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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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

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

Wednesday, June 3, 1981

The above-entitled matter resumed at 9:30 a.m., pursuant to adjournment.

BEFORE:

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

GEORGE A. FERGUSON, Ph.D., MEMBER.

HARRY FOREMAN, M.D., Ph.D., MEMBER.

APPEARANCES:

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

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-and-

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C O N T E N T S

Witness:	Dir.	V.Dire	Cross	Red.	Rec.	Board
Philip S. Justus) Robert E. Jackson) Robert H. Morris) Earl E. Brabb) Darrell G. Herd) Wm. L. Ellsworth) David B. Slemmons) Raman Pichumani) James Devine)						
By Mr. Sullivan	1111					
By Mr. Edgar			1178			
By Mr. Barlow			1183			

Exhibits:	Identified:	Received:
Staff Exhibit 4 - Diagram	1111	
Staff Exhibit 5-A - Photograph	1117	
Staff Exhibit 5-B - Photograph	1117	
Staff Exhibits 6-1 - 6-11	1125	

P R O C E E D I N G S

Whereupon,

PHILIP S. JUSTUS,

ROBERT E. JACKSON,

ROBERT H. MORRIS,

EARL E. BABB,

DARRELL G. HERD,

WILLIAM L. ELLSWORTH,

DAVID B. SLEMMONS,

RAMAN PICHUMANI, and

JAMES DEVINE

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

JUDGE GROSSMAN: The sixth day of the hearing is now convened.

We left off yesterday with cross-examination by Mr. Barlow.

Mr. Swanson, did you have something?

MR. SWANSON: Yes. Actually I guess there may be two ways we could proceed. One is to continue with Mr. Barlow's examination. Perhaps another, and maybe a more efficient way, would be to lead off with a short direct presentation by the Staff as to its interpretation of trench T-1, and then we could complete the overall cross-

examination of this panel. So at this point, as you recall, we have restricted the cross-examination to matters other than trench T-1, and by making a short presentation, we could then open up cross-examination fully.

JUDGE GROSSMAN: That's fine. After the short presentation, we will ask Mr. Edgar to resume his cross-examination, since he had some questions about that. Then we will proceed with Mr. Barlow again.

By the way, let me just say on the ground rules again, after we have been here for so long and eaten so many exotic foods, anyone who wants a recess at any time --

(Laughter.)

-- should just merely say they want a recess, and we'll take five or 10 minutes, without any explanation.

Okay. Why don't we start off with Mr. Swanson's presentation.

MR. SWANSON: I would first like to have Staff distribute three copies of what was formerly marked as Staff Exhibit 3 to the Board. We have had them blown up. The other parties have copies.

And in addition, we will be passing out copies of a modification of that document which I think we will ask the Board to have marked as Staff Exhibit 4, which is Staff Exhibit 3 with some additional reference points and lines drawn in.

And for convenience, it might be easier -- well, we'll probably be making reference to both documents. The document labeled Staff Exhibit 3 is the document that was referred to and made use of when the GE panel was on the stand. There are seven reference points with some notations on it.

Staff Exhibit 4 is not marked on the copy that we have distributed. It is again the same document, but with, I believe, 10 reference points marked on it, and some additional lines drawn in.

MR. EDGAR: May I get a copy of Exhibit 4?

(Document handed to counsel.)

MR. SWANSON: At this time we would ask the Board that the second document be marked as Staff Exhibit 4. It is a blow-up of Figure B-1 of GE Exhibit 2 with reference points and some lines drawn in for illustration purposes. And in the upper left-hand portion of the document, there is also some reference points and estimated distances indicated and, of course, the panel will explain how notations were made and go through the explanation.

JUDGE GROSSMAN: Mr. Swanson, do you have the original for the Board to mark now for identification, or whatever you consider to be the original, for inclusion in the record?

MR. SWANSON: I think perhaps there is no one

original of Staff Exhibit 4. Perhaps if we just take the copy given to the reporter.

JUDGE GROSSMAN: I started to say, which may be a copy of the official document. It has been marked as Staff Exhibit 4.

(The document referred to was marked Staff Exhibit 4 for identification.)

MR. SWANSON: Thank you.

By way of background, this line of questioning is addressed to Drs. Herd and Brabb and Mr. Morris. If one person answers, I will assume if we hear nothing from the other members, we will assume that you both concur in the answer, and if you do disagree, please so indicate.

DIRECT EXAMINATION (Resumed)

BY MR. SWANSON:

Q Gentlemen, when did you visit trench T-1?

A (Witness Brabb) It was in the fall of 1977.

Q At the time of your visit, were any of the trench logs prepared by GE available to you?

A No, they were not.

Q So you did not have a copy of Figure B-1 of GE Exhibit 2 at the time you visited the trench?

A That's correct.

Q When did you receive these logs?

A It was February 1978.

Q So you did not have a chance to review any logs when you were actually in the trenches, then; is that correct?

A (Witness Morris) That's correct.

Q Gentlemen, do you believe in light of the logging you have seen in your visit in the trenches, do you have any reason to believe that the log may not be totally accurate depiction of the conditions in trench T-1?

A (Witness Brabb) We believe that the log does not accurately show some of the soil conditions in T-1, and possibly some of the faulting.

Q I would ask you then to explain why you believe it is not accurate, and in so doing, to explain your interpretations of what does exist in the trench based on the information available to you.

JUDGE FOREMAN: Excuse me. Could I interrupt for a point of clarification? T-1 was the very first of the trenches that was dug?

WITNESS JACKSON: Indeed, that's correct.

JUDGE FOREMAN: Okay. That was my impression, and that helps me.

WITNESS BRABB: Your Honor, may I ask a question? The explanation will involve some discussions of soil formation, and in our report which is part of the SER, there are a series of diagrams that explain how soils are

formed, and the sequence of formation. That may be helpful to the Court in understanding some of the explanation that will be given for T-1. Would you desire that we go through the explanation before we go through the T-1 explanation?

JUDGE FOREMAN: This is Appendix B?

WITNESS BRABB: Yes, sir.

JUDGE GROSSMAN: Well, I think that's a matter for the panel to decide as to whether it would clarify matters, and do it in that progression. And if the panel believes so, then we certainly welcome having you do it that way.

WITNESS HERD: Mr. Chairman, I am going to try and attempt the discussion of T-1, and I think it would be best for everyone if we did have at least a cursory discussion of soil formation processes in relationships of soils to these faults that we observed at the GETR site.

JUDGE GROSSMAN: Well, then, we'd appreciate your doing it that way.

WITNESS HERD: Okay. May I prepare my exhibit here?

(Pause.)

MR. SWANSON: Mr. Chairman, during his presentation, Dr. Herd will also be making reference to photographs that were discussed yesterday. We do not have sufficient copies at this time to offer them into the record,

but I think it would be an appropriate time to have them marked. We did make copies available yesterday afternoon to counsel for the other parties, and they have had a chance to look at them, at least overnight.

Additional copies will be made available during the course of this proceeding to be formally received as exhibits, offered as exhibits in this proceeding. So at this time I would ask the Board to mark for identification two photographs which perhaps I should lay a foundation first for.

These are the two photographs that were discussed yesterday, and perhaps I should ask a couple of short questions of Dr. Jackson to lay a foundation for the photographs.

BY MR. SWANSON:

Q Dr. Jackson, I'm going to show you two photographs and ask you if you could identify those photographs for me, please.

A (Witness Jackson) These are two blow-ups of, I believe, either a Polaroid or a 35 millimeter slide that I took during the trench visit, approximately October 22nd. I believe it was a Saturday. I'm not exactly sure of the date. It was the weekend before the show-cause order was issued, and it was the visit to this trench that was one of the bases for issuance of the show-cause order.

I took these photos of this particular area of the trench because it was a discussion topic that related to an ashen caliche layer; not being a soil specialist, Dr. Herd will discuss the significance of that. And the apparent offset of that ashen caliche layer. So I took a photograph of it.

The discussion of that and its relationship to a fence line which transsected across the trench at the same location. There was some discussion as to whether grazing characteristics had affected the soil development in that area, or forming characteristics.

We, at the time -- or not too long after that, I drew some lines on these photographs, I believe they were in wax pencil. I don't know where the originals are. These are photographs of trench T-1 looking to the north approximately.

Q Could you indicate if this is indeed the west wall at approximately the 125 to 130 foot marker as indicated?

A To the best of my recollection, yes, sir.

Q Let's see. This is the same general area, then, that was covered by Staff Exhibits 3 and 4; is that correct?

A That's correct.

Q And it's your testimony that this is a true copy of the photographs that you took of that area?

A That's correct.

MR. SWANSON: We then would ask the Board -- it's going to be very difficult to distinguish between them. They show the same general area. Perhaps we should have them marked as Staff Exhibits 5-A and 5-B.

If the Chairman has not yet marked them, perhaps the easiest way of identifying them would be for the vertical photograph depiction of the trench -- perhaps we should call that Staff Exhibit 5-A, and the horizontal photograph of the trench could be called Staff Exhibit -- marked as Staff Exhibit 5-B.

JUDGE GROSSMAN: And we will allow you to come up and distinguish them.

(Laughter.)

I can usually tell the difference between horizontal and vertical, but I won't offer to do it with regard to trenches.

(Laughter.)

Dr. Jackson, do those photographs look like the trenches that you observed?

WITNESS JACKSON: Yes. They are not colored, of course, and there was good color contrast in the actual trenches.

MR. SWANSON: After the Board has had an opportunity to inspect them, as I indicated, we do not have

sufficient copies yet to offer them, and the panel, I believe, will need to make reference to the originals as they are testifying. Perhaps they could hold them up and show them to the Board when they need to make references.

(The photographs referred to were marked Staff Exhibit 5-A and Staff Exhibit 5-B for identification.)

BY MR. SWANSON:

Q Dr. Herd, if you would proceed, then, as to your description of your interpretations of the trench, in light of the information that you have had available to you.

A (Witness Herd) Thank you.

I ask your patience through the process. I believe that we need to form a little bit of a base to discuss trench T-1, so I would like to discuss soils and soil offsets first, and the whole of the GETR site, and then discuss their importance to the trench T-1 discussion that we have alluded to in several instances before, and then particularly to offer several interpretations of trench T-1 and its implication to the survey's position in regards to our statement of offset.

So I think first of all it would be useful for everyone to have two documents in hand to talk about soil stratigraphy. In our April 1980 report, which I believe is

Appendix B of the May 23rd, 1980 report, Figure 13 on page 24 is a multi-scene cartoon discussing events which have occurred diagramatically at the GETR site in regards to fault movements, soil formation and offset of soils.

Similarly, I think it would be useful to have nearby a copy of the February 1979 Phase 2 report of Earth Science Associates on the GETR, which I believe is, what? Exhibit 3?

MR. EDGAR: 6.

WITNESS HERD: Thank you. Particularly Figure B-2 on page B-7.

MR. CADY: Excuse me, what page number?

WITNESS HERD: In the consultants' report it would be page B-7, Figure B-2. It's variously paginated, it's toward the center of the volume. There is a color photograph of the trench wall of trench B-2. Mr. Harding is in the foreground pointing to one of the horizons.

JUDGE FOREMAN: Gentlemen, could you give those references again on page 2?

WITNESS HERD: By all means. In the Phase 2 geologic investigation, page B-7, there is a picture which is identified as Figure B-2.

JUDGE FOREMAN: Thank you.

WITNESS HERD: Okay. That's for that report.

MR. EDGAR: It's of the one with the man standing

in the trench. It's Mr. Yaden, incidentally.

WITNESS HERD: Is it? Is it? Excuse me. I can't tell from the back of the head. I thought it was Mr. Harding.

MR. EDGAR: We want to get the scale accurately.

(Laughter.)

Their heights differ.

WITNESS HERD: My apologies, Dick.

(Laughter.)

Okay. Do you also have the survey's administrative report? The Figure 13, the cartoon as well?

JUDGE GROSSMAN: Yes.

WITNESS HERD: Okay, fine. At the GETR site, we have observed that there have been a succession of fault movements, and this is best identified because there is progressive greater offset of older style horizons with depth.

In photograph B-2, Figure B-2, is a picture is a picture of a fault offset of several ages of soils, and let's use that as a beginning point, since Figure 13 is a pictorial chronology of events that are inferred to have occurred in the trench B-2.

First of all, there are two types of units and two types of processes underway at the GETR site.

There are soils that are formed by weathering into material which causes a chemical alteration in the actual physical properties of the soils and prominently causes the coloring of different horizons, basically subparallel to the surface, and then there is also a totally independent process of erosion and deposition of alluvium.

There have been successive periods of erosion which have stripped off and cut the landscape, followed by periods of deposition of material on top of it, subsequent periods of landscape stability during which soils have formed, weathering of the surface of the earth which has resulted in the formation of typical horizons near the surface.

In trench B-2, we have a picture of several horizons of soil right near the surface. If you look above the -- at the top part of the trench wall picture, you will see a gray blackish horizon immediately below the surface of the soil. This is the A-1 horizon. That part of the soil in which the organic material accumulates.

Immediately below it is an A-2 horizon which appears to have a white ashen character. This horizon results from a removal of leachable ions and other clay materials which have been moved downwards from the A horizon into lower horizons below it, the next one, called the B horizon. [illegible] inwater percolation through the soil,

or groundwater movement.

Immediately below that white horizon that you see paralleling that near surface of the earth, there is a darker, more reddish colored horizon which is sharply in contact with the white horizon above it. This is a remnant of an older soil, a B horizon, of a soil which is believed to have formed about 70,000 to 120,000 years ago.

So, in fact, we have here a record of two periods of soil formation quite near the surface and two apparent periods of the landscape instability and colluviation.

With that sort of overview, looking at that, let's just step through the diagrams now in Figure 13, and work our way through segments of events which have occurred. At or inferred from the B-2 trench site, one makes the basic assumption that before 130,000 years ago, the age of this red-colored soil which we see near the top of the surface, there was a pre-existing fault in the Livermore gravels.

Movement occurs along that fault, offsetting the landscape surface there in Step 2. There is erosion which back-wastes that scarp, beveling it, and resulting in deposition of material at the toe of the fault.

Continuing on page 25, we now approach some of the geologic material we see represented in Figure B-2. Some time after this period of erosion which occurred

before 130,000 years ago, there is a period of alluviation where material is actually deposited upon this fault, and it covers it and buries it.

And then on page 26, we have a period of landscape stability where we allow for the formation of soils.

Now I know that the document that you have has been poorly Xeroxed, but if I may just for your amplification show you a color version that originally existed. Presumably at the time when that red-colored soil formed, the one you see nearest the top of the material, there was an A horizon which was probably a darker color, dark brown to black, atop of an intensely developed B horizon, which is this red-colored soil we see near the very top of the trench in the photograph B-2.

This, by the way, is interpreted by Roy Schlemon to have formed during the period of the last interglacial of the Sangamon interglacial, about 70,000, 130,000 years ago.

MR. CADY: Excuse me, Dr. Herd. May I interrupt you for one second?

Were the copies that were presented to the court reporter -- do they include the black and the red diagrams that Dr. Herd is referring to? I think it would clarify the record if those copies were presented to the reporter.

MR. SWANSON: I can answer that. They don't, because the copies that were distributed publicly were the Xerox copies which appear, of course, as Appendix B to the Staff's May 23rd, 1980 SER, and came out only in the form of gray with sort of black islands. I think it's very difficult to distinguish.

We have one copy of the colored version, perhaps.

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WITNESS JACKSON: I have another copy.

WITNESS HERD: If it would assist the Board, I would be more than happy to leave this with you as an exhibit afterwards. I do want to check to be sure that I do not have any annotations or data in the margins which I might have used in other reports, because this is a copy I have had in my office for some time.

JUDGE GROSSMAN: Mr. Swanson, I think we can, even if we close the record, leave it open for the submittal of a subsequent document without disturbing the remainder of the case.

Why do we not allow you to take it back to Washington and have color Xeroxes, or whatever kind of reproduction is necessary.

MR. EDCAR: That is fine with me. I have no objection if just the pages that are discussed are supplemented in just a little separate package.

JUDGE GROSSMAN: Yes. That is what I intended, just the duplication of that.

MR. EDGAR: The color plates of this, sure.

MR. SWANSON: This then would be a color depiction of Plate No. 5 of Figure 13 of Appendix B to the Staff's May 23rd, 1980, Safety Evaluation, which is Staff Exhibit 1-B.

MR. EDGAR: Why don't you make it color plates

of Figure 13, Plates 1 through 11, because he is going to need them to discuss his complete sequence.

MR. SWANSON: We will make that available, then.

JUDGE GROSSMAN: Why don't we mark that sequence of photographs as Staff's Exhibit No. 6.

MR. SWANSON: So again for identification, it would be Plates 1 through 11 of Figure 13.

JUDGE GROSSMAN: That would be Staff's Exhibit No. 6, subs 1 through 11.

MR. SWANSON: That is correct.

(The documents referred to were marked as Staff Exhibit Nos. 6-1 through 6-11 for identification.)

JUDGE FOREMAN: I realize it is a minor point, but for my information can you distinguish between colluvium and alluvium?

WITNESS HERD: No. I have made a coarse generalization there which has been unfair. It undoubtedly is colluvium material that has been deposited here as opposed to alluvium. That is, this appears to be -- the material is poorly sorted and appears to be more typically associated with mass wasting down slopes which has covered it. There may be some alluvial

deposits locally associated with fans that have formed that have formed at the hill front. So I am using alluviation in the broad sense to encompass both alluviation and colluviation, mass wasting and stream action, stream deposits.

Thank you for that point.

Shall we continue?

BY MR. SWANSON:

Q. Yes.

A. (Witness Herd) So sometime in Figure 13, Picture 6, we have an offset which occurs displacing the red colored soil and its overlying and accompanying A horizon.

In Cartoon Step 7 on the next page, sometime during the last glaciation between 70,000 years ago and 17,000 to 20,000 years ago, there is a period of landscape instability where we have erosion.

This erosion apparently strips off all remnants of the overlying A horizon, the darker horizon that we no longer see, and causes the B horizon to appear to be thinned by beveling, stripping it off.

Consequently, there is a formation of a lag gravel along the surface of this erosion front, and this is the so-called "stone line" that we have made reference to several times in the past.

The stone line is interpreted by Earth Science Associates and their consultants to represent a period I believe around 17,000 to 20,000 years ago, sometime during the end of the last glaciation when the conditions were supposed to be wetter, and a period of erosion was underway in the California Coast ranges.

In Cartoon Step 8, we now approach events which are nearer to the present. We have another period of deposition, colluviation and alluviation, which now buries this offset soil and the stone line which truncates it. And into this colluvium and alluvium that is deposited atop of this fault -- atop of this stone line, we have the development of the modern soil profile which we see now in the uppermost part of the trench B-2. That is the photograph that you -- let's just momentarily refer to Figure B-2. That would be, we are now developing the soil that you see immediately below the surface of the land.

JUDGE FOREMAN: That grey material?

WITNESS HERD: That's right. The grey material, the underlying white horizon, all of which are developed in a unit which is deposited atop of that very bright red soil.

The sharp interface in that photograph between the white and the dark red is the interface

between the base of the overlying most recent alluvial colluvial unit, and the top of the truncated buried B-2 horizon below it. And the line that divides them is the stone line, this surface of erosion which is presumed to have occurred sometime near the end of the last glaciation, approximately 17,000 to 20,000 years ago.

Into this overlying material is developed this modern soil which has a grey-black A horizon, and an accompanying A-E horizon or A-2 horizon which is ashen, whitist colored because of the depletion of leachable materials from that horizon, in effect of a chemical action of presumably a vegetation chelating some of the leachable materials and transporting it farther down in the profile.

This horizon sequence -- excuse me. This colluvium/alluvium with its developed soil profile is subsequently offset in Figure 10 -- excuse me, in Cartoon Step 10 of Figure 13. Now notice the consequent effect. There is an apparent offset of I believe it is three feet of the whitish A-2 horizon, but then there is an apparent greater displacement in the underlying horizon below it. That is, the red-colored soil. Because remember that it was offset before 70,000 years ago, and now we've added to it a second offset that has

occurred on top of it. So the effect is to have an apparent increase in the offset as you go with depth in older horizons.

Subsequently, in Cartoon Step 11 in Figure 13, there must be some sort of erosion which occurs, because there is no apparent scarp seen in association with the soil offset in Figure B-2, and I am unaware of it elsewhere. But let me point out that there is a difference of interpretation as to the ages of some of these horizons. I believe Dr. Shlemon has inferred an age of about 8000 to 15,000 years for the A-2 horizon, and considers the A-2 to be offset, but does not believe that the surface A-1 horizon is offset. But that is a minor difference in terms of understanding the sequence of events which we interpret to have occurred in Trench B-2.

So with this sort of foundation, there are two critical elements that we will talk about: The A-2 horizon, this ashen-colored part of the soil profile which has developed nearly parallel beneath the modern land surface perhaps to have formed someplace in the last 8000 years, certainly in the late holocene-- early holocene, perhaps; and the underlying stone line, which is this erosional unconformity which represents the dividing line between the alluvium/colluvium which has

been deposited atop of this buried red-colored soil formed during an interval perhaps 70,000 to 130,000 years ago. That is basically the framework of soil stratigraphy. And with this in reference, let's now go to Trench T-1, if we can.

JUDGE FOREMAN: Before you leave, could you identify the stone line on Figure B-2 for me?

WITNESS HERD: Okay. Figure B-2, the stone line would be that sharp contact which you see in the upper quarter of the picture. It is a sharp boundary which lies immediately atop of the red-colored soil, the red-colored horizon which extends left to right in the picture. It is the abrupt contact between the red and the white above it.

JUDGE FOREMAN: It is approximately 2-1/2 to 3 inches from the left-hand margin, then.

WITNESS HERD: Excuse me? I didn't hear your question.

JUDGE FOREMAN: One could further identify just on coordinates, so to speak, approximately 2-1/2 to 3 inches from the left-hand margin, then.

WITNESS HERD: Okay. The A-2 horizon, this white-colored horizon is offset by this fault so that it appears to be lower above the gentleman than it does to the right of the fault.

JUDGE GROSSMAN: By the way, when you said there was a disagreement, that in place of the 17,000 to 20,000 years I believe your figures are 2000 or 4000 years?

WITNESS HERD: No. It is not the period of erosion which truncates this buried soil; it is the age of soil formation which occurs most recently. That is, the development of the modern profile.

As I understand it, Dr. Shlemon interprets an age increase in the horizons as you go below the surface, or at least there is a reported age of about 8000 to 15,000 years for the A-2 horizon.

We have said that we believe that the modern soil, including the A-1 and white A-2 horizons may be as young as 2000 to 4000 years in age. So we are talking about a 4000-year difference. We are talking about events of soil formation in the last 10,000 years in the holocene.

JUDGE GROSSMAN: Thank you.

WITNESS HERD: I can see Dr. Shlemon cringing, so I hope I haven't miscast your position too badly.

(Laughter.)

WITNESS HERD: All right, now, if we may talk about Trench T-1.

(Pause.)

WITNESS MORRIS: Are you getting feedback [from the microphones]?

