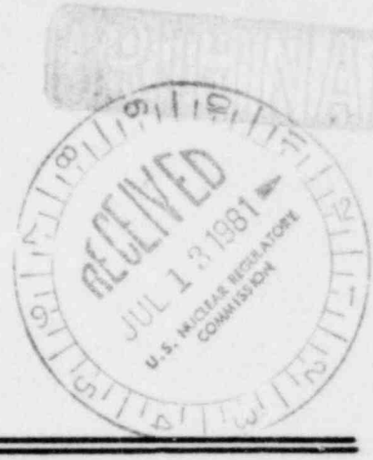
NUCLEAR REGULATORY COMMISSION



In the Matter of:

SOUTHERN CALIFORNIA EDISON COMPANY, ET AL.	)	
	)	DOCKET NO's
(San Onofre Nuclear Generating Station, Units 2 and 3)	)	50-361/362-OL

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

In the matter of:	:	
SOUTHERN CALIFORNIA EDISON COMPANY, ET AL.	:	Docket Nos. 50-361 OL
(San Onofre Nuclear Generation Station, Units 2 and 3)	:	50-362 OL

Stardust Room
Stardust Hotel & Country Club
950 Hotel Circle,
San Diego
California

Tuesday, June 23, 1981

Evidentiary hearing in the above-entitled matter was reconvened, pursuant to adjournment, at 9:00 a.m.

BEFOR:

JAMES L. KELLEY, Esq., Chairman,
Atomic Safety and Licensing Board

DR. CADET H. HAND, JR., Member

MRS. ELIZABETH B. JOHNSON, Member

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On behalf of the Regulatory Staff:

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Deputy Assistant Chief Hearing Counsel
Office of Executive legal Director,
Washington, D.C.

C O N T E N T S

EXHIBITS

PROCEEDINGS

JUDGE KELLEY: We can go on the record now.

Good morning. This is the second day of our seismic hearings on the San Onofre operating license. We concluded yesterday with cross examination of Witness Smith. I would like to go over a couple of things. It will be for you, Mr. Pigott, I think to pick up and if you wish to have redirect I think maybe we should talk about that for a minute, too. But I have a couple of items that I want to go over and then counsel may have a chance to raise things also before we get into the testimony of today.

The first matter was a matter that was argued by counsel orally yesterday and also argued in memoranda of counsel. This goes to the admissibility into evidence of various documents, the two documents offered yesterday by the Applicants, the FSAR, which is the multi-volume document behind us, and also the Applicants' I believe three-volume environmental report. Those documents were offered by Mr. Pigott both for what they are and purport to be and as substantive evidence of the matters addressed in them.

They were objected to by Mr. Wharton for the Carstens Intervention. We heard all part s. The Board's ruling is that both of these exhibits, 1 and 2 they are numbered, are to be admitted both to show that they have been compiled and as substantive evidence of the matters

treated. They obviously are hearsay. On the other hand, they have what is called in hearsay literature various earmarks of trustworthiness, not the least of which is that they are filed under an obligation on the part of the person preparing it to tell the truth. Certainly the circumstances surrounding their reliability far exceeds many of the historic exceptions to the hearsay rule.

The fact that we don't have in the hearing sponsors for the various portions of these voluminous documents does not, in the Board's view, go to their admissibility but rather to the weight that might be given to them.

(The documents marked for identification as Applicants' Exhibits 1 and 2 were admitted into evidence.)

JUDGE KELLEY: Having said that and having admitted these documents into evidence, I would make one cautionary note. This is an on-the-record hearing concerning certain contested issues. We do not anticipate that we would be resolving any major issues by reference to unsponsored portions of these documents. They are there for what they are worth. They are there for this Board's general information. They are there for Appeal Board review. They are there for the Commission to review, for whatever the Commission wants to do with them. But we have gone to some length to refine

contentions and there has been testimony prepared and that is the crux of this hearing and we expect to hear by witnesses and have cross examination on all of the major issues.

Indeed, for example, if we got to the findings of fact and conclusions of law stage and the Applicants found it necessary to rely very heavily on the FSAR, I'm not saying we would do it, but we might consider reopening to hear testimony on those portions that developed into such crucial pieces of evidence.

But with those general remarks, these documents are admitted.

I take it, Mr. Chandler, that when your time comes -- the original stipulation covers the SER and the ACRS letter and some other things -- that will be for you to introduce when that time arrives.

MR. CHANDLER: Yes, sir. We will do it at that time unless the other parties wish us to do it now.

MR. WHARTON: I would like to have the SER admitted into evidence since the basic issue has been decided, if we could have the SER into evidence right now it would solve some evidentiary --

JUDGE KELLEY: It might be simpler just to go over that ground. The original stipulation included which documents, if I may ask? I don't have it immediately in front of me. It was attached to Mr. Pigott's memo on this legal issue.

MR. WHARTON: I have a copy here.

JUDGE KELLEY: Fine. Thank you. I am looking at the Intervenor's memorandum on this subject dated July 5. Attached thereto is the proposed stipulation put forward by the Applicants at the April 29 hearing. It lists the following documents: the FSAR, which is now Exhibit 1 which has been admitted; the environmental report, which is now Exhibit 2 which has also been admitted. The next item is the application of Southern California Edison.

MR. PIGOTT: Mr. Chairman, that was combined in No. 1. No 1 is the application and the FSAR.

JUDGE KELLEY: The FSAR is basically the application.

MR. PIGOTT: Yes, it is.

JUDGE KELLEY: I understand. So that is in. The SER is the Staff's document and I believe, Mr. Wharton, you expressed a desire to have that admitted at this point. Is there any objection to the admission of the Staff's SER?

MR. WHARTON: None.

JUDGE KELLEY: That would include supplements, Supplements 1 and 2 at this point?

MR. CHANDLER: Well, it is Supplements 1 and 2. What I would propose, Mr. Chairman, if the parties are willing to stipulate to the receipt, is I would at this point offer the Staff's safety evaluation as Staff Exhibit No. 1. That is NUREG 0712, Supplement No. 1 thereto as Staff Exhibit No. 2,

Supplement No. 2 thereto as Staff Exhibit No. 3, and the final environmental statement as Staff Exhibit No. 4. Now Supplement No. 1 contains the initial ACRS letter.

JUDGE KELLEY: That stands in my mind on a somewhat different footing, but go ahead.

MR. CHANDLER: All right. And Supplement No. 2 contains the second ACRS letter. The first addresses the geology - seismology portions of the Staff's evaluation; the second addresses the main document, that is, all other matters not addressed by geology-seismology.

JUDGE KELLEY: Does that by chance have anything to do with emergency planning?

MR. CHANDLER: To some extent, the second letter does raise questions about seismic qualification of certain of the emergency planning related equipment. Copies have been provided to the Board and parties on that.

JUDGE KELLEY: Right. My concern, which may be obvious to all counsel, is that at least historically there has been a differentiation. Let me back up two steps. The documents you are offering, with the exception of the ACRS letter, which I want to treat separately, you are offering them to comply with the requirement that you compile it, as it were, and also as evidence of the matters addressed therein, is that correct?

MR. CHANDLER: That is correct. And obviously

with respect to the ACRS letter it would only be the former.

JUDGE KELLEY: Right. That is the point that I was coming to. So what the Board, subject to any comments that the other two parties may have, would propose to do would be to admit the exhibits that the Staff is offering, except that the ACRS letter would only be admitted as proof of the fact that there is an ACRS letter, as it were. It is not offered as substantive evidence of the truth of the matters discussed in the letter. This is an old distinction which I think has been followed in the AEC and the NRC for many years, mostly because (1) these documents are so sort of conclusory and (2) the members of the ACRS are not subject to subpena. And that is the reason for the distinction.

With those understandings, Mr. Wharton, any comment?

MR. WHARTON: I have no objection.

JUDGE KELLEY: Mr. Pigott?

MR. PIGOTT: I stipulate and have no comments.

MR. CHANDLER: Mr. Chairman, it may facilitate matters then if I identified as Staff Exhibit 2-(a) the ACRS letter dated February 10, 1981, which is bound into Staff Exhibit No. 1, that is, Supplement No. 1 to the SER, as Appendix C. I would also offer as Staff Exhibit No. 3(a) the ACRS letter dated March 17, 1981, which is bound into Staff Exhibit No. 3, Supplement No. 2 to the SER, as

Appendix D.

JUDGE KELLEY: Without objection it is so ordered.

(The documents were marked for identification and received into evidence as Staff Exhibits 1, 2, 2(a), 3, 3(a), and 4.)

MR. CHANDLER: I will provide the Reporter with the requisite number of copies.

JUDGE KELLEY: Thank you.

Just one comment on that entry ruling. If appropriate at a later time, let's say in an initial decision, we might give somewhat more lengthy, elaborate explanations of legal rulings, trying to give you here the basic reason for why we are going one way or the other. We may obviously expand on this later on.

One other point to just spend a minute on, hopefully. We want to acknowledge receipt of your memoranda on res judicata and collateral estoppal and other doctrines. I found them all very helpful. It puts me in a much better position to consider these kinds of problems as they arise in the course of the hearing. I don't think we should pause now for an academic debate on these doctrines. I might make a couple of observations that would be appropriate here and then we can move on into the testimony again.

I would say thatone of the topics for discussion

was whether there is a requirement of an identity of parties. If that were so, then those doctrines wouldn't have anything to do with this proceeding, as I understand it, at least as far as any contentions may arise between the Intervenors and the Applicants in particular, perhaps the Staff.

The historic doctrine did require identity of parties. That has been watered down a little bit over the years to in part fit the realities of administrative practice. But just to give an example, it just seems to me that, take this case, if in this case back in 1973 there had been full and fair litigation on some geologic issues at the instance of the Sierra Club, let's say, the Union of Concerned Scientists or whoever you may want to think of as an example. To come back in 1981 at the OL stage and do exactly the same issue on exactly the same evidence with Friends of the Earth or another Intervenor organization, seems to me to be very unproductive and unfair to the applicant and unnecessary and therefore not something that we would be disposed to do.

So I don't think as far as the Board is concerned we need spend a lot of time debating the identity of parties doctrine. I understand that the Carstens Intervenors -- let me make sure I am right about that. Mr. Carstens, were you active in the '73 CP proceeding?

MR. CARSTENS: Yes, I was from the very beginning. For the hearings before the Coastal Commission and subsequent

hearings. Yes, from the very beginning.

MR. WHARTON: Mr. Chairman, I think I may need to talk to Mr. Carstens. The record reflects that he was not any part of that proceeding. I need to talk to him about that. I think he is talking and thinking about the Coastal Commission and litigation involving the Coastal Commission.

JUDGE KELLEY: I am raising it to establish my understanding. The Friends of the Earth, as I understand it, were not in that litigation. I don't know about the other individuals. It is a sort of a by-the-way point because what I just said was it doesn't matter, assuming that the issue was fully litigated, at least in my view.

Mr. Pigott, do you want to say something?

MR PIGOTT: I was going to say I do not believe that Mr. Carstens was a party to the construction permit proceedings before the Nuclear Regulatory Commission; however, I think some of the named individuals who are in that particular Intervenor group, the Vaughn Hadens, Donald May, and a couple of other names seem like old friends from that proceeding. Now we will check the record to be sure precisely who was an admitted party to that proceeding.

JUDGE KELLEY: Okay. As I say, in my view it doesn't matter. But I thought that ought to get said; otherwise, if my view were the opposite, the situation would be very different.

Just a couple of other observations. I think we need in approaching this area some flexibility. One might contend, for example -- a favorite example seems to be the Cristianitos fault and its capability or not. But even assuming that that got litigated in '73, later on you can say well, it is connected to something else or it is not. So it is very hard to put these in totally airtight boxes. That is one point.

I would say to the applicants that if you think you have an objection based on res judicata or some similar doctrine, in view of the fact that so many of these issues are tied up one to the other, it would not be my view that you would waive an objection if you allow some testimony to get into something that arguably could be objected to for this reason. Just in the interests of manageability it would not prejudice your claim at a later point if you foreswore an objection at the first conceivable point you might want to make one.

On the other hand, it seems to me that related to that, and again, going back to the Cristianitos fault, it would be possible for you to waive an objection along res judicata lines by getting into a matter in your own testimony. You know, one might argue that the status of that fault was thoroughly thrashed out in '73. But I notice just in reading the testimony for today, Dr. Ehlig's testimony talks for some

pages about when the Cristianitos fault moved or didn't move. So that if you open up a topic then it will stand as opened up and the other parties would be entitled to get into it to a reasonable degree, notwithstanding the treatment of the subject eight years ago.

MR. PIGOTT: If I might respond, Mr. Chairman. We have an issue which calls for a discussion of the geologic characteristics of the OZD. A full discussion of those characteristics we believe necessarily requires a full discussion of the overall geologic setting. The Cristianitos is incapable, but on the other hand, it is very close to the site and cannot be ignored in a proper discussion of the geology of the area. So you will see reference in our testimony to the Cristianitos and some geologic description of it.

What I would object to and consider to be within the collateral estoppal res judicata rules is the requirement that there be the sufficiency of evidence submitted to reconfirm its lack of capability. I would expect there will be some discussion of the Cristianitos and its history, but in that setting and its relationship to the OZD, which is somewhat considerably different from reopening and reinvestigating the precise question of its capabilities within the meaning of the regulations.

JUDGE KELLEY: I think I follow you making that distinction. In the course of cross examination I apprehend

to be possibly a very difficult thing to do.

MR. PIGOTT: It may be. And I guess I am worried or concerned about when it gets time for decision does this Board have tomake another finding on capability of the Cristianitos. That is the level that I don't want it to rise to because that makes it a new issue. It is one thing for it to be discussed, it is one thing to be ruled into the context. It is another thing for this Board to think that this proceeding has to come up with a conclusion on its capability, absent some kind of new evidence which reopens that.

JUDGE KELLEY: In any case, such evidence would have to be within the contentions that are admitted. Admittedly they are somewhat broad in some respects.

Well, I think that is enough said for the moment on the capability of the Cristianitos fault. Let me ask you a procedural question, Mr. Pigott, before you get back into your case. We hadn't discussed this before and perhaps it doesn't require much discussion. But would it be your proposal as a routine matter that you would present your witness and your witness would be cross examined by both parties, and then you would come back with redirect.

MR. PIGOTT: That would be my anticipation. I may wait, for instance, if we had someone finish in the middle of a day rather than, as conveniently happened yesterday, at the end of a day, to allow the over the night to consider

redirect simply because some of these matters are best examined with a record in front of us and not simply ir the corridor. Other than that, though, I would anticipate the redirect to come either immediately after or shortly after the cross examination of the other two parties.

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JUDGE KELLEY: Mr. Wharton?

MR. WHARTON: That would be agreeable to us, that would be -- redirect is immediately afterwards.

MR. CHANDLER: That is fine with us, Mr. Chairman.

JUDGE KELLEY: All right, well, do you want to proceed?

MR. CHANDLER: Mr. Chairman, before we resume, one final preliminary housekeeping kind of matter. The Staff did yesterday file its views with respect to consideration of earthquakes and emergency planning and EPZ determinations. The document was I believe express mailed to me last night, and I would anticipate receiving it today, and I will make service of that document as soon as I have it available.

JUDGE KELLEY: Thank you. Go ahead.

MR. CHANDLER: Yeah, I would -- I hope that procedure is acceptable in light of the fact that the Board is in hotels, the Staff is in hotels, as is the Applicant, and therefore mailing to the normal office address will I think just encounter more delay, so we would propose to the extent any filings are made from Washington, that they would be sent to me and I will make service upon receipt.

JUDGE KELLEY: That seems sensible. Any objections, Mr. Pigott?

MR. PIGOTT: No.

JUDGE KELLEY: Fine, thank you. Mr. Pigott?

MR. PIGOTT: Very good. We do not have any redirect for Mr. Smith. I would ask that he be temporarily excused. He will be recalled for testimony on a subsequent issue dealing with subsequent geologic discoveries.

JUDGE KELLEY: Very well.

MR. PIGOTT: Before calling our next witness, there was a question on examination having to do with depths of offshore borings. Over the evening we have gathered together what I understand to be the best available information we have at this time, and I think it is accurate, but obviously as Mr. Chandler points out, being away from home base, you can't be a thousand percent sure of these things.

Let me read this information into the record. In 1970, Marine Advisors did four borings of a jet probe type to a depth of 10 to 20 feet. Also in 1970, General Oceanographics did 22 borings by a dart core methodology, to a depth of approximately 6 feet.

In 1974, Woodberg McNeill (ph) Associates did 7 borings of a vibracore type, going from three to seven feet. In 1978, Woodward Clyde Consultants did 10 borings of a rotary wash type, going to levels, I think nine of them went to about 20 feet plus, and one went to 310 feet.

In 1980, Woodward Clyde Consultants did six borings of a vibracore type, ranging in depth from 25 to 41 feet.

And with that, I would call our Applicants' next witness, Dr. Perry L. Ehlig, and could we have the blinds closed again? I think they are distracting to the witness.

JUDGE KELLEY: Yes.

Whereupon,

PERRY L. EHLIG

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

DIRECT EXAMINATION

BY MR. PIGOTT:

Q Would you state your full name?

A Perry L. Ehlig.

Q And your current address?

A My home address is 1560 Via Del Rey, South Pasadena.

Q And do you have before you a document entitled "The Testimony of Dr. Perry L. Ehlig?"

A Yes, I do.

Q And that consists of some 33 pages of text plus a list of publications, and figures denoted PLE-A through PLE-P, is that correct?

A Yes, it is.

Q Do you have any corrections to make to either the figures or the text?

A I have one correction in the text on page nine.

The first three lines, after the first word in line 1, I would like to have the material struck, starting with the period. All of the second line struck, and the first word of the third line. This is repetitious material from the previous sentence.

MR. WHARTON: Would you repeat that again, please, I wasn't able to get that.

THE WITNESS: All right, on line 1, I will read the part that I would like struck. "The juxtaposition is important because the two formed in very different environments." That is a repeat, essentially, of what was said in the previous sentence.

BY MR. PIGOTT:

Q And so, Dr. Ehlig, the testimony would now read starting at the bottom of page eight of your preparation, the juxtaposition of the schist against Peninsular Range basement is significant because the two formed in very different environments, and were probably brought together by lateral faulting. Is that correct?

A That is correct.

Q Are there any other corrections?

A No, there are not.

Q And you are not sponsoring any Exhibits at this point, are you?

A No, I am not.

Q Okay. If you were asked the questions contained in that document, "Testimony of Perry L. Ehlig," this morning, would your answers be the same?

A Yes.

Q And do you adopt that document, including the figures, as your testimony in this proceeding?

A I do.

MR. PIGOTT: I would ask, Mr. Chairman, that the testimony be received as evidence in this proceeding.

JUDGE KELLEY: So ordered.

MR. WHARTON: Mr. Chairman?

JUDGE KELLEY: Excuse me, Mr. Wharton?

MR. WHARTON: I didn't have an opportunity -- I am not objecting to Mr. -- Dr. Ehlig's qualifications, but again, we would like the opportunity of voir dire, of cross-examination, as to qualifications, bias, and other things for purpose of the weight of the evidence, but we don't have any --

JUDGE KELLEY: Yeah, can we establish the procedure that Counsel wish to follow here, and I think one can go various ways, and what we did yesterday was the testimony was admitted into evidence, and then you began with an examination along those lines, and that is satisfactory with me if it is satisfactory with the Counsel.

MR. WHARTON: There is one area that I would like

to get into, if I can find my notes on that, is in some of the testimony, we would have a motion to strike portions of the testimony. Referring specifically in Dr. Ehlig's testimony to references to the hypothesized offshore zone of deformation where the issue is revolving around the offshore zone of deformation, and this Board has previously ruled that it is not hypothesized for purposes of this hearing.

I would move that starting with page one, line 13, there is a mention of the hypothesized offshore zone of deformation, that the word "hypothesized", as used as a modifier of offshore zone of deformation, be stricken from this record.

JUDGE KELLEY: I am sorry, where is that?

MR. WHARTON: I am sorry -- correction, Your Honor. I was looking at the testimony of Edward Heath. I am jumping ahead with that. I don't believe that is in there. I will wait until Mr. Heath on that.

JUDGE KELLEY: All right.

MR. WHARTON: Excuse me.

MR. PIGOTT: Mr. Chairman, for purposes of clarification, I -- when witnesses are presented, adverse witnesses are presented, it may be my intent to voir dire prior to the admission of the testimony, and I would be reserving that right.

JUDGE KELLEY: Very well. So, Dr. Ehlig's

testimony is admitted and Mr. Wharton, if you want to voir dire the witness -- excuse me?

MR. PIGOTT: Do you want -- I assumed that Mr. Wharton was saying he would undertake cross-examination in the nature of voir dire. If that is --

JUDGE KELLEY: I wouldn't draw that distinction, but -- in the nature of, if that clarifies it in your mind, that is all right with me.

MR. PIGOTT: Which would mean that perhaps the next thing is for Dr. Ehlig to do, as Mr. Smith did yesterday, and give the Board and the parties a brief overview and explanation of his testimony.

JUDGE KELLEY: I had frankly forgetten whether the overview preceded the questioning.

MR. PIGOTT: Yes, it did. Yes, it did.

JUDGE KELLEY: All right.

MR. WHARTON: I would prefer that myself, in this particular instance, and in most of them, I believe it would be preferable.

JUDGE KELLEY: All right. Well, then let us have the overview portion of your testimony.

THE WITNESS: All right, my testimony starts with a -- more or less of a recital of the geologic history of the San Onofre region, and the purpose of presenting that history is to give you background since the present geology that we

see today is a product of the sum of the net history of the region.

Now, the history starts back in what we call the Mesozoic, and the oldest rock units we see are about 200 million years old. For those not familiar with geology, I might point out that the margin of the North American Continent lay East of this region, back during the beginning of this time, and that rocks which were deposited probably on an oceanic crust were accreted against the Continental margin by subduction.

Now, this process of peeling off material from an oceanic crust that lay on top of the crust, and placing it against the continent went on for an unknown period of time, but the rocks involved, the oldest are about 200 million years old.

About 120 million years ago, the events changed, the situation changed to where magma, molten rock material, was being formed at depth, and emplaced near the present continental margin, in the form of what we call the Southern California batholith, and if I may have the first slide?

We have a block diagram here, just to acquaint you with what was going on. This slab right here represents oceanic crust, which was being pushed beneath the continental margin. Now, when I say pushed, actually it was going down because of greater density, at least as we understand the

process, which we call subduction. It was going down underneath the continental crust, as a result of having a higher density than the material upon which it rested in the upper mantle.

When the material gets down to a depth, in this particular case, on the order of 125 to 175 kilometers below the surface, it is heated up enough to undergo partial melting, producing magma which rose in the crust, and came on up to the surface to form batholiths at shallow depth, and although we don't see it today, near the surface it would have formed volcanic complex, very similar to the Cascade volcanic range of today, or the Andes.

In the foreground, there would have been a shoreline, into which material was being eroded for the trench off on the offshore area. Now, about 105 million years ago, the subducting slab changed its orientation to pass at a lower angle, and extend further beneath the continent, and at that time, there was a shift in the magnetic activity, which moved it eastward.

In fact, it got east of the -- became east of the Peninsular Ranges by 85 to 90 million years ago. At that time, this region began to cool, and as it cooled, it subsided due to increased density due to contraction, and a line was established, a hinge line, to the west of which the material subsided below sea level, and west of that,

sedimentation could occur within the ocean. To the east, the area was still buoyant and standing above sea level.

May I have the -- PLE figure B (PLE-B), it shows a line west of which rocks that are on the order of 80 million years and younger were deposited. To the east of that line, it was dominantly undergoing erosion. That line nearly corresponds to the western edge of the igneous or batholithic intrusions that occurred during the period between 120 and 105 million years before present.

In the Peninsular Ranges area, and in Baja California, this line was pointed out by two geologists in 1930, and it has been referred to as the Santillan Barrera line, in honor of the two geologists who noted that it was a tectonic hinge line, west of which material was -- the area was below sea level, and sedimentation was taking place, and with time, I might say, that line rotated down, hinged down on the seaward side, so as to drop the sea floor lower and allow more sediment accumulation. Part of the drop was probably due to the weight of added sediment.

Now, this kind of an environment went on, of having sedimentation essentially west of that line, and erosion to the east. Until somewhere in the early Miocene, early to middle Miocene, and to approximately 16 million years ago.

At that time, there was a change in the tectonic

environment as we see it in Southern California. At that time, there was a sudden appearance of Catalina schist at the surface, and erosion from the seaward side of the schist, carrying it, schist material, westward -- I am sorry, eastward onto the edge of the Peninsular Range Province, particularly in the area of San Onofre.

Now, the schist material is called the San Onofre breccia, and if I may have the next vugraph, which is figure PLE-E?

