 horizon from line 6-7 to point 11, to say it may be there and the wedge may be there. And the interpretation we have may be correct.

BY MR. EDGAR:

Q What I can't understand is that when you go in these trenches, you see this very vivid wedge and there's got to be a physical explanation for how you get erosion on buried soil.

A (Witness Brabb) The buried soil is not the issue here. It doesn't enter into the computations that we are

talking about. The buried soil, I think you are talking about the paleosol, is that true?

Q Yes.

A That's in the lower part of the trench. Do we have a confusion in the term?

Q But it is truncated by a stone line.

A (Witness Herd) Okay. I think there may be some confusion here. What you are asking is why isn't there an extension of a wedge-like material 6-11-5 here.

Q Sure. And likewise on the other one where, as you see in B-2 so very vividly with this red horizon.

A (Witness Brabb) No, we are comparing two different units. The red vivid color that you remember from Trench B-2 is this lower unit identified as X, if the consultants are correct, on this diagram. Therefore, you are comparing a vivid unit in B-2 with something that is in the lower part of this trench and not a matter of contention.

Q Well, no. Dr. Herd doesn't agree, I don't think.

A (Witness Herd) Yeah. I think I understand what you are saying. But the most important point that I would raise is that we are not talking about a red soil here. There wasn't a red, vivid, buried soil screaming out at you to be able to follow comparable to Trench B-2.

WITNESS JACKSON: I'd like to add a comment, if I could. It is getting very late and --

MR. EDGAR: I know. I'm sympathetic.

WITNESS JACKSON: I'm not complaining. I'm just indicating that we are talking about something three and a half years ago and our recollections of it. It is very difficult. I do remember in the opening of the other trenches after T-1, T-2 series, puzzling over the reason why we didn't see the obvious things that we saw in B-1, B-2, B-3 and H, especially, where these offsets were very vivid, very apparent.

Now there are other explanations. I think it is absolutely clear to everybody here that there are a lot of alternative explanations to what we are seeing, the faulting being one of the better ones since there are so many shears around this area. One thing that I thought about during this process is that T-1 is located in a somewhat unique area relative to the other trenches. It is in the bottom of a swale. It has a rise on both sides of it. All of the other trenches are located where it is downhill to the west of all of them. So there could have been -- I don't know soils enough to speculate, but there could have been some erosional aspect coming parallel to the fault. In trying to wrestle with this I thought about that.

The other idea is this was a backhoe trench as compared to the others, which were very wide open, bulldozed trenches, which allow you to a great advantage. They are excellent trenches because you can stand back and get a much

better contrast. When you are standing in there with shoring all over you it's not easy to see all the time. I'm generalizing, but it is probably not the best.

JUDGE GOODMAN: I think this is a good time for everyone to have a good night's sleep on that and come back with refreshed recollections tomorrow, unless Mr. Devine, did you have something you wanted to say?

WITNESS DEVINE: I wanted to make a very brief statement. I'm far enough back from this that maybe I can offer a comment that is useful. The others are so close to it they may be missing the point. And you are discussing about the need for erosion and not having it possible because it would not be exposed. As I understand our geologists here, we do not require erosion for the interpretation we have offered. We are saying the wedge is there but it just can't be identified. It has not been removed by erosion.

BY MR. EDGAR:

Q Okay. But then my question is did anyone see it?

A (Witness Devine) And I believe the answer was no, they did not identify it. But they then went on to explain the difficulties of trying to identify it.

MR. EDGAR: Could I make one other suggestion, Judge Grossman? I think I am about seven minutes away from finishing this. It is up to the witness panel, but if they would like to finish, I would be willing to proceed.

MR. SWANSON: Mr. Chairman, we did discuss this possibility earlier, if he could finish with the panel. You may want to finish at least this line. Mr. Cady indicated that he would be available for a few minutes anyway past 5:00. I assume the Board has some questioning. The panel, anyway, is willing, as I understand it, to go for a little while longer to finish this line. One comment, though. Both Mr. Edgar and Mr. Cady indicated that they had completed with other aspects of the panel. I have no questions of either Dr. Ellsworth or Dr. Pichumani. Apparently counsel for the other parties do not have any questions of those two gentlemen. I was wondering if the Board knows if they have questions in that area could those gentlemen perhaps not have to come back?