JUDGE GROSSMAN: We can all hear the feedback. I don't know who is here that can turn it down.

WITNESS HERD: All right, can the Board see this diagram from where you are?

JUDGE GROSSMAN: Yes.

WITNESS HERD: It might also be helpful at this point to have a copy of the log of Trench T-1 now in hand. Now that we have talked about soil stratigraphy and you have a general understanding of the different soil horizons and the like, in particular we would like to talk about the log of Trench T-1, which is Figure No. B-1 in the Earth Science Associates' report of February 1978, "Geologic Investigation: General Electric Test Reactor Site." It is a plate in the rear of the volume.

This chart which I am going to use on the right is an enlargement of part of the central area of that trench.

JUDGE FOREMAN: I don't have that B-1 reference.

MR. SWANSON: That is General Electric Exhibit No. 2.

(Pause.)

JUDGE GROSSMAN: It is not this (indicating) is it?

WITNESS DEVINE: No.

JUDGE FOREMAN: General Electric Exhibit No. 2? This is their --

WITNESS HERD: It is the February 1978 Report of Earth Science Associates.

MR. EDGAR: It is Licensee's Exhibit No. 2.

WITNESS HERD: Thank you.

(Pause.)

JUDGE FOREMAN: Okay, I have Exhibit No. 2 dated February 1978, and the page reference?

WITNESS HERD: It is a plate in the rear of the volume. It is Figure B-1, identified as the "Log of Trench T-1." It is a fold-out in the rear of the volume.

JUDGE FOREMAN: Okay. Thank you.

WITNESS HERD: This diagram that I have here to my right is an enlargement of the area between stations 1-15 and 1-40. This diagram, by the way -- the trench log, as far as I know, has an equal scale horizontal to vertical, so that displacements can be measured in either direction. There is no distortion of the photograph -- excuse me. There is no scaling figure that needs to be involved in looking at the apparent offsets. You can use the scale on the top or the side, and it is an equal amount of measurements.

BY MR. SWANSON:

Q. For identification, Dr. Herd, are you referring to Staff Exhibit No. 4, the blowup with notations?

A. (Witness Herd) Yes. I would like to add some additional lines during the course of this discussion, as well, and I guess we will have to perhaps mark those additional lines onto one of those exhibits.

Q. Okay, but the reference points that you were mentioning up to this point are the reference points depicted in what has been marked as Staff Exhibit No. 4. Is that correct?

A. Right. Thank you. And I will add several additional points, as well, during the course of my discussion.

Okay. Trench T-1 was the first trench that was excavated at the GETR site. It was opened originally to discover whether that there was faulting where the Verona Fault had been projected on my map.

The trench log was completed and released in this February 1978 report and, as has been pointed out previously, we did not have the trench log at hand when we visited the trench. And we are now going to try to interpret some data from this trench log without benefit of visiting the exposure to make our own

annotations on this trench log.

Trench T-1 is important for our discussion because it is one of the elements which concerns the surveys letter of May 8th, 1980, which accompanies our April 1980 report in Appendix B, I believe it is, and if I may just read one of the latter sentences from it:

"The one meter of displacement proposed by the applicant does not appear to be conservative in light of the five feet of movement recognized along the B-1 B-3 fault."

It is this five feet of offset that we will talk about which was interpreted from T-1. We will amend that observation in light of new trench data that -- in light of new photographs that we have seen, and discuss this in a fashion which will hopefully help you understand the uncertainties involved in the interpretation, and the measurements that one might make from this interpretation.

JUDGE GROSSMAN: Without keeping us in suspense, are you going to add on another two feet to that?

(Laughter.)

WITNESS HERD: No, I am not.

JUDGE GROSSMAN: Okay.

WITNESS HERD: I am going to conclude that we feel comfortable with the statement that we had issued

previously, but we will point out some differences in our matter of interpretation and the number of complexities that have been introduced.

At the time that our deposition was -- that Mr. Edgar took a deposition of Earl Brabb and I, we had explained that we had reconstructed an offset in Trench T-1 by extrapolating surface 6-7 to the right of the fault outward to point 9, and measured the apparent offset of that surface along 9-3, which is the extrapolation of the fault at depth.

We had pointed out previously that we felt that there -- we had observed an offset of the stone line and overlying A-2 horizon, and without being able to see an apparent offset documented in the trench log of T-1, we were trying to interpret how much offset we could have inferred from that portions of the trench log with which we felt comfortable.

In doing so, we extrapolated a surface across this step 4-5 which is in the trench log of the Earth Science Associates, and we had then -- well, excuse me. In the subsequent Appendix A of Harding and others' testimony, there is a discussion of why this was felt to be an improper interpretation, pointing out that this step was real and that the manner of extrapolating this surface was unsupported by observations of a continuation

of the sufrace and the so-called "wedge" into the soil above the fault area to the right.

JUDGE GROSSMAN: Could you identify which surface you are talking about?

WITNESS HERD: In other words, this wedge (indicating) which would have been 6-9-3 into the soil.

During lunch yesterday we learned that Dr. Jackson had taken two Polaroid photographs of perhaps a critical area of the trench, and realizing that it might provide us an opportunity to recheck the validity of the trench logging and our interpretation, we asked for the chance to look at it. And we have made some observations and conclusions with regards to that.

I am now going to look at photograph Exhibit No. 5-B. That is, that portion of the trench log which presumably represents more or less the area near Station 1-30. That is, in the area of step 6-5 of the log. I believe that this is that part of the trench log and trench wall, because in Dr. Jackson's annotations on this photograph there is the word "fenceline" above it. And this is the area where, on the consultant's diagram, is "fencepost" indicated. So I believe that that fenceline is the same as the fencepost above the step 5-6 in the trench log.

In this photograph, Exhibit No. 5-B, we can

see a piece of metal shoring to support the wall of the trench. And then to the left of it, a horizontal whitish-colored horizon. This presumably is the A-2 horizon of the modern soil which we discussed previously.

It is associated with a concentration of gravel which appears to be sticking out in the wall of the face of the trench there, which is also apparently the stone line associated with it. So apparently we have left of the shoring here (indicating) an A-2 horizon resting immediately atop of a shoring -- excuse me, atop of the stone line.

JUDGE GROSSMAN: Excuse me for a second. I notice that the GE panel seems to agree that where Dr. Herd has located this photograph is correct with respect to that fenceline?

MR. EDGAR: We would stipulate to that face.

WITNESS HERD: Okay. Thank you.

(Witness Herd and Jackson confer.)

WITNESS HERD: Dr. Jackson said it would be helpful, and I agree, to also just mention photo 5-A. This is almost the identical same area, except in the lower part of the vertical photograph Exhibit 5-A near the bottom of the trench, just above the shadow, you can see a low dipping surface which is marked "fault

surface," and that is presumably the fault that we see in line segment 3-8 on our exhibit here to our right. So the fault that is mapped in -- this major fault that is mapped in this trench is below the white ashen A-2 horizon and the accompanying stone line.

All right. Now let's go back to the horizontal view 5-B. Excuse me?

JUDGE FOREMAN: Is that the basis on which the fault line is depicted on this diagram, the Staff Exhibit No. 4? Is that the basis on which this fault line was drawn? Or are there other bases, too?

WITNESS HERD: This diagram here? You mean line 8-3?

JUDGE FOREMAN: Yes.

WITNESS HERD: No. This was prepared by Earth Science Associates while mapping in the trench. So it is a physical observation and documentation of the fault as they saw it.

MR. EDGAR: That line shows in the original trench, which is Licensee's Exhibit No. 2.

JUDGE FOREMAN: So what you are saying is the observation that you made on that vertical photograph is just a verification?

WITNESS HERD: Correct. Thank you. Right. It is a reference point there.

Continuing with photo 5-B, in the upper part of the shoring there is a horizontal line continuing across the shoring to the right, which has been marked on in probably a Marks-a-Lot pen of some sort. So that would be at the upper right corner of the picture.

Dr. Jackson tells me that this is his recollection of the general position of the continuation of that A-2 horizon, and I interpret that apparent step in the position of the stone line and the accompanying A-2 horizon to be the step which is documented in the trench log as between line segment 4-5 and 6-7.

This document -- these photographs would help confirm the validity of the existence of an intervening step in the trench log, as has been presented by Earth Science Associates, and it would make an improper interpretation of what we had done previously. That is, the extention of surface 6-7 out to point 9, because there was an intermediate step 4-5 that was real.

Now that does not negate our attempt to try to interpret the amount of displacement in this trench. To do so, I would like to look momentarily with you at the overall log of trench T-1, which I asked you to have out before.

MR. EDGAR: That is Licensee's Exhibit No. 2, Figure B-1?

WITNESS HERD: Yes. Thank you. I appreciate your pointing these out.

So to summarize just briefly, the photograph allows us to not only confirm that there is a stone line at line segment 4-5 in the log diagram, but there is also an A-2 horizon immediately above it. And this step that is marked here is not only a step in the stone line, but it is also a step in the A-2 horizon which occurs between 4-5 to 6-7.

Okay. Now in the trench log just in the area below station 100, there are checkmarks going down to the westward continuation of line segment 10-3, which is described as a concentration of dusty caliche along the sharp contact.

end
JWB
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I interpret this to be the A-2 horizon, juxtaposed on top of the stone line, just like we see in the photograph 5-B and A.

So, to summarize, then, it would appear that there is a stone line with accompanying A-2 horizon which more or less is line segment 10-3.

Then there is an abrupt step to 4-5 and an abrupt step to 6-7.

WITNESS BRABB: Excuse me, Dr. Herd and Mr. Chairman. Before he begins the explanation, this might be a convenient time to take a break, if the Court so desires.

JUDGE GROSSMAN: That's fine. We'll take 10 minutes.

(Recess.)

JUDGE GROSSMAN: Dr. Herd, could you proceed?

WITNESS HERD: Okay. Perhaps a moment just of brief summary so we can restore where we were, and then going from there.

Okay. So to summarize, then, we have in trench T-1 a stone line and locally superimposed ashen or A-2-E horizon which is crudely approximated by line 10-3, 4-5 and 6-7. And there are abrupt steps in this at step 3-4, 5-6.

Okay. Now we observed -- we, that is, Earl Brabb

ar3-2

and I and Dr. Morris -- Mr. Morris, in the trench, and I believe other members of the Nuclear Regulatory Commission, that this A-2 was offset by faulting.

So now from this trench log, let's try and look at what we have learned from the photographs and its possible interpretations with faulting is one alternative and other explanations as well.

In this trench log, then, we have an apparently planar A-2 horizon in the western part of the trench that abruptly steps up as station 3-4, continues to 4-5, and then abruptly steps up again and continues to the east at 6-7.

This abrupt step occurs -- one of the steps 3-4 occurs at the position where one of the faults intersects this point. That is fault E-8-3.

Now it is important to talk about not only the offset -- excuse me, the steps in the stone line, but the steps which occur as well in the overlaying A-2 horizon, for remember that there is a tremendous time difference represented between the stone line and the A-2 horizon. The stone line is the surface of erosion which is of the general order of 17 to 20,000 years in age, which has been buried with alluvium-colluvium deposited in the thousands of years subsequent to that, and into that overlying material is developed the modern soil profile, one of these

horizons of which is this white ashen A-2.

The fact that they are locally together is significant, but also important to recognize that we have two time lines then that are closely spaced, one that -- the stone line that's about 17 to 20,000 years, and then the ashen A-2 horizon that may be of the order of 8000 years or greater, or by our interpretation a much younger age for that A-2 horizon as well.

The steps in the A-2 horizon position are rather abrupt. There are two explanations that can be offered for these abrupt steps in the horizons.

Before I go into that, just let me make a brief comment. In Appendix A of Mr. Harding's testimony, there is a photograph which shows an enlargement of the area around point 3. We have been given access to the photograph, and there appears to be some sort of scaling problem difference, and I think that the photograph really shows only a detail in the area around point 3.

We didn't really ourselves learn too much information about soil relationships at that point, so we appreciate the use, but I don't think there's much that we have gained in new information from that point.

Okay. We have then two steps and their interpretations of their occurrence. One interpretation is that the steps in the profile development and the stone

line are simply a function of erosion and subsequent soil formation. That is, that there is no faulting which offsets this upper 17,000 year package of sediments and soils, and that this is just simply a normal process which has allowed for three abrupt steps.

However, I would point out that in looking at the photograph of trench B-2, and in observations elsewhere, I recall a fairly linear uniformity, a parallelism between the A-2 horizon and the land surface, and that where we saw these abrupt steps before, these were associated with faulting as another explanation for their apparent step-up or offset, apparent offset.

So we can interpret these steps here to be simply a function of normal pedogenic processes without faulting onto a land surface that was buried which had some topography in it, and that the soil formation has proceeded to that step of contact.

In looking at soil formation in the general GETR area, I am uncomfortable with that explanation, because I believe that the A-2 horizon in most instances was a fairly uniform and widely extent surface, which was nearly parallel to the surface, and where we did see these abrupt steps, we also saw fault offsets accompanying them.

The second interpretation of this trench log would be that these steps are caused by faulting, which has

displaced a once-continuous stone line and the accompanying over-superimposed A-2 horizon.

Step 3-4 would be readily explained by movement along fault 3-8, which would have caused a once-horizontal continuous surface, A-2 horizon, to have been moved upwards and relative to me, to towards the west, causing an apparent offset in that surface.

At step 5-6, there is no fault mapped immediately intersecting that step, but if you will notice that in the area immediately below station 1-40, a second fault, at least in the area of this figure, is mapped as well. It's a parallel shear which lies above the fault 3-8.

If I were to extend this fault and projection up to point 5, I would more or less intersect this step where 5-6 occurs.

If we were to interpret each of these steps as being occasioned by fault offset, the apparent movement would be measured in the following way:

If at point 5, by extrapolating that fault surface up to its point of intersection with the stone line and A-2 horizon, you were to continue it on upwards to its intersection with the projection at surface 6-7, there would be an additional point which isn't on your logs, and I propose to call point 11. Line segment 5-11 would be the amount of apparent displacement that would have to occur

if movement along that fault now called 11-5 resulted in the apparent step which exists between 5-6 -- between surface 4-5 and 6-7.

JUDGE GROSSMAN: Excuse me. Point 11 would be in between points 9 and 6?

WITNESS HERD: That's correct.

MR. SWANSON: Maybe for clarification, since we are developing a written record now, you could orally explain the position of point 11 with respect to other features on that chart.

MR. EDGAR: Could I ask one more point to be annotated, just to make the record clear?

There is a -- on Staff Exhibit 4, you have a trench above -- I mean, excuse me, a shear or fault above and to the right of the primary fault shown. Then you have extended a line from the end point of that fault to point 5.

WITNESS HERD: That's correct.

MR. EDGAR: Would you mind marking point 12 where the extension line begins, so that we understand that we have defined the line by making two points?

WITNESS HERD: I'll be happy.

(Witness drawing.)

Okay. I have extended then, to summarize, the shear surface which lies above shear 3-8 from point 12 to point 5, the step that occurs in the stone line, and

have continued to extend it in projection, a line with the fault, at point 12 to its intersection with line segment 7-9, which is the extrapolation of surface 6-7 to the fault.

The point of intersection of fault 12-5 with the extrapolation of surface 6-7 is point 11. So if faulting occasioned the offset and the surface and -- excuse me, if faulting occasioned the displacement of the stone line in A-2 horizon between 4-5 and 6-7, the displacement that presumably would have been involved to have accomplished that would have been movement along fault 12-11 which causes the block to override and have an apparent consequent step upwards to the east in that surface.

That is, the buried stone line in superimposed A-2 horizon.

Similarly, the same sort of operation would be involved to document or measure an apparent offset between point 10-3 and 4-5, assuming these were still part of the once continuous horizontally continuous A-2 horizon. Then the calculation of that offset would be first the extrapolation of surface 4-5 to the west towards the fault, which would be line 5-4-9, which is already marked on the document on the exhibit, and point 9 is the intersection of the extrapolation of fault 3-8 with that line segment.

It is also, by coincidence, the intersection of point -- of line extension 6-7 to the west to point 9.

JUDGE GROSSMAN: Excuse me. I noticed that coincidence, and I want to ask you to clarify whether that was a true extension of line 4-5 to point 9, or whether because of the drawing it just approximated that extension.

WITNESS HERD: We have tried to make a careful extrapolation of it, and as far as I know, it is a simple coincidence.

JUDGE GROSSMAN: And are all the angles here basically true angles, such as trench -- I'm sorry, point 12, the fault line there, is that a true angle as to 12 and 12 prime, or whatever you want to call the other?

WITNESS HERD: As far as I know, this trench log is an attempt to accurately portray the apparent angle and dip of the faults as encountered in the walls of that trench. So it may not be the true dip of the fault, it's the apparent dip of the fault in the wall of the trench.

WITNESS BRABB: I'm not sure that's the question he asked.

Mr. Chairman, are you asking whether the fault shown on the trench log that extends to point 12 and in the dashed red line beyond that is a true angle, 180 degrees? Was that your question?

JUDGE GROSSMAN: No, no, I believe Dr. Herd understood my question. The question really didn't relate

to the extension. It related to the fault line 12, and perhaps 12 prime on the other side, whether that was a true angle with respect to the remainder of the drawing, and similarly whether the direction of line 4-5 was a true direction. So that all the projections that managed to hook up happened to do it at the appropriate places.

In other words, the line 4-5, the extension of 4-5 happening to meet the extension of 6-7, because they are true directions, rather than just approximations on the drawing.

WITNESS BRABB: Thank you. I misunderstood. Excuse me for interrupting.

WITNESS HERD: Okay. To recap a moment here, there is a lot of good advice coming from different directions here.

(Laughter.)

All right. It should be pointed out that in addition to fault 3-8 at point 3, you can also see a second break immediately parallel to it, and slightly above it. The apparent offset which occurs between step 4-5 line segment and 10-3 might equally be explained by movement along an extension of that second fault surface as well, and for the purpose of completeness, let's just add that as well, if we may.

So I am going to now label the second additional

fault which lies above line 8-3, I'm going to call this now fault 8-13, point 13 being the intersection of that secondary break where it branches with the intersection of the stone line relative to points we have already discussed.

It would be slightly above and to the right of point 3.

Are you having difficulty?

MR. EDGAR: I'm sorry. I just didn't hear.

WITNESS HERD: Okay, fine. I'm referring specifically to near point 3, there is a forking, apparent forking in the fault line. Fault 8-3 I'm going to call point 13, the point of intersection of the upper part of this fork with the stone line.

MR. EDGAR: Okay.

WITNESS BRABB: Darrell, let me -- do you mean -- I don't think it's been established that that point is the stone line. It's so depicted on the trench log, but I think we have information to indicate that from the records that we examined.

WITNESS HERD: Fine. Agreed. Relative to the mapping and interpretation of the trench offered by Earth Science Associates, that would be the intersection of this step with this line with the fault, right.

I wish to -- we have a disagreement as to the character of the mapping of the critical elements of the

trench, but I was trying to struggle with the identication of this point.

So, to summarize again, point 13 is the intersection of the continuation of the upper fork of fault 8-3, to the point of intersection with the line interpreted by Earth Science Associates and their consultants as being the stone line.

Are we all agreed on that point, just for point of reference?

MR. CADY: Excuse me. For clarification, since there are numerous points in that area of the map, could you place a small "X" where point 13 is to be located?

WITNESS HERD: I already have shown point 13 here on the figure. It would be just above and to the right of point 3. Can you see it? It's right there. I think it would be -- if this was ever reproduced, it would be difficult to have an "X" since we've been using numbers in sequence.

MR. CADY: Is it at approximately 10:00 o'clock above that black dot?

WITNESS HERD: Relative to point 3, it occurs at the position of 2:00 o'clock, relative to the black dot -- there are two black dots.

MR. CADY: Right. The one in the southwest.

WITNESS HERD: The southwesterly black dot

which is connected by a tick to point F, relative to that black dot, it occurs at 12:00 o'clock, immediately above.

MR. CADY: Okay. Thank you.

WITNESS HERD: Okay. Step 4-3 could be equally interpreted by movement along a continuation of fault 8-13 to its point of intersection with surface 4-5 and that point of intersection would be a point which I will now call point 14. Relative to other points on this diagram, 14 appears to be immediately above point 1 on line segment 9-4.

Step 4-3 could be explained then by fault movement on either fault 8-9 or on 8-14, which would accomplish the same apparent offset that we see in step 3-4.

The measurement amount of offset that would be required would be about five feet on either one of these two shears -- on each of these two shears, to accomplish this offset in the step.

Let me express that I am uncomfortable in having created this extrapolation inasmuch as the continuation of surface 6-7 to point 11, or the continuation of line segment 4-5 to point 9, for example, are not documented in the trench log.

However, I believe that the more -- the preferred interpretation from my vantage would be that

these indeed occur on these two faults to occasion the displacement of a once-continuous A-2 horizon.

We had noted previously that we had seen in trench T-1 that there were offsets in the overlying surface soil, the modern soil, the A-2 horizon, and these steps where these soils are offset in an topographic position are places where there are faults intersecting.

end 3

As a consequence, I believe that there is evidence in this trench of displacement of about five feet on each of two breaks, and that even though we have changed our interpretation of the actual calculation of this offset, recognizing that there is an intermediate step 4-5 that is real, we still conclude that we feel uncomfortable with, and believe that there is evidence for more than a meter of displacement on a break in the Verona Fault Zone as evidenced in this trench.

JUDGE FOREMAN: Mr. Herd, these projections that you make are done on a drawing made from a photograph, or on the basis of a photograph. Is the scale that accurate that you can draw the kinds of inferences that you do?

WITNESS HERD: Let's stop a second. We used the photographs to provide references of independent documentation of things we saw in this trench log. In other words, the photographs Exhibits 5-A and 5-B helps us to realize that step 4-5 is real, and that there is at line segment 4-5 and A-2 an overlying stone line there.

The photograph does not really tell us much more than that, but helps to document that one step that is there.

WITNESS BRABB: Can I add some information, your Honor? This is a secondary technique. It is not as good as taking the measurements directly in the trench at the time the trench was open to try to establish these relationships. We are merely trying to do the best we can with the information that is available to determine what is reasonable and approximate.

We have used the term "approximate measurements" to convey to you the uncertainty in the measurement, because small amounts of deflections of the line for example can result in different measurements. In fact, you may observe that there is a mathematical absurdity in the totals of the figures for line 3-1 -- I'm sorry, 3-2-1-9. In one instance, I think it adds up to 7 feet; and in the other instance, it adds up to 6 feet.

The reason for this is that I was very uncomfortable about using precise measurements such as 5.6, and I used approximate measurements. In so doing, we combined two figures that were about one-half, and in so doing when we totaled them we came up with the next higher number.

Again, perhaps we should have used the more precise figures, but I felt it was misleading to try and

convey to you that we can do this exactly; we cannot.

JUDGE FOREMAN: Let me follow up, then, to be sure I understand what you are saying. Then do you have a high degree of confidence that the difference between your projections leading to a number of 2 feet and a number of 5 feet are real differences?