This shows the distribution of the San Onofre breccia, which is in hatchers, and the distribution or probable distribution of the Catalina schist basement just below the surface, and that is a dashed line, and I might say that the known occurrences of Catalina schist, they are known to be west of the Newport-Inglewood zone of deformation, Palos Verdes Hills contain exposures of schist. Catalina Island, of course, contains extensive exposures of Catalina schist, and there are limited dredge samples that have been obtained in the continental border land that indicate schist basement in that region.

The interpretation of this dashed line is in large part based upon the distribution of the angular debris derived from the schist, and eroded, moved eastward, deposited to the east.

You will notice that the Coronado Islands off San

Diego contain the San Onofre Breccia, as we call it. It is also present inland, to the south of Tijuana. That represents very most easterly occurrence of it, then a very extensive body in the San Onofre area. That was derived -- it is non-marine in this area, and was derived from offshore.

Now, the event that brought this about represents a rather drastic change in the tectonic environment, in that the schist was formed, it was metamorphosed, at very great depth, on the order of 30 to 40 kilometers, based on mineral assemblages and what depths we know that they form at, based on studies in the laboratory.

The schist is also a low temperature rock, and we believe it formed at about the same time as the batholithic rocks in the Peninsular Ranges. The radiometric dating indicates a similar age. But the depth of formation and the temperature of formation were considerably different.

Consequently, it would be my opinion that the schist was most likely brought into position next to the batholithic terrain by strike-slip faulting during the middle Miocene, and this is a time, as I say, when there was great disruption of the near-surface geology.

The same time, when you get west of this line, you do not find intact masses of the sedimentary sequence of early Miocene and older, which occurs to the east of that line. We do have bits of it on some of the islands, but in general,

many areas it seems to be lacking. So that was a major event.

Now, following that event, there was a general change in the tectonics of the region, the Los Angeles basin started to open, and on page -- well, in starting around 12, I begin to talk about the Cristianitos fault, and the Cristianitos fault came into being at a somewhat later time than this San Onofre breccia. It came into being around ten million years ago.

It was marked by an abrupt change from base sea floor basin type deposits of the Monterey Formation, very laminated deposits that typically form in a basin that is deficient in oxygen and deficient in bottom currents, so that the beds are not all stirred up, and there aren't animals browsing around, to the environment of the Capistrano Formation and the San Mateo Formation, which is a part of the Capistrano Formation, which changed where adjacent to the Cristianitos fault, sandstone suddenly was being deposited, very massive sandstone.

Near the plant site, there was a very narrow body of sandstone deposited against the Cristianitos fault, and it fans away and fines away so that it appears that the Capistrano Embayment came into being as a result of opening of a basin here, which is called the Capistrano Embayment, or Capistrano basin.

This basin was active for a period of about 10

million years before present until approximately four million years ago, when it became filled with sediment, and activity ceased. Now, at the same time that this basin was forming, a very deep basin existed to the west of the San Joaquin Hills high in the Los Angeles region. The basin had a somewhat larger -- covered a larger area than the present basin, and was very deep.

You will note that in the te timony, I list a locality over here by Newport Beach, where the microfauna analysis by Engle indicates the depth of water was on the order of 10,000 feet. Now, as I interpret the Cristianitos fault, it represents the eastern edge of what amounts to a very large landslide, a gravity landslide, very similar to many other such occurrences in the geologic record.

It represents sliding of the upper crustal material westward toward the Los Angeles basin. I interpret the fault as having a gentler dip as it goes downward, based on the manner in which the sediments deform adjacent to the fault. The fault is down on the west side, and it shows normal drag adjacent to the fault where the beds are upturned along the fault due to normal drag, but then in many places, it shows a slumping down or backward rotation or tilting toward the fault as one moves westward, which is known as reverse drag.

This reverse drag typically forms when a mass

slides out on a gentle plane, and then the plane where the landward side of the mass -- the plane curves upward. Perhaps I can use my hands for a moment.

If you take a surface, sliding surface here, and then curve it upwards sharply, start to move something downhill, a space would open up, and that would be a void space if the rocks were rigid enough to hold it open. But since the sedimentary materials are not rigid, the material collapses back into it so as to tilt the surface back and give what we term reverse drag.

Now, we can see that in the cross-sections in the Capistrano embayment. We also see that as the mass slides down, the center floor of the embayment simply goes down in elevation, and it is possible to develop a fairly deep trough without breaking it up with a lot of faults, whereas if it were a deep-seated pull apart, there would tend to be a lot of faults collapsing on either side, so as far as trying to explain the origin of the Capistrano embayment, it is most easily visualized as essentially a very large landslide moving towards the Los Angeles basin the same time that it was opening up at great depth, and that it simply existed as long as the L.A. basin existed. The Los Angeles basin has subsequently been filled. The tectonic regime has changed in terms of the orientation of compressive stresses, and extensional stresses, and consequently the embayment is no

longer active.

Now, on page 21 and 22, I give my opinion with regard to the reasonable maximum magnitude earthquake that might occur along the OZD.

Based on my observations elsewhere and general knowledge of the region, I believe that an MS-7 magnitude is reasonable and I have stated why, mainly that if the previous maximum magnitude earthquakes had been higher than that, I would expect shearing to propagate to the sea floor, or to the land surface in the case of the Newport-Inglewood zone of deformation, and we do not see that.

I also know from my regional work that both the offshore continental borderland area as well as the Los Angeles basin and greater Los Angeles region are broken into many crustal blocks. There is many types of deformation, a great deal of deep folding going on. I find it difficult to imagine having a nice rigid unit storing up stress uniformly along great lengths of fault.

I think it is much more likely to have small segments break and in fact not even break necessarily along the single line.

On page 23, I discuss wrench fault tectonics. I personally do not like the terminology wrench fault tectonics, and personally would prefer using the term "strike-slip" tectonics, if you wish, because a wrench fault is the

same as a strike slip fault.

My primary objections to wrench fault tectonics is that they are a tectonic concept that was developed back around 1956 for the main development, by Moody and Hill, and they were introduced -- the concept was introduced as a cure-all for explaining overall global deformation. The present concepts of plate tectonics invalidate Moody and Hill's original theory.

Now, the theory makes many simplified assumptions. And these simplified assumptions lead to very simple patterns, but unfortunately, one can explain any pattern of deformation one wants with wrench fault tectonics, given the right scheme, and what it is necessary to do is to put deformation into the context of what is going on within a given region. One must have a good knowledge of the tectonics of a region in order to explain or understand what is happening, and in wrench fault tectonics, many people who apply it try to bypass regional knowledge, and simply start to make conclusions using overly simplified assumptions.

Now, on the page 29 to 33, I discuss the relationship between the Rose Canyon fault and faults of Northern Baja California. Back in 1969, the issue was raised of having a continuous fault zone. In 1979, several publications that came up at the time of the Geological Society of America meeting in San Diego, suggested that there

was an interconnection between the Rose Canyon fault, the Vallecitos fault, and the San Miguel fault in Baja California.

I have looked into this, and the one thing that is very obvious is that the faults in Baja California, at least the ones I have just mentioned, have very small displacement and they do not interconnect, so far as one can tell.

They also seem to have different timing as to when the displacement occurred, and just the last graph here, I find no interconnection, no basis for assuming that there is a throughgoing fault. This is a map by Gordon Gastil and others from the Geological Society of America, Memoir 140. It is which illustration? It is PLE-P, figure PLE-P. In the area of the north edge of the San Rafael Valley, the San Miguel fault trends across the north side of the valley. The Vallecitos fault lies several kilometers to the north, and for scale here, the distance across this intrusion is approximately 10 kilometers, or six miles.

I have investigated this area where they join, they must join. What I found was a dike of probable Cretaceous age can be traced continuously for eight kilometers in this area, blocking any faults. There are overlapping dikes in this region. There is no evidence of the fluton (ph) having a major fault through it. Plus the fact that in working along the Vallecitos fault from approximately the position of the

pointer on across to this road, I traversed the entire length, I find that the apparent offset shown on this map is incorrect. So far as I could determine, although there is a lineament along that line, and may well be minor faulting, I could find no offset of rock units that went across it. There are dikes in that area that trend across it, so far as I could tell, within the limits of exposures they are not offset.

Consequently, it is my conclusion that there is no throughgoing fault within northern Baja California, and no linkup between the San Miguel and the Rose Canyon fault.

With that, I will conclude.

MR. PIGOTT: Unless there are some preliminary questions by the Board on the brief overview given by Dr. Ehlig, I would tender the witness for cross-examination.

MR. WHARTON: Mr. Chairman, I wonder if we might take the morning break at this time, and just go straight through with cross-examination if that would be convenient. Mr. Barlow has asked that he wants to talk to me for about five minutes, so --

JUDGE KELLEY: Well, it is 10:15. It is not a bad time for a 15-minute coffee break. We can do that, come back at 10:30.

MR. CHANDLER: Before we do that?

JUDGE KELLEY: Mr. Chandler?

MR CHANDLER: Before we do that, one thing. I

think it may have been an oversight earlier, with respect to the Staff's documents, as part of Exhibit 4, which is the final environmental statement, there should also be included the Staff's errata to that document. It is a single-page document. I will have that marked as Staff Exhibit 4(a).

JUDGE KELLEY: So ordered.

(Whereupon, the above-mentioned document was marked as Staff Exhibit No. 4(a) for identification and received in evidence.)

JUDGE KELLEY: We will adjourn for 15 minutes.

(Brief recess)

JUDGE KELLEY: Let us resume.

Mr. Wharton, do you want to proceed?

CROSS-EXAMINATION

BY MR. WHARTON:

Q Dr. Ehlig, I am, as you probably know, attorney for the Intervenors, and I am going to be asking you some questions on your educational background, any possible bias that you may have in this matter, your relationship with Southern California Edison, and getting into some definitional matters of terms that you used, for purposes of clarification later on. Mr. Barlow will be asking you questions more in a technical nature.

It states here you are a Professor of Geology at

California State University at Los Angeles. How long have you been a Professor of Geology at that institution?

A Since 1956.

Q Has that been continuous employment at this one university?

A Yes, it has.

Q And what is your present position there, that is, full-time professor, any honorary --

A I am a full-time professor.

Q That is a full professor, is that what they would refer to that as?

A It is a full professor, yes.

Q And you are not there on a part-time basis, that is you are full time --

A I am full time.

Q Okay, as a matter -- when did you start doing work as a consulting geologist of any kind, that is, prior -- even prior to Southern California Edison?

A Oh, about 1954.

Q Have you ever done any consulting prior to being a consultant for Southern California Edison regarding the siting of nuclear power plants?

A I have never been involved as a paid consultant. I have had questions asked me on Sundesert, and participated in some trips, but not as a paid individual.

Q So would it be fair to state this is the first time that you have been a consulting geologist regarding the call it seismic safety, or the geology of a site proposed for a nuclear power plant?

A For a nuclear power plant --

Q Yes.

A -- yes.

Q And when did you start being a consultant for Southern California Edison?

A 1977.

Q And since that time, what amount of your work time or professional duties time have you spent as a consultant for Southern California Edison?

A It would be a little difficult to estimate, but it is a rather small percentage.

Q Let us go back to the first year that you were a consultant for Southern California Edison.

A I devoted perhaps half the summer of 1977 to consulting, to the work I did in San Onofre.

Q Okay, and what did you do with that half that summer, that is, what kind of work did you do?

A It was mapping on Camp Pendleton, geologic mapping.

Q At Camp Pendleton?

A Yes, on the Marine Corps Base.

Q On the Marine Corps Base. Is that in the proximity

to the actual San Onofre site, was that mapping --

A From the site -- well, from about a mile northeast -- northwest of the site, to approximately seven miles southeast of the site, and extending from the coast to about one and a half or so miles inland.

Q Okay, what was the purpose of that mapping that you did that summer?

A They had hired me to find the geologic structure, and to try to solve some stratigraphic questions that had been raised.

Q And they hired you to find, you say, geologic structures, what structure?

A To map the area in more detail than had previously been mapped, and to find out what the geologic structure was of the region.

Q Okay, you are not speaking of a geologic structure, but of geologic structure generally?

A Geologic structure in general. It was a generalized study.

Q But getting back a little bit earlier, how did you become to be a consultant for Southern California Edison? Did they seek you out? Did you seek them out?

A They sought me out.

Q And in what manner did they seek you out?

MR. PIGOTT: I am going to object as being

ambiguous. I certainly don't understand the call of that question.

MR. WHARTON: Well, I will rephrase the question.

BY MR. WHARTON:

Q Was this a situation where someone from Southern California Edison called you, wrote you a letter, how --

A They called you.

Q Okay, and at that particular time, it was that time that arrangements were made as far as what you were to do as a consulting geologist, and what they wanted you to research and study?

A Well, they proposed what I would -- what they would like me to do.

Q Okay, and what did they propose that they would like you to do?

A It was a study to find out stratigraphic relationships near the plant site, and dealt particularly with whether the Capistrano formation, the San Mateo sandstone and Monterey formation occurred along, in different places along the coast, or whether it was all one formation.

They were interested in finding out the general geologic structure, particularly between San Onofre Mountain and the coast.

Q Did they give you any specific problems or questions that they wanted answers from you regarding?

A Well, in terms of the stratigraphic relationships between rocks exposed in the area, there were several possible ways that the rocks might be related, and that was a generalized problem to be solved.

Q Were there any specific problems that they asked you to look into and resolve, or solve, or give your recommendations or your opinions on? By specific, I mean,, looking at, say, specifically looking at activity on the Cristianitos fault, for example. Not saying that that is something that you did. Anything very specific?

A At that time, the Cristianitos fault was not an issue as far as my work was concerned. My work mainly started out going southeastward from the Cristianitos fault. I believe they thought they had the Cristianitos fault pretty well nailed down at that time.

Q Okay, did they give you any specific assignments or problems regarding the offshore zone of deformation?

A Not at that time, no.

Q At any time?

A I have reviewed it in the context of the regional geology. I have looked at the seismic profiles offshore. I have not directl worked with the offshore zone of deformation, in terms of -- well, you can't do field work out there, but I have looked at the data on it. I have copies of the seismic traverses, the reflection profiles. Okay. Your testimony, I

believe, that you -- the question -- a question -- still on page 21, line 14, have you reviewed the earthquake potential of the OZD, and the answer is: Yes, I have reviewed it from the standpoint of what I consider to be the maximum earthquake likely to occur along it, based on its features, geologic strain rate, and regional tectonic setting.

A What page was that again?

Q It was page 21, line 16. I would like to clarify --

A Right.

Q -- your previous answer here, as to whether or not you did specific studies on the OZD for purposes of determining the maximum earthquake on the OZD.

A Probably on the order of 1979, and I am -- the exact timing is not clear to me, I was asked my opinion, as I think probably most consultants have been asked their opinions with regard to earthquake potential, and I have expressed that opinion.

Q When you were asked you opinion, were you asked your opinion with a certain magnitude, in this case magnitude 7, given as what is -- they deemed to be acceptable and for you to double-check that, or was it something you were supposed to do entirely independently and come up with your own number?

A At that time, it was more or less what number

would I come up with. It would -- there was no set figure. This was in fact when -- before a magnitude had been assigned, and at the time, Edison was arguing for strictly, I believe, a Mercali, modified Mercali value for the plant site.

Q Okay, going into the question I would like to ask directly, and get a kind of a definition of what we are dealing with here, is you have -- it says, I have reviewed it from the standpoint of what I considered to be the maximum earthquake likely to occur along it, based on its features, geologic strain rate, and regional tectonic setting. Now, does this word "likely" have a qualifying effect? That is, is this something that you are deciding based upon certain probabilities, or are you looking at for the earthquake, the maximum earthquake that is possible at any time along that fault?

A In judging what is -- let us back up here a minute. One thing is what is plausible, and another one, what is likely to be a real value. Many things that Walt Disney produces are plausible, but when you get down to the real constraints of what you see, they are unlikely.

In this particular case, I have undertaken quite a few fault studies, particularly the San Gabriel fault, and San Andreas fault, in prior engineering geology work, consulting work, I have rendered opinions with regard to what is the maximum probably earthquake to occur along

a particular fault, and I have previously developed criteria that I would use to judge that.

Now, I do not have precise numerical values.

Q Could you state what the criteria are that you are referring to?

A One would be activity, which has been brought up here. Put in again, into a tectonic framework. In this particular case, if a fault shows evidence of having moved only very slowly through time, that is the net accumulated movement, it is in a tectonic environment where the rocks along it are undergoing other types of deformation.

In the case of very slow movement, I would be surprised if a fault moved for any great length during any given earthquake, because the stress field is not likely to be uniform along that fault, and the chances that more than a small segment would reach the critical point to where it would fail are slim.

On the other hand, something like the San Andreas fault, with a very high slip rate, with a very straight trace along most of its length, even along the Big Bend, when you look at the most recent trace, it is very straight.

I have done a lot of work along that, and my own conclusion is that the San Andreas is moving so swiftly that the rocks along it have very little opportunity to bend it out of shape. Whatever little bending does occur, it

simply during any given earthquake straightens itself out by breaking across, making some new breaks, or breaking across old zone.

Certainly the chances of loading a section, a long section of the San Andreas fault up to the critical point, or approaching the critical point, is much greater in any given length of time, than along a fault of very slow slip rate, so slip rate is one of the things, again, it is something that I haven't tried to quantitize. Length is another one. General setting. If a fault is short, if it does not break through the sedimentary cover, that is one of the things that impresses me about the Newport-Inglewood zone of deformation.

When I go along the San Andreas, I find fault scarps propagating up to the surface, including in the Salton trough, in the Mecca Hills.

/// Please continue reading next numbered page. ///

tp #4

Q Dr. Ehlig, I'd like to get back -- I understand that you are going through criteria that you are looking at generally. I'd like to get back to see if I can get a closer understanding of what is meant by "likely". Initially, does the word "likely" as used here have a specific meaning as a term of art or a specific definition that is generally accepted in your professional field?

A "Likely" does not mean absolute. But one might substitute the word "credible" for it.

Q Okay. Now if you use the word "likely" or "credible", if we were to take a given earthquake fault and do all thecomputations and measurement that you have and come up with a figure that you consider to be the maximum earthquake likely to occur, is this the earthquake that (1) is the maximum earthquake that can possibly occur or, for example, is it the earthquake that can occur 2 percent of the time of any earthquake that occurs on that fault, or would it be something, a lower figure?

MR. PIGOTT: Mr. Chairman, I would like to object at this point in time or at least get a clarification. Mr. Wharton has stated that he would be doing the examination for purposes, in effect, of establishing credibility and qualifications and bias, et cetera, and that Mr. Barlow would be doing the technical examination. I have been listening to the last three or four questions and wondering whether or not we

haven't crossed that boundary. I certainly object to the witness in effect being doubleteamed from a substantive standpoint and I would submit that we are into the technical area and that Mr. Wharton either should take on the whole of the technical examination or relinquish the examination to whoever is going to do that portion. I think we have passed the area of qualifications, bias, et cetera.

MR. WHARTON: Mr. Chairman, I note Mr. Pigott's exception, but if I might explain and let the Board rule. If I am getting into what is considered a technical area, I certainly will stop. I am trying to if we can get some of the terms that have been used, technical terms that have been used, to try to get an understanding of what these terms mean in the setting of this licensing procedure. It is very important that these terms have some kind of decision, if possible, that all of us can follow as laymen.

I am trying to see if in fact this is an area where they are getting into probabilities and the probabilities are being decided by a scientist who may not have the qualifications to get into probability, which would be a legal issue. I am trying to define this in terms of whether it is a quantitative statement or a probability statement. That is why I am looking into "likely", to determine whether it is something that is absolute or is somewhere in the range of probabilities from 50 percent to 98 percent.

That is the area I would like to pursue with this. If this seems to be a technical area, I will defer to Mr. Barlow to do that. But I would like to pursue this if I can get some definition of these terms.

JUDGE KELLEY: Well, I think it is a very important thing to pursue. I think it would be cleaner if you can divide technical as opposed to other kinds of questions, but since this has been opened up and since it is sort of quasi-legal, I think it would be useful for you to go ahead and pursue it. I know the Board, having read the testimony and having focused on some of these emerging issues, can see that an awful lot of this is going to turn out to be judgmental in the long run.

I, too, was caught by "likely", and when you said, Dr. Ehlig, that "likely" meant credible, I didn't think that was what it meant. So it can mean that to you, but I would like to get the record as clear as we possibly can on this and nailed down, again, as Mr. Wharton says, are we dealing in terms of art or are we dealing in an individual witness' choice of words, and get that as fairly as possible.

So I am going to overrule the objection to this particular question and allow you to pursue it to its conclusion in the not too distant future.

MR. WHARTON: Feel free to interrupt if I am getting into the technical. I don't want to do that.

JUDGE KELLEY: But then we would like to have the technical questions moved over to Mr. Barlow.

THE WITNESS: Let me try to clarify it, if I can, and perhaps I won't be able to put it in the kind of legalistic terms you would like, but to put it another way, I look at the geologic record and if I see no evidence of such an event having occurred in the past, then I think it is unlikely to occur in the future.

Now I don't think that one can put absolute terms on such things. Just because a meteorite has never struck this building doesn't mean it won't in the future. But it is certainly unlikely. And so the way I use these words is probably not the best legal usage. But if you can get the flavor of where I'm coming from, perhaps that will solve the problem.

BY MR. WHARTON:

Q Okay. If I might just for purposes of clarification again. I understand that when certain valuations are put on there may be a mean standard placed on an evaluation, is that correct? Some kind of mean standard of earthquake on a certain fault?

A That would be correct.

Q Okay. Now what would the mean standard mean in stating what earthquake could occur?

A Are you asking for something in statistics or are

you asking for something in a judgmental?

Q Well, the expression "mean standard" is used by geologists in your field, is that correct?

A Not particularly.

Q Okay. So that is not an expression or a term that you know the full meaning of?

A No.

Q Okay. I won't pursue that any further then. Can you put -- okay. Is it fair to state, then, that the word "likely" here indicates a probabilistic determination on your part of the percentage or likelihood of occurrence of an earthquake on this particular fault?

A I think that would be reasonably fair.

Q Okay. Now going down to page 21, the same line, we are dealing with another word that comes up quite often -- and this is on line 20 -- it says one of the issues is whether M-7 is an appropriate maximum magnitude for earthquakes on the OZD. Question: Do you believe M7 is adequate? and you answer: Yes, I do, for the following reasons, and then you give following reasons. Now in the context of answering this question, what was your understanding of the expression whether MS7 is an appropriate maximum magnitude for earthquakes in the OZD? Could you expand on that, explain what your thinking is for answering the question that yes, it is appropriate, centering on the word "appropriate"?

A I believe that that is the largest value which might occur along the offshore zone of deformation. Again, that is based on my own evaluation of what I see in the geologic record, that it is not exceeded in the past, does not appear to have been exceeded in the past.

Q Okay. When you say the largest that might occur, is that during --

A That's actually probably larger than I would care to place. I would care to place something closer to a 6.5, but --

Q Okay. Just going to the words without going into what you are reciting, that might occur, are you talking about that might occur during the life of the plant or might occur in any time for all --

A Within the present tectonic regime.

Q What do you mean by the present tectonic regime?

A The present orientation of the stress field as we see it and the plate boundary.

Q Now the question I am asking is might occur when? Are you making --

A At any time.

Q At any time? At any time in the forever future?

A That's right.

Q And is "might occur" --

JUDGE KELLEY: Excuse me. I thought you said at

any time during the present stress field.

THE WITNESS: As far as the future goes, I assume that he is not just talking about -- he's expanding it into say the next 100,000 years. In terms of future for human beings, I would say that within the human history I can't perceive of the stress field changing. But if you are going to extrapolate for 5 million years then you've got another --

JUDGE KELLEY: What about a much more modest extrapolation to 40 years?

THE WITNESS: If you extrapolate to a thousand years, I cannot perceive of the stress field changing.

JUDGE KELLEY: I don't mean to be facetious, but that is the normal life of a licensed nuclear reactor.

THE WITNESS: Yes. I cannot perceive of it changing within the next thousand years, 10,000 years.

BY MR. WHARTON:

Q Is it fair to say -- you just used the word "might" -- is it fair to say then using that in determining what is appropriate that "might" is equivalent to the word "likely" as far as coming up with a probabilistic determination?

A Yes.

Q And you don't represent that as a straight quantitative, that is, absolute determination?

A That is correct.

Q You have worked for Southern California Edison

for the past four years.

JUDGE KELLEY: Mr. Wharton, can I just ask one further question as long as we are on this line?

MR. WHARTON: Certainly.

JUDGE KELLEY: Normally I would do it later. I wanted to clarify in my own mind what I think you are saying and what seems to be reflected in what you are testifying. The NRC commissioned some time ago -- or rather the AEC did -- a very elaborate risk study that came to be called the Rasmussen Report. I don't know if you have ever heard of it or not. It was an attempt to quantify risk all the way from dams breaking to nuclear power plants rupturing in some major way. Very specific numbers got assigned. It is also fair to say I think that that study has been criticized quite a bit and is not at least as to the numbers relied upon by the NRC, hasn't been for some time. But it is an example of trying to quantify in very fine detail.