(Pause while the Board members confer.)

JUDGE GROSSMAN: Unfortunately, the Board does have some questions of those two panelists. They are going to have to be here tomorrow anyway. In view of that, does that make any difference as to whether you --

MR. EDGAR: I'm indifferent to it. I'm sure they would be glad to be rid of me.

MR. SWANSON: The panel is I think willing to go seven minutes to finish this line of questioning.

WITNESS HERD: Yes, and I think it would be better to maintain the logic flow.

MR. EDGAR: I think so.

WITNESS HERD: Because we have reconstructed trench logs, et cetera. Fine.

BY MR. EDGAR:

Q A quick one. I handed out Exhibit 43, which is the California Division of Mines and Geology report on the trench -- excuse me -- on the T-1 trench visit. It is dated October 26, 1977 -- I stand corrected -- October 28, 1977, is the cover letter and the enclosed report, by Thomas E. Gay, Jr., State Geologist, is entitled -- is dated October 26, 1977. If I could call the panel's attention to the first page of the attached report and as well page 9 of Appendix D to Staff Exhibit A-2 -- no, Exhibit 1(a) -- I've got A-2 on the mind -- okay. Page 9 of the California Division of Mines and Geology report, which is Appendix D to the first SER.

(Pause)

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WITNESS MORRIS: Is Page 9 a table?

MR. EDGAR: Yes, sir, it is.

The table is entitled: "Characteristics of Thrusts Exposed in the Trenches," and it appears on Page 9 of that report.

BY MR. EDGAR:

Q Now, if you will read down in the memorandum, the Gay Memorandum, under the heading "Trench 1," it says:

"Livermore Gravels (Plio-Pleistocene) overlie and are in fault contact with older soil and colluviium (probably of late Pleistocene age). The fault, which dips gently to the northeast, can be traced in the older soil and colluvium to within three or four feet of the surface.

The younger overlying units did not appear to be deformed, but this could not be established with confidence during the brief period of observation."

Now, turning to Page 9, the table, entitle "Characteristics of Thrusts Exposed in the Trenches," I see in the first row of data Trench T-1 and then in the column that is labeled "Materials Displaced," I read the phrase:

"Stoneline equivalent?"

Is it fair to draw the inference from Exhibit No 43 and the California Mines and Geology Division Report, which is Appendix D to Staff Exhibit 1-A -- Is it fair to draw the inference that the California Division of Mines and Geology

people did not believe that the young soils were offset in trench T-1?

JUDGE GROSSMAN: Excuse me, is there any foundation for this panel to answer for the--

MR. EDGAR: Well, it is part of the SER, so I guess that is a fair question as to whether they know.

WITNESS BRABB: I have a further problem, Mr. Edgar, in establishing the credentials of the two gentlemen that signed the letter, with respect to soils units. It has been my observation in the trenches that these are subtle, difficult things to recognize, and therefore, I would use a weighing process, in terms of the interpretation of these two geologists, who I personally know, in terms of the interpretation of soils.

WITNESS JACKSON: It has been a long time since we attached that to the SER. I do not remember discussing that aspect at all. We attached the SER -- that report in total to the SER, without going through it with CDMG.

BY MR. EDGAR:

Q Okay, I have a simple question now, which I hope will be my last, and it is another one of these layman's questions, and it really deals with plane geometry:

If one assumes that one has two blocks moving along a common surface, and the two blocks consist of prallelograms, and if one slides, one relative to the other, is it true that the direction of travel, the offset, is measured by prallel

lines on the surface of each parallelogram?

Now, that is the question, and I have got a little sketch that will explain my question that I can hand out.

A. (Witness Herd) I have it in hand now.

Q. Okay, all I am trying to establish is that the correct method for measuring the distance of relative motion of those two blocks is to measure parallel to the stationary surface. In other words, one block moves, and therefore, in order to find the total motion, one measures surfaces parallel to one another.