WITNESS BRABB: Yes, sir, we do. We have wiggled the plane back and forth to investigate that possibility that this is just a slop, so to speak, in the methodology. We believe that these differences are real. We think that the order of magnitudes that we are talking about here are the correct order of magnitudes; that it is more on the order of 5 feet than it is 2 feet.

JUDGE FOREMAN: And that is why you keep saying that the offset should be greater than one meter, rather than putting any precise number, because you aren't putting a high degree of faith on that 5-foot number on which you are basing your inferences?

WITNESS HERD: We have documentation, we believe, in trench T-1 and elsewhere that there is evidence that there has been more than 3 feet of movement on a break in the Verona Fault.

WITNESS BRABB: I would like to point out also that there are still different and more complicated

interpretations of this information, as you can well imagine. We have tried to simplify it somewhat, but for example the surface 4-5 offset from surface 6-7 could be explained by faulting along the surface 5-6. This would be a normal type of fault where that surface would be downdrop.

Arguing against that is the presence of the main shear map by the consultants which shows no offset, and therefore this is one of the things that we have considered but discounted based on the information that we have there.

There are other difficulties, as well. For example, the fault projection 11-5-12 and the fault line, the minor fault mapped by the consultants, continues to the right outside of the diagram and can be seen on your trench log. And in the lower part of that log, you can see there are surfaces that have been mapped by the consultants --

WITNESS HERD: "Horizons."

WITNESS BRABB: -- that are not offset. This is a dilemma that we face in the interpretation of the information, and some of the unresolved problem with respect to these measurements. Also, it reflects our conservatism and unevenness in the kind of exercise that we are going through.

WITNESS JACKSON: I would like to make one additional comment to add to what Dr. Brabb said, and for the Board to put it in perspective, why Trench T-1 was approached differently than the latter trenches.

At the time of Trench T-1, there was great concern. The plant was operating, and there was great concern about whether there was or was not a capble fault or an active fault in close proximity to the plant. Our initial traverses of the trenches were for the purposes of determining whether there was or was not the potential of an active fault near the facility, and did it have young movement on it -- Holocene movement; that is probably redundant.

Our conclusion was that it did, and it led to the show-cause order. We were not at that time, and for sometime afterwards, concerned about trying to use the trench data to develop recurrent-movement arguments or total amounts of offset, as a matter of fact.

So as later trenches were put in, we then were looking at them with a very different approach in mind. So then Trench T-1 had been filled for safety purposes.

JUDGE FOREMAN: A follow-up question. In attempting to judge the validity of your inferences,

would information about the time of that so-called minor fault, the fault identified as 11-5-12, the time of that fault, would knowing that add any information to give you confidence in what you are doing?

WITNESS HERD: Yes.

JUDGE FOREMAN: Or doesn't it affect it at all?

WITNESS HERD: Oh, indeed it does. If you envision this trench as having a sequence of time lines that increase in age as you go down because there are a number of buried soils that are in this trench, knowing which age of offset is represented by which fault, where the fault offsets, what particular age would be very critical in understanding it.

If I interpret this log correctly, the existence of this fault 12 in this horizon immediately beneath the stone line indicates that there has been faulting which has at least occurred in the most recent horizon in age that lies immediately below the stone line.

JUDGE FOREMAN: Which is 70,000 years, or something like that?

WITNESS HERD: I don't think we have any clear assurance that this soil here (indicating) immediately below it is the 70,000 to 130,000 year old

soil. I think that is true, and I think it is a good working hypothesis to infer that, but I do not believe there has been any sound documentation of it by comparison to the other trenches. The relationship would be one that you would expect the buried soil immediately below the stone line would be on that order of age 70,000 to 130,000 years ago; but I don't believe. Because Dr. Shlemon did not make a discussion of the soils in this trench, we have no evidence from the consultants in hand to discuss the identification of the soils in this trench. So that is an inference. Okay?

But can I finish out on your one point, if I may?

JUDGE FOREMAN: Sure.

WITNESS HERD: What would be critical to know is: Did fault 12 actually extend up to point 5? And was it continuous off to the right into the lower part? Was it not recognized the continuations of it because of the nature of this rounding material?

Looking at the photographs, it looks like it was a very dense blocking material which is indicated by these hatchered patterns, the blocking character to it. And it may be that, from my vantage point, I think it very easy to have missed the continuation of the fault

in that sort of nature of material.

So as an interpretation, I believe that it is reasonable to think that fault extension from point 12 to 5 is reasonable. And certainly that fault to the right of point 12 did displace the material which is the same age as that between point 12 and 5. It would just be a continuation of mapping of an offset in the same aged material.

Okay, can I summarize just quickly the main points for everyone and from my own vantage point, too, to make sure that I have emphasized the particular points?

With the advantage of the photographs, we feel confident that there are three major steps -- three major line segments in this trench --that is, 10-3, 4-5, and 6-7 -- which are associated with a stone line and a superimposed A-2 horizon.

The steps occur -- one of the steps occurs at the point of intersection of fault 8-3 or 8-13. The other step occurs at the extrapolation of fault 12 to point 5. These steps could be interpreted in two fashions. One, by no fault offset, burial of a surface that had original topography -- in other words, that line segment 7-6-5-4-3-10 represents an eroded surface which has steps which are locally associated with

faulting; and that superimposed on top of that, we have had the A-2 horizon juxtaposed against that contact with an accompanying abrupt step.

We remember in our observation of the trench T-1 that the A-2 horizon was offset. Apparently these offsets occur in areas where these steps 6-5 4-3 are. We believe that pedogenic soil development would suggest that elsewhere the A-2 horizons are fairly linear and uniform in their position paralleling the surface of the earth. And that where we have seen these abrupt steps as in Trench B-2, there has been fault offset.

I personally prefer the interpretation that there was a once-continuous A-2 horizon locally superimposed atop of the stone line that has been displaced at two points by faults -- in this case, by faults 8-3 and faults 5-12 -- and that the movement has been of the order of about 5 feet on each of two faults to accomplish this offset.

So we have differences, then, in terms of the interpretation and logging of steps 6-5, 4-3, and the subsequent interpretation of the fault offset.

JUDGE GROSSMAN: One quick question. I take it your offset of 5 feet is line 9-2, and not line 9-3 which is 7 feet. Is there any question as to what is

the appropriate line?

WITNESS HERD: I said in the general order of nine -- in the general order of 5 feet. Using this detailed diagram, I believe it is more accurate to say that line segment 9-3 is something a little bit more than 5. It is actually six feet, six-point-something or other feet, I believe it is.

But there are ambiguities and uncertainties interpreting where you start measuring that offset. 10-3 is simply an extrapolation of the original land surface into the fault. And I know that Mr. Harding has used point 2 as a point of reference for measurement of this offset in this trench.

So if you measured offset along 9-2, the offset is about 5 feet. If you measured it along 9-3, it is 6-1/2 or more feet, generally rounded to around 7 feet. But I am uncomfortable about making such careful differences between 5 feet and 7 feet when this is an interpretation from trench logs on which we have personal disagreements of the mapping. We have simply taken firm points of line surfaces and extrapolated them into areas where we feel that there has been improper mapping of the relationships of the soil and the faults.

So I recognize that the displacement is more

than 5 feet along 9-3. It may be of the order of approaching the 7 feet. But that is why I couched it in the words, "There appears to be evidence for displacement on two faults of the order of about 5 feet each."

JUDGE GROSSMAN: All right. I had intended to ask a number of questions at the conclusion with regard to the methodology used by the USGS, but Dr. Brabb has mentioned now the secondary method used by USGS which I believe refers to permitting ESA to log the trenches, and then having the USGS review. Is that what you means by the secondary method, Dr. Brabb?

WITNESS BRABB: I said that our method was secondary, in that it was derived from information after the trench was closed. The primary method would be to make the measurements when the trench was open directly on the fault surfaces themselves, so that there could be no question about what the actual measurements are in the trench. So our measurements are secondary in the sense that it is based on an interpretation of the information in the trench, rather than what we actually saw in the trench and measured ourselves.

WITNESS HERD: Could I have a moment to confer with our counsel and the panel?

JUDGE GROSSMAN: Certainly.

(Witnesses confer with their counsel.)

WITNESS HERD: Thanks very much.

If I may, I would like to continue also in a form of discussion to entertain the fault offset interpretation proposed by Mr. Harding and others in their testimony in Appendix A. May I do that now, as well? Or do you wish --

JUDGE GROSSMAN: You can, but I thought you were going to add on to what Dr. Brabb had indicated and I did want to at least establish what we were talking about before with regard to the methodology used, even though I don't want to go into it in detail now and disturb your presentation, which I understand I am doing.

(Laughter.)

WITNESS BRABB: I would like to respond further to you, Judge Grossman, if I may. Trench T-1 is exceptional for a variety of reasons. Dr. Jackson attempted to explain some of them. At the time the Trench T-1 was open, this was very early in the investigation. At that time, we were not formally involved as scientists in the interpretation of the information. But in view of the fact that the trench was going to be closed, we were asked to come out and view the features.

After that time, trench logs were made

available to us. We had them with us in the other trenches and were able to check the measurements that were made by the consultants and assure ourselves that the critical features were correctly shown.

In several of these trenches, we did disagree with the interpretation, and have so stated in our testimony. But this and the other key trenches, T-1, T-2, and T-3, were very early in the investigation when they were trying to establish whether or not there was a reasonable basis for faulting near the GETR.

This is the reason we did not have the logs, and therefore could not verify the measurements directly in the trench.

JUDGE GROSSMAN: Okay. And you did not make your own independent measurements with regard, then, to the T trenches? Is that right?

WITNESS BRABB: Only to the extent of assuring ourselves that there was faulting of the younger soil. We were convinced in what we saw in Trench T-1 that the younger fault -- that the younger soil was faulted, but we did not determine how much.

JUDGE GROSSMAN: Okay, now --

WITNESS HERD: Soil, meaning A-2 horizon.

WITNESS BRABB: Correct.

JUDGE GROSSMAN: Now with regard to the

other trenches, did you go into those trenches when they were freshly dug and check the computations made by -- or the log made by ESA?

WITNESS BRABB: Yes, sir, we did.

WITNESS MORRIS: Yes, sir.

WITNESS BRABB: And in many cases we could verify the information. In many others, we disagreed with the interpretations.

JUDGE GROSSMAN: Where you disagreed, did you make your own logs with regard to those measurements?

WITNESS HERD: If I may just interject a moment, we have pointed out in our April 1980 document a couple of instances, for example in reference to Trench B-1, B-2, where we pointed out in our annotations on logs of those trenches that the fault continued up into those horizons that we believed to be -- that we saw to be offset.

WITNESS BRABB: So I think the correct response is: That in some instances, yes; in other instances, no.

JUDGE GROSSMAN: Okay. I do not care to disturb your presentation any further now, so I will save my questions along those lines for later, Dr. Herd.

WITNESS HERD: There are other lines drawn on Exhibit 4, and I would just like to discuss them

briefly, if only as a matter of reference to an alternative interpretation of this trench as proposed by Mr. Harding and others in their testimony already submitted to the Court.

If I may refer you to the testimony of Harding and others, Figure A-2 on page A-5, Appendix A of the testimony of Harding and others --

JUDGE FOREMAN: That is their exhibit number?

JUDGE GROSSMAN: That should be Exhibit No. 1, I guess, isn't it?

MR. EDGAR: It is Licensee's Exhibit No. 1. Exhibit No. 2 is the Phase II report that we had out with the trench log this morning.

WITNESS HERD: Okay. Once again I would like to refer to Figure A-2 in Appendix A on page A-5.

(Pause.)

(Board conferring.)

WITNESS HERD: Are we ready? Excuse me. Thank you. I was looking off in space here.

Okay, in brief summary, Figure A-2 is a diagram of the events which Mr. Harding and others have proposed to explain the intermediate step 3-4-5 that we have talked about previously in trench log T-1. In particular, I recognize and interpret that step 3-4-5 to be the bend identified in cartoon D at the

bottom of page A-5. If I understand correctly Mr. Harding's argument, if there were faulting in Trench T-1, by his interpretation there was no offset in the stone line, and that this bend was occasioned by movement along the fault plane which caused the surface of the stone -- which caused the stone line to bulge upwards into the overlying soil without an accompanying offset of the ground surface.

I believe that using this interpretation one might come up with a displacement of about 5 feet, as well, if you were to entertain this as an explanation of the origin of this bend. Although I don't believe it geologica-ly probable, I believe it would give the same number.

In the figure on page A-2 in steps B-C, there is a discussion of erosion which forms a surface with a step in the topography left-facing step which is inferred to be the backwasting of fault offset along fault 3-8. This ancestral scarp now buried is supposed to be the point of where the movement occurs along the fault causing it to bulge up.

If I properly interpret this trench log with this diagram Figure A-2, I would infer that this ancestral escarpment or step in the stone line is more or less that step which I see preserved as a relic on

point 6-5. That is, that if I notice between Figures C and D above the bend, there is no apparent change in the angle of that step face.

end
JWB
#4

And I look at this trench log of T-1 and I see that the bulge ends against point 5, so I infer that line segment 6-5 is a representation of the configuration of this ancestral step.

Okay. To recreate what that ancestral step was before bulge 3-4-5 occurred, I have extrapolated that step 6-5 downwards to its point of intersection with the fault plane in the manner shown in this diagram on page A-5.

In other words, I believe that if I interpret this diagram Figure A-2 properly, that in cartoon step C, that would have been equivalent to cartoon -- our figure ancestral step 8-5-6. Okay, to cause step 8-5-6 to bulge outwards would require for there to be movement along a fault bounded wedge of material which would occur between faults 8-3 and 5-12.

This movement would occur relative to the lower portion moving to the west, bulging outwards on the surface. I believe the amount of movement that would be required to cause this bulge would be the amount of distance traveled from point 8 to point 3, which would be the distance of about six feet, rather than the two feet which has been proposed by Mr. Harding.

So, in summary, I believe that an alternate interpretation of the scenario that Mr. Harding has proposed would allow for a measurement of displacement that is about

of the same order that we have independently calculated from a different scenario of events, that is about five feet of movement on each of two faults in this trench.

So, to summarize, then, we believe that Mr. Harding's interpretation is not necessarily at odds with ours, but would lend an independent interpretation if you would continue in the fashion I have, which would support an offset of about five feet on a single shear in trench T-1.

JUDGE GROSSMAN: Of course, that presumes that he was including the bulge at 5-6-7 in his diagram and did not intend to exclude that completely, and merely rely upon bulge 3-4-5 being in the diagram.

WITNESS HERD: Could you repeat that one more time?

JUDGE GROSSMAN: What I'm saying is your suggestion that your theory is consistent with Mr. Harding's presumes that he would not object to including bulge 5-6-7 as part of the one bulge he shows, rather than exclude that bulge and include only bulge 3-4-5.

WITNESS HERD: I understand, if I understand what you are saying correctly, is that my interpretation of bulge 3-4-5 may be at odds with his interpretation? Is that what you are saying?

JUDGE GROSSMAN: No. My understanding of what you are saying now is that your theory is consistent with

Dr. Harding's theory, and that what makes it consistent is your inclusion of the bulge 5-6-7. Maybe we have a problem with my identification of that, but 5-6-7 is the second step, and you are including it and you are saying that makes your theory consistent with his, and I am saying that only presumes that he would permit that inclusion, but he may well have intended to exclude that step or bulge or hump and really restrict his hump to the 3-4-5 bulge.

WITNESS HERD: Understood. I have grossly interpreted the figure A-2 to represent the major relationship seen in this trench.

In other words, I thought that in looking at this diagram, that 6-7 surface is the one described to the right above the fault in Figure C -- excuse me, Figure C of A-2, and that 10-3 represents the surface below the ancestral step, in the same figure illustration.

JUDGE GROSSMAN: Well, perhaps this is the time to clarify it.

Mr. Harding, did you intend to include the entire matter here of all the significant items, including steps 6 including the data between points 5, 6, and 7 in your depiction here in Figure A-2?

MR. HARDING: Yes, that's correct. I agree with including both of those steps in my depiction. However, I don't necessarily agree with the rest of the

interpretation.

JUDGE GROSSMAN: Okay. I just wanted to clarify that.

Well, then, apparently there is no disagreement that your theory may be consistent. However, that's something that Dr. Harding will clarify.

WITNESS HERD: Certainly. Right. The point is, I don't wish to say that I ascribe to this interpretation. I don't believe that this is a geologically-likely explanation for this bulge. I believe that the fault offsets at the surface is a more likely explanation as opposed to some subsurface localized wedge shoving material along a fault. But I have pointed this scenario of events only to illustrate that if I understand that diagram properly, and interpret it in a different fashion, that the amount of offset that would be implied by that scenario of events would be of the same order, the same general amount, about five feet. But I have measured in a totally different manner along these same faults.

May I quit? Thank you.

JUDGE GROSSMAN: I take it, then, Dr. Herd, you have concluded your presentation. And is there any further presentation now to be made before we resume questioning?

WITNESS BRABB: Do you want me to comment on

ar5-5

the significance of this?

MR. SWANSON: The Board just asked if anyone does have any further explanation. If it's necessary. Otherwise --

WITNESS BRABB: I would like to add a brief comment, if I may.

The reason that we have focused on trench T-1 is that it explains some of the principal conclusions of our uneasiness with the one meter of offset. If there is in excess of that amount of displacement in one of the trenches, this relates to our unease with being specifically tied to that figure, and therefore it relates to the amount of conservatism that we would have in the interpretation of the information.

JUDGE FOREMAN: But it doesn't lead you to provide an estimate of what you think the offset might be?

(Laughter.)

Just that it's different?

WITNESS BRABB: We have tried very hard not to do that, your Honor.

JUDGE GROSSMAN: Well, it seems to me that you have tried so hard that you keep using the five-foot figure apparently to be fair to GE, and only as an approximate number, in contrast to the two-foot figure. But when you say five feet, basically from your

ar5-6

approximations, you mean five, six or seven feet, and that if you were forced to come up with a number, it might be somewhere in between those numbers just mentioned; but that you possibly feel not able to give an exact number in view of your not having been in trench 1 and made the exact measurements yourself. Is that basically a fair summary of your position?

WITNESS BRABB: Yes, I think so. We were, of course, in trench 1, but we did not make the measurements at the time. Therefore, we are reluctant secondarily at a later time to be too precise in terms of the exact amount of measurement. But we are convinced that it is not two feet.

JUDGE GROSSMAN: Mr. Edgar?

MR. EDGAR: I'd like to take a short break, if I may.

JUDGE GROSSMAN: Sure. We'll take 10 minutes and be back at 11:50.

(Recess.)

JUDGE GROSSMAN: Mr. Edgar will proceed with the remainder of his cross-examination.

WITNESS BRABB: Your Honor, before Mr. Edgar begins, may I make a correction for the record?

JUDGE GROSSMAN: Certainly.

WITNESS BRABB: I had indicated that we had made

no direct measurements in trench T-1. My colleague corrects me, that he had estimated the offset of one of the older horizons in trench T-1 at the time that he was there.

JUDGE GROSSMAN: Your colleague being Dr. Herd?

WITNESS BRABB: I'm sorry, it would be Mr. Morris, and I'll let him speak to that.

WITNESS MORRIS: This was -- the estimate was made, I believe, on the second visit to the trench after the walls had been picked off, but still without the advantage of the log and without having a good handle on the stratigraphy, We took some crude observations of the maximum displacement of older horizons, and I can't equate those with what we know about the stratigraphy today, but my estimate was 15 feet.

CROSS-EXAMINATION (Continued)

BY MR. EDGAR:

XXXX

Q And, Mr. Morris, were those on -- is it your belief, recognizing the limits of uncertainty associated with the then-identified soil strata, but is it your belief that that 15 feet would be associated with the lower contact of these so-called paleo soils?

A (Witness Morris) Yes, sir, that would be true.

Q I will go on and try to define some of these terms, so if I start using the geologists' language, it will be similarly defined?

ar5-8

MR. SWANSON: Excuse me. Before Mr. Edgar begins, I wanted to point one thing out, and that is that Dr. Slemmons, who has an important part of the Staff's testimony, has a report, of course, appended to the May 23rd, 1980 report, is unavailable to sit with this panel after today. He will return on the probability panel and, of course, could be asked questions at that time, but it's important in terms of the overall perspective to have him on this panel, and in the event that this question and subject may not be concluded today, I would like parties to perhaps keep that in mind, and if there are questions directly related to worldwide data on surface offset for earthquakes, it would be much appreciated -- perhaps we could try to accommodate Dr. Slemmons on this point today.

MR. EDGAR: I could make a suggestion here. I have completed all my questioning other than the questioning on T-1, and I would be perfectly willing to defer to Mr. Cady and let him ask all of his questions of Dr. Slemmons. That wouldn't be any problem at all. It would also give us some time to review things.

MR. SWANSON: It might be difficult to try to single out questions to him, but --

MR. EDGAR: He can go ahead. That's fine with me.

JUDGE GROSSMAN: Okay. After lunch, I'm sure

Mr. Cady will have a decision as to whether he wants to proceed then, or he wants Mr. Barlow to proceed that way.

MR. CADY: I will direct Mr. Barlow to have his early interrogation of this panel go along those lines, as outlined by Mr. Swanson.

MR. SWANSON: One other point that would not be readily apparent. I think all of the geologic and seismic material in the Staff's evaluation is contained in the Staff Exhibits A and B, with one exception. I want to point this out, so there wouldn't be a concern later when we get into other parts of the hearing.

In the October 1980 -- October 27, 1980 section of the Safety Evaluation, that would be Exhibit 1-C, it's Section C, page 12 -- that's page C-12 -- the first two-thirds of the page represent the results of an analysis of time histories, seismic scram analysis and it's dependent on an analysis of seismology, and that appears as one page in a document which otherwise deals with structural material. That section deals with the time histories in reaching certain levels of acceleration during a recorded event.

I just wanted the parties and the Board to be aware that there was a section on seismology, so that if there are any questions on that, this, of course, is the panel to address that matter to.

I just wanted to clarify that one point.

JUDGE FOREMAN: Page 12, did you say?

MR. CADY: C-12, your Honor.

JUDGE GROSSMAN: Mr. Edgar, in view of the fact that you really want to break at noon, we don't see anything profitable about starting now and putting three minutes on, so why don't we adjourn now and come back at 1:15 today.

Thank you.

(Whereupon, at 11:55 a.m., the hearing was recessed, to reconvene at 1:15 p.m., this same day.)

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AFTERNOON SESSION

(1:15 p.m.)

JUDGE GROSSMAN: The hearing is in session.

Mr. Cady, did you decide whether you prefer to have Mr. Barlow cross-examine to begin with? Or would you want Mr. Edgar to continue?

MR. CADY: I would prefer to have Mr. Barlow get through with as much as he has today, and I believe Mr. Edgar will be able to finish up with whatever questions he has tomorrow.

JUDGE GROSSMAN: Fine. Mr. Barlow, would you proceed?