Is it your practice as a geologist and, beyond that, would you be able to say whether it is the practice of geologists generally to attempt to quantify with any precision the likelihood of an earthquake of any given magnitude, the likelihood of that happening? Do you speak in terms of 1 in 10,000 or 1 in whatever or rather is it more likely you would give a judgmental conclusion about such a thing without reference to numbers?

THE WITNESS: I don't think that there is any basis for assigning values so that they can be handled statistically, unless you are talking about lesser than the maximum possible earthquake. If you speak of the highest value being 7 and consider that as a real earthquake and not just something higher than what you really expect, then there is a relationship between the numbers of 6's that you would expect and the numbers of 5 magnitude earthquakes you would expect and from that, if you know slip rate, one can predict the recurrence interval and from there go into what is the likelihood of how many 5's or 6's versus 7's, that sort of thing.

But the problem that I would have in the kind of work I do is that I have no precise numerical way of evaluating the probability of say a 7 along the zone. I can look at the record and say whether I think a 7 might have occurred. I can look at something along the San Andreas and see evidence of compression ridges and things that toss ground that say hey, it was a big magnitude earthquake, it threw things up in the air. I can look at something like Newport-Inglewood and say I don't see that kind of evidence.

But within that, it is a very shady area. All I am saying is that from what I have seen in looking particularly along the Newport-Inglewood and looking at the subsurface data, I really see no evidence that anything got up to magnitude 7,

at least what I would expect from a magnitude 7 in the past. Therefore I will predict in the future that it won't.

JUDGE KELLEY: But you wouldn't attempt to assign a number to that?

THE WITNESS: No, I would not.

JUDGE KELLEY: Thank you.

BY MR. WHARTON:

Q Okay. You state that you have worked for Southern California Edison for the past four years?

A That is correct.

Q What percentage of your income, that is, your overall income, personal income, during the past four years has been derived from Southern California Edison?

A It is a very small percentage. Precise values I don't know; I suppose 10 percent, perhaps.

Q Now have you personally written anything that was published in the FSAR that was admitted into evidence this morning?

MR. PIGOTT: I'm going to object on the relevancy of that question, Mr. Chairman. We have identified his direct testimony and I don't believe we have to go beyond that.

MR. WHARTON: Mr. Chairman, I believe you made an indication this morning that you aren't going to be relying on parts of the FSAR of witnesses who were unavailable and I would like to know if Dr. Ehlig participated in writing the

FSAR for purposes of the record later on when we have to come up with findings of fact and conclusions of law, whether parts of the FSAR that he wrote can be relied on in or will be relied on by the Board in writing findings of fact and conclusions.

JUDGE KELLEY: Well, that is not quite what I recall having said this morning. What I said was that we did not envision reliance, strong reliance on the FSAR as opposed to whatever is produced in this hearing as to important major points, if you will.

MR WHARTON: If I may --

JUDGE KELLEY: But with that, we did admit that exhibit into evidence. And I guess I am still -- I don't think the question has anything to do with Dr. Ehlig's testimony but, apart from that, I am not sure where you are going with this.

MR. WHARTON: What I have down this morning, my statement as I have it down -- and correct it if it is wrong -- is the Board will not be resolving major issues based on unsponsored documents of the FSAR. I am asking if the -- Dr. Ehlig is a sponsor of part of the FSAR, for purposes of reliance later on or did he write it any of it.

JUDGE KELLEY: So what if he says yes, I wrote section whatever? Then where do we go?

MR. WHARTON: Then if he wrote section such and

such that is on the record. That may be something he can rely on. If he says he didn't write any of it --

JUDGE KELLEY: I am really concerned about the consumption of time here more than anything else. If there is a quick question with a short answer, go ahead.

MR. WHARTON: I am not going into the FSAR. I just want to know if he did any part of it and what part did he do. That's all.

JUDGE KELLEY: All right. Go ahead.

MR. PIGOTT: Could I have a further definition of the question, then? When you say writing, are you talking about -- we need a specification of that. Take a look at the FSAR and you can just imagine that it is not done by committee and it is not done by people parcelling it out. So I think "writing" is probably a poor term.

JUDGE KELLEY: It's not identified, is it, piece by piece, of the authors?

MR. PIGOTT: No, it is not. So I would think that the form of the question, asking for "writing", is probably not clear in this context.

MR. WHARTON: I will try to rephrase the question.

BY MR. WHARTON:

Q Are you aware of the existence of what is called the FSAR?

A Yes, I am.

Q Have you reviewed the FSAR?

A Not recently.

Q Have you reviewed any parts of the FSAR regarding geology and seismology?

A By review do you mean brief through it or do you mean review in the context of --

Q Just say read through it, just to read it.

A Yes, I have.

Q Are there parts of that particular document you have read through that you would consider would be sponsored by you, that is, they are based upon your work product, your research, and your writings? Is there anything in the FSAR that would indicate that?

A As best I recall, there are parts that would have been contributed by me or at least the major writing. But I would have to review it specifically with that in mind to really check it out.

MR. WHARTON: I would ask merely, and not go the question any further, that if Dr. Ehlig can review, if he would give me what it is that he contributed to the FSAR so we can put that in the record, and that would be the end of the line of questioning.

MR. PIGOTT: I'm going to object. There is no showing as to the relevance. We have submitted his issue. We have submitted his testimony, rather. The Applicants are

not at this time relying on Dr. Ehlig to sponsor any particular language in the FSAR and this seems to me like an unwarranted burden to place on the witness at this time of the trial. He is here to stand examination on what he has put together, not to go back and review things that may or may not have been put together over the last four or five or six years.

JUDGE KELLEY: Mr. Wharton, if you had in hand a piece of paper from Dr. Ehlig saying I basically wrote the following sections of the FSAR, what does that do for you?

MR. WHARTON: Mr. Chairman, that for me --

JUDGE KELLEY: Maybe I should see it, but --

MR. WHARTON: Mr. Chairman, I would like to go through this line of questioning briefly for purposes later when we have to come up with findings of fact and conclusions of law, if findings of fact and conclusions of law are submitted which rely on parts of the FSAR rather than testimony written here. I want to have a document to review to determine which parts of the FSAR have been sponsored at this particular hearing or indicated that they have authorship that has been designated or that part which has not so that I can make a proper objection to whatever findingsof fact or conclusions of law are submitted. That is the purpose.

JUDGE KELLEY: Well, if the Applicants end up not relying on the FSAR in their proposed findings and conclusions then this won't serve any purpose, will it?

MR. WHARTON: That's correct. It would not.

JUDGE KELLEY: So let me ask you, Mr. Pigott, if you had your preferences between doing this now and doing it later -- and I'm not saying that you necessarily have to, but if you had to do one or the other -- which would you prefer to do?

MR. PIGOTT: I'm not sure what the question is. Prefer to do what?

JUDGE KELLEY: The question is, when you file your proposed findings and conclusions if, with regard to various findings, you are relying on the FSAR would you then -- what would be your view about footnoting it and saying this section was written by Dr. Ehlig or whoever?

MR. PIGOTT: I'm not --

JUDGE KELLEY: As opposed to doing it now and writing up a lot of material that may not serve any purpose.

MR. PIGOTT: I'm not even sure if I could do that. The way these things are put together, I would have difficulty pointing to a particular paragraph and saying that this is the work of a single person. The usual situation is that when a question comes up or there is a submittal being made it is perhaps initially drafted by one person or organization, but it is subject to many reviews, many changes, comes from the -- comes back and forth from client to consultant to other consultant for review and back and forth until finally it gets

into a form that, as Mr. Moody testified to yesterday, it is reviewed through the management scheme and is submitted as the official best word of the corporation. Now I would be willing and will of course, if I submit a finding of fact which relies solely on the FSAR, to be identifying that portion of the FSAR. But as I sit here right now, I'm not even sure that I would be able to identify the person who actually drafted that language.

Now I could probably get someone who would be able to sponsor the conclusions and who would have done the appropriate research, if that ever became necessary on an evidentiary basis.

MR. CHANDLER: Mr. Chairman, I would like to note that of course not only is it possible that the Applicant might desire to rely on the FSAR, but I certainly would anticipate the Staff may have the need or the desire to do so as well. Furthermore, I think at least a certain level of inquiry into the contents of the FSAR may be appropriate. We may have questions of weight to be given, inconsistencies in statements. I think at least to a certain level a certain amount of probing is appropriate by Mr. Wharton on that.

JUDGE KELLEY: This would be -- let me follow Mr. Chandler's suggestion. Mr. Wharton had reached the point in questions whereby he wanted to know what parts Dr. Ehlig authored. Now you are suggesting a step beyond that,

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as I hear you, whereby not just identification but probing about the adequacy of a section of the FSAR, is that right?

MR. CHANDLER: Well, there may be weight questions that come up, credibility questions that come up. If Mr. Wharton is satisfied to rely on an identification of the sections that Dr. Ehlig is responsible for, I will be satisfied with that. All I am suggesting to the Board and representing for the Staff is that we wouldn't consider it appropriate necessarily to cut off any inquiry into the FSAR merely because it is an institutional document.

JUDGE KELLEY: Let me just follow this through. The staff, like the other parties, is going to come in here with witnesses who they will sponsor and who have testimony, correct?

MR. CHANDLER: Correct.

JUDGE KELLEY: Is the Staff suggesting that beyond that we should review various sections of the FSAR at the hearing?

MR. CHANDLER: No, sir.

JUDGE KELLEY: Then I am not sure I follow you.

MR. CHANDLER: Well, when you say other sections of the FSAR, maybe I misunderstood the Board's question.

JUDGE KELLEY: You are putting forward for yourself a section of the SER on seismology and geology on this issue, correct?

MR. CHANDLER: That's correct.

JUDGE KELLEY: You are not offering in your affirmative case, as far as I am aware, anything from the FSAR. Of course that is the Applicants' document. I understand that. But you could rely on it, I assume. Do you envision cross examination on the FSAR from witnesses -- I'm not sure which ones -- in this hearing?

MR. CHANDLER: Staff witnesses? I'm a little bit lost, Mr. Chairman.

JUDGE KELLEY: Well maybe we are both lost. I'm lost on your suggestion that we should probe the FSAR in some fashion.

MR. CHANDLER: What I am suggesting is that certain use I think is appropriately made of the FSAR in this proceeding. It is not merely a document that should sit on the shelf back there. I think reference to it is appropriate in the context of examining Dr. Ehlig. There may be statements in that document that may not be consistent with what Dr. Ehlig has testified to at this time. I don't know.

All I am suggesting is that --

JUDGE KELLEY: Okay. Those examples I understand.

MR. CHANDLER: That's the only point I was making, sir.

JUDGE KELLEY: All right.

MR. WHARTON: Mr. Chairman, if I may point out,

the problem as I argued against the admissibility is I am up against an anonymous document that could be relied on later on. I think we are entitled to know if there are certain witnesses that are here to testify, if they participated in writing the FSAR for purposes of they can refer to it or they may refer to it as part of the background. My saying, I am asking about the FSAR to see whether or not he wrote any part of it. I don't think -- I am not going into technical aspects of the FSAR, what it says. That is Mr. Barlow's position.

But I don't think that if he has -- I think if he has written part of the FSAR, that if Mr. Barlow sees parts of the FSAR -- I'm not saying that we are even going into that because we really haven't got that much time to do it -- but he should be able to get into that part, if Dr. Ehlig wrote it. That is part of his testimony and it is being offered as testimony.

He is testifying now (1) if there is any inconsistencies they should be pointed out and (2) if there is something that we want to probe further on the FSAR, I think we should be able to do that. We are not anticipating doing that. It is not part of the main case that we have, mainly because we haven't been able to review all of it. But it is open. You admitted it into evidence. It is something that can be relied on. We are entitled to go into that.

I am asking for identification of the author so

that we can go into it. If he can't comment on it I can't ask him about it.

JUDGE KELLEY: I am a little concerned as a matter of the orderly conduct of this hearing how that would be done. It seems to me that let's suppose that there is a section of the FSAR composed by Dr. Ehlig and it is inconsistent in some fashion or appears to be with his testimony. Then obviously in the normal course of things you could ask a question about that.

But we are here this morning and he is now subject to cross examination and we are in the position of you are now attempting to identify what sections he wrote. Do you envision that -- suppose you had right now, I wrote sections 2, 3 and 7. Do you think you could effectively use them in cross examination here today?

MR. WHARTON: At this particular point, no. That doesn't mean if we can review that -- this will be the first time we find out who wrote this part of the FSAR. If there is something in the FSAR that turns out to be totally inconsistent with what he says now, I think we have a right to recall him for purposes of impeachment. That is one of the reasons we need it. I have to know what that document -- who wrote this document. I can't deal with an anonymous document.

MR. PIGOTT: Mr. Chairman, I think we have been through several times now how the document is prepared. And

Dr. Ehlig is not sponsoring the FSAR that's being sponsored by the Applicants generally as an institutional type of a document.

If Mr. Wharton has problems with a particular portion which he thinks is something that should be subjected to cross examination by Dr. Ehlig, then he is free to use that as the basis of his cross examination.

What I am objecting to is the burden that he would impose on Dr. Ehlig and on the Applicants to go through the six-plus volumes comprising the FSAR and in effect you would never find anything I don't think actually written by Dr. Ehlig. The best you would have to do is find out which portions he closely reviewed as a part of the submittal and I submit that that is just not a fair approach to examination of that document. If he has problems with the document and it appears to go to the area that Dr. Ehlig is addressing, then fine and good, let him go ahead and do the cross examination based on that document.

But for Dr. Ehlig or any of our other witnesses, Mr. Heath, Dr. Smith, Mr. White, to have to go back through that document and identify section by section for the convenience of counsel in preparing their case, I think that is totally unwarranted and a burden on the applicants that shouldn't be required.

MR. CHANDLER: Mr. Chairman, I would note my

general agreement with what Mr. Pigott said. I think it is consistent with what I was suggesting earlier. I don't think we need delay the proceeding or suggest the need to recall witnesses. But I think use can be made of the document. There are certainly sections in there which relate to the subject matter which Dr. Ehlig is testifying to now.

To the extent a brief identification can be promptly provided, certainly it may be helpful. But we would not envision theneed to or make the suggestion that specific identification of chapter and verse is necessary or appropriate to accomplish what Mr. Wharton wants.

JUDGE KELLEY: Maybe not chapter and verse. I must say that having participated in the drafting of an awful lot of colleagal documents over time, I have not experienced the degree of difficulty that you suggest, Mr. Pigott, in remembering who wrote what. I always remember exactly what I wrote. And it goes through various types of review and permutation, but it is not my experience, anyway, that it is all that hard sometime later to say yes, that is what I wrote.

If this document is divided up as I assume it is into subject matter areas, I suppose most experts could just say well, most of this I know nothing about, and rather quickly get to the portions that are in their various -- moreover, it is not, given the nature of the beast, that it is

colleagal, you would not be it seems to me under any obligation to absolutely certainly identify, but rather to indicate this is the kind of thing that I believe I reviewed. That might be enough, I would think.

I think on that basis -- well, let me check with my Board members a moment.

(Pause while the Board members confer)

JUDGE KELLEY: I believe we are ready to resume. The Board has concluded that we are not going to require the various witnesses to identify different sections of the FSAR they may have participated in. You have the FSAR, Mr. Wharton, and you are of course free to cross examine any given witness by reference to the sections of the FSAR, let's say for example, that might be inconsistent or viewed as inconsistent with testimony. But a breakdown seems to us to be unwarranted.

So if you would then go ahead with your questions.

MR. WHARTON: Okay. I will just note for the record I take exception to the ruling and that I am now being forced to review an anonymous document. I have no further cross examination at this time and I will turn it over to Mr. Barlow.

JUDGE KELLEY: Mr. Barlow?

CROSS EXAMINATION

BY MR. BARLOW:

Q Dr. Ehlig, in looking at your testimony I would

like to just ask you a few background questions about the presentation that you gave this morning. On page 4, line 5, you are -- beginning on line 4 -- you say that your studies indicate that the geologic evolution of the San Onofre region began about 200 million years ago when the western edge of the continental crust terminated in that area. When you say western edge of the continental crust, are you referring here to the North American plate as it is currently described?

A I'm talking about the North American continent. When you say North American plate, it would be part of the North American plate at that time; however, what you have to realize is that the North American plate did not have the same configuration then as today.

Q I understand that. But it is a similar block within the crust that is today referred to as the North American plate?

A Yeah. It might be considerably more south than the present location.

Q So would it be reasonable to infer from this that plate tectonics of one sort of another have been occurring in thisregion for 200 million years?

A So far as we know, plate tectonics have been going on for the last at least 2 billion years.

Q Two billion years? Okay. Thank you.

MR. PIGOTT: I might ask Mr. Barlow to speak a

little louder. I am having difficulty following his questions.

JUDGE KELLEY: Yes. I will endorse that. Mr. Barlow, you do tend to speak in a very low voice.

MR. BARLOW: Sorry. Perhaps if I put this closer. Can you hear it better?

JUDGE KELLEY: Yes. It is just volume, I think, sheer volume.

MR. BARLOW: I will try to talk louder.

MR. PIGOTT: The loudspeaking system really doesn't help those of us that are parallel to you. We have to rely on just the regular acoustics, unfortunately.

MR. BARLOW: Okay. I will try to talk louder.

BY MR. BARLOW:

Q Dr. Ehlig, on page 7 of your testimony, line 20, you state at the beginning of the middle miocene, about 16 million years ago, conditions changed radically on the southern California coast and adjacent offshore borderland. The change may have been brought on by the passage of the East Pacific Rise beneath this part of the continental margin or by divergent transform faulting postdating the overriding of the rise. Could you explain this historic event in the geologic history of this region and in particular the concept that the East Pacific Rise passed beneath this part of the continental margin? Could you explain what that means?

A As plate tectonic reconstructions are understood

at this time, prior to 30 million years ago a spreading center existed in the Pacific Ocean off North America. The plate to the west of that spreading center was referred to as the Pacific Plate, the plate to the east was referred to as the Farallones Plate. With time, North America has shifted westward relative to the spreading center and the two came in contact at about 29 million years ago, based on reconstructions by Tanya Atwater in a publication in 1970 and another publication by Atwater and Mohlner.

Now the reconstructions that we make indicate that the spreading center, the East Pacific Rise, was likely to have passed beneath the continental border at about 29 million years ago. But in terms of the onshore record in the San Onofre area, in fact, in southern California, we really don't see evidence of anything much happening at 29 million years.

What we do see is that about 16 million years ago -- and that is give or take about a million -- things suddenly started to change in southern California. The former uniform deposition in a westward direction or seaward direction was disrupted. There was extensive volcanism. And suddenly the Catalina Schist, which had been formed at great depth, appeared at the surface and was uplifted and shed debris onto the continental margin.

Now plate reconstructions permit motions to change

through time relative plate motions. It is entirely possible that at the 16 million year old time period the movement of the Pacific plate relative to North America was slightly divergent. It was moving a little more rapidly northwestward than North America was moving westward. These are relative to a fixed point, in the Atlantic, say.

We don't know the exact reconstruction or exact nature other than we know many basins started to form, the borderland was broken up into many small plates at that time and that there was major tectonic events taking place in California.

Q Okay. At what point in geologic history did the Baja Peninsula begin separating from the mainland of the Mexican part of the North American continent?

A The main separation that we note today started about 4 million years ago at Magnetic Anomaly 3. Now in the literature you will see that is based on the magnetic striping at the mouth of the Gulf of California and you will note in the literature that it has been opened approximately 240 kilometers in the last 4 million years. There is evidence of what we call a protogulf that dates back as early as 10 million years. There is a possibility that some movement occurred along the gulf boundary and then up along the San Andreas fault as long as 10 million years ago.

Q I see. But am I correct in understanding you to

say that during the past 4 million years there has been 240 kilometers of separation in the Gulf of California?

A In the Gulf of California, based on magnetic striping, it is approximately 240 kilometers. That is a figure I understand that the most recent data gives slightly more than that at the mouth. But that is essentially correct.

Q Thank you. On page 27 of your testimony, line 3, first of all, line 1, the question is would you discuss how the OZD fits into the wrench tectonic system, and your answer, assuming the OZD marks the boundary between the peninsula range basement and the Catalina schist, the OZD originated about 15 or 16 million years ago during the middle miocene. And further down the page you say that on line 15, the OZD was probably part of a system of right lateral faults which formed the Pacific North American plate boundary within the California continental borderland during middle miocene, thus the OZD probably originated as a wrench fault.

Can you tell us whether or not this analysis of the geologic history of approximately 15 million years ago, I assume -- excuse me. Let me rephrase the question. At what time did the OZD probably originate as a wrench fault?

MR. PIGOTT: I'm going to object to the question. We have gone through a two minute build-up. I don't know whether the rephrasing lost the build-up or are you rephrasing the last call of the question. It is very disconcerting, Mr.

Chairman, to have one question posed with a long preamble and then the second question apparently going off at about 30 degrees.

JUDGE KELLEY: Mr. Barlow, just restate what your question was.

MR. BARLOW: Okay. Well, I have two main questions on this section.

JUDGE KELLEY: Two questions. Take them one at a time.

BY MR. BARLOW:

Q One is at what time in geologic history did the OZD originate as a wrench fault?

A As I would interpret the data, the OZD originated as a right slip fault or had right slip along it starting around 15 to 16 million years ago.

Q And do you agree that the OZD is currently experiencing right lateral strike slip motion?

A Let me back up one second here. I may be ambiguous. Off the site of the plant the OZD. I would not agree to something like a Rose CAnyon fault being part of the OZD because it does not mark the boundary between the two different basement types. So you need to qualify. When I am talking about OZD here I am talking about offshore from the plant.

Q Would you be comfortable referring to that as the

South Coast OZD?

A Yes. That is also a speculation that that is the boundary, speculation on my part. There is no absolute proof that that is where the basement contact is. But yes.

Q Would you agree that in the current tectonic regime that the Newport - Inglewood - South Coast OZD is experiencing right lateral strike slip movement?

A There is right shear going on in the Newport-Inglewood zone of deformation, as shown by the pattern of folds. I personally haven't seen enough data to demonstrate to me that the area offshore from San Onofre is experiencing right shear. Since we are in a region where right shear is more or less the rule along northwest trending faults, I would assume that it might experience right shear if it was active. But as far as proof, I know of no direct evidence that proves it is experiencing right shear.

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BY MR. BARLOW:

Q Okay, looking back a bit to the review of your testimony on page 27, did you mean on this page that in geologic history, the OZD formed -- well, on line 15 through 19, am I correct in interpreting that to mean that the OZD at one time formed the Pacific North American Plate boundary during the middle Miocene?

A Well, I conjecture that it was part of the plate boundary during the middle Miocene, yes.

Q And currently the main plate boundary is along the San Andreas fault and its branches, would you agree with that?

A That is correct, and its interconnected branches.

Q Would you agree that the south coast OZD and the Newport-Inglewood fault zone are related to -- or excuse me, are part of the present plate boundary system?

A I do not relate them directly to the plate boundary, but rather to drag phenomena associated with -- so when you say plate boundary system, they are not part of the primary plate boundary.

Q What would you assign as the width of the plate boundary in current time?

A It depends upon what area you are looking at. The main plate boundary is the San Andreas fault in most areas. Within Southern California, the plate boundary appears to have

two branches, in southeastern California. The San Jacinto fault appears to be assimilating part of the movement, and the San Andreas fault is assimilating part of the movement.

I would consider the block between the San Jacinto and the San Andreas to constitute what we would term a microplate right now. It is not internally broken up particularly, but there is a shunting of motion along each of the zones.

The problem of how the San Jacinto ties in with the San Andreas down in southeastern California, southern end of the Salton trough, is fairly complex. I don't think t has been totally resolved, but in most places, I would place the boundary as maybe a mile wide. It is -- actually, the modern San Andreas is a very discrete fault.

Q At the bottom of page 27, line 25, you state, in either case Quaternary deformation along the OZD, continuing on page 28, is a secondary effect of interaction between the Pacific and North American crustal plates, and the theory of wrench faulting is not applicable.

Do you agree that the San Andreas fault is a wrench fault.

A It is a strike-slip fault. Some people like to choose the word "wrench fault" in place of strike-slip fault.

Q Okay. Let us turn to the section of your testimony dealing with wrench tectonics. Again, it is on page

23. You say that you are familiar with present-day theories of wrench tectonics and you discuss these theories. I assume you have read the article by Wilcox, Harding and Seely entitled "Basic Wrench Tectonics," is that correct?

A That is correct.

Q Did you also -- or excuse me, was that in the Bulletin of the American Association of Petroleum Geologists?

A That is correct.

Q Did you read an accompanying article that followed that article, entitled "The Newport-Inglewood Fault Zone?"