A. Correct -- Excuse me, what you are doing is -- the amount of offset, which you have in your figure, would be measured along line where the letter "t" in "offset" occurs--

Q. Yes.

A. Is that what you are meaning?

Q. Yes.

And this is where I am having trouble: If I look at Staff Exhibit No. 4, and I look at the projected line five, four, nine, and if I assume that I have thrust faulting, that is, the upper surface moving parallel -- or the upper sruface thrusting over the lower surface, and I have got another illustration here, I cannot understand how one would measure offset by projecting line five, four, nine, rather, it would seem appropriate, as a matter of plane geometry, to measure the distance along the offset which is intersected by the line five,

four, one.

A. Fine, I understand your question.

The point is that in your simple block model, you assume no deformation. What we see in our trench log here is indicattion that surface four, five is not planar -- horizontal, rather, but is dipping, inclined to the east, to the right of the trench. What we have then is to caicualte the offset of surfaces that we assume to have been once continuous and horizontal. So we have not only simple horizontal -- offset of the horizontal surface, but accompanying rotation, apparent rotation of that surface, too. So that what was originally once flatlying, if four, five was originally continuous with ten, three, and there had been no rotation as the faulting had occurred, then the offset would be appropriately measured from four, one across, but as was pointed out, I believe, by Chairman Grossman earlier that you just simply don't want to make a simple extrapolation -- I mean a straight line across, in a horizontal fashion from four, one. You have to use the line segment four, five, and a 180-degree angle straight outwards from there.

Q. But aren't we talking about, even though it hasn't been observed, there is a wedge of soil here. In theory, we have got a wedge here.

A. We have talked about this earlier.

Q. And if there is a wedge, then the only way you can

do this measurement is parallel to the line ten, three.

A. Is parallel to the line ten, three?

Q. Yes.

A. Just hold it a second. I have lost ten, three.

Q. There is no rotation, if we are dealing with a wedge.

WITNESS BRABB: Can I answer the question?

The information that we feel confident about that we know is that there is a surface four, five that is inclined, therefore, the fault movement has not been in the form of a prallelogram, in the way that you described, but there has been some rotation. The wedge that you mentioned is in our mind not been established one way or another, therefore that is information that we are uncertain about, because there is no information of the character of the soils in that part of the trench. But we do know, based on the photographs and the information presented on the trench log that surface four, five slopes in the direction indicated. This is a known quantity, as against a quantity that we have no information about.

BY MR. EDGAR:

Q. Okay, but granting that it slopes, what I can't understand is the theoretical -- The theoretical model here is that there is indeed at the time of thrusting, there is a wedge of soil thrust over. The wedge that I am referring to

is five, four, one three. That wedge is thrust over a block of soil. If indeed that is true, in thrust faulting, isn't it correct that one should measure between the parallel lines five, four, one, and ten, three.

MR. DEVINE: As I understand what you have described, Mr. Edgar, that requires that the slope four, five is tilted prior to the displacement along the fault. Then you could measure from four to one. Otherwise, I don't see how you can.

You see, if you have four, five tilted in the same angle it is now, but progressed down along fault nine, three, eight, prior to the movement it is already tilted, and then you move that prallelogram up along surface eight, three, toward nine, the amount it would move would be the four, one line, but that presupposes that the four, five line is tilted prior to the movement along fault three, eight.

WITNESS BRABB: I also think that requires then a fault from line six, five to eight, in order to downdrop that surface in relation to the six, seven surface, and the trench log indicates that the main fault that extends across there is not offset, therefore, this argument that it is tilted beforehand appears to be implausible.

WITNESS JACKSON: I will add a further complexity, I think I have to. It relates to the discussion with Dr. Slemmons yesterday, when this -- the fact that as you move

it -- and it relates to the feature five, eight has the same problems that we expressed in talking about a line from twelve to five. You would have difficulty in identifying a fault that is in that particular area. And what Dr. Slemmons was indicating was you could have back-rotation, if you like, around the toe of a thrust.