Whereupon,

PHILIP S. JUSTUS,

ROBERT E. JACKSON,

ROBERT H. MORRIS,

EARL E. BRABB,

DARRELL G. HERD,

WILLIAM L. ELLSWORTH,

DAVID B. SLEMMONS,

RAMAN PICHUMANI,

and

JAMES DEVINE

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

CROSS-EXAMINATION (resumed)

BY MR. BARLOW:

Q. I think that Dr. Herd and Dr. Brabb deserve a break, so I am going to start with questions to Dr. Slemmons.

Dr. Slemmons, in your testimony on page 3, you state that --

A. (Witness Jackson) Glenn, could you give us a chance to find that document and get it before us?

Q. Sure. Tell us when you have it before you.

A. Is that Appendix E to the Staff's SER?

Q. No, but if you could have that on hand, I also have questions on that. It is in NRC Staff Testimony of David B. Slemmons.

A. (Witness Slemmons) Yes, I have it.

Q. On page 3, you state there at the end of the top paragraph: "The worldwide data and the San Fernando earthquake data suggest that the offsets could be as much as 2 to 2.5 meters. The associated earthquake would be about 6 to 6.5 magnitude."

Could you explain how you arrived at the suggestion that offsets at the GETR site on the Verona fault could be as much as 2 to 2.5 meters based on worldwide data and San Fernando data?

A. Yes. First, let's take the San Fernando

earthquake data. The analogy has been made by the Staff and by others with the San Fernando earthquake. This type of correlation I believe is very conservative in that there are a number of activities and characteristics for the San Fernando earthquake that indicate that it has a greater capability of producing a large earthquake than the Verona Fault Zone.

So my using analogies there and taking the 2.5 meters, which was the maximum observed in the San Fernando event, you arrive at a value that would be in my opinion greater than any that you are likely to obtain on the Verona Fault.

The worldwide data includes a great deal of scatter. And in an attempt to try to refine the worldwide data base that I used in 1977 in the state-of-the-art paper published with the Corps of Engineers, I have re-examined the data that I complied at that time and have, in addition, added more recent events, or events for which data was not readily available earlier. And when one plots the data, you get a band of dispersed data points. And by linear regression, if you fit the best point to that curve, or construct the line that would give the correlation between either the fault length or displacement and the surface wave magnitude, one obtains what would be the best fit value, or the

most likely value with 50 percent chance of the actual event being greater than or less than that particular value. One finds that the scatter and data involves a considerable range.

If one attempts to apply this to the Verona Fault, we have several problems that need to be resolved. First of all, what is the length of the Verona Fault? There have been two widely used values, both of which have merit.

One would be to extend the zone from the Las Positas zone and connect it in some fashion. They cannot be exactly defined by the present data to, or toward the Calaveras Fault. And if you extend it for the length of the range to the edge of the Livermore Valley near Pleasanton, one gets a length which would be approximately 8 kilometers.

This length would be defined perhaps by a compressional folding and upthrusting model which would cause the hills there to rise in a somewhat symmetrical fashion, and it gives what I think would be a minimum reasonable length.

A second alternative -- and, by the way, if one were to use that, you would have to have some sort of a truncating or a cross-fault at the edge or near the edge of Livermore Valley to the east of Pleasanton.

The second possibility would be that it would continue to join with the Calaveras Fault Zone. And if you used that model, you come up with a length of about 12 kilometers.

The first model would be reasonable for a reverse slip, or a reverse oblique slip type of mechanism. The second might get into a more strike-slip type regime. And the calculations that you make, or can make, then, would come up with magnitude values from minute data for an 8-kilometer length of about 6-1/2 and about 6.7 for the 12 kilometer lenth.

I have also done computations using world-wide data, North America data -- and these are included on pages 12 and 13 of my letter of April 28th, which is appended as Appendix C, I believe --

Q. I believe it is E.

A. Appendix E in the SER. And the values that one can obtain from the 8-kilometer length would be scattered from about 5-3/4 to approximately 6.67. That higher value, by the way, is for reverse and reverse oblique. And the data base for that type of faulting is the poorest, because we have the least number of good, well-studied examples. And if one wants to average these kinds of values, you come up with a mean of approximately 6.1.

For the 12-kilometer length, you can come up with values that would range from about 6 to about 6.8, and a mean value of 6.33.

Well, taken together with my newer data, this would suggest that the magnitude to be expected on the Verona Fault would be somewhere from somewhat above 6, 6-1/4 to approximately 6-1/2.

Working at it from another direction, if you take the displacement that has been observed on B-1, B-3, or on B-2, or on H, you come up with maximum displacements that are about 3 feet, approximately 1 meter or less. And depending upon which of the data bases that you use, strike slip or reverse oblique or combined reverse oblique and reverse, you come up with figures that would run between approximately 6 and about 6-3/4.

Taken together, this leads me to the conclusion that the most likely event would have a magnitude of approximately 6-1/2. And normally from the worldwide data base, this would then correlate with displacement of about 1 meter.

However, in view of the scatter of data points, if you were to simply make the plot and use a standard deviation on the worldwide data base, that one meter would have to be cushioned with a plus or minus of say

one sigma, and this would probably embrace a range from somewhat less than a half a meter to somewhere around one-and-a-half to, or perhaps two-and-a-half meters.

The best fit, in my opinion, comes from the real hard evidence in the trenches of the amount of offset. And on the basis of that, I would project that the high probability that you would be involved with a cyclic type of faulting event in which you would have the greatest tendency to have a repeat for a similar kind of displ-cement in the future. Namely, something in the range of 2 to 3 feet.

In summary, then, I would say that the best fit is 2 to 3 fit, but the worldwide data base suggests that there is some much smaller possibility of a displacement that may get up to as much as 2 or 2.5 meters.

A. (Witness Jackson) I would like to add an addition, and I hope that Dr. Slemmons might comment a little further on it.

I would like to offer a caution and comment and I think Dr. Slemmons will comment a little further, on the caution of using fault length versus magnitude -- I did not want the Board to be misled -- as a sole basis for determining magnitude. It is a technique. It is one of many techniques for determining a magnitude from

previous ruptures that have occurred on other faults at other locations. There are other methods, such as moment calculations which depend on the area of the fault -- not only its length, which is one-dimensional, but a two-dimensional or a three-dimensional picture.

The other item is to look rated on slip rate as plotted against magnitude. Those are all other methods that can be used, also.

An important observation in Dr. Slemmons' data sets is that the displacements plotted are maximum displacements observed during that event, and not necessarily displacement on a given scarp or fault plane, but often can be a calculated displacement across a zone of faulting, sometimes of a significant difference -- distance, excuse me.

A. (Witness Slemmons) Those are both good points. The question that would come from that, then, would be: Are the 2 to 3 feet measurements in an area where you are likely to obtain a maximum measurement? Or are they in fact in a place where you might expect from the geological situation of the fault to have a reduced value, and not have a representative sample.

Actually, the trenches have trenched the shears in a number of places with some significant amount of length of the fault being exposed by the trenches, and

the trenches are in approximately a point that is opposite the highest point of the Livermore Hills there, the Vallecitos Hills. So geologically, where you've obtained the greatest height of the range is where you should expect the maximum displacements to occur on the fault zone.

So that in my opinion, the measurements are taken in an area where the most likely match is likely to occur for the maximum displacements.

Another factor that I guess should be considered is that of judgment value. During my experience, I have observed approximately one-half the cases throughout the world where surface faulting has occurred historically, some 45 or 50, of about 100 data points, or 100 events.

And in looking at these faults, looking at the topographic expression, the recency of the most recent movement, the amount of movement that has occurred through periods such as the Holocene, or the late quaternary, getting a feeling for the fault within its regime, the relationships to other faults and structures in the region, one can get a feeling whether a fault is a big fault that is likely to result in large magnitude events and large displacements, or whether it is a relatively minor, or subordinate, or

a branching lower-order-of-magnitude type structure.

In that regard, the Vallecitos Fault is definitely a fault of rather short length. It does not show the dynamic and size of relationships that one would expect from a fault that would produce large earthquakes.

As a judgment value, I feel that a fault in that type of a situation is likely to produce a relatively small earthquake, 6, 6.5 perhaps would be a better value, and the displacement is reasonable for the 1 meter range.

Q. Dr. Slemmons, just for the record, could you tell us what the magnitude of the San Fernando earthquake was?

A. Would you repeat the question, please?

Q. Yes. What was the magnitude of the San Fernando earthquake of 1971?

A. 6.4.

Q. Thank you. And you said that the maximum observed offset there was 2.5 meters?

A. That's correct.

Q. Thank you.

Turning to page 10 --

A. (Witness Jackson) Excuse me. I would like to add to that. That is not the maximum observed offset,

I don't believe. That is the calculated net slip.

A. (Witness Slemmons) That's correct. This was indicated in a paper by Bob Sharp.

Q. Okay. I think we will come back to that. I would like to turn first to your Appendix E in the SER, your letter to Bob Jackson dated April 28th, 1980, which is Appendix E. Do you have that before you?

A. Yes.

Q. On page 10, Section 4 entitled "Surface Faulting Concepts related to Potential for Surface Rupture," in the second paragraph of Section 4 you begin by saying: "In addition, three alternatives appear to be reasonable for surface rupturing on the Verona Fault." And if I may just skip to the last sentence in that paragraph where you conclude: "Any of these alternatives lead to a capable fault classification for the Verona Fault."

Could I ask you if your definition of "capable fault classification" comes from the Appendix A of 10 CFR Part 100?

A. Yes, it does.

Q. Are you -- I mean, I would like to get into this discussion that you have outlined in the next couple of pages, and I would like to preliminarily ask you: Was the definition of "capable fault" for any of

these -- for your conclusion regarding any of these alternatives based on the Appendix A definition where it says that: a fault may be considered a capable fault if it has a structural relationship with a capable fault such that movement on one fault may be reasonably assumed to cause movement on the other fault?

I paraphrased the last part of that. I don't have Appendix A right in front of me. Maybe I should look --

A. I think the most conclusive way of defining "capable" here is in terms of the definition which indicates one displacement during the last 35,000 years, and more than one in the last half-million years. I think on that basis that it clearly falls in the "capable" category.

Q. Okay. We may come back to that question.

In that paragraph on page 10, the three alternatives that you suggest appear to be reasonable for surface rupturing on the Verona Fault, alternative number one states that: "The Verona Fault connects the southern part of the Las Positas Fault with a Livermore Valley boundary structure near Pleasanton with a length of about 8 kilometers."

And in your discussion today, you mentioned that this would have to be a -- or have to involve a

truncating or a cross-fault at the edge of the Livermore Valley. Could you please explain the fault geometry that you envision for this relationship you have described?

A. I am basing that primarily on the topographic expression for the northwestern end of the fault bend. The hills come down to the planar edge of the Valley. The Valley at Pleasanton is quite wide. It is covered with young alluvium, so it conceals any fault relationships either with regard to the nature of the boundary of the rather steep northern edge of the hills.

I would envision a fault -- a possibility of a fault running near the base of the hills, and truncating the Verona Fault to the east of Pleasanton.

Q. Now this hypothetical fault that you're proposing here I assume is what you are labeling as a "Livermore Valley boundary structure near Pleasanton"?

A. Yes.

Q. And is that also what you referred to verbally today as a "truncating or a cross-fault at the edge of Livermore Valley"?

A. Yes.

Q. Could you point to any evidence that indicates the existence of such a hypothetical fault?

A. No, nothing other than the topography. The

topography shows a very abrupt steepening of the range in that area. The hills look as though they've been rejuvenated -- that is, recently uplifted. An alternative possibility could be the lateral planation of Niles Creek as it goes past the town of Pleasanton, but the capacity of that stream to erode to me seems limited in view of the width of the valley and the alignment of that face through the range.

Q. Thank you.

In your second alternative, you state that: "The Verona Fault connects the south end of the Las Positas Fault with the Calaveras Fault with a length of about 12 kilometers."

I am having difficulty in picturing exactly this fault geometry that you are characterizing here. Could you explain it?

A. Yes. It would essentially involve an extension to the northwest of the Verona Fault, past GETR, coming out to the edge of the valley near Pleasanton, and connecting to the south of Pleasanton with the Calaveras Fault.

Q. I see. And in this proposed fault geometry, does the Verona Fault have to connect to the Pleasanton Fault for that concept?

A. It would not have to; no.

Q It would not. So this is not dependent on a Pleasanton Fault and Verona Fault connection?

A No, it does not.

Q Okay.

A I did not list in these alternatives the possibility of a connection with the Pleasanton Fault.

Q Thank you.

If in your alternative number two, if your alternative number two were valid, and there was a fault geometry such that the Verona Fault connected the Las Positas Fault to the Calaveras Fault and was approximately 12 kilometers long, do you have a concept of the tectonic relationship between those three faults in such a proposed fault geometry?

A There would have to be a relationship of connection and activity on each. The activity has been discussed earlier, and is verified for each of the three zones.

Q Okay. If --

(Witnesses Jackson and Slemmons confer.)

A (Witness Slemmons) This has been referenced earlier, by the way, by Darrell Herd in his testimony, and I believe that that is, in my opinion, the most plausible of the models.

Q Are you saying that Darrell Herd proposed a

fault geometry in which the Verona Fault connects the Las Positas to the Calaveras Fault?

A. I believe you showed that in one of the figures, or came very close to showing that, did you not? I've forgotten the exact figure that was referred to earlier, Darrell. Would you like to comment?

A. (Witness Herd) Give me a moment to look at my figures.

(Pause.)

In particular we are talking about Figure No. 38 in Appendix B of the May 23rd, 1980, SER. It does not hook up, right, in this diagram.

MR. EDGAR: What does not hook up to what?

WITNESS HERD: Thank you. I'm sorry. Okay. The Verona Fault as depicted in my Figure 38 is not shown as being connected to the Calaveras Fault.

BY MR. BARLOW:

Q. Dr. Slemmons?

A. (Witness Slemmons) Might I continue? What I would propose is that at the end of a thrust fault you would have to terminate the fault somehow, and in many cases the fault rolls over and becomes a tear fault that could make a connection at least at depth.

Q. I see. And this is common in thrust faults in their relationships to other faults?

A. Yes.

Q. And would it be common to find this relationship between a thrust fault and a strike-slip fault?

A. Yes. In fact, if one goes -- that is common. And you also find that many strike-slip faults are, in places, reverse faults as well. This was observed, for example, on the Owiterary (phonetic) fault in New Zealand, and I have been to one field occurrence south of Dublin where the Calaveras Fault at the front of the hills there dips into the range at an angle of about 45 degrees, and is in fact a thrust fault. So in that look, that is a reverse right reverse fault.

Q. That is the first time I've heard about that. I would like you to explain it a little more. You are saying that you observed in the field near Dublin, which is just north of the GETR reactor a few miles, I assume, you observed along the Calaveras Fault Zone characteristics or components of thrust faulting on the Calaveras Fault Zone?

A. That's correct.

MR. SWANSON: Excuse me. I think before answering the question, there is at least one assumption in there that ought to be separated out, that being the distance of Dublin from the GETR site. It is a

multi-part question.

MR. BARLOW: Perhaps I could rephrase the question and strike that part of it.

MR. EDGAR: Yes. And define "few," also.

MR. BARLOW: If I could strike that part of the question --

JUDGE GROSSMAN: Did you give a distance near the GETR site? I'm sorry, I didn't hear that distance if you did.

MR. BARLOW: I just approximated or assumed a distance, actually a direction that Dublin is north of the GETR along the Calaveras Fault Zone. That part of the question is not necessary to the question at all. But perhaps I could separate it into two questions, and it might be answered either Dr. Slemmons or Dr. Herd or Dr. Brabb.

BY MR. BARLOW:

Q. Could you give us an estimate of the distance between the site near Dublin where you observed thrust faulting and the site on the Calaveras Fault opposite the GETR reactor?

A. (Witness Slemmons) The location was approximately halfway between Dublin and the town of Pleasanton where the location on the Calaveras Fault Zone nearest Pleasanton is. I have not studied extensively the Hayward

Fault and the Calaveras Fault, but I understand -- and perhaps one of the other members of the USGS could comment on whether the reverse fault concept is important there -- but I have made that one observation.

Q. Okay, in general, therefore, it is possible to have a strike-slip fault like the Calaveras Fault with thrust-fault components?

A. Correct.

Q. Therefore, it would not be unreasonable to postulate that a connection between the Verona thrust fault zone and the Calaveras strike-slip fault zone would not be an unreasonable configuration?

A. There would be a possibility of a tectonic intertie.

Q. You commented that the valley near Pleasanton north of the GETR is covered with young alluvium. If the Verona Fault -- Well, first let me preface this with another question.

Did you go to Trench E?

A. Yes.

Q. Did you see a topographic escarpment to the southwest of Trench E?

A. No, I did not observe that locality that has been discussed.

Q. Do you agree that it is possible that the

Verona Fault curves at that point and would be just beyond Trench E to the southwest?

end

JWB

#6

A I have no basis to make an observation on that.

Q Would you agree that it is possible that Verona fault zone in the Valley between the Vallecitos Hills and the Calaveras Fault could be buried or covered with young alluvium?

A I think that is a question I would defer to Earl Brabb who has mapped in the area. I have seen no evidence for other structures in that area.

Q I did not ask you about evidence. I asked about the possibility.

A It would be possible if the alluvium is very young.

Q Could you describe for us the techniques or methodology that would be available for researching for such evidence in such a situation? I mean if you had an unlimited budget or a budget to do this research, what sort of research could a geologist or a seismologist do to look in an area where young alluvium might be covering a fault to see if there is a fault at depth or beneath the surface there?

A The method that is most commonly used under those conditions would be a seismic profiling method, but it does not always have a resolution in the kind of materials that are present in that area.

Q Is that method similar to what is used by oil

companies in exploring for oil?

A Yes.

(Panel conferring.)

MR. EDGAR: The question was predicated on an unlimited budget which today is a physical impossibility, I think.

MR. BARLOW: Excuse me. Is it possible to strike that question?

JUDGE GROSSMAN: I'm sorry, I didn't get the tenor of that.

MR. EDGAR: The question was predicated on an assumed unlimited budget, and we all know that's not true any more.

BY MR. BARLOW:

Q Dr. Slemmons, could you possibly estimate what it would cost to do a seismic profiling study between trench E and the Calaveras Fault?

MR. SWANSON: Objection. There is no basis for the relevancy of that question.

JUDGE GROSSMAN: I think Mr. Barlow is probably going to tie that -- connect it later on, and I'll allow him some leeway.

Could you answer that, sir?

WITNESS SLEMMONS: I have no basis, I have no experience in running programs that have called for my

personal arranging for that type of survey being made.

BY MR. BARLOW:

Q Would anyone on the panel be able to estimate the cost of such a research project?

A (Witness Morris) I have some very rough estimates, but it would probably be on the order of $100,000, at the minimum.

A (Witness Devine) That's a difficult question to speculate, because I have no idea what type of equipment you are talking about, how many profiles we'd need to understand what we're after, the kind of terrain that we'd be working in, the permits we'd need. I think it's a question that's impossible for any member of the panel to estimate, with what we know right now.

Q Thank you.

A (Witness Jackson) I might point out that GE did run some refraction studies, if I recall, in that general area. I'd have to pull out a map and look at the actual cross-section. But there were refraction studies which I do not recall exactly where they went in the definition that you asked it from the end of trench E to the Calaveras. I think they were more to the other -- my recollection is they were more to the east of that, but I'm not sure.

A (Witness Brabb) It's my impression that the

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seismic profiles do not apply to the area that is the subject of the question of Mr. Barlow.

Q Dr. Herd, do you know of any research methodologies that would be available to geologists or geophysicists to study this question that we're discussing, the possible connection between the Verona Fault and the Calaveras Fault in a valley covered with young alluvium?

(Panel conferring.)

A (Witness Jackson) Did you direct that to Dr. Herd?

Q I'm sorry. If I may, I meant to direct it to Dr. Brabb.

A (Witness Brabb) If we make the assumption that we're trying to find a fault in an area covered by a young alluvium, certainly geomorphology, specifically the study of the stream systems in the area and aerial photography, looking for liniaments, discolorations of the soil, discontinuities of any kind, would be the types of techniques that would likely be applied as a first phase.

Later, if there are some indications of faulting, you would commonly follow it up with trenching.

Q I see. So it's possible that trenching would reveal evidence regarding the structural relationship?

A Yes.

Q Thank you.

Dr. Brabb, have you examined the evidence which Dr. Slemmons mentioned of thrust faulting in the Calaveras Fault zone halfway between Dublin and Pleasanton?

A Excuse me. My colleague wishes to comment on the last question. I'd like to give him an opportunity, if that's permissible.

A (Witness Herd) No, I decided not to. Thank you.

A (Witness Brabb) I'm now confused on the question you asked. Can you repeat the question for me?

Q Either I could repeat it or the court reporter.

(The reporter read the record, as requested.)

WITNESS SLEMMONS: Thank you.

The answer to that is no.

BY MR. BARLOW:

Q Dr. Herd, have you examined that area?

A (Witness Herd) Yes, I have visited it briefly.

Q Did you, Dr. Herd, examine that area after Dr. Slemmons had observed it and brought it to your attention?

A I'm totally unfamiliar with the description that Dr. Slemmons is making reference to here today. My mapping of the Calaveras Fault zone predates the 1978 date of release of my open files which you have included in part as one of your exhibits.

Q Thank you.

So -- well, let me ask it this way:

Dr. Herd, have you ever discussed with Dr. Slemmons his observations in that area of the Calaveras Fault zone?

A I don't believe in any detail, no. We've never had occasion to before.

Q Thank you.

Dr. Slemmons, returning to your Appendix B in the SER, page 12, your list of three alternatives for the tectonics of the region. You, in your alternative No. 2, for Verona Fault reverse slip to possible strike slip, with 12 kilometers length from the Las Placitas Fault to the Calaveras Fault west of Pleasanton -- in your calculations at the bottom of page 12 and top of page 13, do those calculations of maximum possible magnitude relate to that proposed structural relationship?

A (Witness Slemmons) Yes.

Q At the top of page 13, my copy of your letter in Appendix E reads magnitude 7.3. Is that a typographical error, or is it supposed to be?

A No, that was a calculation for purely reverse slip, not reverse oblique, and it was based on a very small number of observations. I think the worldwide data had something like 7 or 8 observations and this, as was the case with the compilation by Benella of U.S. Geological Survey, both suffered from inadequate data, and that has

been remedied in my newer plot, and for that plot you come up with something near 6.8, I believe.

Q 6.8. Is that correct, 6.8?

A 6.7.

Q 6.7.

A (Witness Jackson) Mr. Barlow, could I ask a question? On your previous question --

JUDGE GROSSMAN: Yes, any time you think something's gone by on which there has to be some correction or elaboration, I wish you would comment.

WITNESS JACKSON: I want to make an observation. Mr. Barlow was asking a question, six or seven questions in which he has used the term "structural relationship."

Now I have a very good idea what that is, because I'm familiar with Appendix A to 10 CFR Part 100, in which a structural relationship is a term that's used in there. The other members of the panel may not be, and may be referring to it in a more loose sense than was intended in the question by Mr. Barlow.