A Yes.

Q Do you agree that the subtitle of that, or the full title of the accompanying article was "Newport-Inglewood Trend, California, an example of wrenching style of deformation," by T.P. Harding? Are you familiar with that --

A I am not sure of the question right there.

Q Are you familiar with the article that followed the article which you reference in --

A Yes.

MR. PIGOTT: Objection. Are you -- we have had a series of questions. Are you asking whether or not there is an article that this witness is familiar with, or are you asking whether or not the title of a particular article is such as you have just read?

MR. BARLOW: I am sorry. I will strike the question regarding the title. The intent of the question is whether or not the witness is familiar with the article. I believe --

THE WITNESS: Yes, I am.

MR. BARLOW: -- he answered yes.

JUDGE KELLEY: What article is this, for the record? Is that from a magazine or --

MR. BARLOW: It is the Bulletin of the American Association of Petroleum Geologists, 1973. The first article, which Dr. Ehlig references on page --

JUDGE KELLEY: Is this once a year, or is that some issue --

MR. BARLOW: Well, it is a regular publication. I am not sure how often it comes out --

THE WITNESS: It is monthly.

MR. BARLOW: It is monthly.

JUDGE KELLEY: It is monthly?

MR. BARLOW: Yes. And the article referenced by Dr. Ehlig was followed by another article, which I read the title of.

BY MR. BARLOW:

Q Dr. Ehlig, would you agree that in the article following the one that you referenced, that the Newport-Inglewood fault zone was described as an example of wrenching

style of deformation?

A Certainly as described by Harding. It previously was used as an example by Moody and Hill in 1956, as a style of deformation one finds in sediments overlying a zone that is undergoing right shear.

Q Okay, let us look at your discussion of the article by Moody and Hill. On page 24, line 23, you say that among the most controversial aspects of wrench fault tectonics is the theory proposed by Moody and Hill. Now, this theory was proposed in a publication by them in 1956, is that correct?

A Yes, a Geological Society of America bulletin.

Q All right, and then you go on to discuss on page 25 and 26 this 1956 article and theory by Moody and Hill, and you argue against several of their assumptions and ways of looking at wrench tectonics.

Are you arguing here that the concepts or theories of wrench tectonics did not evolve after 1956, after the -- this theory by Moody and Hill?

A In the Wilcox, Harding Seely article, they don't really evolve any new theory on wrench fault tectonics. I think everybody agrees that Moody and Hill were incorrect in their efforts to explain all worldwide deformation by the theory.

What Wilcox, Harding and Seely are trying to do is

demonstrate that one can find productive oil fields in structures that overlie zones of lateral shear, of wrench faulting, as they call it, and they cite ways in which to discover such zones.

But as far as deriving any deep-seated concept of wrench fault tectonics, they do not develop it. In fact, my great objection to applying this to deep-seated deformation is that the people who have utilized a theory are dominantly petroleum geologists who are looking for structures in the sedimentary cover that covers the basement, and they model all of their experiments by placing clay plates, cakes, I am sorry, layered clay over the top of rigid plates, which then they proceed to deform, or in some cases over elastic sheets that they proceed to deform.

The underlying material that would represent the crystalline basement is not an appropriate model of any normal rock, and it tells you nothing about what is going on at depth, so you can't extrapolate downward from their surface structures and really know what is going on at depth, so that is my main objection.

Q In your discussion of the theories by Wilcox, Harding and Seely, you say on line 14, quote, their interpretation --

A What page is this?

Q I am sorry, it is on page 23, line 14. You say,

their interpretations are based on the deformation produced in clay models by moving tin sheets beneath a clay cake.

Do you mean to say here that Wilcox, Harding and Seely in their 1973 report relied solely or primarily on models, clay models using tin sheets and clay cakes?

A That is how they illustrate it, and they -- those are essentially their words. They use what amount to cookie sheets.

Q Did they not also use numerous maps of faults, wrench faults and strike-slip faults from throughout the world?

A They used the models to then extend the analogy to what we see at the surface. Now, in most cases, the -- perhaps all cases, the examples they cite are long-accepted examples of strike-slip faults.

Q But they did use observations of strike-slip faults from throughout the world in their article.

A That is correct.

Q Did they also use radar images and aerial photography in analysing these faults?

A Well, one often uses aerial photography, or where it is suitable, radar imagery, to get pictures of surface deformation that allows you to see the pattern of folds, and then you can relate those to the deformation one gets in deforming a clay on top of a couple tin sheets, or whatever the

model mechanism is.

Q Did Wilcox, Harding and Seely also use fault maps, actual maps of fault zones to discuss these clay models of wrench tectonics?

A They did in a number of cases, and in one case that I am familiar with, the San Gabriel fault, where they have worked that in, I would disagree with their chronology. I think they have misused it. I didn't review their article so I didn't have a chance to give my input.

Q Did these authors also use cross-sectional analysis of various stratigraphic layers in the earth, and the way that the fault zones proceed into the -- below the surface?

A They show the faults propagating up through the sedimentary cover, yes.

Q Did their use of these sort of diagrams and figures make use of data and research that had been done by oil and gas companies looking for petroleum deposits?

A Certainly. Their work is perfectly valid for oil exploration, when one is working with a sedimentary cover that overlies a zone of deformation. You might note on that that the Newport-Inglewood zone would be in the early stages of wrench faulting as they portray it, without a throughgoing fault.

Now, other models might place it as having ... a

throughgoing fault back in mid-Miocene and then draped by sediment with a slow movement causing deformation of the overlying sediment. We don't know the answer on that.

Q How did you reach the conclusion that their interpretations were based on clay models when they used all these other sorts of maps and diagrams and below-surface research by oil companies, and aerial photography?

A They acheive strain in clay models and then make an analogy and say the strain we acheived in our clay models is very similar to the pattern one sees in nature, therefore the pattern that we see in nature was produced in the same way in which the pattern was in the clay model.

They are making analogies from a laboratory modeling experiment over to what you see in natu-

Q You seem to argue that the concept of wrench tectonics is -- does not allow for the more complex faulting that occurs in Southern California, and yet would you -- based on your familiarity with the article by Wilcox, Harding, and Seely, would you agree that they discussed three general styles of wrenching, including simply parallel wrenching in which crustal blocks move parallel with the wrench fault, and secondly convergent wrenching caused by blocks moving obliquely towards the wrench fault, and third, divergent wrenching resulting from oblique movements of the blocks away from the wrench fault, and that all three of these styles

develop on both local and regional scales? Is that a fair description of your understanding of --

A Those patterns, you are correct that they had blocks, that they moved parallel without any convergent or divergent motions, in other cases, they pulled them apart or pushed them together.

The styles that they get are things that develop directly above the shear zone at depth. When you extend it to a broad crustal region, then you run into problems, and among my objections is that one cannot extrapolate for great distances away from the fault, and attribute all of the regional deformation to their little simple experiments.

Q In -- within the concept of wrench tectonics, does one observe in nature of series of parallel strike-slip faults?

A That is a rather broad question. A series of en echelon faults that are essentially parallel may develop above a zone undergoing right or left shear in the basement, and these would have a stepped, right-stepping or left-stepping arrangement. Do you mean that sort of arrangement, or do you mean --

Q No, I was thinking more in terms of the San Andreas fault, the San Jacinto fault, the Whittier-Elsinore fault, the Newport-Inglewood OZD fault zone, the Palos Verdes Coronado Banks faults, and the San Clemente fault being parallel strike-slip faults which are wrenching the blocks between them.

Would you agree that these strike-slip faults would be wrench faults under the definitions used by these authors?

MR. PIGOTT: I am goir to object. I think that Mr. Ehlig has, or D.. Ehlig has made his statement as to what he considers to be wrench faults. I don't think it is up to Dr. Ehlig to substantiate what may or may not be said in an article. If Intervenors have a case they want to put on through an article, I think there are other ways to do it, and this is not an appropriate one.

MR. WHARTON: Mr. Chairman, I would first ask for the basis of the objection, and secondly, I think this question is very straightforward and direct and asks his professional opinion, which he is here to testify as to, the particular features and what they are. It is a totally appropriate question.

MR. PIGOTT: He is not asking for an interpretation. He is asking for a confirmation of what is said in an article and that can be done in a number of ways, the worst of which is to ask somebody what he read in it.

JUDGE KELLEY: I did not hear it quite that way, Mr. Pigott. I would agree with you, and a couple of questions before have been asked, do you agree that, and such, and has read a long description, that it was in the article, which I think is not the thrust of what you have here anyway.

The question is phrased in terms of do you agree

that certain faults or certain types of -- can be characterized in a certain way, I think is a legitimate one. Could you rephrase it and --

MR. BARLOW: Certainly.

JUDGE KELLEY: -- enumerate your faults?

BY MR. BARLOW:

Q Dr. Ehlig, would you agree that the series of parallel strike-slip faults, from the San Andreas fault to the San Clemente fault, are strike-slip faults?

A You have just stated that they were. In the case of the Palos Verdes fault, I know of no evidence or no proof that it is in fact strike-slip. There is evidence of reverse movement in the Palos Verdes Hills, which I am familiar with.

There are a large number of faults of varying ages that are northwest-trending, where we do have data with regard to the displacement on the faults. Most of them tend to have right slip, or show evidence of some right-slip movement along them.

They have not all been active simultaneously. They have not been necessarily all part of the plate boundary, and may be secondary faults. Some have been part of the plate boundary at one time. Others are part of the plate boundary at another, but within that -- those reservations, yes, I would agree.

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Q Thank you. Would you agree that wrench fault tectonics involves both extension and compression, complex secondary faulting, and co-existing normal faults, reverse faults, and dip/slip components, in addition to the main strike-slip wrench fault?

A Objection. Compound, complex, and generally unintelligible.

MR. WHARTON: The question may be nintelligible to Mr. Pigott. The question asked, does he agree with this particular statement, and the statement runs through a series of things that all have to be agreed to. I think it is a perfectly appropriate question.

MR. PIGOTT: It is still compound, complex.

JUDGE KELLEY: Could you break it down into pieces?

MR. BARLOW: Certainly.

BY MR. BARLOW:

Q Dr. Ehlig, would you agree that within the concept of wrench tectonics as described by the three authors who you have referenced in your testimony, that wrench fault tectonics involves complex secondary faulting?

A I believe they borrowed that directly from Moody and Hill, and Moody and Hill came up with a scheme that was a kind of panacea for all kinds of deformation. They worked out ways in which they could get every type of deformation we

see, and that is one of the objections I have to the theory.

Unless one looks at the details on a local basis, one cannot conclude whether or not something is the result of complex motion in a right shear system or a left shear system, but yes, as the theory was worked out by Moody and Hill, one could get any type of deformation one wished out of it.

Q Would you agree that a main strike-slip fault can be accompanied by co-existing normal faults, reverse faults, and dip-slip components?

A They can be accompanied by secondary faults of various types, yes.

Q Including normal faults and reverse faults?

A That is correct.

Q Do you agree that the Newport-Inglewood fault zone could -- are we supposed to wait?

MR. PIGOTT: I am sorry, could we have the question again. It is -- there was a distraction there.

JUDGE HAND: Yes, and I think perhaps we might wait for a moment.

JUDGE KELLEY: Ladies and Gentlemen, we have been asked to clear the room. Conveniently, it is the lunch hour. Apparently there has been some sort of bomb threat. I don't know -- that is all I know, but if we have been asked to, eat lunch. So, I would suggest coming back at a quarter after one, if the room is still here.

(Recessed at 12:15 p.m. to reconvene at 1:15 p.m.)

AFTERNOON SESSION

JUDGE KELLEY: We are ready to resume.

Mr. Barlow, before you start, let me just ask you a couple of questions about the scope and intent and time of cross-examination. I note that on your cross-examination plan, you reference some nine different areas into which you wish to inquire, and I don't believe you have finished the first one yet.

And looking down the road, I am beginning to run into a little bit of concern about time. You and Mr. Wharton and all parties have to have time to cross-examine and make a case, but we do have to pay a little attention to how time is going, and that is just a general observation with regard to how you might be a little selective with questioning.

Beyond that, let me just ask you as to Dr. Ehlig, would you have an estimate of about how long it may take you to complete your cross?

MR. BARLOW: I imagine it will take about an hour.

JUDGE KELLEY: Oh.

MR. BARLOW: It is possible it would go longer, though, maybe an hour and a half. It depends on the answers.

JUDGE KELLEY: Don't have any problem with that. I was extrapolating from one to nine, and thinking it looked a little bit longer, but --

MR. BARLOW: Well, some of that outline is also

for Mr. Heath, so --

JUDGE KELLEY: Very well, why don't you resume.

Whereupon,

PERRY L. EHLIG,

the witness on the stand at the time of recess, resumed the stand and, having been previously duly sworn by the Chairman, was examined and testified further as follows:

CROSS-EXAMINATION -- RESUMED

BY MR. BARLOW:

Q Dr. Ehlig, are you familiar with the testimony by Edward Heath in this proceeding?

A Yes, I am.

Q Are you aware that in his testimony, he describes the OZD as being representative of wrench-style tectonics?

A Yes, I am.

Q Am I correct in understanding that you disagree with Mr. Heath?

A I don't really disagree with hi . I don't personally prefer to use the term "wrench style", and I don't feel that the zone that is -- the entirety of the OZD all the way along its length necessarily shows evidence of right or left slip, in this case everybody is interpreting as right slip, but I don't necessarily feel that the entire zone shows clear-cut evidence of that sense of shear.

Q Is part of your concern with this a matter of

definition of terms? I mean, would you be more comfortable with the -- with a description of strike-slip faulting, rather than wrench faulting?

A I prefer that. However, my concern is more that I have not seen what I would consider conclusive evidence that the zone is everywhere marked by right shear, that there is -- that there is indeed a throughgoing fault along the trend all the way along, that is referred to as the OZD.

Q I believe you said earlier that it was difficult to go on field trips offshore from San Onofre to investigate the surface expressions of the South Coast OZD, is that correct?

A One cannot see it from the -- from above the sea surface, and I don't do SCUBA diving.

Q Is it true that, according to your testimony, that the area now occupied by the San Onofre reactors and the area offshore, which has been designated the OZD, have been above and below sea level many times during the past few million years?

MR. PIGOTT: What do we mean by many? It is a little bit of a generalized --

BY MR. BARLOW:

Q Well, perhaps you could quantify for us how many times in your estimate the --

A If we go back to approximately 15 million years

ago, right after the San Onofre breccia was deposited, the area went below sea level. It remained below sea level until at least -- continuously, until at least 2 million years ago.

Now, depending upon the location on San Onofre Mountain, as the area rose relative to the sea level, the shoreline oscillated because of changes in sea level associated with glaciation and deglaciation, so that there were some oscillations.

Right at the plant site itself, I don't know how many times it has been exposed by glaciation with causing the lowering of the sea level and then deglaciation raising the sea level, but it is probably on the order of three or so times.

Q Could you give us an estimate of the most recent time during which the sea level was further offshore, nearer the OZD or the shelf edge?

A The most recent time would be during the midpart of the Wisconsin glacial epoch. That would be about 20 to 23 -- 20 to 23,000 years ago, is a commonly accepted value for the lowest end. At that time, it was about a hundred meters lower than it is today, or a bit over 300 feet lower than it is today, so it would be very close to the OZD at that time.

Q This puzzles me. I have heard other presentations by other consultants to the Applicants, where it was noted

that the sea level was out near the shelf edge 13,000 years ago. What is the discrepancy there?

A There is no discrepancy --

MR. PIGOTT: Objection.

THE WITNESS: The -- I mentioned the lowest stand. Sea level stayed relatively low until about 11,000 years ago, and then rapid deglaciation caused a rapid rise in sea level between 11,000 and 8,000 years ago, so it would have migrated -- I gave you the furthest out position, it then migrated shoreward, slowly, until about 11,000 years ago, and then started a rapid advance toward the present coast.

BY MR. BARLOW:

Q I see, so until about 11,000 years ago, the sea level was close to the OZD, is that correct?

A Well, it would -- you would have to look at the maps, and particularly the subsea profiles to see where the old marine bench is. It is quite a ways offshore, but whether it is midpoint between the two, I -- the shoreline and the OZD, I really couldn't say now without looking at a profile.

Q When the sea level was further offshore, would you expect that the creeks in the area would cause the deposit of layers of young Holocene sediments, in the area that is now beneath the ocean offshore from San Onofre, between the reactors and the OZD?

A During low stand of sea level, San Mateo and San

Onofre Creeks downcut their channels, and were depositing at a position offshore and carrying sediment into the ocean. Now, the ocean would redistribute it.

Only during a very low stand might the creeks spread an alluvial fan out over the old shelf, off to the sides. Now, I am not sure what you are driving at there, as to the location, whether you are talking about non-marine and marine sedimentation.

Q Well, the intent of my line of questioning is to try to understand how a scientist would go about analyzing the -- any evidence that might be available for surface faulting along the OZD. A scientist of contemporary time, looking at the OZD and trying to determine the nature of movement on this fault, whether it is a strike-slip fault or not, how recent the movement was, this sort of thing, and in that context, I would like to ask you, would you agree that when a fault zone is beneath the ocean, that it can be covered with young saturated sediments of sand or mud that would tend to obscure surface rupture evidence on it?

A If it is in an area where the surface was very flat on the sea floor, yes. The OZD is right at the shelf break, and well, it just depends upon what part of it you are looking at, but I think you would have to get to specifics.

There is no easy way to answer that one. If you

looked at specific profiles, one could say.

Q Do you agree that offshore from San Onofre, along the OZD, there are layers of soft saturated sediments on the surface of the sea floor?

A Just in from the OZD there certainly are. Right at the OZD, if you are talking about the shelf break, in places the bedrock is exposed and a number of the dart cores picked up bedrock.

Q But between the OZD and the reactors, there -- it would be -- tend to be covered with soft sediments?

MR. PIGOTT: I am going to object at this point, unless we can tie it into Dr. Ehlig's testimony. He is now getting into the precise interpretation of the sea floor offshore of San Onofre. I think that is probably better handled in Dr. Moore's testimony, which is admittedly in a different contention, but I don't believe this witness is necessarily the best witness to be asking about precise soil types in the various -- right at the vicinity of the OZD.

MR. WHARTON: Mr. Chairman, on this issue, the witness has testified as to one of the bases for his determination of the size of earthquake is looking at the amount of surface displacement, and what would be expected --

JUDGE KELLEY: Where is the testimony located?

MR. WHARTON: I am just looking for the pages right now, and I am just -- they don't jump out at me.

MR. BARLOW: Page 21 at the bottom.

MR. WHARTON: Yes, page 21, line 23, one, the absence of extensive and/or throughgoing fault ruptures in the near surface strata along much of the OZD was typical of faulting associated with earthquakes of less than M-7. This is one of the very bases that he determined the maximum would be seven. What Mr. Barlow's line of questioning goes to is the fact that because of surface sediment build-up and the depth of surface sediments, that those particular extensive and throughgoing fault ruptures in the near surface cannot be observed, because they are covered, and I think that is -- if that is what he is testifying to, it ... the basis of, the very basis of what he is saying, and I think he should be able to get into that.

JUDGE KELLEY: Objection is overruled. Proceed, Mr. Barlow.

BY MR. BARLOW:

Q Could you answer my question, Dr. Ehlig?

A Would you repeat the question?

Q Would you agree that along the sea floor between the San Onofre reactors and the OZD that the -- any faulting there would tend to be covered with layers of soft, saturated young sediments?

A I would certainly agree that they are saturated and soft if they are young sediments. One can see at least

two generations of terrace deposits on the sea floor in that area, and Dr. Roy Schlieman has presented testimony on the ages of those sediments.

Q Okay. I would like to turn to another line of questioning, if you could turn to page 21 of your testimony.

First of all, on line 17, where you refer to the maximum earthquake likely to occur along the OZD, would this be -- are you familiar with terms that have been used that are discussed in the Intervenor's testimony, and many other places, maximum credible earthquake, and maximum probably earthquake? Are you familiar with those two terms?

A I am aware of those two terms, yes.

Q Would you assign -- or would you equate your term maximum earthquake likely with either maximum credible earthquake or maximum probable earthquake?

A I would tend to equate it with maximum credible. I am not sure that there is a significant difference between the two, when actually applied to a situation such as this, using the criteria that I have used.

Q Does likelihood indicate some degree of probability?

MR. PIGOTT: Objection. I believe that we went over this ground rather thoroughly this morning. It is asked and answered.

MR. WHARTON: Mr. Chairman, could I ask for an

offer of proof from Mr. Barlow on this line of questioning to see how it differs from what I was talking about this morning --

JUDGE KELLEY: I think we can pursue it a little more. I do think this is an extremely important thrust here, as to among other things, to distinguish between terms of art and personal preferences in language, and I think that should be spelled out as clearly as possible, so go ahead, Mr. Barlow, not at exceedingly great length, but go ahead for a little bit.

MR. BARLOW: I am trying to hurry.

BY MR. BARLOW:

Q Do you recall the question, Dr. Ehlig?

A Well, would you repeat it? I don't.

Q Does the use of the term "likelihood" imply some degree of probability?

A Yes, it does.

Q Then would you term maximum earthquake likely be interchangeable with the term "maximum probably earthquake?"

A That would be a reasonable substitution of words, yes.

Q Thank you.

JUDGE KELLEY: May I just come back in on this, Dr. Ehlig? Do you also equate maximum credible with maximum probable?

THE WITNESS: In this terminology, yes.

JUDGE KELLEY: Because in ordinary English, it seems to me that those are not the same, in fact, they seem to me to be quite different. Now, if that is the way you want to use the term, you certainly can do it, but I would have guessed from a layman's standpoint that if someone told me that the maximum credible earthquake along a certain fault was seven, he was telling me that yes, it is conceivable, but it isn't likely, whereas, if he said that the maximum probable was, he was telling me, yeah, that probably will happen within some time range. And those to me are very different things.

Now, should I for purposes of your testimony equate the two?

THE WITNESS: Not in the context that you just put them. I would say that probably the highest value one could possibly expect would be a 7. I don't expect that to be the maximum value. I expect a somewhat lower value to be the maximum, whether that is 6.5 or 6.8 is another -- obviously, my technique of evaluating earthquake probability or magnitude probability and credibility is not a type of technique that will allow one to establish a precise value.

It is imprecise. The only thing that I would try to state here is that I see no evidence for a magnitude as large as seven having occurred on the zone, in that I don't see features that I would expect to be caused by such an earthquake.

JUDGE KELLEY: I have the sense you and I are talking about two different things. I think I understand this differentiation between 6.5 and 7. What I have difficulty with is what terms does one employ when one wants to talk first about what you think probably will happen as opposed to what you think is conceivable or is credible, if you will. Those are different things, aren't they, in your mind?

THE WITNESS: That's true. And if you were to ask me what do I feel is the greatest magnitude earthquake I would expect in the next thousand years along the zone, then I could give a fairly precise answer as far as my own expectations go. One problem that I have dealing with the zone offshore from San Onofre is that I am not certain that the zone is particularly active at the present time. I know it has been active in the geologic past. I would feel more comfortable if I were being asked about the Newport-Inglewood zone of deformation because there we have had a 6.3 and certainly one might anticipate another 6.3, though I would not anticipate another 6.3 for several hundred years in the same location.

I just don't know what the activity is in the offshore zone in the present regime. Now Ed Heath will be testifying on the basis of his studies. Nothing that I have seen says it is particularly active.

MR. WHARTON: Mr. Chairman, I might raise this

issue now that we had talked about collateral estoppal this morning. I think one of the areas we are getting into the collateral estoppal is the basic finding, the explicit finding that was made at the construction licensing hearing and that was -- we don't have any problem with identity of parties there because this particular decision stated in the Staff comments on res judicata and collateral estoppal, and there the Staff cites the record of the construction stage and says from these explicit and implicit findings, the Staff concludes it would be permissible for the operating stages to relitigate the evidence-- it would be impermissible to relitigate the evidence then relied upon regarding inactivity of Cristianitos fault and the basic characteristic of the OZD as an extensive linear zone of deformation at least 240 kilometers long, extending from the Santa Monica Mountains to at least Baja, California.

The testimony that has been submitted and the testimony being given right now appears to be an effort to again segment the OZD into three different sections and treat them differently. I think the finding was specific that it is an extensive linear zone at least 240 miles long and, for purposes of hearing, we have to treat it that way -- kilometers long -- it would be treated as one throughgoing fault. That is the way it was treated before and I think that is the way it should be treated now.

JUDGE KELLEY: Let me be sure I am following you. Are you quoting from the Staff's memo?

MR. WHARTON: Yes. I am quoting from the Staff's, who is quoting from the transcript. It's on page four of the Staff's submission on the comments on res judicata and collateral estoppal.

JUDGE KELLEY: I have it now. Can I look this over just a moment?

(Pause)

JUDGE KELLEY: Mr. Pigott, would you care to comment on that? Let me take a step back. You are directing this comment, this objection to a portion of Dr. Ehlig's testimony, is that correct?