I personally have difficulty dealing with this kind of material in a rigorous geometric sense. That is not at all what these things behave like.

MR. EDGAR: All right, that concludes my questions.

I would like to offer into evidence Licensee's Exhibit No. 44.

I would like to have marked for identification -- There are two rough drawings I handed out: One, is two parallelograms. The other is a parallelogram against one with a hump on it.

I would like the first one to be discussed to be marked for identification as Licensee's Exhibit No. 45, and the second marked for identification as Licensee's Exhibit No. 46.

(The documents referred to were marked for identification as Licensee's Exhibits Nos. 45 and 46.)

MR. EDGAR: I would like to offer

Licensee's Exhibits Nos. 44, 45, and 46.

JUDGE GROSSMAN: I have no problem with Exhibit No. 44. My problem with Exhibits 45 and 46 is that my recollection is that you made some assumptions here, and I don't believe that anyone on the panel adopted these assumptions.

MR. EDGAR: Well, I don't think that goes to admissibility, it just illustrates what the question was. They were used for the purpose of illustration, that is all.

JUDGE GROSSMAN: Okay, and in order to have a complete record, you would like them in--

MR. EDGAR: That is all.

JUDGE GROSSMAN: Okay, is there any objection to that?

MR. SWANSON: No objection.

MR. CADY: No objection.

JUDGF GROSSMAN: Okay, they are all admitted.

(The documents referred to, having been previously marked for identification as Licensee's Exhibits Nos. 44, 45, and 46, were received into evidence.)

JUDGE GROSSMAN: Mr. Edgar, we won't hold you to your having concluded, due to the fact that there were some answers that you weren't prepared for.

Fine, we will adjourn till tomorrow at--

MR. SWANSON: Before we decide that, could we have a qualitative estimate, as opposed to a quantitative estimate from the Board, as to the examination of this panel?

The problem is -- as we had indicated in a telephone conference call, in advance of this hearing -- we did have an availability problem of Dr. Hall, on the subject of the effect of acceleration, and also to the extent that he would participate in the structural review.

We originally had set aside today and tomorrow for this examination of his analysis.

I understand from Counsel for the parties that we don't have a very long examination, probably, by the parties of him, but we do want to make sure we get through him tomorrow.

Now, it would have been the preferable course of events, obviously, to finish up with this panel, without a break, but we do have an alternate consideration, if the Board thinks that it has extensive examination, in terms of large numbers of hours, and perhaps we have this other consideration also.

JUDGE GROSSMAN: I am sorry, was there anything new to add?

MR. SWANSON: I was just trying to, I guess--

JUDGE GROSSMAN: It doesn't appear to us that we

would be examining the panel for more than an hour.

MR. SWANSON: Maybe I could just get an indication from Mr. Edgar if he has extensive examination of Dr. Hall?

MR. EDGAR: No, about ten minutes, 15 minutes.

MR. SWANSON: Well, in that case, I guess the thing to do would be to finish with this panel the first thing in the morning.

JUDGE GROSSMAN: Yes, now do you have anyone following Drs. Hall and Martori?

MR. SWANSON: Yes, we sure do. We have a probability account flying in tonight that would be available to come on, if we finish up with Dr. Hall and Mr. Martori.

JUDGE GROSSMAN: All right, fine. Why don't we adjourn then until 9:00 o'clock tomorrow morning.

(Whereupon, the hearing in the above entitled matter was adjourned, to reconvene at 9:00 a.m., Friday, June 5, 1981.)

-oOo-

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US NUCLEAR REGULATORY COMMISSION

in the matter of: GENERAL ELECTRIC COMPANY (VALLECITOS NUCLEAR CENTER)

Date of Proceeding: Thursday, 4 June 1981

Docket Number: 50-70 SC

Place of Proceeding: SAN FRANCISCO, CALIFORNIA

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

Jane N. Beach

Official Reporter

Ann Riley

Official Reporter

Official Reporter