I just want it to be clear, or that he make it clear what he is referring to, and if he uses it in that context, should refer to within the meaning of Part 100 or within the context. It's just for clarification of the record.

WITNESS SLEMMONS: For clarification, I'm using

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the term in a general sense, and not in terms of any specific definition in CFR 100.

BY MR. BARLOW:

Q Thank you.

If I may define the term as I'm using it by referencing 10 CFR Part 100, Appendix A, Section 3, Subsection G, No. 3 --

A (Witness Jackson) Give us a chance to open that.

Q Certainly. It's page 547 of the 1980 version.

A (Witness Justus) Could we have the paragraph designation, please?

Q Yes, it's Appendix A, III, Definitions, Subsection G, Subsection 3. Have you found it?

A We have.

A (Witness Jackson) We'd like to read it.

Q I'd like to read it for the benefit of the panel who may not be familiar with it.

"A structural relationship to a capable fault, according to characteristics one or two of this paragraph, such that movement on one could be reasonably expected to be accompanied by movement on the other."

Now the context of this definition of structural relationship is in Subsection G, which is a definition of a capable fault, and the -- I should read this, I guess. It reads:

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"A capable fault is a fault which has exhibited one or more of the following characteristics: (1) movement at or near the ground surface, at least once within the past 35,000 years, or movement of a recurring nature within the past 500,000 years."

Section 2 deals with macroseismicity, and it is not my intent here to go into that. But Section 3 as read is the definition of the term "structural relationship" that I am referring to.

MR. SWANSON: May I ask Mr. Barlow exactly what he means by definition of structural relationship? He gave a possible definition of capable fault within the meaning of that particular section, but he is asking the panel to use a definition of structural relationship within the meaning of Part 100, and I just don't see that defined. I see a use of the term, but not a definition, and I think if he is going to require the panel to use a definition, I think it's absolutely certain that they understand what he wants them to use, and I would ask Mr. Barlow if he would define it further.

JUDGE GROSSMAN: My reading of this indicates that there is in fact a definition of structural relationship which means that the movement on one could be reasonably expected to be accompanied by movement on the other, and even though they don't call it a definition --

now maybe I'm wrong. Is that the sense that Dr. Jackson gets from that?

WITNESS JACKSON: This is the problem with Appendix A and definitions of terms. A large number of people would think a structural relationship -- some geologists, I learned this yesterday --

(Laughter.)

-- some geologists would indicate that a structural relationship would require connection between two faults. Others would mean that it's in the same tectonic regime or reasonably connected through a series of other connections. So there are a number of definitions and geologists use them differently. And all I was requesting is that when that question is asked, that it be defined in the question that's being asked. If it's a legal definition, that's one thing. Many of the panel members here are not involved in legal proceedings that often.

JUDGE GROSSMAN: Okay. I believe Mr. Barlow started off by attempting to use the definition in Appendix A, and then we got a little sidetracked on that.

Now is it your intention, Mr. Barlow, to be using the definition as in Appendix A?

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

JUDGE GROSSMAN: Dr. Slemmons, was there anything

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that you have already said in which you referred to a structural relationship that is inappropriate with regard to the definition or what appears to be a definition in Appendix A?

WITNESS SLEMMONS: Yes, your Honor. I did not mean in any sense to imply that movement on one would immediately and directly cause movement on the other. Interrelationships between faults very often involves the building up and release of strain, placing new strain on another fault or branch or system, and then later as the area is subjected to continued strain, it then, although it has a related tectonic cause, would perform independently, and I intend that sense in my comments.

JUDGE GROSSMAN: Okay. In other words, you are qualifying the word here accompanied by and your answers did not mean that it would be accompanied by, at least not immediately, but it could be in the future? Is that basically the difference?

WITNESS SLEMMONS: That's correct.

WITNESS JACKSON: Other members of the panel have used that in the last several hours. Dr. Brabb used it a few minutes ago, and I believe Dr. Herd earlier today. So I don't know the context of the question, but I am concerned about it.

WITNESS BRABB: I'll respond for myself. My

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qualifications are similar to those of Dr. Slemmons. I would further emphasize that in some cases a structural relationship in the meaning used by Mr. Barlow and taken from the appendix might mean something that happened 50 million years ago and hasn't happened since. So that there is definitely a qualification in any use that I have made of the term "structural relationship."

MR. BARLOW: Judge Grossman, if I might say that I did not have the intention of applying this definition retroactively to former discussions. I would like to proceed from this point.

MR. SWANSON: Well, I just want to make sure, is the panel clear, then, when he says to use this definition of Appendix A, what he means, if the structural relationship has to exist in a certain period of time, which is say perhaps more recent than what members of the panel might otherwise use the term? I want to make sure that it's clear so the record is clear when we get an answer what they are referring to.

WITNESS BRABB: If the questions are asked of me, I would prefer to be reminded each time of the meaning.

MR. BARLOW: Okay.

WITNESS JACKSON: I would contend that it is a very difficult definition for an average --

(Laughter.)

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-- or above-average geologist or seismologist to utilize, because it requires a lot of experience and past experience in dealing with this kind of a definition.

In fact, most geologists don't like to deal in this forum with this kind of a regulation. So I don't think the testimony should be restricted, as long as it's clear what they're responding to.

JUDGE GROSSMAN: I don't want your testimony to be meaningless now, and I foresee the possibility that all of your answers are going to be in the negative, assuming that you never think that there must be motion accompanying on one interrelated -- on one structure interrelated with another one occurring simultaneously. And so I think that kind of thinking is going to get us all negative answers when it's not intended.

So, you know, if the regulation is poorly written and you can't really use it, I don't want to get you to commit yourself to that, Dr. Jackson, but nevertheless if it is written in such a way that you can't use it, let's find out about it now, rather than have answers that are meaningless.

WITNESS JACKSON: I'm not trying to infer that at all. I'm trying to indicate that it is -- it needs to be applied with knowledge and experience of this particular definition, and I think the people here can do that now,

knowing how it is defined.

MR. EDGAR: Isn't the trouble stemming from the fact that we are playing hide-and-seek with the definition? Perhaps if the question is asked directly of Dr. Brabb, for example, "Do you expect simultaneous movement," then he as an expert can give a clear answer.

I think it is unfair, perhaps objectionable, to ask experts questions on a legal definition, and I am very sympathetic to this witness panel being placed into this hidden box of having to play with the definition.

The problem stems not from the witnesses' responses, but rather from the question, and if the question is asked clearly as to not using the term "structural relationship," but if they ask these witnesses, "Will movement on one be immediately followed by movement on another," every man here can give a clear answer.

JUDGE GROSSMAN: Without accepting Mr. Edgar's characterization of what was wrong, I will allow the technical examiner to phrase the question as precisely as he is able to.

MR. BARLOW: Thank you, your Honor. I am afraid that we may have stumbled into unnecessary confusion, and I would like to ask a couple of questions to perhaps clear up the confusion.

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BY MR. BARLOW:

Q Dr. Jackson, does the NRC Geosciences Branch characterize the Calaveras Fault as a capable fault under the definitions of Appendix A?

A (Witness Jackson) Yes.

Q Dr. Jackson, does the Staff characterize the Verona Fault as a capable fault under the definition of Appendix A?

A No, not under the definition of Appendix A. Appendix A was not applied to this site.

Q Without the term Appendix A, does the Staff characterize the Verona Fault as a capable fault?

A There is no definition called capable fault in the common geology and seismology literature.

Q Dr. Jackson, does the Staff consider the Verona Fault capable of movement during a future earthquake?

A Absolutely.

Q Thank you.

Dr. Herd, regarding a discussion that occurred yesterday during cross-examination, could you explain how the Calaveras Fault could connect with or have a branching or structural relationship with the San Andreas Fault in the area of Hollister, and yet not have any available evidence of surface faulting in the area of intersection or branching?

MR. SWANSON: Can we have a clarification of the word? Structural relationship was used again, and I want to make sure that the answer is responsive to the question, and I think we need a clarification.

BY MR. BARLOW:

Q Perhaps I should preface that question with another question.

Dr. Herd, in your opinion, could movement on the San Andreas Fault trigger sympathetic movement or be accompanied by movement on the Calaveras Fault?

(Panel conferring.)

A (Witness Herd) There has been some discussion at the table. Could I have the question repeated once again?

(The reporter read the record, as requested.)

WITNESS HERD: Yes, I suppose that's possible, although I know of no documentation of that happening.

BY MR. BARLOW:

Q Does any other member of the panel know of any documentation of movement on the Calaveras Fault, either in the form of aftershocks or sympathetic faulting that occurred during movement on the San Andreas Fault?

A (Witness Jackson) I don't ask for clarification, but by movement -- we have switched back from surface faulting, which I'm sure the Board is now conditioned to

listen to, to earthquakes occurring on a fault at depth, which obviously have to have some movement accompanying. You're meaning either/or?

Q Either/or, and I would especially like to hear Dr. Ellsworth's opinion.

A (Witness Ellsworth) I am unaware of any instance in which movement on the San Andreas Fault has resulted in either co-seismic movement or a movement that is followed within a few days in the Calaveras Fault in a causal relationship.

Q Dr. Ellsworth, could that be from lack of -- excuse me. Is it true that instrumental recordings of seismicity in this area did not begin until the 1930s?

A No, that is not true.

Q Could you tell me when that did begin?

A The first instruments to record earthquakes began operation, I believe, in 1887.

Q 1887.

Can you approximate the decade in which instrumentation was placed in the area near the junction or intersection of the Calaveras Fault and the San Andreas Fault?

A Could you define instrumentation, please?

Q Either accelerometers, seismographs or other precise instruments that are used to measure earthquake

motions or seismicity.

A The early instruments that I referred to were established at Mount Hamilton which is in the vicinity of the Hollister -- of Hollister, California. More detailed instruments were installed by perhaps the late 1950s or early 1960s. I'm not sure about the date.

Q Either the decade of the '50s or the '60s?

A That's correct.

Q In the 20th century.

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

I think there is a problem here of preciseness and quantities that bears on this question. The instruments installed at Mount Hamilton at the turn of the century could be used, and were used, to look at events on the San Andreas Fault. They did not have to be in the vicinity of Hollister in order to do that.

Seismographs record signals from earthquakes from wherever they occur, and can be used to locate that event, even though it's thousands of kilometers away. So the instruments at Mount Hamilton and elsewhere in the world were recording earthquakes on the San Andreas Fault, so it wasn't just after we put instruments in at Hollister.

Q Thank you, Dr. Devine.

A It's Mr. Devine.

Q Mr. Devine.

My interest was more in the seismicity on the Calaveras Fault in response to seismicity on the San Andreas Fault, rather than measurements of earthquake on the San Andreas Fault, and my question to Dr. Ellsworth was more specific to that relationship.

A I was responding to your question, when was it you started recording earthquakes on instruments in California that applied to the San Andreas Fault, and my answer is still applicable.

Q Well, without going back to the original question, I believe I asked about the specific region near where the Calaveras and San Andreas Fault join.

A And my answer is applicable.

Q Thank you.

A (Witness Jackson) Mr. Barlow, are you going to change your line? I wanted to correct my testimony. Earlier I meant to correct a response to you. You asked if the Verona Fault is a capable fault, and we clearly have used that definition in this proceeding. Although Appendix A, as we have indicated in the SER, investigative requirements of Appendix A have not been met in a rigorous sense, the definition we have used as a way of characterizing, I think based on our latest SER, you could conclude that the Verona Fault is an active fault. We have concluded it has

Holocene movement, which is an earlier movement than 35,000 years.

I didn't mean to mislead either the Board or Mr. Barlow. I answered too quickly.

JUDGE GROSSMAN: Well, Judge Foreman, in fact, has found the place in which you referred to the Verona Fault as being --

WITNESS JACKSON: It's in our conclusions No. 3. We wrote that.

JUDGE FOREMAN: I will hand it to you.

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BY MR. BARLOW:

Q Dr. Ellsworth, would you agree within the concept of seismic cycles that there was a period of seismic quiescence from 1906 to 1955 in the Bay Area?

A (Witness Ellsworth) The observational record of earthquakes that we have assembled for the San Francisco Bay region in the period that you refer to indicates that there was a very low level of moderate size earthquakes during that period, meaning earthquakes with Richter magnitudes of 5 and larger.

Q There was a l level of earthquakes of magnitude 5 and larger?

A When compared with the previous century, yes.

Q Is it reasonable, therefore, to conclude that the San Andreas fault was relatively quiet during that period in the area of interest that w have been discussing?

A Could you define the area of interest? You referred to the --

Q Near the connection between the Calaveras fault and the San Andreas fault.

A I think I can answer the question directly. The 1906 earthquake did not appear to have a measurable effect on seismicity within the Hollister region The earthquake activity at or near the magnitude 5 level appeared to continue without interruption through the time of the earthquake.

Q Okay. Thank you. Dr. Herd, getting back to the question that got us into all of this, yesterday we were discussing the area where the Calaveras fault approaches the San Andreas fault, near Hollister. You noted that there was no evidence of surface faulting in the area of intersection or branching. Could you explain why this phenomena could occur where two major faults could come close to each other and where you have characterized the Calaveras fault as a branch of the San Andreas fault and yet there is no evidence of surface faulting in the area of branching?

(Pause)

MR. SWANSON: Can I ask a clarification? You meant branching from the San Andreas to the Calaveras, is that correct?

MR. BARLOW: Well, yesterday Dr. Herd characterized the Calaveras fault as a branch of the San Andreas fault. That's what I was referring to.

MR. SWANSON: I just wanted a clarification of the specific area you are referring to.

WITNESS HERD: Okay. Let's take it in steps, if I may. No. 1, I used the description of Calaveras as a branch of the San Andreas in a figurative sense. If you look at a map pattern, the Calaveras fault in a gross sense appears to splay off, to branch from the San Andreas. However, if you look closely and map closely in the area immediately adjacent --

immediately between the two faults, you will discover that the Calaveras and the southerly continuation, the Paicines fault, closely parallels but never actually intersects the Calaveras -- excuse me - the Calaveras and the Paicines closely parallel but never intersect the San Andreas. There is a close parallelism. There is an apparent increase in motion, thatis, fault creep, along the Calaveras - Paicines fault, as you go northward, as fault creep diminishes on the San Andreas fault.

So apparently slip is distributed in a broad manner between the two, across this intervening crustal block, so that motion is not direct but transferred over a considerable distance. Perhaps Dr. Ellsworth might be able to comment directly on some of the geodetic information that bears particularly on that question.

BY MR. BARLOW:

Q Yes, If I may ask a question based on what you said --

A (Witness Herd) I would prefer if Dr. Ellsworth were to comment just a moment, if he has anything to add.

Q Okay.

A (Witness Ellsworth) Would you repeat the question so I know which I am answering?

JUDGE GROSSMAN: I'm not sure that he asked a question of you yet, and I am going to let Mr. Barlow control his part of the questioning rather than have the panel do it.

Mr. Barlow, who would you like to respond at this

point?

MR. BARLOW: Well, I would like to hear from either or both Dr. Herd and Dr. Ellsworth, but I believe that Dr. Herd answered my last question and then went on to explain further something which raised an area that he would like Dr. Ellsworth to comment on and it wasn't -- I did not have an outstanding question. If Dr. Herd wanted Dr. Ellsworth to comment on what he had said, that is fine with me.

JUDGE GROSSMAN: Oh, okay. That's fine, then.

WITNESS HERD: I feel no need for that. The main point was just to be sure I had answered your question properly.

BY MR. BARLOW:

Q Just so that it is clear, Dr. Ellsworth, I'm not asking you this question but I just want you to understand the context, I had asked Dr. Herd in my last question to explain how the Calaveras fault could come so close to the San Andreas fault and be considered by some to be a branch fault and yet there is no evidence of surface faulting in the area of connection or branching between these two faults. And in Dr. Herd's explanation he mentioned something that I would like you to elaborate on further. If Dr. Herd has any comments, or Dr. Drabb, either one, I would be glad to hear them.

Dr. Herd said that there was some sort of transfer of movement from the San Andreas fault onto the Calaveras fault in the form of creep in this area and --

A (Witness Herd) No, I didn't say in creep. I said there was a diminution in creep on one as one apparently increased on the other.

Q Okay. Let me put it this way. Dr. Ellsworth, could you explain to us the relationship between movement, either microseismicity movement at depth or creep movement or any other sort of movement on the Calaveras fault zone in the area along that fault zone near the area of Hollister, where the two faults come close to each other?

A (Witness Ellsworth) Let me make sure I understand. You are asking to understand the mechanism of the transfer of motion between the faults?

Q That's correct.

A Well, permanent deformation of earth materials does not require that faulting act as a continuous process, that there can be permanent strain imparted in the material. The geodetic data that we have in the region where the Paicines and San Andreas faults are in close proximity and approximately along parallel courses would indicate that there is permanent deformation that occurs between the zones and it is thought that this permanent straining occuring within the zone accomplishes the transfer of some of the motion from the San Andreas fault onto the Calaveras and Paicines faults.

Q So in your opinion there is some transfer of strain or deformation from the San Andreas zone to the Calaveras zone?

A The deformation is distributed. I wouldn't say that it is transferred.

Q Okay. You said -- letme put it in the form of a question. Could you explain what you meant by the transfer of movement or strain between two faults is not necessarily based on faulting? Did you mean that it is not necessary to have at the surface evidence of a faulting connection between two faults in order for there to be transfer of strain between those two fault zones?

A That was not the intent of my answer. I will attempt to explain again that slip within a system of faults may also involve permanent deformation in the materials around those faults and that these two mechanisms acting together may transmit the motion that occurs at great distances across the fault zone.

Q Okay. If we could step back and approach this from a different perspective --

WITNESS JACKSON: Mr. Barlow, I was going to ask, I think -- I know I shouldn't speak back to you this way, but one of the problems is that I think the individuals on the panel are trying to be, you know, scientifically very precise. If I infer what you are trying to ask is youare asking is in some way movement on the Calaveras system of faults equivalently related in some driving mechanism sort of way to the San Andreas fault. I think if you ask that question I think most of us

would say yes. I'll let the others speak for themselves. I don't personally think that maybe if you want the precise answer as you are asking it -- that's fine. But I think that's what the difficulty is. I don't want the Board to interpret a lack of responsiveness to your question, okay?

MR. BARLOW: Okay. That's why I was going to rephrase my question in a simplified way.

WITNESS JACKSON: Thank you.

BY MR. BARLOW:

Q Dr. Ellsworth, is it your opinion that movement on the San Andreas fault zone can be transferred in any way to movement on the Calaveras fault zone?

WITNESS HERD: Excuse me. May I ask for a five-minute break?

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

(A brief recess)

JUDGE GROSSMAN: On the record.

MR. BARLOW: Your Honor, could I ask if there was a question pending that was not answered? I believe there was. Could the Court Reporter read it back?

(The question was read back.)

WITNESS ELLSWORTH: It is my opinion that movement on the San Andreas fault zone could be transferred to the Calaveras fault zone, but other relatively long periods of

time. I am aware of no evidence that would suggest coseismic movement on the San Andreas fault and sympathetic movement on the Calaveras fault.

BY MR. BARLOW:

Q Do you know of any evidence to disprove the hypothesis that movement on the San Andreas fault could be accompanied by movement on the Calaveras fault?

MR. SWANSON: Objection. I would like to hear what the foundationwas for that hypothesis before we go on on the record.

JUDGE GROSSMAN: I couldn't even hear the question.

MR. BARLOW: Would you like me to repeat it?

JUDGE GROSSMAN: Yes, please.

BY MR. BARLOW:

Q Dr. Ellsworth, do you know of any evidence to disprove the hypothesis that movement on the San Andreas fault could be accompanied by movement on the Calaveras fault?

MR. SWANSON: I just want to know what the basis was for the hypothesis. I don't believe he got it from this panel.

JUDGE GROSSMAN: Overruled. The witness can answer the question.

WITNESS ELLSWORTH: It is of course very difficult to negate such a hypothesis; however, there have been ample opportunities to observe either surface offsets on the Calaveras fault at the time of earthquakes on the San Andreas fault that

have not been observed and there have also been numerous opportunities to observe microearthquakes occurring at the time of earthquakes on the San Andreas fault and, again, those have not been observed either.

BY MR. BARLOW:

Q Dr. Ellsworth, do you or any other member of the panel know of any instances in California faulting episodes where earthquakes on one fault have triggered sympathetic surface faulting on another fault?

A (Witness Ellsworth) Sympathetic surface faulting has apparently been triggered by earthquakes in the Imperial -- on the Imperial fault, on the Superstition Hills fault and on the San Andreas fault and on those same faults by earthquakes on the San Jacinto fault.

Q So it is a common occurrence among faults of the San Andreas system for an earthquake on one fault to be accompanied by surface faulting on another fault?

A Very, very minor, apparently surficial movements have been triggered on occasion in the Imperial Valley region. I am not aware of similar observations elsewhere in the San Andreas system, with the exception of the possible association of movement on the Las Positas fault with the January 24, 1980 earthquake on the Greenville fault.

WITNESS SLEMMONS: The movements of that sort have always been quite trivial in the world on a global scale or in

worldwide data there are several examples of conjugate faulting where your faults are more in a rectangular pattern where two faults may move at the same time. But for branching faults such as those of the San Andreas system here in, in New Zealand, those that are similar in Japan, movement in one has historically always been independent of others.

BY MR. BARLOW:

Q Dr. Ellsworth or Dr. Slemmons, have you studied in detail the Borega Mountain earthquake of 1968?

A (Witness Slemmons) No, not in detail.

Q Has anyone on the panel studied that earthquake in detail?

A (Witness Brabb) No.

A (Witness Jackson) I have not.

Q Okay. Dr. Slemmons, going back to Appendix E in the Staff SER and your letter of April 28, 1980, on page 12, where we were looking before, in this part of your report on pages 12 and 13 you have used three different alternatives that you characterize as appearing to be reasonable for surface rupturing on the Verona fault and alternatives suggesting maximum earthquake magnitudes. In your results of maximum estimated earthquake magnitudes you have calculated magnitudes on -- well, in the version that I have up to magnitude 7.3 you said in relation to that that you later recalculated that estimate to a magnitude 6.7. Therefore, I will overlook the estimate of 7.3 and discuss

the others. Do you have -- you have estimated potential maximum magnitude earthquakes on the Verona fault zone under various alternative circumstances of magnitudes 6.9+, 6.5 and 6.95. Would this lead you to estimate or characterize the potential maximum magnitude earthquake on the Verona fault as a 6.5 plus or minus .5?

A (Witness Slemmons) I have a hard time picturing a very much larger earthquake than 6.5 for this zone. I hesitate to put plus or minus figures on it. These really represent a statistical treatment of the data and the average value is essentially a best fit from the data.

Q Does this mean that in the worldwide data set there are situations where characteristics as you have discussed here have resulted in earthquakes of those larger magnitudes larger than 6.5?