MR. WHARTON: Yes. It probably will come up again with the use of hypothesized OZD, but it is also at this point Dr. Ehlig, my understanding of his testimony just given was that he would prefer to look at the Newport-Inglewood zone of deformation for purposes of activity and what he is looking at. But if you are looking at the South Coast offshore zone of deformation, he has some problems determining whether or not this is an active fault.

Now the question of whether or not South Coast offshore zone is an active fault and connects with the Newport-Inglewood zone has already been settled in this matter and we should not be relitigating that now.

JUDGE KELLEY: But you are not referring, for purposes of clarity, to anything he said in his direct testimony?

MR. WHARTON: No. I am referring to the statements that he had just made in response to one of your questions. For purposes of -- I guess it would be for purposes of a motion to strike, would be to strike any testimony that is stating that the offshore zones are separate zones, which he just seems to have testified to. I don't think we should be getting into that, nor should we hear evidence about that.

JUDGE KELLEY: Mr. Pigott?

MR. PIGOTT: Well, first of all, Mr. Wharton was not active at the construction permit stage and so I will assume that he is not deliberately attempting to mislead the Board with respect to the findings and the status of the use of the OZD and HOZD. But very clearly the Applicants have never accepted as a matter of substance the throughgoing nature of the offshore zone of deformation.

If we go back to the construction permit, you will find that it was continually called at that time a hypothesized zone because there was an effort not to litigate the underlying geology but rather to assume for purposes of setting design that it was in fact a throughgoing linear zone of deformation. We are not attempting to relitigate that particular question at this time and it does not appear in any of the issues.

However, we are litigating whether or not a maximum magnitude MS 7 is appropriate for this zone of deformation and in doing so we have examined the geologic characteristics of those -- of that zone as they actually exist and not under some hypothesis. Now we are not looking, again, in much the same manner as the Cristianitos, we are not looking for this Board to undertake a determination that the zone is in three segments or that there really is no zone or that there is a zone. That is not an issue.

But we do have to look at the characteristics of the zone and the characteristics vary from portion to portion. It is 240 kilometers long that we are dealing with. And it changes from, as you move from northwest to the southeast of the zone. And those characteristics are being discussed, but I think you will find they are being discussed without drawing the conclusion as to whether or not they are in fact connected. We are asssuming as a part of the earthquake potential that they are connected. But we are not trying to litigate in this proceeding whether or not that is the fact. But we are setting forth how the characteristics change from one end to the other and the effect that that has on the earthquake generating capability of the overall zone.

JUDGE KELLEY: Are you not submitting some evidence on particularly these seismic profiles, if I am choosing my term correctly, that have some bearing on the characteristics

of the OZD?

MR. PIGOTT: Certainly.

JUDGE KELLEY: Is there new information which you are proffering which has a bearing on the statement that Mr. Wharton is objecting to?

MR. PIGOTT: Well, what I am objecting to, first of all, is the absolutely incorrect mischaracterization -- total mischaracterization -- of a finding of fact coming out of the construction permit stage. That is just not correct, for a beginning. That is the first vice that I address.

When you ask what our testimony is, we are not attempting to litigate whether or not it is throughgoing or whether it is blocked off. We are litigating its earthquake capability and doing so by looking at the various geologic characteristics throughout the entire length of the zone.

JUDGE KELLEY: Yes, but what I am asking is if we have significant new information, maybe information that is perfectly consistent with what was in the prior proceeding, but if there is new information and it is relevant, that seems to me that has a bearing on whether or not the statements of the kind Dr. Ehlig made would be admissible.

MR. PIGOTT: The only new information that would have come forward would probably be additional detailed information showing that in fact the various segments do not connect. And that would be from the new seismic profiling.

But we are not putting --

JUDGE KELLEY: That is exactly what I am asking you about. That's what I want to know.

MR. PIGOTT: But we are not pushing that. We are still for purposes of earthquake generation purposes assuming that the three general areas are not disconnected. We are assuming that they are connected. But we are also saying that because they are different in different parts, you have to take that into consideration when looking at the earthquake potential of the overall zone.

JUDGE KELLEY: Mr. Wharton -- let me go over to Mr. Chandler. You started all this by writing a memorandum.

MR. CHANDLER: Mr. Vogler wrote the memorandum, sir.

JUDGE KELLEY: Well, there is a quote here from the licensing board decision. In its context, was this -- well, you lead in by talking about explicit and implicit findings.

MR. CHANDLER: Yes, sir.

JUDGE KELLEY: And then you pick up a quote. The notion that the OZD was a long, continuous fault, was that an explicit finding?

MR. CHANDLER: Yes, sir. There was an explicit finding by the Board, I believe I have identified the finding numbers on the prior page. In Finding No. 61 of the Licensing

Board decision, which is LBP 73-36, found at 6 AEC 929. At 943, the Board explicitly found that the appropriate geologic model was that set forth in the Staff safety evaluation report. It then made reference back to its finding 59, which is at 6 AEC 942, and it quoted a summary portion of the statement out of the Staff safety evaluation report, in particular, the characterization of the geologic model set forth in the USGS report in Appendix C, from which I have distilled the quotation there, the explicit finding from the Board being it is an extensive linear zone of deformation at least 240 kilometers long extending from the Santa Monica Mountains to at least Baja, California.

JUDGE KELLEY: And is it your point, Mr. Wharton, that extensive linear zone of deformation means extensive, if you will, continuous linear or is that not it?

MR. WHARTON: I believe that that is what Mr. Chandler just said, speaking from the Staff. It is in the safety evaluation report at that time. And that is what we are saying and that is what -- we don't want to relitigate whether it is continuous, extensive, 240 miles long, or any of that. That has already been decided.

MR. CHANDLER: If I may, let me just stress the point that at the construction permit stage, as at this point in time, the Staff has not stated or equated the offshore zone of deformation with a continuous fault, as Mr. Wharton

suggested earlier. We continue to be of the view that what we are talking about is a continuous zone of deformation.

JUDGE KELLEY: We will get into that in your case, I expect.

MR. CHANDLER: I expect we will. We believe that what the Applicant has proposed in its testimony is indeed consistent with the Board's finding at the construction permit stage. That is to say that the use of collateral estoppal would not bar the evidence that is being presented by the Applicants. In the first instance magnitude was not a consideration.

JUDGE KELLEY: Excuse me?

MR. CHANDLER: Magnitude was not a consideration at the construction permit stage. The case was based on intensities and ground acceleration. So this is indeed a new consideration.

JUDGE KELLEY: But you are saying that continuity of the fault was a consideration?

MR. CHANDLER: Continuity of the zone of deformation.

JUDGE KELLEY: Of the zone of deformation.

MR. CHANDLER: Not to be equated with the fault.

JUDGE KELLEY: Would you excuse me a moment. I believe I have a copy of the decision.

(Pause)

JUDGE KELLEY: Well, I'm not going to take the time now. Mr. Pigott, were you going to say something else?

MR. PIGOTT: Yes, I was. With respect to the same finding, which I think Mr. Chandler points to the appropriate finding, and I would refer that to your careful reading on this issue because it is rather important, but I would not some of the additional language other than as cited by the Staff. It says, quote: The Applicants ultimately prior to the hearing agreed to accept the Staff's more conservative view as the basis for the design. Accordingly, they agreed to the stipulation cited in paragraph 1 supra, which specifies that the adequacy of the design basis earthquake will be litigated in the framework of the geologic model set forth by the regulatory staff's evaluation. This model, of course, is the one set forth by the USGS in the quoted section of the report in paragraph 59 supra.

The Board has reviewed the information in the record and the Staff evaluation of that information and finds the Staff's model is the appropriate one for use in evaluating the effect of these facilities on the health and safety of the public. We note the Applicants' reluctance to concede that the Staff's model is a true representation of the situation. This was indicated by their effort to introduce prepared testimony attempting to counter the Staff's model and specifically stated in the Applicants' reply to the Staff's

proposed findings. We stated above the interpretation of the geologic data is susceptible to differences of opinion and future discoveries may well prove the Applicants' interpretation to be correct. Indeed, there may have been a small preponderence of evidence presently in their favor. The importance of the matter from a safety point of view and lack of overwhelming evidence that the Applicants' interpretation is correct however, require this Board to adopt the more conservative position, i.e., that the Staff model is the one to be used in evaluating the propriety of a .67G design basis earthquake -- end of the quote.

I would point out to the Board again that the model was accepted for purposes of litigating the appropriate seismic design basis. It was never accepted for the truth of the assumption. We are not now trying to controvert the model, but we are stressing that the model even as accepted one must look at the various geologic characteristics within that overall zone, and that is what we are attempting to do.

JUDGE KELLEY: Just so I understand you -- and I am familiar with that opinion that you just quoted from -- you are saying that at the CP stage at least on this issue -- did you litigate geology really at all?

MR. PIGOTT: No, we didn't.

JUDGE KELLEY: Or did you just argue about the model?

MR. PIGOTT: Rather than litigate geology, we assumed the model. In fact, if we had had to litigate it it would have been a donnybrook because we just simply don't agree with that model. But we can accept it for design purposes.

JUDGE KELLEY: And you did.

MR. PIGOTT: And we did.

JUDGE KELLEY: And now you are arguing that the geology is open in this proceedingand is litigible as such?

MR. PIGOTT: No. We are looking at the characteristics but we are not litigating whether or not it is purely segmented, whether or not it is one linear zone or whether it is three separate faults. That we are not litigating. We are assuming that it is linear. We are assuming that it is not blocked off. But we are looking at the characteristics as they change from one portion of the zone to the other.

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JUDGE KELLEY: Well, we are going to have some very, very tough decisions over the course of the next who knows how long. I am going to deny your motion to strike for a couple of reasons, Mr. Wharton. One, it is not at all clear to me that there was any explicit finding on this point. I am, as I said, I am familiar with that decision, and there was a stipulation as to model there, which I think has a bearing on this. It also seems to me that this is not something even that the Applicants are proferring, but rather it is an answer that you got when you asked a question on cross-examination, and sometimes you get something that you don't want, but that is a part of that process, so the motion is denied, and Mr. Barlow, why don't you go ahead.

BY MR. BARLOW:

Q Dr. Ehlig, you have two degrees in geology, is that correct?

A That is correct.

Q Do you have a degree in seismology?

A No, I do not.

Q Do you have a degree in geophysics?

A No, I do not.

Q Do you have a degree in tectono-physics?

A I don't even know whether such a degree is offered.

Q Okay. I assume you don't have one, then. In your

testimony on page 21, you offer a -- our method of estimating the maximum likely earthquake magnitude on the OZD, and you state, beginning on page 24 -- excuse me, page 21, line 24, quote, the absence of extensive and/or throughgoing fault ruptures and near-surface strata along much of the OZD is typical of faulting associated with earthquakes of less than about magnitude MS-7, and then you go on, for larger earthquakes, the high rate and large amount of ground displacement during such an earthquake would favor propagation of faults to the surface, and would also favor extensive secondary faulting and lurching near the surface.

Has this method of yours, of estimating the maximum likely magnitude, has this method been established in the scientific literature? Have you published on this?

A I have not published this particular method. On the other hand, there are lots of published articles which indicate the degree of ground deformation associated with various earthquakes, and I am familiar with quite a few of them.

Q Has your technique or method which you use in your testimony for estimating the maximum likely magnitude been subjected to peer review?

A In terms of the way I stated it earlier, I personally have not had my method subjected to peer review, no. I believe one can find plenty of excerpts in the

literature that would use the same criteria.

Q Can you quote other authors who have established such a relationship in the scientific literature?

MR. PIGOTT: Excuse me, could we have a clarification? Are you talking as to the use of the general approach of Dr. Ehlig to establishing the magnitude, or are you trying to focus in on a particular finding with respect to this zone and this location?

MR. BARLOW: Well, at the moment, I am discussing the method which Dr. Ehlig presents in his testimony for estimating maximum likely magnitude.

MR. PIGOTT: Okay, so you are not now tying into the -- into this particular determination?

MR. BARLOW: Well, he applies it to this particular situation. I am trying to establish whether or not the method has been subjected to peer review, and whether or not he can quote authors who have established this method in the scientific literature.

THE WITNESS: One can cite examples such as Shor and Roberts on the San Miguel, and the surface deformation there, which was associated with the 6.6, 6.8 earthquakes in Baja California. One can use the Tehachapi Arvin (ph) earthquake of 1952 as examples of the kind of deformation. That is -- Division of Mines put out a bulletin on that. I believe it is bulletin 152, if I recall, it was California

Division of Mines.

There was a great deal of published information on the San Fernando Earthquake, in which that fault did propagate up through a sedimentary section. Certainly, the 6.3 1933 earthquake has quite a little information on it.

As for specific authors, using these kind of arguments offhand, I can't quote any name of anybody who has attempted to use this specifically. I believe Burt Slemons in his report on earthquakes has cited geologic features that are associated with various magnitude earthquakes, and I think you could find that in many fairly generalized texts, as what is or is not associated with a particular size earthquake.

BY MR. BARLOW:

Q On page 22 of your testimony, line 2, where you use the term "high rate," could you define that term?

A Which line is this?

Q Line 2, page 22.

A The velocity with which the crack propagates is a function of magnitude, to some degree, and with the larger magnitude earthquakes, the shear propagates at a higher rate than at a lower magnitude earthquake, and there is a close relationship between amount of displacement and magnitude of earthquake, so the higher the magnitude, the faster the crack propagates, the more brittle the material behaves, brittle-ly, and the higher the magnitude the more

displacement and the more likely it is that the material will be stressed to the point where it ruptures.

Q What mechanism of rupture are you assuming to conclude that magnitude 7.5 earthquakes would rupture into the shallow sediments?

A Would you repeat that?

Q What mechanism of rupture are you assuming to conclude that magnitude 7.5 quakes would rupture into the shallow sediments?

A I am not sure I understand what you mean by mechanism. I would assume that the rupture would occur on a steeply inclined plane at depth in the basement rack, and that it would propagate from a point of rupture, perhaps five to seven, five to ten kilometers below the surface, would propagate both upward and laterally.

Q Could you state what data base you are using to determine the difference between magnitude 7.0 and magnitude 7.5 earthquakes in terms of surface rupture observable?

A Oh, I could refer back to statements by Slemmons. When you say data base, I do not have a specific collection of earthquakes that I am referring to.

Q Do you have a specific set of faults that you are referring to?

A Certainly I could refer to the San Andreas fault, the San Jacinto fault. I could refer to the Newport-Inglewood

zone of deformation. I could refer again back to various literature, published literature, in textbooks, generalized statements.

Q Have you compared --

MR. PIGOTT: Excuse me, has the witness completed his answer?

THE WITNESS: Yes.

BY MR. BARLOW:

Q What is your evidence that higher magnitude earthquakes travel at a higher rate?

A I believe you will find statements on rate in -- I am trying to remember the name of the text now. The Hausner -- right now the names of some of the authors escape me, but there are many references which deal with the subject of rate of propagation, how fast a rupture surface propagates, as versus magnitude, and distance with which it goes.

Q Have you compared your model with events on the Imperial fault, including the 1940 El Centro event of magnitude 7.0 or larger?

MR. PIGOTT: I am going to object to the characterization of the witness's testimony as it being a model. I think he has given us a judgment and an opinion, but I do not see anything that would rise to the level of a model, and I object to that characterization.

JUDGE KELLEY: Are you referring -- I don't recall "model" either.

MR. BARLOW: I could rephrase the question, Your Honor.

JUDGE KELLEY: All right, go ahead.

BY MR. BARLOW:

Q Dr. Ehlig, in applying your method for calculating the maximum likely earthquake on the OZD, have you tested your method against the data base available regarding surface rupture and earthquakes on the Imperial fault in the Imperial Valley?

A I am dealing with this from the basis of geologic observations. I have seen the Imperial fault. I noted that it ruptured to the surface with a magnitude of less than seven. It goes through a very thick section of sediments. We know that the Imperial fault can have magnitudes slightly higher, or suspect it can have slightly higher than 7, so I am not sure how that relates directly with the Newport-Inglewood, or -- zone of deformation, or the offshore zone.

Q Have you studied the geologic evidence along the Hosgri fault zone, which is offshore of California?

A No, I have not.

Q Are you postulating with this method that earthquakes smaller than approximately magnitude 7.0 would not

leave evidence of surface faulting?

A No, I am not. Evidence of surface faulting certainly has occurred on lower magnitude. The --

Q What threshold would --

MR. PIGOTT: Again, I am not sure that the witness has completed his answer.

MR. BARLOW: I am sorry.

JUDGE KELLEY: Perhaps you could pause a little longer. Go ahead.

THE WITNESS: Okay, on this case of the San Fernando Earthquake, of February 9, 1971, that propagated to the surface, and depending on what data you look at, it is about a six point four magnitude.

Things as low as 5.5 are known to propagate to the surface, so it varies with the site conditions. There are other earthquakes that have occurred of magnitude in the range of six and a half that have not propagated to the surface.

Newport-Inglewood, of course, the Long Beach earthquake, may or may not have propagated to the surface.

BY MR. BARLOW:

Q Is there any evidence today along the Newport-Inglewood fault zone that an earthquake of magnitude 6.3 occurred there in 1933?

A Yes, there is.

Q Could you explain that to us?

A I personally have not seen the direct evidence. I think that Ed Heath would be an appropriate one to discuss evidence, at least for the Long Beach earthquake. I have certainly seen fissures -- or rupture surfaces in places like Cherry Hill, but as far as Baldwin Hills, I have looked at surfaces there. They are not, by the way -- the surfaces I have seen are not directly related to the proposed Newport-Inglewood fault. They are not along exactly the same trend. They are subsidiary faults.

Q Would you expect earthquakes of greater than magnitude 7.0 to always propagate to the surface and leave evidence of surface rupture?

A I should think by the time they got to that magnitude, they would tend to, if they were nearly vertical faults.

Q In terms of geologic evidence that you might examine long after earthquakes had occurred, would you be able to tell the difference between displacement that resulted from two magnitude MS-7 events versus evidence that might have been left by one magnitude 7.5 earthquake?

A One might be able to tell that there were two events versus one, if surface ruptures at, say the effects of liquefaction, had resulted in deformation of near-surface sediments, and then new layers of sediment were laid over the

top, and then another earthquake came along and disrupted the younger sediments. You would see two different events, and might see a very disturbed horizon, perhaps with some of the fault traces coming up to the top of that horizon, and then truncated or overlain by a younger horizon, which would then have only the youngest fault breaks propagate through it, so you might do as Kerry Sieh did at Pallette Creek, and be able to work out a sequence of history.

Q Would you be able to tell the difference between surface displacement that occurred during five MS 6.5 earthquakes, and one MS 7.5? Looking only at geologic evidence?

A I am not -- would you repeat that?

Q Okay, to put this in context a bit, if the surface displacement that occurred during a number of smaller earthquakes equalled the surface displacement or slip that resulted on the same fault during a one larger earthquake, would you be able to tell the difference between five MS 6.5 events and one MS 7.5?

A You might or might not, depending on what kind of record you were working with, what kind of sequence of rocks you were working with. If you were dealing with something that was entirely within basement rock and not producing scarps or other features, you might not be able to tell how many earthquakes were represented by any given displacement,

one needs ideal conditions with soft rocks overlying the zone, or a continuous sedimentary history synchronous with the earthquakes in order to be able to work out the number of events.

Q When you stated an opinion that you would expect to observe extensive evidence of near-surface faulting for magnitude 7 or greater earthquakes, what degree of certainty do you have for that expectation, and what evidence do you have for that expectation?

A Again, from the one standpoint in working along the San Andreas fault, one sees essentially continuous ground rupture. One sees many effects from local ground deformation. When one gets to smaller faults, the effects are less pronounced. Now, one of the things, there is extensive liquefaction associated with larger earthquakes, and one ought to see the evidence for that.

Again, going back to the general literature, I believe there is an ample literature to indicate that in the case of the larger earthquakes, when one gets above about 6.5 or so, they tend to propagate to the surface, and tend to cause surface rupturing, particularly when they are vertical faults.

Q Would this apply to zones with thick sediments?

A The thicker the sequence of sedimentary rocks over the zone, the -- as long as they are soft sedimentary rocks,

the less likely it would be that the shear surface would propagate to the surface during any given event.

If the shear surface has propagated up partly up through the sedimentary pile during a previous event, then there would be uncoupling between the two sides of the shear surface and during a future event, one might expect the crack to continue propagating upward, so that it probably would not take long with large events to propagate it to the surface, but if one were starting a brand-new fault beneath a section that was 15,000 feet thick, it might take a while to propagate it up.

Q Would the 1956 earthquake on the San Miguel faults zone, in Baja, which you referred to earlier, necessarily have to have ruptured through a thick layer of overlying sediments?

A In that particular case it did not. The basement rock is very close to the surface.

Q Is it possible that the San Miguel fault zone is an incipient fault zone which is working its way toward the surface?

A Well, it has ruptured through to the surface. It has a very small displacement on it. One of the problems with referring to the San Miguel fault is that the fault which broke in 1956 shows evidence of good surface fault features. It shows that it has been recently active. The features from the '56 break are very well preserved. When one

goes to the north part of what is called the San Miguel fault, and the two do not interconnect, as far as I can tell, from reviewing aerial photographs, one sees no surface evidence for young rupture. What one sees at the southeast side of San Rafael Valley is old alluvium going across the fault zone.

So far as I could tell, the fault zone has been inactive for a very long period of time, so I am not sure that it is the same continuous fault zone, but what it does appear is that the north branch or north path of the San Miguel fault has been around for a long time. It has got an old alluvial surface across it with a soil zone on it.

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tp #11

Q You are saying that the San Miguel fault zone is not an active fault zone?

A I am saying that the one which -- part which broke in 1956 is clearly active. I am saying what has been called the San Miguel fault zone along the north side of the San Rafael Valley is not necessarily an active fault zone.

Q Dr. Ehlig, have you attempted to quantify the slip rate and the amount of displacement along the San Miguel fault zone and/or the Vallecitos fault zone in Baja?

A No.

Q Have you studied the slip rate or amount of displacement on either of those fault zones?

A The amount of displacement I have looked at, yes.

Q Could you quantify that for us?

A In the San Miguel fault in the area just southeast of what was the community of San Miguel, the displacement appears to be on the order of oh, at most 200 meters. There is an unconformity between the basement and the overlying sediments that has downdropped on the southwest side. The slickened sides that are exposed in an excavation that was made across the fault plunge, that is, they go downhill, at about 41 degrees where I measured in one of the trenches. It would appear that the south branch of the San Miguel fault has about equal amounts of dip slip displacement and right slip displacement. The dip slip is a normal dip slip with the

west side down. And it is on the order of a matter of say maximum of about 200 meters, I think. I actually believe it is less than that but I don't have precise control there.

On the north part of the San Miguel fault, near the east edge of the San Rafael Valley, the zone is exposed in an area of basement where one can trace dikes and actually a septa of metamorphic rocks within a granitic pluton that are offset. The displacement of nearly vertical dikes is on the order of 240 meters. Immediately to the west of a major stream, the zone is overlain by old alluvium and so far as I could tell, the alluvium was not offset.

So there we are looking at something with a very relatively small total displacement as far as faults go and no evidence of holocene activity. Now I haven't dated the old alluvium, but just to look at the degree of weathering and all, again, by comparisons based on my field work, it would appear that the terrace deposits or the old alluvial deposits are many thousands of years old.

In the case of the Vallecitos fault, along its southern part I was unable to detect any offset in dikes. Now the exposures are not so good that I could tell that I could spot a fault say with 10 meters of displacement. But I hiked along the zone, I looked for evidence of offset. There is a lineament there and I will assume that there is a fault there. But the striking thing you see is a valley. It lies along a

valley that has old alluvium with a very deep red soil developed in areas where gabro was present or gabro debris was present. So far as I can tell, there is no evidence of young displacement. In fact, if you go back to Gordon Gastell's work in the memoir of 140 of the Geological Society of America and to other publications by him, you will see that there is no evidence that he has found or others working with him have found for any quaternary displacement along the Vallecitos.

So so far as we can tell, it is -- in fact, I believe he states that there is no evidence for cenozoic displacement. It appears to be a relatively old, inactive fault.

Q Is it possible that there is a data gap there in terms of sediments being older sediments without the presence of younger sediments, which would make it difficult to determine the age of most recent displacement?

A At the north end of the Vallecitos there are eocene rocks. Exposures are not sufficient to really be sure that ruptures don't go into the eocene rocks, but there is no geomorphic evidence or anything that one can see in field exposures to indicate that the rupture does go into the eocene rocks. In the area that I worked along the Vallecitos at its southeast end, the terrace deposits I suppose could be dated, but they have a very mature soil horizon on them, a lateritic type soil, very, very good development of hematite. I think that in a -- without being precise, one can conclude

that such soil horizons take many thousands of years to develop. They are on top of old gravels which are deeply incised. They are just remnants of valley fill.