A The larger numbers that we see here appear in those cases where the data base is poor and particularly that 7.3, which is modified to 6.7, and that same figure comes up in the first fitting of the 6.9 value. One of the reasons why fitting the data for the Verona fault has been so difficult is this type of reverse slip faulting is rather poorly known from the standpoint of surface faulting. Alternative methods that are widely used are to use worldwide data of all fault types where you sort of average out the relationships or to use the North American data, and the reason for that is that the North

American data base is of much higher quality than is typical of the worldwide data base, although there are many well studied earthquakes there as well. So many compilations will utilize, in addition to the fault slip type involved, either the North American or the worldwide data or a combination of the two or three.

Q Okay. I notice in your three alternative groupings of magnitudes there that in Group I and Group II the largest magnitudes that were estimated or arrived at were accompanied by a notice of poor data base. However, in Group III, the last one in which you arrive at an estimate of magnitude 6.95 for the strike slip width, there is no mention of poor data base there. Could you explain whether or not there was a good data base or poor data base?

A There was in part a poor data base. This included the possibility of a combined earthquake for a reverse slip earthquake generated along the 8.2 kilometer wide zone as well as along the strike slip zone, so it would be sort of a composite or integrated unit. And this does include a poor data base.

Q Within that characterization, could you define strike slip width and strike slip length or zone?

A The term "width" has been used there to assume that the movement was as a combined rectangular slab of the southern edge of Livermore Valley, that it would include the Vallecitos Hills on the northwest trending Verona as well as the southern

edge of the Valley along the bounding Las Positas fault. So I considered the longer length of the Las Positas, the 15 kilometer long length, and considered the width to be the 8 kilometer wide Vallecitos Hills. This particular interpretation is an awkward one to make because it is based on subsurface assumptions which cannot be based on any hard data. That particular number is one that I don't place very high credibility in.

Q Okay. But not looking at the number but at the words that you have used, am I correct in understanding that the strike slip length that you are referring to is the length of the Las Positas fault?

A Yes.

Q And the strikeslip width is the width of the Verona fault?

A Correct.

Q Okay.

WITNESS DEVINE: I think he means it's the length of the Verona fault, not the width of the Verona fault.

WITNESS SLEMMONS: The length of the Verona for the long axis. In other words, having an earthquake generated primarily by the Las Positas driving, buckling or decoupling at the end of the Valley. In that event, if you essentially decouple shallow, surficial materials of the Livermore gravels, actually the single figure for the length of the strike slip Las Positas would give the most reasonable value for the

magnitude and this would be on the order of 6.2 or 6.3.

BY MR. BARLOW:

Q Continuing on the next page, 14, of your letter there, at the top of page 14, the second sentence and the third sentence, you refer to reports dated 1978. Is that a typographical error? Is that supposed to mean 1979?

A (Witness Slemmons) It was 1977. It is a typographical error.

Q I'm sorry. I'm not referring to the -- let me clarify. There are three occasions in that paragraph where the 1978 figure occurs. I was referring to the second and third.

A The second would be 1979.

Q Would the third also be 1979?

A Yes.

Q Okay. And the first would be 1977?

A Perhaps Bob Jackson could clarify this.

Q I assumed it was a typographical error.

(Pause while the panel members confer.)

A (Witness Slemmons) The first one is correct. I would have to refer back to the record to verify the dates on the other two.

Q Okay. Well, it's not tremendously important except in the context of the change in position between the NRC Staff report of 1979, the SER input, and the SER in which this report occurs. And in that paragraph you say that any future ruptures

could have maximum offsets of 2 to 2.5 meters, as noted in the show cause report of September 29, 1978. I want to know if that should read 1979.

A That may. I would have to go back to the record and verify the date.

Q Okay. On page 15 of your report you state in your Section 8, Summary and Conclusions, on Point 3 in your summary, quote "Many of the assumptions presented in the JVA probabilistic analysis are reasonable and conservative, but the overall effect of the GETR foundation geometry and possible errors in inferred soil ages appear to make the overall probability assessment a non-conservative valuation."

Could you explain what you mean by "possible errors in inferred soil ages"?

A I understand that this topic is to be considered later in the week.

Q Well, I understand that probability is to be considered later, but I am limiting my question to the words, quote: "possible errors in inferred soil ages". And I believe that we have discussed soil ages here today because the Licensee has their soil consultant here.

A Okay. We can explore that, if you wish.

Q Could you explain to me what you mean by "possible errors in inferred soil ages"?

A I indicated I believe in one of my earlier reports

that the soil ages are essentially based on a soil stratigraphy with the numbered series of stages that, as you count down from the surface to subsequently deeper soils, each earlier one can then be indirectly correlated with the worldwide sea level change data of Shackleton and Updike. I indicated in my reviews earlier that this assumes a complete record and that each unit will appear that the local geological situation would not eliminate the record of any of the units nor would local accidents -- nor could local accidents add extra soil forming intervals. And that under these conditions, it would be possible to have, for example, the addition of an extra soil that might then automatically give errors in the inferred ages of older soils.

The errors could go in either direction, more commonly perhaps they would be in the direction of a conservative result. But I can conceive of landslides, local catastrophic flooding and other events that might give a non-conservative effect. The statement there should be a possibly non-conservative valuation rather than a non-conservative valuation.

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Q Dr. Slemmons, could you define a tear fault?

A (Witness Slemmons)A tear fault is where you have a thrust fault structure that terminates against a cross trend and this would be a terminating fault of a different orientation.

Q Could the cross fault be a strike-slip fault?

A It could be and commonly is.

Q Is this common to find this situation in California faults?

A I haven't mapped in any of the young tertiary sediments where you have anti-clinal structures but I've seen cross-sections with them represented.

Q Okay, I think that's all right now for you, Dr. Slemmons, if I could switch over to questionning Dr. Herd and Dr. Brabb.

Dr. Herd, does the U.S.G.S. have the capability in terms of laboratories to date soils or estimate the age of soils found in trenches?

A (Witness Brabb) The U.S. Geological survey has two radio-carbon laboratories which do perform some analyses of soil ages.

Q Are either of those labs in the Menlo Park facilities?

A There is one.

Q Are you qualified to use that laboratory to

estimate the ages of soils.

A Laboratories are available for routine analysis of ages of materials that are needed to complete geologic investigations supported by the geologic division.

Q Dr. Herd, are you qualified to estimate the ages of soil?

A Qualified? I've had quite a number of courses in soil mapping and soil identification, soil genesis -- when you say to estimate the soil age, there are a variety of data which apply to that answer. So, yes, in a general sense, I guess.

Q Are you experienced in estimating ranges of ages for soils in various locations in your work?

A I have been involved in a number of instances where soil ages have been estimated either by my colleagues or I have made approximations of my own.

Q At the GETR site, the ages of soils in the trenches have been estimated by a consultant to the licensee.

Did the NRC ever ask you Dr. Herd or any other soils scientists from the U.S. Geological survey to estimate the age of soils in trenches at the GETR site independently of the estimates presented by the applicant's consultants?

A As part of the review, I certainly did look into the problem of the age of the soils. Although, we did not

perform independent assessments of -- for example, the radio-carbon agents of the soils. The raw data were provided to us by General Electric.

Q Did you or any other member of the G.S. perform a laboratory analysis of any soils on the GETR site or any of the trenches associated with it?

A I am unaware of any samples having been taken by geological survey personnel that was from the GETR area which were used to date soils.

Q Were you or any other G.S. scientists ever asked to do that? In the context of the GETR review?

(Pause)

A I hate to ask this but would you please have the last question repeated?

(Pause - playback)

I don't particularly remember a direct mandate to do something of that sort but I certainly was not precluded from it.

EXAMINATION

BY JUDGE GROSSMAN:

Q Dr. Brabb?

A (Witness Brabb) I'd like to respond to that and add to what Dr. Herd said just very briefly. We would not normally be asked to do that as part of our concept of what our role was in this investigation. It was to review

the work of the consultant, therefore, the implementation and supplementation of information would not normally be asked of us. It was not in this instance.

Q Mr. Morris?

A (Witness Morris)

As coordinator of the geologic review, Bob Jackson from the NRC never requested that information from us.

MR. BARLOW: Thank you.

MR. SWANSON: Mr. Chairman, so that there's not a confusion, you've heard the term consultant term used by two different members of the panel and I'm quite sure they are used in different ways.

Dr. Brabb, I think --- if he indicated what he meant when he said the consultant just so that the record's clear.

WITNESS BRABB: Yes, to the best of my knowledge, in all isntances where I have used the word consultant, it's been consultant to the General Electric Company.

In this instance it consists of earth science associates, Dr. Jahns and a number of other people.

JUDGE GROSSMAN: I don't recall it being used --

MR. SWANSON: Well, I think members of the staff used the word consultant, by meaning consultant to this staff, the NRC staff. I didn't want that to be any confusion in the record.

BY MR. BARLOW:

Q Mr. Morris, your testimony for this proceeding on page 5 could you look at that?

A (Witness Jackson) Could I add to the last questions?

Q Sorry, I'm sitting here thinking.

A We did not, I can confirm we did not ask that independent age dates be assessed for the GETR site. It is -- routinely we do not do it in an audit review of the site that we're -- of any site, unless there's a particular concern about the age dates. In this particular case, we adopted those estimates provided by Dr. Herd and Dr. Brabb after their review of the information provided by the licensee. I think in most instances we came up with younger dates than the licensee's consultant, as an assumption.

Q Dr. Jackson, would you agree that there is a disagreement between the U.S.G.S scientists and the licensee's consultants when estimating the ages of soils in the trenches?

A I hate to answer that in a general way. I think there are differences of interpretation of particular layers.

I don't know if they disagree over the particular age dating.

Q If I could make it more specific, do you agree that

there is a disagreement between the U.S.G.S. scientists and the consultants of the licensee regarding the age of the recent movement on the fault planes in the trenches?

A That's my understanding, yes. The estimate of most recent movement.

Q Mr. Morris, are you looking at page five of your testimony?

A (Witness Morris) Yes.

Q There I read, the last two sentences, "However, we have contended throughout the proceedings that one meter of surface offset is not a conservative estimate of the total amount of offset that occurs, that could occur along the Verona fault. The total amount of offset will not necessarily occur along any one fault, plane or strand of the Verona fault."

Could you explain that statement?

First, let me ask you. Is that the position of the U.S.G.S.?

A Yes, it is.

Q Could you explain it please?

A Well, we have agreed or contended from the outset that a postulated one meter for design purposes was not a conservative boundary. I think that's sufficient to explain that part.

The second part I recall answering yesterday was

that we would expect movement not to be restricted to any one strand of fault in the -- several strands that we know in the trenches and that would be distributed amongst those.

Q Okay, thank you.

A (Witness Devine) I'd like to make what I believe is a correction to what Bob said and ask for him to agree or disagree. The words that we used were that we do not believe that one meter of surface offset is not a conservative estimate for the total amount of offset that could occur on the Verona fault.

Bob said for design purposes and that's not what we have said as a statement. What we have said is what he said on page 5.

And I believe he mis-paraphrased it.

(Witness Morris) I'll stand corrected.

JUDGE FOREMAN: I would like to ask a quick question.

With respect to the last sentence, the fact that the total offset wouldn't necessarily occur on one fault plane, is that a result of a conservative situation of a deleterious one?

Would it be better from the viewpoint of the design characteristics, from the viewpoint of minimizing damage in the plant for it to curve up on one fault plane or have it occur on several?

WITNESS MORRIS: If the fault movement were distributed over several strands, let's say, take an arbitrary number of 20 feet and assign 6-1/3 feet to each strand of one of three strands, in otherwords, you can have six feet on each of those, that would be distributed.

Accumulative, it would be roughly 20 feet of movement on one strand and that would be the worst case, probably.

BY MR. BARLOW:

Q Dr. Herd and Dr. Brabb, I have a series of questions based on the deposition which you went through on March 25th, 1981 in Washington D.C., I assume -- no, in Bethesda, Maryland, I see.

A (Witness Jackson) I'm having a hard time hearing you.

Q Oh, okay, I'll talk louder, I'm sorry.

Do you have a copy available of the transcript of your deposition?

Yes, I'd like you to look first at page 80?

A (Witness Herd) Excuse me, what page?

Q Page 80.

A 80?

Q 8-0.

Line 11, you say, Dr. Herd, similarly, the Verona

fault may join with or intersect other faults which if you viewed the area on the fault as a cumulative value, might increase the area of the fault to a greater distance. But we see the thrust component through a certain kilometer length and in that thrust element component there is comparability with the rupture link of the San Fernando event of 1971.

So on line 20 you continue, so what we are comparing is the thrust -- oh, I'm sorry, that was Mr. Edgar. Strike that last part. Could you explain what you meant in lines 11 through 18 on page 80 of your deposition?

A (Witness Herd) May I have a moment to study the context in which these sentences occur?

Q Certainly.

JUDGE GROSSMAN: Off the record.

(Discussion off the record.)

JUDGE GROSSMAN: On the record.

Could you start again, please, Dr. Herd?

WITNESS HERD: As best I see these sentences, they occur in the context of the comparison of San Fernando, the San Fernando earthquake of 1971 to the Verona fault.

The answer that I have put in the context of line 6 through 18 is an attempt to further explain the relationship there, to paraphrase if I may the context of that as well as other sentences in this context.

I pointed out that the length of the Verona fault that I was aware of or I had personally interpreted was of a length comparable to the ruptured length of the San Fernando fault event of 1971, that is of the order of twelve kilometers; I believe to be specific, the San Fernando rupture has been variously estimated between twelve to nineteen kilometers in length.

Secondly, in paragraph 1' through 14, that was an attempt to try and depict properly the actual area on the fault and I had -- I'm not sure if it was in previous pages or elsewhere, I had talked about the calculation of an earthquakes size of a particular area -- or had compared it by the area of the fault that might generate an earthquake and I was trying to point out that the Verona fault might join with others which could increase the area of the fault plane.

In particular, I would presume I was making reference at least mentally to the Las Positas fault.

MR. BARLOW: Thank you.

JUDGE GROSSMAN: I just want to again point out on the record, I didn't realize we were offered that portion, that your references to the deposition, the exact quotes of the deposition will not be on the record unless you quote it.

Now what's absent from this discussion now is any

number that may have been in there and of course, that's up to you whether you wanted it there but I'm not sure that the discussion is meaningful without further elaboration on what is in the deposition and you might want to keep that in mind for your further questions.

BY MR. BARLOW:

Q Dr. Herd, on the following page you were discussing the number associated with the length of rupture in the Verona fault zone and the San Fernando fault zone and on line 20 of page 81, you stated and I quote, "I don't think we really know what is the full thrust link on the Verona fault. There are a number of ways to calculate it but we simply don't have sufficient field evidence in which I feel comfortable to give you an exact value earning or an effective limiting value."

And a few lines below that on page 82 on line 10 you say, the last part of the sentence, "there are critical bits of information missing in these calculations."

Can you explain -- well, let me continue on that page.

On line 16, page 82 you continue, "I believe the mirimum value that we have reported in the figure is something on the order of 10 to 12 kilometers. However, if you -- the Northwestern end of the Verona fault is as of yet unestablished so it is not clear whether there is continued

thrusting--thrust faults."

Then you restated, "It is not clear whether there is a Northern continuation of thrust faulting in the Verona fault to the Northwest that somehow might intersect the Calaveras or be limited by it in some fashion or sub-parallel faults like the Pleasanton fault and similarly the intersection or presumed intersection of the Las Positas fault also gives complications in terms of the full length because it may bend or intersect with a perpendicular angle."

Then continuing a few lines beyond that on line 16 on page 83, "In our first report of 1979, -- which," I would like you to clarify which report that is, but you continue, "I believe you reported a value for the Verona, Las Positas fault system which would be the family of faults, the two joined together as having a possible link of the order of 29 kilometers."

Now, in the context of these quotes, I would like you to explain some of the things that you said in trying to determine the length of the possible faulting on Verona fault zone and could we start with an explanation of which report of 1979 he meant. Was that included in the SER's input of 1979?

MR. SWANSON: Mr. Chairman, we've been pretty loose in dealing with the stipulation before but we now have

questions that are dealing directly with one of the items of the stipulation, that being item F, which reads the Verona fault including the Northwesterly projection along possibly splays of the Pleasanton fault has an estimated maximum surface length of 12 kilometers. The Intervenors are signatories of course to the stipulation. The Board I realize has balancing interests to consider in terms of this -- of it's own determination of the case.

But, I think to allow one of the signatories to the stipulation to violate it by probing extensively into one of the items that it has agreed to, is simply just making a mockery of the stipulation itself.

JUDGE GROSSMAN: I don't think that -- I will consult with my fellow board members on that.

Did you want to speak to that Mr. Cady?

MR. CADY: I believe Mr. Barlow's line of questionning deals with the length of the Verona fault, taking into consideration the length of the Las Positas fault and if he directs his questions as to the length of the Las Positas fault and then add that on to the stipulated 12 kilometers of the Verona fault, I think we can short-cut any future questionning along these lines.

MR. SWANSON: The obvious problem that we have is that he's stretching the definition of the Verona fault. The stipulation of course, speaks for itself. The Verona

including a certain projection of surface length of 12 kilometers -- the question is going to the fault length of the Verona.

JUDGE GROSSMAN: Without even consulting with my fellow board members, it seems to me that we have a semantic problem that will be clarified by the answers and I don't -- do you persist in objecting to that explanation?

MR. SWANSON: Well, the problem as I understand it is, we're adding on other faults contained as part of the Verona and we're just -- I'm afraid opening a door to a wholesale probbing -- development of an issue by at least one of the parties on an item which I believe has been resolved by stipulation.

I fully recognize that the Board has interest of it's own course to pursue and may have interest in this area, but my concern is one of the signatories starts asking questions in an area which is clearly covered by the stipulation -- we have a problem.

JUDGE GROSSMAN: Do you have something further to add, Mr. Barlow?

MR. BARLOW: Yes, Your Honor, if I might approach this line of questionning with the stipulation in mind and agreeing to the stipulation, I'll try to formulate my stipulation in mind. And, in agreeing to the stipulation

try to formulate my questions in a way that allows for the stipulation.

MR. CADY: Excuse me, Your Honor, I believe what he's trying to say is, is that we will take the Verona fault as stipulated to 12 kilometers, if we can just establish what the length of the Las Positas fault is, then we can go on to other areas of inquiry.

MR. SWANSON: That's a different question, I have no objection to that.

JUDGE GROSSMAN: Go on to the Las Positas fault.

BY MR. BARLOW:

Q Dr. Herd, when you referred to the first report of 1979, are you referring to the document known as SER Input of 1979 in which a report by you and Dr. Brabb was included?

A (Witness Herd) A moment to check?

(Pause)

I'm sorry to take a moment but I really want to be sure of the context of everything.

(Pause)

This may take a minute, if you please.

MR. BARLOW: Sure.

WITNESS BRABB: Your Honor, may we take a 5 minute break?

JUDGE GROSSMAN: Yes.

(A brief recess was taken.)

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JUDGE GROSSMAN: Mr. Barlow, you may continue.

BY MR. BARLOW:

Q Dr. Herd, did you discover whether or not the report you referenced was from the 1979 SER input?

A (Witness Herd) Let me be specific. In the deposition I refer to in our first report of 1979. I believe we reported a value for the Verona - Las Positas fault system which would be the family of faults, the two joined together, as having a possible length of the order of 29 kilometers. Specifically, in our report entitled "General Electric Test Reactor Vallecitos Nuclear Center, Vallecitos, California", with a cover letter from Dr. Menard, Director of the Geological Survey, to Mr. Denton, dated the 5th of September, 1979, we made a point in the conclusions -- unfortunately, the manuscript is unpaginated -- but Conclusion No.3 of this '79 report was a conservative position based on information available is that the Verona - Las Positas fault system extends from the Calaveras to the Greenville fault, a distance of at least 29 kilometers. The hypothetical fault proposed by the Licensee is restricted to a distance of 8.2 kilometers with a reference given.

Q Thank you. Was that measurement of the Las Positas fault based on the assumption that the Las Positas continued to the Calaveras along its projected trend or that it curved into and joined with the Verona fault?

A I'm sorry. I really don't recall.

Q Okay. If you measured the length of the Las Positas fault from the Greenville fault -- I'm sorry. Dr. Brabb?

WITNESS BRABB: Excuse me. I think I recall the answer to that question. It was from the intersection of the Verona and Las Positas fault eastward to where the Las Positas fault is truncated by the Greenville fault.

BY MR. BARLOW:

Q Could you tell me the length from the intersection of the Verona fault to the Greenville fault on the Las Positas fault? How long is the Las Positas fault from the Greenville fault to the intersection with the Verona fault?

A (Witness Herd) I'm sorry. I'm not certain of the answer that my colleague just gave. Would you please repeat Dr. Brabb's answer?

JUDGE GROSSMAN: Mr. Reporter, could we do that, repeat Dr. Brabb's answer?

(The answer was read back.)

WITNESS HERD: I believe Dr. Brabb is incorrect in making that statement. I think he did not mean it in the way he said it.

BY MR. BARLOW:

Q Okay. To clear up the confusion, could you tell us where the ends of the length measured as 29 kilometers begin and end in the report that you referenced? Is that the combination of the Verona fault and the Las Positas fault?

A (Witness Brabb) I would like to go back to the question that was originally asked so that it would be clear what we were responding to. I had thought it did relate to the amount of the Las Positas fault that was included in the 29 kilometers of measurement and you are questioning whether or not that measurement included the portion that extended beyond the Verona fault in the vicinity of the San Antonio Reservoir and asked whether or not that amount was included in the overall measurement. My answer was no, it was not, that the measurement was taken from the eastern terminus of the Verona fault where it intersects the Las Positas fault. That distance eastward, is what I said -- it is actually northeastward -- to where the Las Positas fault is terminated by the Greenville fault, that is the amount of measurement that was used to come up with that 29 kilometers.

You add to that distance, and I don't recall what it is, the I guess stipulated measurement of 12 kilometers for the Verona fault and that's where the figure of 29 kilometers comes from.

Q Okay. Would you take the number 29 and subtract 12 to arrive at the length of the Las Positas fault from the intersection with the Verona to the intersection with the Greenville?

A That's my recollection.

Q Thank you.

WITNESS HERD: Unfortunately, it will not give you the right answer.

WITNESS JACKSON: Mr. Barlow, could I suggest that we have Dr. Herd's map here and Dr. Brabb and Dr. Herd certainly can scale it off. It may solve -- make for a cleaner record.

WITNESS HERD: May I just comment a moment? The problem is you are asking us to recollect a calculation that was done several years ago at the time when there were multiple interpretations and hypotheses as to how to link it up. I do recall that the Las Positas fault has a mapped length of the order of about 15 kilometers. Now, where that point of intersection is we can measure off on a map, but then when you come to a calculation of what the length of the Verona fault is it would be then a simple subtraction. But I know it is not certainly of the order of 17 kilometers. That is unreasonable.

So there must have been some sort of different interpretation of the turn on the end of the Verona fault to have accomplished that measurement. So rather than have it appear that Dr. Brabb and I are at difference, the problem is one of recollection and I am afraid it's rather difficult from my vantage point to reconstruct how that was done.

MR. BARLOW: Okay. Thank you.

BY MR. BARLOW:

Q Dr. Brabb, you visited the trenches at the Vallecitos

site, did you not?