Again, I have no way of dating it precisely. I have not done the kind of work that would lead to precise dating, but certainly there is no suggestion that I know of that the fault has moved within the holocene and probably within quarternary time.

Q Earlier you stated an opinion that the San Miguel fault zone has a low slip rate. Could this be based upon your analysis of the amount of displacement on the San Miguel fault zone?

A I don't recall that I stated it had a low slip rate.

Q I'm sorry. Let me ask you then. I thought you had said that. Considering your discussion of the amount of displacement on the San Miguel fault zone, with 200 meters on the southern section and 240 meters on the northern section, would you -- would it be your opinion that the San Miguel fault zone has a low slip rate relative to the San Andreas fault system faults?

A My assumption, of course, is that it does have a relatively low slip rate compared to the San Andreas. In order to establish slip rate, one must know the period of time during which the fault was active and take the total

displacement and divide it by the length of time during which the fault was active. I don't know how long the fault has been active. But one point I was trying to make is that the period of activity for the southern San Miguel seems like it is different than what the period of activity for what is called the northern San Miguel -- and in the case of Vallecitos, there is no geomorphic evidence for young displacement. I don't know how long it was active, though. Nor do I know, in its case, what the total displacement is, other than it is very small, probably less than 100 meters in the area where I looked in the southeast portion of it.

Q Considering the southern San Miguel fault zone, can you estimate the period during which it was active?

A The last bit of activity was very recent, obviously, because one can still see the effects of the 1956 earthquake. They are amazingly well preserved, including juniper trees that are tilted and uprooted and, if I were to look at it and try to guess when the earthquake occurred, I'd put it much more recent than 1956. But other than that, I don't know of any special way of knowing when it was previously active.

Q Are you familiar with the -- first of all, before I change lines. Do you know of a method to estimate the period during which the southern San Miguel was active?

A It would take a considerable amount of field work,

I suspect, to try to find data. One thing might be that streams cross the fault. One might be able to find a sequence of offset stream channels, stream deposits. I have not done that.

Q Are you familiar with the Tijuana lineament?

A I know what is referred to as the Tijuana lineament.

Q Would you agree that the projected strike of the Vallecitos fault zone is parallel or subparallel with the Tijuana lineament?

A The Tijuana lineament, so far as I know, does not have a precise trend. It merely is parallel to the Tijuana Valley. One could get a fair range of trends if one wished to out of such a broad feature. The Vallecitos fault is roughly parallel, but lies to the west or projects to the northwest of the Tijuana Valley.

Q Is the Vallecitos fault also roughly parallel with the San Miguel fault zone?

A Yes, they are approximately parallel.

Q Then collectively are the San Miguel fault zone, the Vallecitos fault zone and the Tijuana Valley lineament all three roughly parallel with each other?

A Well, one could draw lines that would be nearly parallel. Again, the valley itself is a very broad feature. It has a more northerly trend than the faults themselves.

Q Is the trend of these three features, the San

Miguel fault zone, the Vallecitos fault zone, and the Tijuana Valley lineament, is the trend which could be parallel of those three features roughly parallel with the projected strike of the Rose Canyon fault zone?

A The Rose Canyon fault zone, where it has been traced, in San Diego Bay tends to go more southerly and again it would not hook up directly with the Tijuana lineament. The La Nacion fault zone would probably hook up better with the Tijuana lineament.

Q Would that form an en echelon type pattern between the Rose Canyon fault zone and the Tijuana Valley lineament?

A The Tijuana -- well, since they are staggered or they are not in alignment, one could make them en echelon. That would be true of any two lines that aren't overlapping -- in fact, they can overlap -- any two lines one draws that are subparallel can be said to be en echelon. They are in a somewhat staggered arrangement.

Q Are you familiar with any other fault zones in southern California or Baja where there is a similar pattern of en echelon parallel faults?

MR. PIGOTT: I'm going to object. That is an assumption that the three faults that he has been talking about comprise a fault zone and I think that is an assumption of something not in evidence.

MR. BARLOW: I did not use the word "fault zone".

JUDGE KELLEY: Would you care to rephrase it or restate it?

MR. BARLOW: Okay.

BY MR. BARLOW:

Q Dr. Ehlig, you stated that you studied faulting along the San Andreas and San Jacinto fault zones. Are you aware of en echelon patterns of faulting along those two fault zones where you have a similar situation as to what we have been discussing, where you have strike slip faults that are parallel or subparallel in an en echelon sidestepping fashion?

MR. PIGOTT: I'm going to object again. There is still this -- it is extremely ambiguous as to whether or not this same situation implies some kind of a connection of these faults and that has never been agreed to by the witness. Again, I guess it is argumentative is what it is and I would object.

JUDGE KELLEY: Well, I am going to overrule the objection. I think that the witness can build in the concern you have expressed and he can disagree if he wants to.

THE WITNESS: Along the San Andreas fault on a very small scale there are many en echelon breaks. The scale we are talking about is quite different along the San Andreas or San Jacinto fault, it is quite different from

the scale we are talking about along the northern San Miguel versus the Vallecitos. Northern San Miguel is about, as I recall, 7 or 8 kilometers to the west of the trend of the Vallecitos. In the case of the San Andreas fault or the San Jacinto fault, we are talking about breaks that are hundreds of meters apart generally or the thing splits and then rejoins in some way, not necessarily in an echelon pattern. But the scale is quite different.

BY MR. BARLOW:

Q You say that the separation between the Vallecitos fault and the San Miguel fault is 7 or 8 kilometers?

A As I recall, it is.

Q Are you familiar with the distance between the Imperial fault and the Sierra Prieto fault?

A I don't know the precise distance between the two.

Q Would you characterize those faults as en echelon faults that are parallel or subparallel within the plate boundary zone?

A The two faults, as I recall, are separated by a spreading center and, consequently, there is a mechanism transfer the motion from one, the Sierra Prieto, through a spreading center and then on to the Imperial fault. No such mechanism exists in Baja.

Q Is it possible that the en echelon surface patterns along the San Andreas fault zone and the San Jacinto fault

zone are surface expressions of a deep linear basement fault? Would you agree with that characterization?

A I suspect a depth along both faults there is a fairly throughgoing fault surface. Now in the case of the San Jacinto, it looks like it has broken in segments through time and one cannot be sure that there is in fact at great depth a continuous throughgoing shear surface as versus a more or less overlapping shear surface with a deep seated mylonite zone or a crush zone in between.

In the San Andreas there are local complications that are the result of compression across the zone or secondary deformation that may well be disrupting the zone at depth. If you had specific examples of en echelon faults that you wanted to give me, I might be able to give a more precise answer.

Q Well first I will ask you this question because this is more to the intent of the line of questioning. Would you agree that in southern California and in Baja California that it is possible to have a deep linear fault in the basement that does not express itself at the surface in terms of -- with evidence of continuous surface rupture?

A Only if the displacement, total displacement is very small and only if the rocks are reasonably flexible. If we are talking about very rigid rocks, i.e., basement rocks exposed at the surface, then I don't think it is

possible to have a throughgoing zone without having some combination of surface interconnections betwee the various breaks.

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Q Would you agree that it is theoretically possible that there is a deep linear fault zone in the basement rocks beneath the San Miguel, Vallecitos, and Rose Canyon fault zones?

MR. PIGOTT: Objection again to the characterization of something not in evidence, that the three are fault zones.

JUDGE KELLEY: Could you restate that for me, please?

MR. BARLOW: Okay.

BY MR. BARLOW:

Q Dr. Ehlig, would you agree that it is theoretically possible that there is a deep linear fault zone in the basement rocks beneath the Rose Canyon fault zone, the Vallecitos fault zone, and the San Miguel fault zone?

A I would agree that each of those zones extends to depth, but I would not agree that there is or could be an interconnection between the zones individually.

Q Do you agree that those three fault zones are parallel when you project the strike of the faults within the zone?

A They are roughly parallel to each other.

Q Are they roughly in line with each other when you project the strike of the three zones?

A No, they are not in alignment with each other.

Q Are there in an en echelon pattern of alignment?

A Yes. The San Miguel to the Vallecitos is a right-stepping alignment. We would have to project the Vallecitos quite a ways to get to the -- Rose Canyon. I am not sure how it would line up.

Q Would that projected strike of the Vallecitos towards the Rose Canyon fault go through the Tijuana Valley lineament?

A I believe it would lie to the west of the main part of the Tijuana Valley. I would have to look at a map.

Q You said that you have examined aerial photographs and made a limited geologic reconnaissance of this area between the Rose Canyon fault and the Vallecitos fault.

A That is correct.

Q Do you see in that area the possibility of an alignment of an en echelon fashion between the Vallecitos fault and the Rose Canyon fault?

A The two do not project down close enough to really fit an en echelon model, so far as I can tell. Now, I am having difficulty answering your question, because the geology is such that the two do not come close to each other. The Rose Canyon does not come close to the Vallecitos.

Q By that answer, do you mean that there is a data gap between them?

A Not a data gap, but a gap in which we do not know

of any throughgoing faults with the northwest or north-northwest trend.

Q Is that lack of knowledge due to lack of research?

A That is a philosophical question. As far as looking at surface exposures and looking at air photos, colored air photos of the region, one does not see any features going through. In fact, now that you mention it, there are some northeast trending faults that have been mapped by Minch and are well known, that lie along part of the zone, that extend from the coast into the Tijuana River, and these would tend to block any throughgoing faults that would be along the Vallecitos trend.

Q Do those faults cross the Tijuana lineament?

A Are using the river as the Tijuana lineament.

Q Let me ask you, are you using the river as the Tijuana lineament?

A I didn't invent the term. A lineament refers to a line-like feature. In Gordon Gastil's publication --

JUDGE KELLEY: Excuse me just a moment. Madam, I am afraid that you will have to take the baby out if you can't get it quiet. We do have to have an atmosphere in which the witness can focus. Thank you.

MR. PIGOTT: Does the witness have the question in mind?

THE WITNESS: Yes. In the references to the

Tijuana lineament, the lineament refers to the Valley in general, I believe. If you have the text, you might go back to it, but it is not specific as to where the line is supposed to be. It just mentions a number of features that occur on either side the Valley, that various people apparently have noticed, that seem to suggest that there might be differences across the valley.

BY MR. BARLOW:

Q Well, if we call it the Tijuana Valley, do these northeast-trending faults that you mentioned cross the Tijuana Valley, are they evident on both sides of the Valley?

A They extend to the main -- about to the Tijuana River, or have been traced that far. When one gets east of the Tijuana River, the terrain has rather extensive soil cover and landslide-affected soft sediments that are not too conducive to tracing some of these features.

Q Is it therefore possible that these northeast-trending faults are truncated by a fault beneath the Tijuana Valley?

MR. PIGOTT: I am going to object. I don't believe that we have established the fault going along the floor of the valley, and I also object to the continued form of the question, is it possible. I am assuming that the witness is answering it in the realm of realistic possibility, but as everybody who has played this game knows, anything is

possible.

MR. WHARTON: Mr. Chairman, if I may comment on this, an expert witness can ask -- can answer the question. If he cannot say it is possible, if he cannot say he can answer it that way, he could state that. Anything -- an expert witness can also answer, and give an opinion on something based on a hypothetical if it is a hypothetical. It doesn't necessarily have to be evidence which is firmly established in the record as of this time. It seems to me like it is an appropriate question.

JUDGE KELLEY: What portion of the testimony is this directed at?

MR. BARLOW: It is directed to number 9 on the outline.

JUDGE KELLEY: You have been on 9 for some time, right?

MR. BARLOW: Yes, sir. I am almost complete with it.

JUDGE KELLEY: And but what portion of the witness's testimony does this really relate to?

MR. BARLOW: Oh. He has a discussion of the Vallecitos fault zone, which goes from page 29 to page 33.

JUDGE KELLEY: And would you just repeat the question?

MR. BARLOW: Yes, sir.

BY MR. BARLOW:

Q Dr. Ehlig, in the context of our discussion of a possible structural relationship between the Vallecitos fault and the Rose Canyon fault, we have postulated a possible fault running through the Tijuana Valley, which is parallel to the Vallecitos fault and to the Rose Canyon fault. In discussing this postulated fault, you said that that fault might be truncated by, or blocked by northeast-trending faults, which ran between the coast and the Tijuana Valley.

My question to you is if you do not observe these northeast-tranding faults on the east side of the Tijuana Valley, and you only observed them on the west side of the Tijuana River, therefore is it possible that the postulated fault beneath the Tijuana Valley truncates these northeast-trending faults which you have mentioned?

JUDGE KELLEY: I am going to overrule the objection.

MR. PIGOTT: So long as it is clear that the postulations are those of the Intervenors and not the witness.

JUDGE KELLEY: That was my understanding. It is your postulation, correct?

MR. BARLOW: We postulate the fault in the Tijuana lineament.

JUDGE KELLEY: Right. Very well. Go ahead, Dr.

Ehlig.

THE WITNESS: It is easy to hypothesize such a fault, and I would have to agree. However, if such a fault exists, it should continue southward through the area south of the Tijuana Valley, and in searching through the basement terrane there, I can find no feature that passes on thrcugh the basement terrane that is underlaying -- it is Allecitos (ph) Formation, volcanic rocks of Mesozoic age, and they are -- I see no evidence for a fault zone going through there.

BY MR. BARLOW:

Q Do you see any evidence that would contradict the possibility of a fault zone going through there?

A There are a number of cross-cutting features that would suggest that a fault zone does not go through.

Q What is the -- excuse me.

A Joints, linear trending features. There are certainly similar rock types all the way across the zone. There is a sequence of Eocene conglomerates in through there that show no evidence of offset.

If one were to postulate a fault of very small displacement, say on the order of tens of meters, I suppose it would be possible for it to go through there without seeing solid evidence for it, and I won't argue one way or the other on that.

On the other hand, if one were to try to

hypothesize a very young fault through there, I would expect to see some geomorphic evidence of offset strains or other features which would -- could be seen on air photos, or readily seen on the ground, that would indicate the presence of an active fault, and I don't see those features.

Q In your testimony on page 29, you state that you have made a -- it is line 15 -- you say, quote, you have made a limited geologic reconnaisance of the area. Can you tell us the limitations or describe the extent of the research that you have done in this area?

A Well, at the time this testimony was prepared, I had spent, oh, I guess ten days of field work since last Christmas, in Baja California. Most of that was the week between -- from Christmas until New Years, of which the better part of one day was spent actually looking at the area, near the south part of the Tijuana Valley.

I had previously been down there on geologic field trips, but not specifically for the purpose of trying to determine whether a fault went through. I had looked at the geology on a number of occasions in the past.

Since the time of the prepared testimony, I have spent two days specifically working in that area to see whether I could find any throughgoing features, and that work included travelling across roads that extend between the Tijuana to Tecate, the highway that extends off from that to

valley, Las Palmas Valley, and along various roads throughout that area, as well as some hiking, as well as further review of the air photographs, and as a result of that work, I have found nothing that would suggest a throughgoing fault in that area.

Q You said that you spent one day looking at the southern end of the Tijuana valley. Did that include the area between the Tijuana Valley and the Vallecitos fault zone?

A When you say one day, that was at the time of this testimony. I had spent, oh, about three years ago, time looking at the Vallecitos fault, and had gone very close to the north end of the Vallecitos looking at that, and then at around Christmastime, just after that, I spent the better part of one day looking in around the south end of the -- of the Tijuana Valley.

Now, what kind of information specifically did you want on what areas I looked at at that time?

Q Well, I was trying to determine the extent of your research between the northern end of the Vallecitos fault zone, and the southern extent of the Tijuana Valley lineament.

A Okay, I have approached the north end of the Vallecitos via dirt roads, very close to where it can last be traced in Las Palmas Valley, up from the north end, where there is Eocene conglomerate, and have travelled, I think,

a better part of the dirt roads in that area, as well as hiking off to the sides where it seemed appropriate, as well as looking at air photos, and that is all I can say on that.

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tp #13

Q What is the distance between the northern end of the Vallecitos fault zone and the southern end of the Tijuana Valley?

A I would really need a map to measure that, but I would guess it is on the order of 15 - 20 kilometers. I'm not sure.

Q You would approximate 15 to 20 kilometers?

A That may be a little on the long side, but I'm not sure.

Q It could be less than that?

A Yes.

Q What is your estimate of the slip rate on the Vallecitos fault zone since cretaceous time?

A I have no basis for estimating the slip rate. If one were to average -- you say since cretaceous, which is the last 65 million years -- if one were to merely take the displacement along the Vallecitos -- and I'm not sure where one would get solid data on how much displacement. My data would suggest that it is a very small displacement, less than a kilometer. If one were to take say a kilometer and divided it by 65 million years, you could come up with a slip rate, but that wouldn't necessarily mean anything because it probably wasn't active during all that period of time.

Q What would you estimate the slip rate to be during the past 5 million years?

A I don't know that it has been active during the past 5 million years.

Q Okay. Moving on to a different subject, the final area to discuss with you, you describe the Cristianitos fault as a listric normal fault. Would this mean that the Cristianitos fault is likely to flatten with depth?

A That is correct.

Q Considering this -- let's see, did you have a figure that shows this?

A I had no figure to -- well, I have -- PLE-L shows a cross section that I have used in the argument. I show an example for reserve drag I showed PLE-K and for another example of features for a flattening of a fault, PLE-N, but that is the Pelican Hills fault. I do not show a cross section of how it extends at depth.

Q In the PLE-K figure in your testimony, if you were to draw this Cristianitos fault into the deeper layers what would it look like? Could you possibly draw that on that figure or would you need a larger sheet of paper?

A It would start to flatten as one proceeded downward on the fault. Whether you could actually construct a precise cross section from the data given, it was a matter of conjecture. I think one could probably suggest that it would flatten a few hundred feet down it would start to flatten in this particular case. This is probably due to

local flattening of the surface. It is certainly not a deep seated flattening.

Q Perhaps your figure PLE-M seems to show a deeper section. At what depth would the listric normal Cristianitos fault begin to flatten?

A The flattening normally is a progressive sort of thing and when you say when would it start to flatten, it is curved. It is a characteristic feature of a listric fault. It is entirely possible that it would get down to a bedding plane at a depth of oh, 10,000, 15,000, 20,000 feet. I don't have enough control here to say where, exactly where it flattens down to a bedding plane, say.

Q What sort of research would allow you to determine the depth at which the Cristianitos tends to curve or flatten?

A Well, within the main part of the embayment there are enough oil wells or drills, holes drilled for oil, that there is fair control down into the cretaceous strata. In those areas one could hypothesize where the fault might flatten or what depth one might expect the fault to be at any given distance from its surface trace. You would probably need deep drill hole data to really get precise control. Seismic would probably not give definitive results.

Q Looking at your figure PLE-N, where you look at a crossection showing the Pelican Hill fault zone, I believe you said in your testimony that there are a number of normal faults

in this region which are similar to the Cristianitos fault and you cited this fault zone as one of them. Would you characterize the Pelican Hill fault zone as a listric normal fault?

A Based on what I see, yes.

Q Could you tell me approximately how far this fault zone is from the Cristianitos fault zone?

A I would need a map to measure distances.

Q Is it within the Capistrano Embayment?

A If you will turn to PLE-O, in the San Joaquin Hills you will see an 'X' right below the word "sand", below the 'n' in "sand". Right there is a dot 'P', which represents Pelican Hill. The fault passes through Pelican Hill and is trending -- it is dipping off to the west in that area. So perhaps 20 kilometers from the Cristianitos.

Q Thank you. Would you expect these two listric normal fault zones to be similar in terms of the depth at which they begin to curve?

A They might or might not be. I see no reason why they necessarily would be. It is like landslides where the sole of the slide is along bedding. Nothing dictates that one slide can't be deeper than another.

Q Have you studied the faults in the Capistrano Embayment which were mapped by Jack West for the Applicants?

A I have reviewed Jack West's work, yes. When you

say study them, most of the faults do not come up to the surface and therefore at least not within the embayment. Therefore one cannot study them at the surface.

Q I see. Are you familiar with the faults mapped by West entitled the Shady Canyon fault and the Dana Point fault?

A Yes.

Q Would you describe those faults as listric normal faults?

A In the case of the Dana Point fault, there is a fault at Dana Point and right offhand now I do not recall whether the fault I am thinking of is the same one that Jack referred to as the Dana Point fault. You might refresh my memory as to whether the Dana Point fault is one exposed onshore or not.

MR. PIGOTT: Mr. Chairman, I think the witness has now been under examination for almost an even two hours. I would think it appropriate that some break be considered.

JUDGE KELLEY: Yes. I think we are moving up on that time. I am under the impression that you are pretty close to wrapping up. If you are not, I think we should break now. If you can finish it up in -- well, what kind of time would you estimate?

MR. BARLOW: Well, Your Honor, I don't expect it to take very much longer, perhaps 15 or 20 minutes. But I

would appreciate a break at this point, if it is okay.

JUDGE KELLEY: Well, I think it is almost unanimous. Let me make just an observation that we have had -- a couple people in the audience have brought signs into the room and we can't allow that because it does detract from the ability of the witnesses and the Board and others to focus on what we are about here. So if you need to bring signs into the room, I would appreciate your leaving them in the back of the room or putting them down on the floor.

And with that, let's break for ten minutes.

(A brief recess)

JUDGE KELLEY: Mr. Barlow, you have some questions left. Let me just note, though, when I asked you about likely duration when you began, you said an hour to an hour and a half and you have had about two. So I am going to ask you to wrap up in about five or ten minutes.

MR. BARLOW: Five or ten?

JUDGE KELLEY: Yes.

MR. PIGOTT: Mr. Chairman, could we have one preliminary thing? I believe the order yesterday on page 704 of the transcript is that the prepared direct be copied into the record at this time. That doesn't appear to be happening. I wonder if we could have that clarified as to whether or not we are going to get the volume of the direct transcripts, the direct testimony.

JUDGE KELLEY: By all means, let's clarify exactly what we are going to do. We do have, as I understand it, all of the direct bound up together in a single volume. That was the first thing that we did.

MR. PIGOTT: Except we never received it. It has not been done. It has not been done.

JUDGE KELLEY: Let's go off the record.

(Off the record)

JUDGE KELLEY: On the record.

MR. PIGOTT: Mr. Barlow's ten minutes are up.

(Laughter)

JUDGE KELLEY: I think we needed that. Mr. Barlow, go ahead.

BY MR. BARLOW:

Q Dr. Ehlig, we were discussing the Cristianitos fault and your description of it as a listric normal fault. We were trying to determine at what depth the Cristianitos fault may curve or flatten out. And you estimated approximately 10,000 feet. Would that be along basement rocks or in the Williams formation?

A I don't -- well, maybe I indicated 10,000. I indicated it would depend upon how far west one was from the surface trace as to what the depth was anyplace. I suspect that it would be at the base of either the cretaceous or within the Santiago Peak volcanic sequence which to the

west does interfinger with marine rocks and might well have a well developed bedding plane in it. So it could be deeper -- should be deeper than 10,000 feet that ie flattens in I think most areas and could I suppose be over 20,000. I would have to really look at a crossection, which I don't have in front of me or with me, to see what would be a reasonable depth. But it is on that ballpark of more than 10,000, I'd say.

Q And you said at some points it could be in the SAntiago - Silverado formation?

A No. The Santiago Peak volcanics. It would below the Cretaceous and into the upper jurassic.

Q Oh, I see. But it also could be in the cretaceous level?

A Perhaps at the base of the cretaceous. There are wells that -- oil wells that were drilled well into the cretaceous and show evidence of backward rotation. I believe that exhibit that I submitted which was a crossection by Jack West.

Q At --

A Yeah, that shows wells going down into the cretaceous and the fault is continuing below that.

Q Are you looking at PLE-L?

A That is correct.

Q Looking at that for a moment, could you tell me

what you estimate to be the width of the Cristianitos fault zone at the point it has the Forester Branch, which I believe is portrayed here?

A The Cristianitos fault per se is a very discrete fault that passes down through that section and is dipping westward. The Forester Branch would be a subsidiary fault, probably coming up from the curve Cristianitos at depth. Now there is a certain amount of interpretive license in utilizing that data to show exactly where the Forester branch is in the crossection and how much displacement is there.

Q Okay.

MR. BARLOW: I would like to request that the witness be given a copy of the exhibit, the Applicants' Exhibit No. SB-1, by Dr. Sean Behler.

JUDGE KELLEY: Is that a separate volume from this large volume?

MR. BARLOW: Yes, sir. It is an exhibit that is a separate volume and it has a stratographic map by West. It is the same sort of map that we are looking at here and I think it would be better.

MR. PIGOTT: A clarification. Is it appended to Dr. Behler's direct testimony or is it one of his exhibits?

MR. BARLOW: Exhibit SB-1. It is a separate volume.

JUDGE KELLEY: It is not just a page after his

testimony. I see.

MR. BARLOW: While we are waiting for that, in the interests of time --

JUDGE KELLEY: You have some other questions?