A (Witness Brabb) Yes.

Q Did you observe the shear zones in Trenches B-1, B-2 and H?

A Yes.

Q You are familiar with the estimated width of the Verona fault zone in the SER?

A Is that figure 2200 feet?

Q No, sir. In the 1980 version of the SER on page 5 I believe it reads "at least 3200 feet wide, based on the latest USGS report".

(Pause while the panel members confer.)

WITNESS JUSTUS: I can comment on that. That's a typographical error that was corrected during an ACRS meeting. We can apologize for not making sure that that correction was noted here. The correct number is 2200 feet.

MR. EDGAR: What's the correct number, Dr. Justus? I didn't hear you.

WITNESS JUSTUS: Two thousand two hundred feet.

MR. SWANSON: I might just mention or remind the Board that Stipulation Item C is that, geologic data indicate that the GETR site is located within the zone of faulting (the Verona fault) which is at least 2200 feet wide.

JUDGE GROSSMAN: That doesn't, of course, exclude 3200 feet.

MR. SWANSON: No, but I just wanted to indicate that I think -- I don't think this is the first time parties are hearing that number, 2200 feet. I didn't want there to be a misconception.

WITNESS JACKSON: I would like to add something. It is an estimate of 2200 feet. I don't think we sat down to be extremely precise in that, just to make sure that qualifier is attached.

MR. BARLOW: Thank you.

BY MR. BARLOW:

Q Dr. Brabb, in your opinion is it possible that other branches or shear zones associated with the Verona thrust fault zone could exist between Trench H and the Calaveras fault zone in the Vallecitos Valley?

A (Witness Brabb) Yes.

Q Do you think that it would be possible to conduct investigations to see if there were other shear zones in the Vallecitos Valley associated with the Verona fault zone by additional trenching in that direction?

A Yes.

Q Dr. Brabb, do you think that it is a reasonable hypothesis that the Verona fault zone could be wider than 2200 feet in the direction of the Calaveras fault zone?

A I'm sorry. I don't have the geometry clearly in mind. Would it be possible to refer to a map so that I can see

specifically what direction you are referring to?

Q Certainly. In the SER of 1980, the report by you and Dr. Herd, I believe it is Figure 1, Appendix B.

A And what was the question?

Q Okay. Looking at Figure 1 and looking at the width of the Verona fault zone extending from Trench B-3 to Trench H and where the words read Vallecitos Valley, going in that direction, which is towards the Calaveras fault zone, do you think it is a reasonable hypothesis that the Verona fault zone is wider than the Trench B to Trench A width?

A My difficulty in responding is that I am not aware of any direct evidence for faulting in that area; however, I am also not aware or I am not convinced that a thorough and complete search was made for faults in that area. Therefore, I would say that it is possible for some limited distance beyond Trench H to find additional splays. However, I wouldn't think that this would extend much further than Trench H, say all the way to the word "Vallecitos" in "Vallecitos Valley" at the scale of this map.

WITNESS JACKSON: I would add a comment, Mr. Barlow. We did look at aerial photographs of the site area and I think at some point in the proceeding, in the review, we did question hills or very weak linears that were further to the southwest than Trench H. I don't recall that they were anywhere near as strong or definitive as those that led us to require trenches

at other locations on the site. But there definitely was the potential.

BY MR. BARLOW:

Q Therefore, Dr. Jackson, is it reasonable to conclude that the Verona fault zone could be wider than 2200 feet?

A (Witness Jackson) I think you could conclude that, but I think I agree with Dr. Brabb that it is a possibility. I don't think we have any strong positive evidence to indicate that. I wouldn't rule it out.

WITNESS JUSTUS: Also I would like to add for clarity that it is my understanding that you are referring to the out-crop width of this fault zone. Am I correct in that assumption?

BY MR. BARLOW:

Q Could you define the term "outcrop width"?

A (Witness Justus) That's the width of the furthest separation of the fault traces measured perpendicular to the traces at the surface of the earth.

Q Yes. I am referring to that width.

A Thank you.

Q Dr. Brabb, in your Appendix B of the 1980 SER on page II --

A Do you mean ii as opposed to II?

Q Yes. Lower case Roman numeral ii. At the top of that page no. ii, it reads, quote, "The absence of faults opposite the GETR in the B-1 Trench 280 feet northwest of the

reactor does not preclude the existence of faults beneath the GETR that either do not extend north to the trench or that are older than the 70,000 to 130,000 year old alluvium in the trench" end quote. Dr. Brabb, is it correct to interpret this statement to mean that there could be a fault beneath the reactor which was not observed in any of the trenches that were dug?

A Theoretically possible, yes.

Q Do you think it would be helpful in terms of investigating the possibility of faults directly beneath the reactor to have dug another trench on the other side of the reactor opposite the B-1 trench?

A Yes.

Q Is it possible, Dr. Brabb, that faulting in the Verona fault zone could occur in an en echelon fashion such that faulting could exist directly beneath the reactor but would not show up in the B-1 trench?

A Yes.

Q Thank you.

WITNESS JACKSON: I would like to add a comment to that, if I may. I think throughout the case which related to requirements for digging trenches we have interacted. I would agree with Dr. Brabb on the first trench, the trench to the east of the reactor would have been helpful. On the other hand, there were specific bases which led us to agree upon the

trenches that GE had proposed in the location they were to be put and those were based on an examination of photographs which provided rough estimates of linear features that could be seen on the aerial photographs. This is what led us to conclude where those trenches should be put.

Included in that decision in part was an understanding that any throughgoing faults of any significance in terms of similarity to the major throughgoing features that we could see in Trench T-1, as an example, should be intersected by the Trench B-1 or B-3, if I have the trenches correct, projecting from the hillfront down to the southwest. We did not notice any of those projecting through the site. The closest was a linear feature which, when trenched in the B-1 Trench, turned out to be a channel fill deposit, which could result in a topographic or a linear feature being observed on the aerial photographs.

Now in all fairness I have to point out that the photographs we looked at were photographs which were taken after the plant was built, so there was a highway put in there, a parking lot next to the GETR, which would to some extent have restricted your ability to see a linear feature going through there. But my feeling is very strong that the basis that led us to concludewhere the trenches were, which we had I believe -- the NRC and the USGS had fairly good predictive ability, we found faults in most locations where we expected to find

them, based on our air photos, we did not see such a linear that would have intersected the trench, B-1.

Now the other element is that the depth of the trench may nothave been deep enough to reach something, but that would also then begin to infer an older age of offset.

BY MR. BARLOW:

Q Dr. Jackson, would you agree that a rather critical point that is being examined in this proceeding is whether or not a fault exists directly beneath the reactor at the GETR site?

A (Witness Jackson) The question is obviously -- the answer is obviously yes, although I say critical element. We have considered surface faulting under the plant. There are excavation photographs which indicate that possibility. The probabilistic studies that have been done in part depend on whether or not -- may in part depend on whether or not there is or is not a fault under the reactor. But some of the probability analyses indicate that the probability would be extremely low even if there were a shear existing under the reactor.

Q Dr. Jackson, do you agree that various people in this proceeding have taken the position that future offsets will most likely occur on observed historic offsets that were observed from the trenches?

A I don't recall that term being used, no. My recollection is that the future movements are most likely to occur

on the existing shears, since I was one of the prime authors of that term. What was intended was shears like those that we see at the break in topographic -- the hillfront and the break in slope where the trenches were put.

Q Okay. Dr. Jackson, using your term "existing shears", do you agree that if there were an existing shear that trended directly beneath the GETR that it would be a critical factor in the probability studies?

A My understanding is that it could be. But it may not have to be.

Q Dr. Jackson, can you recall the date on which the Staff was notified of the existence of photographs of the GETR foundation excavation?

A I do not recall, but we could look it up. We have it with us, I believe.

Q While that data is being looked up, I would like to ask you the relevant question here. Dr. Jackson, do you recall whether or not after the staff learned of the existence of these photographs of the excavation and looked at these photographs, whether or not the staff ever asked the Licensee to consider digging an additional trench on the opposite side of the reactor from the B-1 trench?

A Let me look at the date first. The photographs were mailed to Mr. Victor Stello of NRC on January 5, 1978, from Mr. R. W. Darmitzel. My recollection -- and I must admit, it

nas been a long review for this particular facility and I have been involved in it since the show cause order -- there have been many discussions about what study should or should not be done and we have debated this with the USGS and the Licensee and what was required and what is not required. It is usually our position in the branch that we will review -- we request and we review proposed studies by the Licensee. Now obviously we have a very strong encouraging role in that in which we demand certain things be done.

In this particular case, there was a long period of time between the time that we had trenches T-1 and the other trenches on the site. So there was a long period of time in which we had verbally requested trenches and they were not dug. I think that is a fact. Now I recall there were discussions as to whether or not a trench was needed to the east of the GETR. I don't recall any specific discussion or meetings or anything like that that resulted in it. I thinkit was one of those that we were -- looked at in an evolutionary fashion and we did not make a decision "yea" or "nay" as to whether to require such a trench.

JUDGE GROSSMAN: Excuse me. Just for a second, I want a little clarification. Are your standards the same? You have mentioned a number of times as to what the usual procedures are. Are the standards the same with regard to show cause proceedings as they are with regard to licensing

proceedings as to how much you rely upon the licensee's information?

WITNESS JACKSON: I'm afraid I can't answer very completely. This is the first show cause proceeding I have been involved in. I feel that we, as a staff and as a review board, have done a far greater amount of independent analysis and development than would normally be done for a power plant license review in say an operating license stage. In other words, we have injected ourselves -- "we" including our advisors and consultants -- into the process moreso than would be done by the applicant or licensee submitting information under a REG Guide 1.70 type format. Yes.

JUDGE GROSSMAN: Okay. But when you are talking about the usual situation, then you are comparing it to the usual licensing situation.

WITNESS JACKSON: It is the only comparison I have right now.

JUDGE GROSSMAN: Did you have something to add, Dr. Justus, to that?

WITNESS JUSTUS: No, I was just trying to clarify with Dr. Jackson a particular point about applying Appendix A.

WITNESS JACKSON: I think I have answered your question as best I can. No decision yes or no was made as to whether a trench should be put there or not.

BY MR. BARLOW:

Q Okay. I have a related question to that. After the time -- following the Staff's receipt of the photographs of the excavation site, did GE dig any trenches after that?

A (Witness Jackson) I do not believe so.

Q Did you say it was January, 1978?

A Yes.

Q Were all of the trenches dug at that point?

A I do not know. I would have to check each one. I don't recall.

(Pause)

WITNESS JACKSON: Let me confer for one minute. Maybe we can tell.

(Pause while the panel members confer.)

WITNESS JACKSON: I am reminded that there was a sequence of trenches put in -- I believe the trenches in the hillside that looked at the back scarp area of the proposed landslide hypothesis and the trench E and the trench A, the ones on the extreme limits of well -- testing the length of the fault to some extent.

I don't -- and I'm reminded those were in the fall of '78. That's our best recollection. I'm sure there are in this people that can say.

CROSS-EXAMINATION(Resumed)

BY MR. BARLOW:

Q Would that be approximately nine months after you received the photographs?

A (Witness Jackson) Yes.

Q Were there also a series of small minor trenches dug closer to the reactor following January 1978?

(Pause)

A I think the best way -- we're guessing and that is not a good way to testify, we could look them up if you want and tell you tomorrow, if that's what you would like.

Q I would be interested in knowing how many trenches or what trenches were dug after the staff received photographs of the excavations and pending calculation of the number, the real question is, why was the decision

made --

JUDGE GROSSMAN: Excuse me.

You may want to consult with G.E.'s people in a -- if they can refresh your recollection at all, I'm speaking to the panelists now. Perhaps you can come with a definitive answer.

MR. EDGAR: I wonder if it wouldn't be more fair to the panel to give them the phase 2 geologic report which is exhibit 6 and licensee's exhibit 6 and allow them to refresh their recollection. Your asking people to remember a time sequence that occurred three years ago.

WITNESS BRABB: I think it's a little more complicated than that Mr. Edgar. We appreciate the suggestion of help. There are two elements to it.

One is when the trenches are dug, the other is when they were closed. Are both those elements in that report?

MR. SWANSON: I was referring to the summary --

MR. EDGAR: The closure isn't in the report.

JUDGE GROSSMAN: I believe the questions are directed to the panel and we want the panel to answer.

If there's anything there that can lead them to a quick and definitive answer, that's fine, but since

there isn't, I think we'll wait until tomorrow and get an authoritative answer on it.

BY MR. BARLOW:

Q Dr. Jackson, the actual dates of the digging of the trenches is not what I was trying to get at other than the fact that there were trenches dug after the staff receives the photographs of the excavations.

The real question that I'm struggling with here is how did the staff decide or did the staff discuss and decide to not ask General Electric to dig a trench on the other side of the GETR reactor from the B-1 trench after the staff had received and reviewed the photographs of the excavation in which a possible existing shear was observed?

A (Witness Jackson) Let me ask one question of my colleagues.

(Panel Conferring)

(Pause)

My recollection is those photographs came -- were evidently submitted at that point in time -- my recollection is they probably were not reviewed for a long period of time. In otherwords, they came in, were in the docket but not reviewed.

Q Were you involved in the decision to not ask the licensee to not dig another trench to look for

existing shears beneath the reactor?

A There was no such decision.

Q Was that ever discussed?

A As I answered earlier in my testimony, I don't recall any meeting or decision pro or con as to whether such a trench was needed. I think I would welcome comments from the other panel. We -- from what I asked my colleagues was when did we finally send the photos to the U.S.G.S. to ask their assistance in review. It was considerably late in the review so I have a feeling -- it's not a feeling. It appears very well, that these photos were on the docket for some time period before they were reviewed and it related to staff shortages in my branch.

Q One final question along this line, Dr. Jackson.

A I would like to amend that just a little bit.

I really believe strongly that everything we learned on the site we learned by making certain estimates of where we expected to gain information.

It's been my view on this that it's a very low likelihood like those we see in T-1, B-1, H or B-2 exist in the reactor facility. It is a possibility. But I don't think anyone -- I certainly do not believe that is the case.

Q Do you agree that in the context of these

proceedings and the decisions that are to be made and in the context of analyzing the probability studies that have been done, that it would be helpful to all parties here to know whether or not there is an existing shear beneath the reactor?

A I just want to -- you said, would be helpful?

Q Yes.

A It certainly would be helpful, yes.

Q Do you agree it would be useful in estimating the probability of future offsets beneath the reactor?

A I think that's a question you should refer to the experts who did the probability analysis. I do not know.

Q Dr. Slemmons, do you agree that it would be useful in estimating the probability of future offsets on shears beneath the reactor to have investigated the possibility of whether on echelon thrust fault shear existed on the opposite side of the reactor from the B-1 trench?

A Yes.

Q Thank you.

(Pause)

MR. BARLOW: Your Honor, could we take a five minute break?

JUDGE GROSSMAN: Yes, that's fine. Off the record.

(A brief recess was taken.)

JUDGE GROSSMAN: On the record.

Mr. Barlow, you have given us some hint about some things contained in photographs. I take it you intend to pursue that in more detail later?

MR. BARLOW: Well, I believe it has been discussed during other cross-examination in the presentation so I wasn't intending to pursue it any more.

JUDGE GROSSMAN: Oh, okay, fine.

MR. BARLOW: I think it has been pretty well explored as far as the photographs go.

WITNESS JACKSON: There are two comments that we would like to add to the last answer. I will make one, Dr. Brabb.

The purpose of the trench program that was put in in the latest stages was, as you know, it had a continuing controversy over the origin of the features on the site whether they're landslide or faulting and the purpose of the trench excavations were put in in that stage. We directed to that end in mind. Now, the probability analysis was not being considered at that point in time.

And, therefore, we -- I'm trying to stumble -- we did not know in that point in time that that would be an element of any consideration in the proceeding, so I recall in talking with our colleagues here that there

were meetings held. The licensee proposed a certain investigation program and involved myself, the U.S.G.S. and I don't know if Dr. Slemmons was involved. The Advisory Committee on Reactor Safeguards was involved and that program was essentially approved at that point in time.

But, I think you make decisions at given points in time based on your needs at that point in time.

I just wanted to clarify that.

BY MR. BARLOW:

Q Dr. Jackson. Is there more concerning this?

A (Witness Brabb) Yes, I'd like to make a modification to my answer where you are asking about the possibility of trenching in the area southeast of the reactor to preclude the possibility of a fault.

It's my recollection that there was some trenching in that area that is not a part of the official record.

There was, as I recall a trench on the B-1 T-1 shear for example, that was dug. We had a chance to examine it but to the best of my recollection, there were never any logs prepared for that trench.

I have an even vaguer recollection that there may have been some trenches in the road area of the reactor southeast of the reactor but we did not have an opportunity to examine them.

Q So did you say there may have been some trenches in the area southeast of the reactor along the road?

A (Witness Brabb) In that general area, yes.

Q Do you recall when that was that you knew about it?

A It probably would have been in 1978, but I really don't have a clear recollection, either of the date or if in fact there were trenches.

JUDGE FOREMAN: Are you talking about Highway 84 or the road between the GETR and highway 84?

WITNESS BRABB: No sir, this would be the small utility road in the general vicinity of the reactor itself.

MR. BARLOW: Mr. Devine, --

WITNESS JACKSON: Let me just add to that.

I don't recall those trenches, except for one. I vaguely recall one trench on the east side of the road and I believe it was the trenches that were being put into chase B-2. In otherwords, there was an exposure in B-2 and we had postulated that it had an extent to it east/west and there was a trench put in very close to the road. I don't know if there was one on the eastern side of the road, but I believe the purpose of those trenches was to chase B-2.

BY MR. BARLOW:

Q Dr. Jackson, could you avoid skipping around?

I'd like to ask a follow-up question on something you said a moment ago.

You said that the purpose of the trenching in 1978 was to investigate the landslide hypothesis I believe. Was it not a concern of yours and other members of the geo-sciences branch and the staff at that time that there might be "existing shears" beneath the reactor itself that could be found in a possible trench?

A (Witness Jackson) I'm sure that was the concern, it always has been the concern. I believe the decision was based on the B--hold on a moment, I want to make sure I'm referencing the proper trenches.

(Pause)

I think I've been referring to -- the trench I've really been referring to is the B-1 trench. The B-1 trench was in close enough proximity of the GETR and I could scale it off of this figure, that it would very likely be suitable to indicate the presence of any shear such as those that we saw at the base of the hill front. I'm referring to figure 5 of the geologic investigation phase 2 by General Electric company, February, 1979.

MR. EDGAR: Licensee's Exhibit 6.

(Pause)

WITNESS JACKSON: On this figure it appears that the trench B-1 is about 200 -- well, we have one figure which is 280 feet and we just scaled it off, it looks like 220 feet from the GETR foundation. It is in that range.

So that it was our view I believe at the time of the consideration and that's been quite awhile ago, that if there were to be likely a fault like we're seeing along the base of the hill front, the primary offset of the Verona fault, or like we see in B-2, we would also see that in the trench B-1 unless it were shorter than 280 feet long which would make it a much smaller fault in terms of it's potential offset.

BY MR. BARLOW:

Q Dr. Jackson, do you agree that in a thrust fault zone you can have en echelon faulting?

A (Witness Jackson) In generalities, yes. I don't recall that exactly happening at this site.

Q Dr. Jackson, do you have any reason or evidence to dispute the statement by Drs. Herd and Brabb that we quoted before which is, "the absence of faults opposite the GETR in the B-1 trench, 280 feet Northwest of the reactor does not preclude the existence of faults beneath the GETR?

MR. SWANSON: Could we have an identification of where that statement came from?

MR. BARLOW: Yes, that's from the 1980 SER Appendix B, page ii or lower case Roman Numeral II.

MR. SWANSON: Thank you.

WITNESS JACKSON: If you say does not preclude I think I have no choice but to say yes.

I would say that good sound geologic reasoning does not lead me to conclude that I must have a fault there.

MR. BARLOW: Thank you.

BY MR. BARLOW:

Q Mr. Devine, during the deposition of March 25th, 1981 on page 122, you made a statement of interest in this regard.

(Pause)

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BY MR. BARLOW:

Q The quote that I'm interested in occurs in the secord paragraph on page 122, where you say a concern is that we make sure we understand the fact that we may not have been able to -- that "we" being the consultants and us and anyone else -- to have identified all the shears that may exist. So my answer is not limited to just the three shears that have been identified, but to existing shears, whether we know they are there or not. So yes, it is more likely to occur on an existing shear. We may not have identified them all.

Mr. Devine, do you agree that there may be existing shears beneath the GETR reactor that were not identified due to the lack of trenching?

A (Witness Devine) Yes, also, though, it may also have -- there could be shears not identified by the lack of trenching that went deep enough to get the old shears, as referred in the statement by Dr. Brabb earlier.

Q Thank you. Okay, I think we will leave that subject and go to one quick question for Dr. Jackson. In the context of that deposition on page 145, on line 13, page 145, Dr. Jackson, you say "the Oakshot paper we have been aware of for a short period of time on the staff and we discussed it again yesterday. As noted in the SER, we were not aware of any direct net slip measurements at the time we made this analysis. We will be looking at the Oakeshott and others paper in the

future." Did you make this statement on March 25, 1981?

A (Witness Jackson) Yes.

Q When did it come to the attention of yourself and the staff that the existence of the Oakeshott paper?

(Pause)

WITNESS JUSTUS: To be more specific, this reference to the Oakeshott paper is actually not quite correct. There is a paper in a journal that was edited by Mr. Oakeshott and that is a paper by R.V. Sharp.

BY MR. BARLOW:

Q Could you give us the title of it?

A (Witness Justus) "Displacement on Tectonic Ruptures" by Robert V. Sharpe.

Q Do you know what date that paper was published?

A In 1975.

Q That paper is the one that is referenced by you, Dr. Jackson?

A (Witness Jackson) Yes. Could I expand a little bit on that?

Q Certainly. If you could describe the paper and its contents and whether or not it has been examined since the deposition date by the staff.

A I wanted to expand on your previous question; then I will let Dr. Justus answer that. He is better prepared on that. When we prepared the initial SER, it is an important

point for the Board to understand -- we prepared the initial SER. The argument that had prevailed for a long period of time was whether the faults were landslide induced or tectonic induced. In fact, that has been the prevalent approach that is taken throughout the review. In an attempt to begin to work with recurrence or estimating surface offset that might occur as a result of tectonic movement, I initiated a study on my own to look at a way of estimating that, and that was to look at available offset data on the San Fernando, a classic paper in 1971. Included in that was Barrows and others paper.

Now we were clear -- I was clear in that review to specify that we did not have net slip, actual net slip measurements. Now when Dr. Justus came on the staff this obviously was an interesting point and he was assigned to the GETR review. We began to pursue this as a way of estimating faulting. In the correspondence with Dr. Barrows, who is at California Division of Mines and Geology, he noted in a response from a letter from him to us that we should look at this paper that Mr. Sharpe had done. I had not previously been aware of it. And in fact, in none of our discussions with the USGS or ourselves or other consultants had it come up.

So we then looked at it and Dr. Justus has worked on that paper since then.