MR. BARLOW: Yes.

JUDGE KELLEY: Go ahead.

MR. PIGOTT: Just a second. I would prefer that the questions didn't proceed until I was --

JUDGE KELLEY: All right.

MR. PIGOTT: What page are you on?

MR. BARLOW: Figure 19. It doesn't have a page number on it.

MR. PIGOTT: I'm not sure that Dr. Ehlig is familiar with that, so he might take just a minute or so to make sure that he is.

BY MR. BARLOW:

Q Dr. Ehlig, have you seen this figure before?

A Yes, I have.

Q Looking at this figure, could you tell me where you would project the curve or flattening to take place on the Cristianitos fault if it is a listric normal fault within these layers and depths shown on figure 19?

A The most likely area would be below the trabuco formation and down in the upper part of what is termed basement here. Now in this particular area no oil wells have

been drilled through the tribuco and consequently, the details of stratigraphy are not known at depth along the Cristianitos fault in this particular area. Sean Behler shows the shallowest possible projection of the Cristianitos.

Q Does that shallowest possible projection agree with your concept of listric normal faulting?

A I would -- it agrees in that in close to the Cristianitos, I would carry the Cristianitos pretty much down as shown in this crossection, particularly in view of the Exxon well that is shown going down to a depth of between 2,000 and 3,000 meters.

Q Is it possible that the curving or flattening of the Cristianitos fault zone which was listric normal could place the Cristianitos fault, the curved part of the Cristianitos fault at depth, directly beneath the hypocenters of the earthquakes which Dr. Behler is discussing here?

A It would certainly place the fault surface going beneath the hypocenters. How deeply or how close to the hypocenters is a matter of conjecture. The deeeper of the two hypocenters might well be fairly close to what would be a feasible projection of a fault.

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BY MR. BARLOW:

Q By close, within what range of closeness do you mean now?

MR. PIGOTT: I am going to object to this kind of a question. It is one thing to talk generally of the application of Dr. Ehlig's testimony to the general area that Dr. Biehler is discussing, but I think it is quite another thing to ask for him to come up with particular distances that are certainly not a part of his direct examination. I would object to his going too far beyond the scope of the direct.

MR. WHARTON: Mr. Chairman, the testimony offered on cross-examination clearly indicates that the Cristianitos fault curves, and that the curve of the fault would lead it at depth close to or very near the hypocenters of the movement in the Cristianitos area. I think it is a very important issue and critical issue that we get the best testimony we can from Dr. Ehlig regarding how this particular phenomenon would work and how that would be his best estimate of how that would be portrayed on the map, of how that -- how the -- how it would work.

JUDGE KELLEY: I will overrule the objection.

THE WITNESS: This is a matter of speculation, but if I were to draw a curve, I would tend to pass it beneath the most easterly hypocenter at a depth of perhaps 5,000 meters, as a reasonable kind of projection.

MR. WHARTON: Mr. Chairman, we would request that Dr. Ehlig do draw that curve on this particular map, and that after drawing the curve, that it be identified as the Intervenor's first Exhibit.

MR. PIGOTT: I am going to object. The Intervenors can put together their own exhibits. I think this is an improper way for a showing on behalf of the Intervenors.

This is not Dr. Ehlig's map. He has made some general projections. I think it is an unfair burden to put on him, and an inaccurate type of material to have in the record.

MR. BARLOW: I think we could proceed without a request for drawing.

JUDGE KELLEY: I was going to sustain the objection anyway, so go ahead.

BY MR. BARLOW:

Q Dr. Ehlig, what sort of error bar would you ascribe to your projected curve of the Cristianitos fault?

A It would be quite a large one. It could be significantly deeper than I have suggested here.

Q Could it also --

A I have not tried to make a cross-section extending from the surface trace of the Cristianitos to the Los Angeles Basin area, to try to tie down a precise depth at which such a fault would run -- extend. What I have noted

are the features which infer that the fault plane does tend to flatten with depth, and that was the critical part of the argument, not how deeply the actual plane extends.

Q Okay, thank you, so just to make sure that I understand you, you said that the projected flattening or curving of the Cristianitos fault would be approximately at a depth of 5,000 feet?

MR. PIGOTT: Objection, that is a misstatement. I believe he stated that --

THE WITNESS: 5,000 meters is a possible position, not feet.

BY MR. BARLOW:

Q 5,000 meters, excuse me, that is what you said, 5,000 meters?

A Is a plausible position.

Q Okay. Thank you. Dr. Ehlig, is it possible that if the Cristianitos fault had ceased to be active as a normal fault, that it could be reactivated as a thrust fault under the current stress regime in Southern California?

MR. PIGOTT: I am going to object to this as going far beyond the scope of the issue. The issue here is the general geology in the area of the OZD, including the regional area, and it is not -- it is not capability of the Cristianitos fault. I believe that we are attempting here just a fishing expedition of possibility on possibility in an

attempt to raise that which has been decided a long time ago.

The issue is, concerning these two events, is in issue, and will be addressed by Dr. Biehler. I don't think that is an appropriate question for this witness.

MR. BARLOW: Your Honor, we are looking at a map that analyzes the earthquakes which occurred close to the Cristianitos fault zone in 1975 and we are trying to determine the sense of faulting along this zone, which Dr. Ehlig is an expert in, and he has testified in his written testimony concerning the nature of the Cristianitos fault zone, and we are just following along on that in terms of the evolution over geologic time of this fault zone.

JUDGE KELLEY: Well, I think it is marginal, but I will allow this question, but at the same time say to you, Mr. Barlow, that I want you to wrap up with another two or three questions in the next three or four minutes.

THE WITNESS: In response to the question, I know of no evidence that the Cristianitos fault has experienced movement within the Quaternary, and in addition, the orientation of the fault is certainly not what I would expect for reactivation by reverse movement. The stress field is such that I would expect reverse movement to occur along a more east-west or northwest trending structure, if one were to hypothesize stresses sufficient to cause reverse faulting.

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BY MR. BARLOW:

Q In general, is it possible that a listric-normal fault can change its sense of movement and become a thrust fault with reverse faulting?

A Well, generally listric-normal faults do not extend deep enough to be in the zone where earthquakes would be generated, and in fact one would require very special mechanics in order to reverse the movement.

Q Okay.

A I think it would be extremely difficult to reactivate such a fault.

Q Have you studied the northern end of the Cristianitos fault zone, and in this regard, let me rephrase the question, excuse me.

Do you consider it possible that at some point during the geologic evolution of this region, that the Cristianitos fault zone formed a structural relationship between the Newport-Inglewood OZD fault zone, and the Whittier-Elsinore fault zone?

MR. PIGOTT: I am going to object. This is something I guess we got to -- close to the other day, but this I believe extends beyond the issue currently before the Board. I think it is an attempt, a back door attempt to put into this case some kind of an allegation of different or new structures by putting something new on what has been the

traditional mapped extent of the Cristianitos. If there is an issue, I believe it is issue number three, which has an opening for the Intervenors, upon a threshold showing, to show additional new discoveries other than the five or six delineated there, and I would object again that this goes beyond the scope of this issue,and attempts improperly to put a new issue into this proceeding.

JUDGE KELLEY: Could you just restate the question?

MR. BARLOW: Certainly.

BY MR. BARLOW:

Q Dr. Ehlig, in your opinion, is it possible that at some point during geologic history, the Cristianitos fault zone formed a structural relationship between the Newport-Inglewood OZD fault zone and the Whittier-Elsinore fault zone?

JUDGE KELLEY: I am going to sustain the objection. I believe you can bring it in at a later point on a showing of relevance, as Mr. Pigott has suggested. This, after all, is rather general introductory testimony.

It is very important testimony, but it is rather general, and I think that you are getting beyond, in this regard, and so I am going to sustain the objection.

MR. BARLOW: Could I ask a question; are you saying that it would be relevant under contention three, or the other contention?

JUDGE KELLEY: I am saying it might be. I am not making that ruling, but it may be, upon a proper showing.

MR. WHARTON: You didn't give me a chance to refer to the record to show the relevance of the matter, and I am referring to page 29, where he is testifying that under existing conditions, the Cristianitos fault is buttressed and cannot move. Consequently movement on the OZD would not cause movement on the Cristianitos fault. I believe with his testimony, it is appropriate to be able to pursue his discussion of it, the relationship of the Cristianitos fault and the OZD, which was the thrust of the question.

MR. PIGOTT: I would submit, Mr. Chairman, that Mr. Wharton is at the wrong end of the Cristianitos fault. The testimony here goes to the south end.

JUDGE KELLEY: You were at, Mr. Wharton, the end that goes down into the ocean, right?

MR. WHARTON: That is correct.

JUDGE KELLEY: And out toward the OZD. You were asking a different question, I thought.

MR. BARLOW: Well, I was including that in the question, but I was looking at the northern end of the Cristianitos fault, where it comes very close to the Whittier-Elsinore fault zone.

JUDGE KELLEY: Next question.

MR. BARLOW: Okay.

JUDGE KELLEY: Next and almost last. Next to the last.

MR. BARLOW: Okay.

BY MR. BARLOW:

Q Dr. Ehlig, would you agree that water-saturated near-surface sediments offshore from San Onofre would tend to heal or cover up surface displacements that occur on the OZD -- that occurred on the OZD before the Holocene times?

A I am not aware of things being healed, rocks being able to heal themselves the way people heal themselves. No, I would not agree.

Q Excuse me, I was not discussing rocks. I was discussing water-saturated near-surface sediments.

A Anything that is below the water's surface is going to be saturated with water, but that doesn't mean the grains are going to readjust in any way so as to destroy the rock evidence. If you want to talk about marine organisms burrowing in the upper foot or so, I would agree that the upper foot or so is frequently disturbed by bottom dwellers, and can lose evidence, but not if you go down several feet.

Q One last question. Do you consider it possible that San Onofre Mountain is a product of dip slip movement in a similar way to the way that Mount Soledad is a product of dip slip movement?

A That question loses me in that San Onofre

Mountain is composed of extremely erosionally-resistant San Onofre breccia which is tilted seaward. You are inferring that there is a fault present, but I -- I have -- I am at a loss to know what fault you are talking about.

Q Well, the Cristianitos fault runs along the northern flank of the San Onofre Mountain, is that correct?

A It cuts it obliquely, yes.

Q Does the Cristianitos fault have any dip slip movement on it?

A The Cristianitos fault is a dip slip fault. It is what we call a normal fault.

JUDGE KELLEY: With that, I believe we will pass on to Mr. Chandler. Mr. Chandler, do you have any questions of this witness?

MR. CHANDLER: If I could just have one moment.

No questions, Mr. Chairman.

JUDGE KELLEY: No questions.

MR. PIGOTT: Mister -- oh, I am sorry. The Board may have questions.

JUDGE KELLEY: I just have one easy question, Dr. Ehlig. You were talking about the San Miguel fault, and you used the term slicken sides. Whatdoes that mean?

THE WITNESS: A grooved surface that is produced along the fault as it moves. It is the actual fault plane. Because of grains projecting out or harder materials, it leaves

a grooved surface, and when you talk about the slicken sides, it is the trend of the grooves.

JUDGE KELLEY: Thank you. Mr. Pigott?

MR. PIGOTT: Yes, Mr. Chairman. We have had a full six-plus hours, I guess, almost, of cross-examination of Dr. Ehlig. I would request, as I indicated I might this morning, that before redirecting, I would like the evening recess to go over the record. Certainly it would shorten it and probably make a more organized redirect if in fact that is even necessary.

JUDGE KELLEY: Are you going to have any redirect of Mr. Smith at this point?

MR. PIGOTT: No, Mr. Smith is not going to have redirect.

MR. WHARTON: Mr. Chairman?

JUDGE KELLEY: Yes.

MR. WHARTON: Is Mr. Pigott proposing to move on to another witness --

MR. PIGOTT: I will -- I am prepared to call my next witness, yes.

MR. WHARTON: I would propose then, that if Mr. Pigott has redirect, that he do it at this time, for purposes of expediting the proceedings, for purposes of the witness's testimony being fresh in the cross-examiner's mind, that we have redirect at this time.

JUDGE KELLEY: Any comment from the Staff?

MR. CHANDLER: We have no preference on that, Mr. Chairman.

MR. PIGOTT: I would only point out that the proposed procedure which I did discuss this morning met with no objection. In fact, it was a stated no objection this morning. If we are looking to expedite the record, I think you would have an expedited situation if we can come in with a few short questions, rather than perhaps asking for a recess now to see whether or not the witness has some areas he feels should be redirected, and probably take longer than it should.

Of course, we could do that subject to a few minutes of conversation with the witness. I would far prefer at this hour of the day to call the next witness, do the preliminaries, perhaps even get his general discussion of his testimony into the record before proceeding.

JUDGE KELLEY: I am going to rule in your favor, Mr. Pigott, with the observation, obviously, that you be able to do the same thing further along with one of your witnesses, if you choose to do so, Mr. Wharton.

But I think it would expedite things in all likelihood for you to have overnight to think about what you want to put on, and also from the standpoint of Dr. Ehlig, who has had kind of a long day on the stand, and so with that,

I will thank you and excuse you at this time, Dr. Ehlig, with the understanding that you will or you may be back tomorrow morning.

MR. PIGOTT: I think he probably will for a very short period of time.

JUDGE KELLEY: Right.

MR. PIGOTT: One thing before I call him, Mr. Chairman, I will give you copies of the memorandum which I said I had not been able to receive as of yesterday concerning the memorandum on the size of the plume exposure pathway, and if I could just hand that out.

JUDGE KELLEY: Fine.

MR. PIGOTT: And I would ask Mr. Heath if he could take the stand.

Whereupon,

EDWARD GEORGE HEATH

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

JUDGE KELLEY: Mr. Heath, we are happy to have you with us.

DIRECT EXAMINATION

BY MR. PIGOTT:

Q Would you state your full name?

A Yes, Edward George Heath.

Q And your current business address?

A It is at 4000 West Chapman Avenue in the City of Orange.

Q Do you have before you a copy of the document entitled "Testimony of Edward G. Heath," which is composed of 28 pages, and figures EGH-A through EGH-L, some 12 figures?

A Yes. I also -- there is one more figure, M.

Q I am sorry I missed it. Okay. 13 figures. Do you have any corrections to make in either the figures or your prepared testimony?

A No, I do not.

Q If you were asked those questions today, would your answers be the same as contained in that document?

A Yes, they would.

Q So you adopt that as your testimony in this proceeding?

A Yes. I do.

MR. PIGOTT: I would ask that Mr. Heath's testimony be accepted into the record as evidence.

MR. WHARTON: Mr. Chairman, I would just move to strike the reference to the word "hypothesized offshore zone of deformation," since the contention as stated goes to the OZD and there was a specific question regarding whether it is hypothesized, or the OZD in its pre-hearing conference, the Board specifically deleted the letter "H" from that, and I would move that the letter "H" and the word "hypothesized"

be removed from the record at page 9, line 3; page 11, line 10; correction. The page 1, and that is line 11, and then page 10, which refers to this figure EGH-B.

MR. CHANDLER: I am sorry, Mr. Chairman, on page 1, "hypothesized" appears on line 13, is that not correct?

MR. WHARTON: It starts on line 11. Page -- line number 13, that is correct.

JUDGE KELLEY: All right, let me make sure I know what you are referring to. I have got page 1, line 11, or line 13. Now, what is the other reference?

MR. WHARTON: The other reference is to page ten, I believe -- okay, line 24.

JUDGE KELLEY: Line 24, page ten.

MR. WHARTON: That refers to figure EGH-B, location map hypothesized OZD.

JUDGE KELLEY: All right, and then what else?

MR. WHARTON: And the attached map that that refers to also.

JUDGE KELLEY: Which also uses the figure "H" OZD?

MR. WHARTON: Yes, it does.

JUDGE KELLEY: And was there something else, or is that it?

MR. WHARTON: That is it.

JUDGE KELLEY: Mr. Chandler, you were involved in

this deep issue some time back. What is your view?

MR. CHANDLER: We would note that we would support Mr. Wharton's motion to delete reference to the "hypothesized OZD." We believe it inappropriate.

JUDGE KELLEY: As I recall, though, one of the reasons you offered back in the pre-hearing was that the OZD, in quotes, had been recognized as such, was referred to as such by geologists, and it was just confusing to have an "H" in front of it, that was part of your reasons, wasn't it?

MR. CHANDLER: Yes, that is correct.

JUDGE KELLEY: Was that all of it? What other reason?

MR. CHANDLER: Well, I think part of it goes to the position that we have indicated in our comments with respect to the applicability of collateral estoppel and res judicata to this proceeding. We are not here to relitigate whether this is a hypothesized zone or a real zone or something else.

That matter was, we believe, disposed of at the construction permit stage. We believe that at least since that point in time, the offshore zone of deformation, or OZD is a recognized structure as such. It is not a hypothesized zone.

We recognize that the Applicant has not necessarily acceded to that in an academic sense, but certainly for

purposes of this proceeding, we understand that it is accepting it as the characterization appropriate for determining the design of the facility, and for that reason we do not believe that we should be referring to it as hypothesized.

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MR. PIGGOT: If I might be heard?

If I can go back again to the construction permit stage, the issue was avoided, by use of a stipulation for purposes of hering the seismic portion of the case. There was never a hearing on whether or not, as a geologic fact there is such a thing as a continuous offshore zone of deformation.

It has been accepted for design purposes. It has never been accepted, it has never been found in any proceeding to infact be the kind of a model of a zone that is depicted for purposes of seismic design. And for that reason, we cannot accede to willy-nilly, on the basis of time passing, and people using a term for a long period of time, a name that i fact depicts it as a geologic structure. It is just not true. It has just never been litigated, and we construe it as "hypothesized," and until the occasion arrises, which I hope never does, that it is litigated, I think that both sides should be able to refer to it by whatever name they want.

By using the word "hypothesized," and you will notice in lower case on the first page, and I don't think there is any other evil associated with its use in Figure EGH-B, we are maintaining our position that this is not something that has been determined as a matter of fact in a NRC proceeding or in any other proceeding.

We should be allowed to continue to characterize OZD in the manner that we deem fit.

MR. WHARTON: Mr. Chairman, just one point: if the Board is going to be consistent, this issue was raised at the pre-hearing conference, where they were asking that the contention be in terms of "hypothesized offshore zone of deformation." The Board, at that time, after hearing argument, ruled that it was not to be "hypothesized offshore zone of deformation," but the "OZD," and we have prepared our case on the basis of "OZD," not "hypothesized zone." That is the contention. I submit that that should be consistent all the way through, that it is "OZD" and not the "hypothesized zone."

JUDGE KELLEY: I am looking for the May order, and I don't have my hands on it.

The pre=heraing conference order, which was then superceded by the order of May 28th.

I would like to take a look at that, because I frankly don't remember the couple or three sentences of reasoning that came after the decision to knock out the "H." I remember that we said something, and I just want to look at it.

MR. WHARTON: Is that the May 8th? I have a copy of the May 8th.

JUDGE KELLEY: Do you? Could I see that, please?

Let me just take a moment to look at this.

All we did here pregermit (ph.), as lawyers say, the question of whether res judicata applied here or not.

And although not stated, part of the reason for this was the understanding that OZD was the more commonly used term, rightly or wrongly, and that for the purposes of writing a contention, it would be understood. "OZD" was better than "HOZD."

I fail to see whether this really makes much difference, or any difference at all. Here is a witness for the Applicant using a term that he prefers. There is no confusion here as to what he is talking about. I assume he could call it the "so-called OZD," if he wanted to, and everybody would know what he meant.

MR. PIGOTT: He has called it that.

JUDGE KELLEY: But I don't really see much potential here for confusion in the record.

You are not testifying, and correct me if I am wrong, you are not testifying substantively that there is really no OZD out there.

THE WITNESS: No.

JUDGE KELLEY: That is not what you are talking about. So with that understanding, I am going to deny this motion to strike, and let the testimony in the respects referred to stand.

MR. PIGGOT: So the testimony isnow in evidence.

JUDGE KELLEY: That is correct.

/ / /

BY MR. PIGOTT:

Q Mr. Heath, have you prepared a general discussion and description of the testimony that you plan to give here?

A Yes, I have. I can run through a brief review of my testimony.

Q Mr. Heath, Just one second, one more thing:

I am advised that in all this flurry of forensic action I have forgotten to ask you if you sponsor any exhibits with respect to your testimony at this stage?

A Yes, I do.

I have the exhibits marked 3 through 9 --

Q Yes, those are Applicants' Exhibits Nos. 3--

A Applicants' Exhibits marked 3 through 11, I believe that were read into the record.

MR. PIGGOT: Exhibits 3 through 10, Mr. Chairman, they are EGH-1 through 8.

BY MR. PIGOTT:

Q Were those exhibits prepared either by you or under your supervision and direction?

A Yes, they were, with the exception of one exhibit, which contains data prepared by Western Geophysical, but the exhibit is essentially a review of that data, and that was prepared.

MR. PIGOTT: I would ask that the exhibits be allowed into evidence.

JUDGE KELLEY: Mr. Wharton?

MR. WHARTON: No objection.

MR. CHANDLER: No objection.

JUDGE KELLEY: So odered.

(Applicants' Exhibit No. 3, EGH-1 through EGH-8 was thereupon received into evidence.)

BY MR. PIGOTT:

Q Now, Mr. Heath, do you have a brief explanation of your testimony?

A I have two corrections to those exhibits.

Q To the exhibits?

A To the exhibits, that I would like to point out.

Q Which exhibits?

A Okay, the first one is Exhibit No. 3, which is marked EGH-1, and on that I would like to turn to the table that is 361.38-3.

Q That is the table entitled: "Synthetic Plot Based on Slip Rate Versus Half Fault Length," in Slemmons 1977?

A No, this is a table. It is Table 38-3.

Q That is the one entitled: "Comparison of Zone Characteristics North to South, Along the Hypothesized Offshore Zone of Deformation"?

A Yes, it is.

I have two corrections for that table:

Under the column marked "South Coast Offshore Zone of Deformation," and opposite the maximum segment length, the 48 should be changed to 27-plus-or-minus kilometers.

And under the column headed "Rose Canyon Fault Zone," and opposite the line marked "Maximum Segment Length," the 35 should be changed to 48-plus-or-minus kilometers.

That corrects that table so that it is in agreement with the table on the preceding page.

Those were typographical errors.

Q Do you have another correction?

A Yes, I do.

That is in Exhibit No. 6, marked EGH-4, Figure 361.61-3.

Q What was the table number again?

A It was 361.61-3.

Q Is that a table or a figure this time?

A This is a figure. It is entitled: "Horizontal Geologic Slip Grade, Seal Beach Field, Newport-Inglewood Zone of Deformation."

The title itself is in error. It should read: "Horizontal Geologic Slip Rate, Long Beach Field," substituting for, or in place of "Seal Beach."

Q Do you have any other corrections?

A No, I do not.

MR. CHANDLER: Mr. Pigott, I have a question, if I may:

The copy of the testimony, of the exhibits that I have goes from Figure 361.61-1 to 361.61-3.

Is there no -2?

MR. PIGOTT: There is supposed to be.

If we could see you afterwards, we will see that you have a complete set, Mr. Chandler.

MR. CHANDLER: Thank you.

THE WITNESS: That is a similar figure on the Seal Beach and Hunting Beach Fields. It should be in there.

BY MR. PIGOTT:

Q With the testimony admitted, the exhibits admitted, and the corrections made, Mr. Heath, I think we are in order to prepare to hear your talk concerning your testimony.

A Okay, thank you.

My testimony is on the evaluation of the maximum magnitude for the offshore zone of deformation, opposite the site. And in this study several methodologies were considered, in evaluating the maximum earthquake applicable to the OZD. My specific approach uses both a quantitative and a qualitative comparison of geologic features as a means of differentiating and ranking faults and thus evaluating the earthquake potential of the OZD.

In this evaluation, I also evaluated the rupture length versus maximum magnitude methodology, the displacement per event methodologies, however, due to the limited data available on surface displacement, and length of the fault segments within the OZD, neither of these methods could be used solely on their own.

The degree of fault activity methodology for estimating earthquake magnitude that we use is based on coparing the degree of fault activity on the OZD with that of similar faults in the Southern California area, and in strike slip faults and similar tectonic environments around the world.

This approach, for a specific fault, considers evidence of fault behavior in basically three steps:

First we look at the tectonic style in the tectonic environment of the fault of concern. Then we look at fault activity and geologic perameters for these faults and similar faults around the world. Then the degree of activity perameters are compared, so that the fault of interest is ranked relative to other faults.

The degree of activity is measured best by the long term or geologic slip rate on the faults, and from this relationship a maximum magnitude limit can be estimated for each of the faults so compared.

I would like now to just briefly run through these

three steps, starting with the tectonic setting, style of faulting for the OZD and the other similar strike slip faults in the Southern California area.

If I could have Figure No. EGH-A.

(Slide projected on wall)

WITNESS HEATH: This figure is generalized fault map of the Southern California area, and on it I have highlighted the broad red line extending from the north -- I guess I had better say from the upper left-hand corner, down to the lower right-hand corner is the San Andreas Fault, which extends down to the Salton Sea area, and then made the offset to the Imperial Fault, and on down into the Gulf of California.

To the west of the San Andreas, and connecting with it, is the San Jacinto Fault. These two faults together comprise the major portion of the plate boundary between the North American Plate, which is in the upper right-hand part of the slide, and the Pacific Plate, which is to the lower left-hand corner.

The plate motion is such that while the motion is at the Pacific, the plate is moving to the northwest, so that you develop a right slip or right strike slip along the San Andreas and the San Jacinto Fault Zones.

As you can see on the map, there are several other faults to the west of the San Andreas and the

San Jacinto Fault. We have the Whittier-Elsinor Fault Zone, a long linear series of en echelon faults. Further to the west, we have the offshore zone of deformation, which extends from the Malibu Fault in the north which is the southern edge of the Transverse Range in that area, down to the San Diego area, in what is called the Rose Canyon Fault, and then it then it trends offshore across Coronado, towards the international border.

Also, there are several additional faults in the offshore area. The one closest to the San Diego area is Coronado Banks Fault, and then further west we have the San Clemente Fault. These are, at least, the major faults in the area that we will be talking about, and it does show the parallelism of these faults in the Southern California area.

I might note also, to the northwest -- pardon me -- northeast of the San Andreas, in the Mojave area, there are several also northwest southeast trending faults, such as the one marked the Hellendale Fault there, several of those.

I might say that, as Dr. Ehlig has said, and also Jay Smith, that the San Andreas appears to be taking up the major amount of the plate motion, but there is a drag effect, which causes lesser amounts of displacement on these other northwest southeast faults. The general displacement can be characterized as right lateral strike slip, with lesser and varying amounts of normal displacement.

I would like to now move on to discuss in a little detail the OZD, if I could have Slide B.

Now, realizing discussions that we have had in the meeting, on segmentation of the zone, or the lack thereof, I would like to say that this slide simply is used to designate the names that have been applied to the various portions of the OZD.

In the north, we have the Newport-Inglewood Zone of Deformation. In the offshore area, opposite the site, we have the South Coast Offshore Zone of Deformation, and in the San Diego area, into the north and the offshore area, we have what is called the Rose Canyon Fault Zone.

One thing that is obvious from this slide shows that the zone is made up of a series of discontinuous, disconnected faults. What is not shown on here is that there are also a series of folds, some of them associated with the faults, commonly lying in the areas between the faults, where the folds are taking up some of the lateral motion across the zone.

We look first at the Newport-Inglewood Zone of Deformation, which is Figure C.

This map is a generalized sub-surface map of the fault pattern and folds along the Newport-Inglewood Zone of Deformation, as prepared Harding, in a publication of 1973, which was referred to earlier in the proceedings.

It shows a series of en echelon left-stepping faults, which are highlighted in red, and a series of northwest trending anticlinal type folds, which are more or less in a left-stepping mode, along the fault.

Harding, in his article, describes this as a typical example of a wrench fault. I would like right now to basically give you my definition of a wrench fault:

Here we are dealing with a deapseated right lateral fault, and it has -- in the basement rocks -- It has deformed a thick series of sediments, ranging up to about, I think, 14,000 feet thick that have been deposited with time while the fault was moving. So that has deformed the rocks a different amount, depending on the age and depth of the rocks in the Los Angeles Basin.

This deformation, as you can see, follows a relatively narrow band, along the zone of deformation, and in places you have the anticlinal folds associated with the faults, and in some places they are more or less between them.

The wrench tectonics here is primarily the amount of deformation that is occurring in this sedimentary section, above the deeper strike slip fault. So, when I refer to "wrench faults," this is the type that I am referring to, and I am using the definition as used by Harding in his paper.

We look at the offshore area on, I think, it is No. D -- This is a similar map, but this is prepared from

offshore geophysical data of Western Geophysical Company. They ran the profiles and preapred the contour map.

Now, the orientation here is turned around, so that north is off to the upper left-hand corner. The trend of the two zones is sub-parallel. You see a similar pattern of folds along the zone, and short, discontinuous faults. And this is also -- this horizon is a sesmic reflecting horizon, near the top of the Miocene, and is therefore similar to the one that was presented by Harding in his paper.

This pattern of faults and folds is very similar to that in the Newport-Inglewood Zone of Deformation, and therefore I think it is very probably the result of deeper seated right slip on a fault underlying the zone.

/ / /

I would like to point out here, however, that the folds here are much more gentle. The flanks of the folds have lower dips. The total deformation, therefore, of the sedimentary sections, is less, suggesting that there has been more movement on the Newport-Inglewood fault than there has been on the offshore zone.

We go down to and look at Rose Canyon fault, which is shown on figure EGH-E. Pardon me. I would like the other one back. I forgot.

The northern offshore portion of the Rose Canyon fault is shown on the right-hand portion of this slide, but it trends in towards the coast to the right-hand side, follow it on down there to the right, where it goes onshore in the La Jolla area.

And the next slide shows the fault as it goes through the San Diego area, past Mission Bay which is a little inlet there in the upper left-hand portion of the slide, and then as it goes on through the San Diego area, it trends off more in a southerly direction across Coronado and breaks up into a series of faults there labelled the Spanish Bight, Coronado, and Silver Strand faults.

The character of the faulting in this area appears to be primarily dip slip, with a displacement down to the east into the San Diego Bay area, and the fault appears to die out in the area of the -- as shown there, before it gets

to the U.S.-Mexican Border, or the projection of it offshore.

As noted here, this map is taken from Kennedy and Welday in their publication of 1980.

I would like to move on now. In my testimony on page 16, I briefly summarize that the Newport-Inglewood zone of deformation was picked as a representative model of the OZD because of the similarities in structural style among the three elements of the zone, and because of the extensive and high quality data available regarding the style and amount of deformation along the Newport-Inglewood zone.

I noted the large number of folds and anticlines along the Newport-Inglewood zone of deformation, and the majority of the anticlines are oil-bearing and have been extensively drilled and studied by the oil companies, and we have utilized this vast store of information to help us understand the structure of the Newport-Inglewood zone, and the history of faulting and the amount of displacement with time that has occurred along this zone since the initiation of at least the current -- pardon me. The initiation of the current tectonic system, which started in the Los Angeles basin four to five thousand -- million years ago, so we have a good history on that.

It has also been the seismically the most active portion of the OZD, as has been stated, it has had several large earthquakes, a destructive earthquake in 1920 in

Inglewood. I think it was estimated to be a magnitude of 4.9, but did a lot of damage, the 1933 Long Beach earthquake, with a magnitude 6.3, and numerous smaller events have been recorded along the Newport-Inglewood zone. Therefore we think it is an appropriate model. We think it is a conservative model, and it has abundant evidence to help us come to a -- derive an estimate for the maximum earthquake that it might be capable of, and we also feel that in having derived such an estimate, transferring this event off to the offshore zone opposite the site is also adding conservatism to the estimate, because we see less deformation in the offshore zone, and less seismic activity.

And the second step in the study was to derive and compare the different parameters of the faults that relate to their degree of activity, amount of displacement they have had.

To move along, these are summarized on a table here, just a second while I find this. This would be EGH-F and we can put that up. I don't know if we can read it off of the vugraph. We might want to turn to that table.

I won't go into any detail on the table, but simply summarize the type of information that we have. We have compared the major faults that we have good data on in Southern California. That is the San Andreas, San Jacinto, Whittier-Elsinore, and OZD. We put down the dimensions of the

fault segments, as could be measured from both surface and subsurface maps. The total displacement, this is of geologic units across the faults, to compare those. The distance from the San Andreas fault, which is the plate boundary, the historic rupture lengths that have occurred on these faults, historic displacements.

A qualitative description of the continuity in geomorphic features that one can observe on the surface along these fault trends, because there is a difference that can be noted.

The historic seismicity, the maximum historic magnitudes that have been recorded on these faults, and the last but certainly not least is the geologic slip rate, which shows the -- how far and how fast essentially these faults have been experienced displacement over the last four to five million years.

On the following page, we have table 361.38-3. This summarizes in a little more detail many of the same types of features for the three portions of the OZD, the north, central, and southern portions.

The general conclusions that can be drawn from comparing the geologic and degree of activity parameters as presented on these two tables are the major plate motions between the North American and Pacific Plate, as occurring along the San Andreas and San Jacinto fault zones, and has

continued to do so for at least the last five million years.

This active plate motion is particularly well-demonstrated by comparing the total displacement across the faults, and the long-term geologic slip rates of the faults, and that there is a consistent decrease in essentially all of these measurable parameters, westward from the major plate boundary to the OZD, suggesting that the OZD is a less significant fault with much lower level of earthquake potential than the more active faults along the quake boundary.

I would like to now look at some of the other methods that we considered in determining the maximum earthquake.

These methods including the maximum historic earthquakes that have been recorded on these faults, and comparing them; the fault rupture length versus magnitude relationship; displacement per event relationships; and the long-term or geological rate -- slip rate, pardon me, on a fault versus the maximum historic earthquake.

I revieved briefly that the maximum earthquake recorded on the zone was the Long Beach earthquake. The aftershock zone ruptured approximately 30 kilometers, from Newport Beach up to the Long Beach area, and the maximum magnitude was 6.3.

The longest segment that we have on this -- in the

subsurface on this zone, is essentially the same area that extends a little further north, for a total of about 36 kilometers.

Therefore, the subsurface rupture that occurred in 1933 is close to the total segment length, or the longest segment of the zone, suggesting that the 1933 event may be close to the maximum that that zone is capable of producing.

We look at the rupture length and displacement per event methods. We can look at the empirical relationships that were developed by Dr. Slemmons in his start-of-the-art paper. It was published in 1977.

We can see that the syle of faulting and tectonic setting directly affect the relationship between the magnitude and length of rupture and the amount of surface displacement. That is, the different types of faults, the strike-slip faults, normal faults, thrust faults, plot differently on the graph that he prepares, and he develops a regression line for each type of faulting, so we see that we have to be careful in comparing faults that we are not comparing, just say apples and oranges.

We need to define the style of faulting before we start using these fault length or displacement per event relationships.

We also see that in the same report, and also in the SER, where Dr. Slemmons has prepared a portion of that,

that the number of faults in Southern California, particularly the San Jacinto fault, generally is not believed to rupture only half of its -- you know, full half length, but generally yes, usually in a -- maybe between 20 and 30 percent of the fault length is all that ruptures on the major events.

Therefore, use of the commonly prescribed half-length method is not appropriate for these types of faults, even the ones that are long and continuous, and particularly for a zone of deformation such as the one we are talking about, where we do not even have continuous faulting, we have short faults which are interrupted by gaps, with no faults, or folds which are taking up some of the displacement.

If we -- we can learn something about -- from the fault length relationships, however, because we do have -- recognize segments of faulting within the zone of deformation. We can measure the lengths of these segments. We can go to the relationships established by Slemmons, and see what type of an earthquake it might have taken to produce this rupture.

Now, most of these ruptures that we are measuring are in the subsurface. We can presume that the surface rupture would not have been longer than what is now in the geologic record in the subsurface. And if we simply take the longest segments along there from the three portions, 36 kilometers in the Newport-Inglewood, 27 in the offshore

area, and 48 in the Rose Canyon area, and draw the Slemmons curves, we come up with maximum magnitude estimates ranging from 6.6 to 6.9, based on the fault length relationships.

We look at the displacement per event. We have more of a problem, because nowhere along the zone do we find good evidence of the amount of displacement that has occurred on single events, so that relationship is not well applied to the zone of deformation.

That brings us to the fourth method, which we call the degree of activity method, by itself. In order to assess the degree of activity of the various faults, in terms of geologic slip rate, we made a literature search to estimate the displacements and develop the slip rates for the various faults. This was done for the strike-slip faults we have talked about today, a number of other ones in Southern California, and others around the world from similar tectonic environments.

We restricted the tectonic environments to those of Southern California style. Generally, we are talking about plate edge here we have essentially strike-slip motion, so that we would be dealing with very similar faults.

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WITNESS HEATH: After compiling the data and because slip rate is perhaps the most quantitative measure of the degree of fault activity and we were comparing faults, as you recall, by their degree of activity, we made a plot, the slip rate versus maximum historical earthquakes of magnitudes that have occurred on these faults. This is shown in Figure EGH-J.

On the lefthand we have a scale which relates in millimeters per year the rate of movement that we have measured along these various faults. Across the bottom we have an earthquake magnitude scale which records the maximum event that is recorded or estimated for pre-recording but historic earthquakes. Many of these faults have experienced lesser earthquakes, but only the maximum or the largest earthquake that they have recorded has been plotted on this. And you see that there appears to be an increase in the magnitude with an increase in the slip rate, suggesting that you might be able to put a bounding limit on the scale.

We have done so on the next figure. This line would simply represent the maximum historic earthquake limit that comes out of the data set. If you assume that some of these faults have had the maximum earthquake that they are capable of, then the line begins to take on the significance that maybe it really represents some limit to what these faults are capable of, depending on their degree of activity,

as measured by their slip rate.

There is little conservatism built into this curve. In order to add conservatism and to take into account the variations in estimates of magnitude and variations in estimates of geologic slip rates by the number of researchers that have studied that, we plotted a box around here which shows a variation in slip rates as measured. This is on Figure EGH-L.

These boxes in a vertical sense, so the variation in geologic slip rate and in a lateral sense .2 magnitude unit variation in estimated magnitude. If we put a line now to bound these, which is shown on my last figure -- believe it or not, I am coming to the end here -- we have a line that that bounds the complete data set, the variations in the data set is moved over about a half a magnitude unit to the right of our previous line and we suggest indicates a maximum earthquake limit line for this style of faulting in this tectonic environment. This is one of the methods that we have used to make a numerical estimate of the maximum magnitude. The most conservative way to use this is to take a line off of the Newport-Inglewood fault, which is, incidentally, marked No. 7 there, second from the bottom. It has an average slip rate of .5 millimeters per year, a range of .3 to .68 millimeters per year.

We take the maximum slip rate, which would be the

top of the box, extend it to the right til it intersects the line marked MEL and then drop down to the magnitude scale. It comes out with a magnitude of MS-7, as an estimate.

That basically is the conclusion of my testimony. There are several different methods that were used to derive this estimate.

MR. PIGOTT: Mr. Chairman?

JUDGE KELLEY: Yes?

MR. PIGOTT: One additional thing before we tender Mr. Heath for cross examination. At the prehearing conference of the 19th then primarily addressing emergency planning I believe the Board expressed an interest in having one large map depicting the general geology or the main faults in the southern California area. We do have such a map. We have compiled it from the official maps of the California Department of Mines and Geology. I would think that Mr. Heath is probably the appropriate one to have it identified through and I would -- I have a large one. Perhaps we can bring it up and put it on one of th easels. I also have photographic reductions which can be given and served to the parties.

I would ask that when you see it that this map be identified as Applicants' next-in-order, which will be, I think, 33 (EGH-9). I will leave it to the Board as to whether or not they want to have it admitted as evidence. I will go as far as identifying it and, if you want it in the record,

obviously we have no objections. It is an official publication of the State of California.

(The map was marked for identification as Applicants' Exhibit No. 33 (EGH-9).)

JUDGE KELLEY: Thank you, Mr. Pigott. My thought when I mentioned this was to aid understanding. Mr. Wharton?

MR. WHARTON: Yes. We would like an opportunity to be able to review the map before anything is done. We would like to have an opportunity to review it this evening with Dr. Brune, discuss it, and see if there are any problems with the map rather than taking any action on it right now. We haven't seen it yet.

MR. PIGOTT: That's fine.

JUDGE KELLEY: That seems reasonable, to be sure.

MR. PIGOTT: That's fine. I only want to identify it.

JUDGE KELLEY: So it stands identified. Why don't you take a look at it and we might then raise the subject again tomorrow, sometime tomorrow.

I wanted to ask Mr. Heath just one question for the sake of clarity in the record or perhaps for the sake of at least educating me a little bit. When you refer to magnitude it is an MS measurement I think throughout your testimony.

THE WITNESS: That is correct.

JUDGE KELLEY: I wonder if you might explain a little bit just what that means, and let me add a couple of other thoughts. I think the public or a lot of people think of the Richter scale. Is that the Richter scale or is that some other scale? The Staff's document refers to an ML measurement. There are a number of measurements of magnitude, as I understand it. If you would just comment a bit on just what it is you are referring to, is that the surface wave measurement?

THE WITNESS: Yes, it is. I'm not sure I can give you a complete and adequate description of that because I am a geologist and not a seismologist. But I understand from talking with our seismologists they have recommended that we use the MS scale as a pure representation, particularly in the higher magnitudes. The ML scale, which is the local magnitude, tends to saturate due to the nearness of surface waves, as I understand it. In the near field it saturates and doesn't give you true readings for higher magnitudes, where the MS, which is recorded at a longer distance, gives you a better scale.

But beyond that, I think it would have to be handled by a seismologist. I believe we are prepared to do that.

MR. PIGOTT: Mr. Chairman, I believe we previously

scheduled for Wednesday morning sort of a geology - seismology 501 course and that would certainly be covered at that time, which will be tomorrow morning.

JUDGE KELLEY: That's fine.

MR. CHANDLER: Mr. Chairman, if I may suggest, I think there is -- I won't describe it in any way -- there is a rather extensive discussion which I think is fairly useful from purely an educational standpoint which may be found in the recent Appeal Board decision, ALAB 644, dated June 16, in the matter of Pacific Gas and Electric Company, Diablo Canyon Nuclear Power Plant, starting somewhere in the vicinity of page 39, which I think sheds quite a bit of light on the differences between ML's and MS's and all that kind of stuff.

JUDGE KELLEY: Have you got some extra copies?

MR. CHANDLER: The document is somewhat in excess of 200 pages, Mr. Chairman. I do not.

JUDGE KELLEY: Fine. I appreciate that. Then the next question is whether we continue this afternoon and move on with Mr. Wharton's cross or whether we quit at this point. Let me see what the Board's sentiment is.

In any event, I don't think we want to go very mcuh longer. Counsel?

MR. WHARTON: I would prefer to start cross examination tomorrow so it is continuous all the way through.

JUDGE KELLEY: There is some virtue in that.

MR. PIGOTT: I didn't hear what counsel said.

MR. WHARTON: I would prefer to start cross examination tomorrow so that there is continuity all the way through it.

MR. PIGOTT: That will be fine with me because we may be wanting to put Dr. Ehlig back on for just a few minutes.

JUDGE KELLEY: Then let's stop for the evening. Are there any other housekeeping matters that we need to --

MR. WHARTON: Are we going to have an instruction session tomorrow morning?

MR. PIGOTT: I have on other thing that I would like to bring up before we go off the record. Applicants have filed a trial brief. I think we have adequately apprised all parties of our order of proof. We have received the testimony of the Intervenors. We are aware that a number of subpenas have been issued to people that the Intervenors wish to have called. I would like not later than -- pretty soon, and I would like it tomorrow, as a matter of fact -- some idea of the order of proof of the Intervenors.

I would also like to know whether or not I can expect to obtain some prepared direct testimony with the people who are under subpena. It is not my understanding that a subpena automatically takes you off the hook for

filing prepared direct testimony. I would submit that in the absence of prepared direct testimony I should be entitled to some kind of an outline of the proposed examination of subpenaed parties. But in any event, I think I need some guidance as to where, when and what the Intervenors plan to say beyond what we have seen in their prepared direct.

MR. WHARTON: Mr. Chairman, I think that Mr. Pigott is entitled to the same consideration given me, which was I received the trial brief Thursday before the hearing starting on the following Monday, as to his witness list. I am in a position right now -- our planning at this point is to issue the subpenas and then, after the subpenas are actually served, we will know exactly who we are going to have here. You are not always sure that you are going to be able to serve all the subpenas.

We are also in the middle of the hearings on this part, the middle of the hearings on the Applicants' part of the case. I would propose that I would be able to present a trial brief to the Board somewhere in the neighborhood of I figure around July 6 I figure on being able to prepare a trial brief, reviewing all of the witnesses we will have at that time. I just don't know how many are going to be able to make it at this time.

JUDGE KELLEY: How about you, Mr. Chandler? Have you already given a sequence of witnesses?

MR. CHANDLER: No. We haven't specifically laid that out. And quite frankly, because of the obvious limitations on our resources out here, it is not going to be really possible for me to provide anything formal or extensive. Of course we will be presenting Drs. Kennedy and Green on June 29. So a portion of Contention 2 will be addressed at that point in time. I will be happy to try and sketch out a somewhat more meaningful game plan over the next couple of days and provide it to the Board and parties. If possible, I will do so in some formal fashion; otherwise, perhaps some statement on the record to indicate our order of presentation.

JUDGE KELLEY: Let me get this mutuality straight. When was your sequence of witnesses presented, Mr. Pigott?

MR. PIGOTT: My sequence of witnesses was presented Thursday, the 19th, although the full direct prepared testimony was presented I think it was June 8 we filed it. I might say that what I am asking for is pretty simple. All I would like to know is 1, 2, 3 for the ones that have been -- had direct testimony filed. I would like to know which ones come in first, second, third, which certainly can't be that much of a burden.

And with respect to the subpenaed witnesses --

JUDGE KELLEY: Stop there. That's no problem?

MR. WHARTON: No, that's not a problem. We could probably do that by tomorrow.

MR. PIGOTT: The second thing is with respect to the subpenaed ones, I'd like to know if we are going to get prepared direct. I have a feeling that Intervenors are going to argue that they can't comply with that. But in the absence of that, surely they must have had some anticipation as to what they would ask these various gentlemen if they were called. That being the case, I think I am at least entitled to some kind of an outline as to the type of proof they would expect to elicit from the people they will subpena. If they don't subpena them, obviously nothing hits the record. But I do feel that I am entitled to some kind of a notice as to the case the Intervenors want to put on.

JUDGE KELLEY: Mr. Wharton?

MR. WHARTON: Yes. I don't anticipate receiving -- preparing written testimony for the subpenaed witnesses. The review so far with the witnesses is that they neither have the time nor the inclination to sit down and write the kind of prepared testimony that would be needed, nor do we have the resources to pay someone to do that kind of thing if one demanded to be paid.

Our situation with the testimony received, I must remind the Board that we received the --

JUDGE KELLEY: Let me stay on this point, though. If you are dealing with subpenaed truly involuntary witnesses I understand the difficulty in having prepared testimony.

Perhaps it is a contradiction in terms almost. But Mr. Pigott's point is you must have some idea -- you must have a reason for calling this person and what you expect him to say. Some indication of that sort it seems to me he is entitled to.

MR. WHARTON: I would agree. Again, I would look to when we received the prepared testimony, which was a week before the hearings began.

MR. PIGOTT: Two weeks.

MR. WHARTON: No. I received them seven days -- I received them the Monday before the hearing was to start.

MR. PIGOTT: No, you didn't.

MR. WHARTON: And during that particular week we had the pre-hearing conference plus considerable preparation for the pre-hearing conference on emergency planning. It came down to I had four days to review all of the testimony regarding the seismic issues in the hearing right now. I am not talking about just to get even or anything, but I am talking about let's make things a little bit equal.

We have -- they have had a long period of time to review our written testimony which was served on them, I believe, on the same Monday that I received ours.

JUDGE KELLEY: Without counting exactly what the dasy are, you have an advantage, it seems to me, in that you do get written testimony for all their witnesses.

MR. WHARTON: That is correct.

JUDGE KELLEY: And so you know exactly what they are going to say. Now if the most that can be expected from you is some kind of outline of what you think this witness may say, that if we are seeking equity here, disadvantages the Applicants to a rather considerable degree, in my view.

MR. CHANDLER: The Staff would also like to receive a copy of this information as well.

JUDGE KELLEY: Oh, yes. I am not intending to exclude you at all.

MR. WHARTON: May I give you a report on that tomorrow morning when I can expect to put together an outline of what our witnesses will testify to?

JUDGE KELLEY: Yes, that is satisfactory. But I do think that we should discuss it tomorrow and then resolve it.

Anything else?

Thank you very much. We will resume then tomorrow morning here at 9:00 o'clock.

(Thereupon, at 5:30 p.m., the hearing was adjourned, to reconvene at 9:00 a.m., the following day, Wednesday, June 24, 1981.)

This is to certify that the attached proceedings before the

NUCLEAR REGULATORY COMMISSION

in the matter of: SAN ONOFRE NUCLEAR GENERATING STATION

Date of Proceeding: June 23, 1981

Docket Number: 50-361/362-OL

Place of Proceeding: San Diego, California

were held as herein appears, and that this is the original transcript thereof for the file of the Commission.

Tom Willis

Official Reporter (Typed)

Tom Willis

Official Reporter (Signature)