BY MR. BARLOW:

Q Dr. Justus, does that 1975 paper by Robert Sharpe

provide data that is significantly different than the 1981 paper by Dr. Sharpe?

A (Witness Justus) No.

Q Can you explain to me why -- and perhaps you will need help from Dr. Jackson -- why the 1975 paper did not come to the attention of the staff until March 25, 1981?

A Well, we just, as we just explained, the Sharpe paper came to my attention in correspondence on September 5, 1980.

WITNESS JACKSON: I would like to amplify. You know, in the course of this review I have looked at thousands of papers. This was one I did not happen to find.

BY MR. BARLOW:

Q Dr. Justus, have you analyzed the contents of both the 1975 and the 1981 reports by Robert Sharpe?

A (Witness Justus) Yes.

Q Are you familiar with his conclusions regarding the offset data from the San Fernando earthquake?

A In which paper?

Q In either one.

A Yes.

Q In the 1981 paper, do you agree that he says on page 3 -- do you have a copy of that available?

A Yes. Are you referring to Open File Report 81-668?

Q Yes, I am, which was distributed by the staff last week. On page 3, in the middle of the page, it says within the

Sylmar segment I reported five measures of net slip ranging from 2.0 to 2.5 meters, Table 1, in Sharpe, 1975. And in the following paragraph it says the calculated net slip from 2.0 to 2.5 meters are representative of at least 1.4 kilometers of the 2.9 kilometer length of the Sylmar segments. Do you agree with his calculated net slips on the Sylmar segment?

A I have no reason to question the statements that you just read.

Q Thank you. Could you explain -- are you familiar with the Sylmar segment of the San Fernando earthquake?

A I am from literature research, yes.

Q Can you explain how thrust faulting could occur on a 2.9 kilometer segment within the context of that earthquake? To put that question in context a little more, if the total rupture length in the San Fernando earthquake were approximately 12 kilometers, would the Sylmar segment be an en echalon segment of thrust faulting?

A I can't answer that question directly and I should explain why and then try to answer at least part of your question. The Sylmar segment is a part of the San Fernando fault system and has been referred to as a segment on a geographic basis. In fact, from the map of the San Fernando fault system that ruptured in 1971, the Sylmar segment can be considered to be en echalon within the fault system on a geographic basis, that is to say, it is offset from the con-- the adjacent segment,

called the Tujunga segment. That is one part of the -- one aspect of the fault. The other aspect that I think should be made clear is that the Sylmar segment and the Mission Wells segment adjacent to it are the most unlike thrust fault portions -- the Mission Wells and Sylmar segments are more like strike slip faults. They are principally strike slip portions of the San Fernando system. The Tujunga and the Lakeview segments are mainly thrust fault segments. It is the Sylmar and Mission Wells segments that show the greater net slip movements -- I should say actually just the Sylmar. The Tujunga and Lakeview segments, which are most like the Verona, actually show substantially less of a net slip.

But for the moment, I will stick to just a discussion of the Sylmar. But I would like to be sure that it is kept in the context of the entire fault system.

Q Within the context of what you were just talking about, Dr. Justus, do you recall the discussion earlier today with Dr. Slemmons regarding the possibility of strike slip components on the Verona thrust fault zone?

A Could you be more specific? Which statement or statements were you referring to?

Q Well, in Dr. Slemmons' Appendix E we were discussing various alternative tectonic concepts for the Verona fault zone. One of them involved strike slip movement.

A Yes.

Q Okay. Would this tectonic concept be comparable to the description which you have given us that the San Fernando thrust faulting event involved strike slip components on the Sylmar segment?

A No, I don't believe so.

Q Could you explain why you do not believe that?

A In the case of San Fernando, we are discussing one fault system that has four parts. And these parts have moved during the same event. In discussions with Dr. Slemmons previously there was discussion of strike slip faults that may or may not be related to the Verona fault. In any case, I believe we were talking about or he was talking about strike slip faults that were -- that have not been mapped as the Verona fault or part of the Verona fault system.

Q Okay.

WITNESS JACKSON: Mr. Barlow, I would like to add a comment. Since I did go in the trenches at GETR and Dr. Justus did not, we have postulated the possibility of some oblique component of movement on the shears at GETR. But I do not recall seeing any evidence or strong evidence that there was an oblique component of slip. In fact, I think all of the evidence that I am aware of indicated a total dip slip movement. Now for the Board's knowledge, this is very complicated in terms of talking about net slip, dip slip, vertical and the like. What we are calculating is the vector-type approach

to the amount of offset that is taking place on the surface.

If I could describe, we have a figure that I think would be helpful in terms of the total proceeding that we could provide, if it would be desirable. It would help me explain the movement. Is that acceptable?

JUDGE GROSSMAN: That's fine.

WITNESS JACKSON: It's only a descriptive chart. It has no testimony in it.

BY MR. BARLOW:

Q Could you describe it?

A (Witness Jackson) It's a block diagram which indicates the terms that we have been using all day in terms of net slip, dip slip, break. In fact, it's got too many terms on it. But I think it would be helpful.

MR. BARLOW: I have no objection.

JUDGE GROSSMAN: Well, we are generally familiar with those terms and I don't want to take thetime because I think there may be some questions directed to Dr. Slemmons, who is not going to be here.

WITNESS JACKSON: It can be entered in at any time, if so desired. All I wanted to indicate, that there is an oblique slip component in San Fernando; there is no strong evidence for an oblique slip component at GETR.

JUDGE GROSSMAN: Fine. My fellow Board members do want the thing now, if it won't take too much time.

WITNESS JUSTUS: I will have to make one correction. I believe that Dr. Jackson may have forgotten some map evidence for oblique slip on the Verona system. That does appear in Appendix B of the May SER. I believe they are shown on Figure 1, the summary of them is shown in Figure 1.

I think, if I can elaborate a little bit on why we think it is important when making the comparison of the Verona fault and the San Fernando fault to specify the nature of movement -- and perhaps it would also be useful to point out or to remind the Board that the San Fernando fault system is an analogous system. It was never meant to be a model, a one for one model, you might say. The details of the San Fernando fault system are different from what we know of details of the Verona system. Our intent in invoking the San Fernando fault analogy was to try to employ the best available comparison known at the time of a thrust fault system that had suffered earthquake movement and reverse oblique slip, for which we had thought the Verona would have similar characteristics, that is to say, capable of an earthquake and of reverse oblique slip movement.

In detail, there are segments on the San Fernando that are not as comparable as other segments on the San Fernando. We will continue to run into, I think, a bit of a problem when we generalize for the whole San Fernando and compare it now to the Verona. There may be questions for Dr. Slemmons, though.

WITNESS JACKSON: It's probably not my position to comment but Dr. Slemmons will not be here tomorrow and I assume that there's --

MR. SWANSON: Just so there isn't any misunderstanding, I had asked the parties to accomodate us because he would not appear again with the panel.

He will be appearing with the probability panel and it's pretty likely that we are not going to finish up with this panel today. I think in the general context, though, it is becomming quite obvious that there's an inter-disciplinary approach to this problem and many times it's very helpful to have the various members there to respond to a question and that's why it was helpful to have Dr. Slemmons on but he will be available later and I just wanted to clarify that.

MR. BARLOW: Your Honor, may I ask a question of Dr. Slemmons?

JUDGE GROSSMAN: Yes.

BY MR. BARLOW:

Q Dr. Slemmons, are you familiar with the two reports by Robert V. Sharpe dated 1975 and 1981?

A (Witness Slemmons) Well yes, I've seen them both. I haven't reviewed them carefully.

Q Dr. Slemmons on your testimony on page 3 as we discussed earlier today, do you conclude, "the worldwide

data and the San Fernando earthquake data suggest that the offsets could be as much as 2 to 2.5 meters."

Do you have any reason to believe in the proposal to exclude the data from the Sylmar segment of the San Fernando earthquake data set?

A Would you clarify the use of the word exclude the data from the Sylmar segment?

Q Okay, maybe I'll rephrase the question.

Would you have any reason to exclude the Sylmar segment data from any analysis of the data set from the San Fernando earthquake in your research?

A No, I wouldnot exclude it and I'll point out the reasons why and Dr. Herd may wish to elaborate on that further.

I visited this earthquake zone in the spring following the earthquake and indeed there are the differences that Dr. Justus pointed out in that the section along the Tujunga section is a combined reverse and oblique type of slip movement. We have observed almost two meters of horizontal components in combination with about two meters of uplift. The section at Sylmar is more strike-slip but I believe from my impressions and Dr. Herd has a map he prepared and a chart that will verify the details of it.

It seemed that trench H had more of an oblique

slip component, than B-1, B-3 and B-2 and so, I believe there is a somewhat analogous situation although the angle of plunge is much steeper on the hills at Vallecitos.

Dr. Herd, did you want to comment further?

(Witness Herd) All I would do is make reference-- I believe the figure your're referring to is the figure 1 in our 1980 report which has annotated onto the map of thrust faults the attitudes of slickensides that were documented by General Electric's consultants.

WITNESS JUSTUS: Incidentally, I think it was implied that in my analysis of the San Fernando fault system, my way of it being an analog to the Verona, that I may have excised the Sylmar segment or values of net slip made on the Sylmar segment from my analysis and that is not so.

I was referring to the different character of the Sylmar compared to the others but in my analysis I included the data from the Sylmar, nevertheless.

I suppose I should comment further, and perhaps I'm jumping the gun but my assessment of Dr. Sharpe's data which were looked at subsequent to our SER have not changed our opinion of the significance or the conservatism of the San Fernando analogy for the Verona in that the conclusions of the characteristic net slips of the San Fernando system based on another set of data which we

did refer to in the SER, that of Barrows and others, has not changed by incorporating Sharpe's data. Furthermore, after we investigated Sharpe's data, we thought that we might make another search and make sure that there weren't any other values hidden in closets or drawers or whatever and indeed, we incorporated a total of four sets of data including Kam, Kam's data which were referred to in the Livermore portion of this hearing and another set which I can refer to more specifically if need be.

Altogether, we have not changed our opinion as stated in the SER about one meter of net slip being the mean or characteristic net slip for the San Fernando fault system.

BY MR. BARLOW:

Q Dr. Slemmons, could you look at the staff SER of 1980, page 5?

(Pause)

Could you read section 5 out loud please?

A (Witness Slemmons) One meter of reverse oblique net slip along the fault plane which could vary in depth from about 10 to 45 degrees provides an appropriate description of surface displacement which could occur on a Verona fault strand splay beneath the reactor during a single event.

Q Because number 5 has a star by it, could you also

read the --

A The star indicates, denotes positions that have been modified since September, 1979, report, letter from H.R. Denton U.S.N.R.C. to R. W. Darmitzel, G.E.

Q Dr. Slemmons, are you aware of the change in position by the NRC staff between the 1979 letter from Mr. Denton of the NRC and the position stated here in the 1980 SER?

A Are you referring to the change in dip? Or the change in the amount of displacement?

Q The change in the amount of displacement.

A Yes, I am aware of that.

Q Could you describe the change in estimates of amount of displacement in terms of net slip?

A I can't describe the basis of their making those changes were, but I can give you my opinion of the basis of what I feel is a validity for such change.

Q Okay if you would give us your opinion for that please?

A I think that first of all, there was the use of the San Fernando analog which was very heavily used in the interplay particulary in the earlier stages.

The San Fernando event is I think if you were to scale it a much larger kind of event that can occur. First of all, it's part of the Santa Susanna, Santa Mater or

Sierra Madre fault zone which is over 100 kilometers in length in contrast to a very short length, 8 or 12 kilometers for the Verona. The amount of base offset is much greater. It's segmented at both ends by geological structure but it continues in a much longer trend. The rocks of similar age and they include continental sediments are uplifted in the hills behind the hills in San Fernando to a much greater height than we see for the Vallecitos Hills.

The down thrown block has over 10,000 feet of tertiary sediments in contrast to the projected much shallower depth to bedrock on the south side of the Verona.

The rate of slip or the strain rate for the San Fernando area is a much higher rate than has been determined from several sources for the Verona fault zone. The slip-rate for the Verona fault zone is approximately three milimeters per year and the workers from the U.S.G.S and Dr. Herd or Dr. Brabb can give you figures and have a-almost in order of magnitude higher strain rate across the fault zone.

The topographic expression of the Verona fault zone is very subdued. It's a very subtle feature that does show up on aerial photographs but it -- even with low sun angle illumination is not a harsh and conspicuous feature in contrast with San Fernando fault zone which is

extremely marked and conspicuous. The amount of movement during the most recent slip has been verified from numerous trenches along the Verona fault zone as from about 2 feet to 3 feet, in contrast to 2.5 meters at several places along the San Fernando fault zone.

If you were to apply my fault length displacement data, to the San Fernando zone, in order to project what you might expect from the San Fernando, you come up with an event that is approximately 7 magnitude, just under 7 magnitude and as I've indicated earlier, I feel the same kinds of applications of data to the Verona fault zone would be closer to a 6.5 so for a number of reasons, I feel that the use of that analog is very conservative and therefore, I feel that scaling down from the 2.5 meter value from the earlier document has some validity and I think nothing is more conclusive and site specific that could tie in better to any seismic cycles and mechanisms than the physical observation at the fault itself and repeated measurements show that the last offset or possibly more than one offset accumulated has been between 2 and 3 feet and so there's a much greater likelihood of a repetition of that sort of event than something scaled much higher, so for these reasons, I concur that the staff decision is a reasonable one and I will support their number 5 statement.

I might just interject a conjecture and that has to do with trench T-1. I did not examine that trench. It had been closed prior to my coming on board. That is in location to the east of the B-1, B-3 shear and the B-2 shear and could be in a zone which is at the sway -- it may be on the convergence of those two and it's the photographs that we saw in the interpretations by Dr. Herd earlier today, suggest to me at least the possibility that at least two of the events are shown by that shear and one may be a strand that continues on to become B-1 and B-3 and another splay active at a different time could be the one to go onto B-2.

So the apparent and not yet resolved status of T-1 could be featured and could fall within the bounds of multiple events -- none of which would necessarily exceed a three foot displacement.

Q Dr. Slemmons, if it were determined that offsets in trench T-1 were either 5, 6 or 7 feet, would that influence you to change your estimates of the potential offsets for the Verona fault zones?

A I would want to consider that issue carefully before making a judgment.

Q You said the San Fernando earthquake was a magnitude 6.4, is that correct?

A That's correct.

Q It was on a thrust fault zone with strike slip component?

A That's correct.

Q And, in your testimony, you say that worldwide data in the San Fernando earthquake data suggest that offsets could be as much as 2 to 2.5 meters? Did you mean there that offsets at the GETR site on the Verona thrust fault zone could be as much as 2 to 2.5 meters?

A No, that would be in the zone, not onto the plant.

Would you repeat that question again?

Q Did you mean by the statement in your testimony that offsets at the GETR site are not directly beneath the reactor, but within the Verona thrust fault zone, could be as much as 2 to 2.5 meters?

A That statement was based on an extrapolation of worldwide data to anyone of the three strands, the B-1, B-3, B-2 or H and that is taking worldwide data and extrapolating to the faults at the site.

Q Is an important component of your worldwide data the San Fernando earthquake data in which there are several instances which you said there were 2.5 meter offsets?

A It is one of approximately 16 to 18 data points depending on the particular analysis displacement or length

and so it's weighted in with the others. If you plot that particular event relative to the worldwide data, the displacement there was very large. The 2-½ meters was very high for the 6.4 magnitude and the rupture length was fairly close but a little long.

(Witness Jackson) I'd like to add one brief comment.

The net slip number of 2-½ meters that Mr. Barlow has been using is a calculated net slip across a zone of faulting. It's not a measured amount of net slip along a single fault splay.

Q How long was the Sylmar segment that this occurred on?

A (Witness Jackson) I'd have to check and see where maximum observed net slip was. I don't know. Do you know which one it was for the record?

Q Do you know the width of the Sylmar segment?

A The width of the Sylmar segment?

The number 2-½ that has been referred to is a calculated net slip which was measured, I believe in the Tujunga segment.

(Witness Justus) No.

Give me one moment.

Q Perhaps while Dr. Justus is looking that up, this is a time when I could ask Dr. Slemmons while he is

available, Dr. Slemmons, you mentioned your opinion that there are several locations along the San Fernando fault in which there were as much as 2-½ meters of offsets.

Do you recall the number of locations and the names of the locations?

A (Witness Slemmons) No, those can be verified by looking at the tabulations of Bob Sharpe, R. Sharpe.

In addition, I believe I saw in the field a similar measurement area where a measurement had been made in Tujunga canyon on the upper scarp or fault zone of the two main strands.

The size value appeared in more than one measurement point but as I recall, tabulation of 50 or so data points gave that extreme value for only about two or three of the data points and the numbers dropped off very rapidly from that. They were sort of spikes on a series of variation along the fault trace.

Q Are you saying that the early characterization of several locations is two or three locations or can you recall more?

A That is what I recall, but I would defer to checking the actual documents.

Q There may be more than two or three?

JUDGE GROSSMAN: I'm sorry, but did the witness answer that last question? There may be more than 2 or 3?

BY MR. BARLOW:

Q Do you agree that there may be more?

A (Witness Slemmons) I don't think there are more than two or three spots. Several of them occurred on streets as I recall on the Sylmar segment and these had more of a strike slip component and they were relatively near -- well, they were in an area of about one kilometer or two kilometers west of the hills, I think near Hubbard Street or Hubbard Avenue. I don't recall the name properly and one other at a point near a Middle Ranch on the Osborne Road going up Little Tujunga Canyon.

(Witness Jackson) Mr. Barlow, I'm having difficulty answering the previous question I've been looking up.

The reason is, in your question, you kept referring to 2-½ meters and I assumed that that 2-½ came from that discussion that I had provided, that we had provided in our SER which was a discussion of a 1971 report on the San Fernando earthquake that indicated that the 2-½ or 2.4 meters of net slip had occurred across a zone 200 meters wide.

Q 200 meters wide?

A 200 meters wide. It was a calculated net slip which means that you did not go out with a ruler in

the field and measure it. Now, we could --

(Witness Slemmons) I think this is almost essential or necessary when you have an area that's been urbanized and the slabbing of pavement and curbstone and buildings across the fault zone, make the surface expression much more spread out.

MR. SWANSON: Before we get into anymore questions, I would like to again mention the stipulation does cover this matter. Item H contains a statement concerning the San Fernando event and vertical displacement for this location is distributed across a zone of breakage 200 meters wide which is complicated by a zone of shearing and thrusting and a zone of extension.

WITNESS BRABB: Your Honor, we have a problem in this matter as well, in that we do not agree with the conclusions of the staff.

I'm not sure of the mechanisms for bringing this information out, but in view of the short time to question Dr. Slemmons, I would merely like to note at this stage that we have an opportunity to explain our disagreement with this figure.

JUDGE GROSSMAN: Are you objecting to that, Mr. Swanson, that he be allowed to explain his disagreement with the staff?

MR. SWANSON: My concern is that Mr. Barlow is

asking about the very matter that they had stipulated to.

(Pause)

JUDGE GROSSMAN: I will have to consult with my colleagues on this but when the question comes up as to whether people on a professional panel have to sit there and accept matters that they really don't agree with, I personally would be inclined to allow them some leeway in explaining their professional positions.

MR. SWANSON: I understand that. I think the record is better served by allowing Dr. Brabb to indicate that, what he feels he needs to on that point.

MR. EDGAR: I'm confused. I wanted to express my support for Mr. Swanson's position to the extent that we have questionning going to the questions of the 2.4 meters observed at San Fernando across a 200 meter width. I'm not sure what it's adding to the record. That's stipulated.

Are we overlapping two things?

MR. SWANSON: May I ask just one thing, Mr. Chairman? It's getting about 5 o'clock anyways. I think maybe a discussion is necessary to find out if we are talking about 2 different things. My objection goes to Mr. Barlow's questionning of a matter which is

covered under stipulation. I would promise to the Board however, that if our discussions indicate that there is a -- this again among our professional panel that we will indicate that to the Board tomorrow morning.

JUDGE GROSSMAN: Yes, but to the extent that someone on the panel states a position and it is in response to a question by Mr. Barlow and then someone else on the panel has a different professional opinion, I think he ought to be entitled to speak to that.

For one thing, while sitting on a panel he is associated with those answers under the ground rules that we've laid down. So I don't want to have Dr. Brabb to have to sit there and associate himself --

MR. SWANSON: No, I fully agree.

JUDGE GROSSMAN: Of an opinion that he doesn't subscribe to.

Before we leave --

JUDGE FOREMAN: I just have one quick question of Dr. Slemmon.

EXAMINATION

BY JUDGE FOREMAN:

Q You had been asked a few moments ago whether you had changed your mind. I believe this was about the conservative of the one meter displacement. In the face of a possible 5 to 7 fault or 5 to 7 foot displacement,

in the T trench as opposed to two. You said you'd like to think about it and I'm asking if indeed you would think about it when you come back next time.

A (Witness Slemmons) Okay, I've thought about it but I'll think about it another day and I'll think about it.

JUDGE FOREMAN: Indeed, I would like to hear what you have to say.

WITNESS JACKSON: Mr. Chairman, since I initiated the discussion of the offset, I was only trying -- Mr. Barlow is the one who kept using 2-½ meters. I think most of us made an assumption that that had come from a generally used term of maximum offset that has been referred to many times in the San Fernando records.

I think to help us prepare for obvious questions tomorrow on this topic that it's important that we know on this panel what that reference is being made to. Again, it's a problem that we are making suppositions. We are aware of a great deal of data on San Fernando and that 2-½ meters could come from a number of things. I would request a definition of that if I could.

MR. BARLOW: I believe, Dr. Jackson, that Dr. Justus and I discussed this in the context of Robert Sharpe's report where it says 2 to 2-½ meters of offset.

WITNESS JACKSON: The open file report? Okay, I'm sorry.

JUDGE GROSSMAN: Okay, I take it that no one has any pressing business now that we can --

MR. EDGAR: I'd like to approach the bench if I could if we're through.

JUDGE GROSSMAN: Did Mr. Cady have something?

MR. CADY: No, I was just going to suggest that if we are going to terminate this this afternoon's examination that we begin the session tomorrow at 9 o'clock instead of 9:30 so that we can get more business taken care of in the mornings, because we aren't getting very much in between 9:30 and 12 o'clock and towards the end of the afternoon people are beginning to lose some form of energy.

JUDGE GROSSMAN: Can the technical examiner manage to be here at 9 o'clock?

MR. CADY: I will try to bring him personally Your Honor.

JUDGE GROSSMAN: Is there any objection to this starting at 9:00?

(No Response)

Okay, we'll start at 9:00 and we'll have a bench conference here after we adjourn. We are adjourned.

(Thereupon, at 5:09 p.m., the hearing was recessed to reconvene at 9:00 a.m. the following day.)

This is to certify that the attached proceedings before the

US NUCLEAR REGULATORY COMMISSION

in the matter of: GENERAL ELECTRIC COMPANY (VALLECITOS NUCLEAR CENTER)

Date of Proceeding: June 3, 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.

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