UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

In the matter of:

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

Docket No.

Subcommittee on Extreme External Phenomena and Diablo Canyon

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UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

Subcommittee on Diablo Canyon
- and -
Subcommittee on Extreme External Phenomena

The Papagayo Room
Los Angeles International
Airport Holiday Inn
9901 South La Cienega Blvd.
Los Angeles, California

Thursday, May 24, 1984

The meeting of the Subcommittees on Diablo Canyon and Extreme External Phenomena convened at 8:30 a.m., Dale Okrent, Chairman of the Subcommittee on Extreme External Phenomena, presiding.

ACRS Members Present:

D. OKRENT
C. SIESS
J. EBESOLE
W. KERR
M. CARBON
H. ETHERINGTON

ACRS Consultants Present:

B. PAGE
G. THOMPSON
G. THOMPSON
J. MAXWELL
M. TRIFUNAC
E. LUCO

Designated Federal Employees:

R. SAVIO
J. McKINLEY

PRESENTERS:

H. SCHIERLING
J. KNIGHT
S. BROCOUM
S. ISRAEL
D. McMULLEN
B. ROTHMAN
B. JACKSON
D. PERKINS
J. CROUCH
D. HAMILTON
A. CORNELL
P. SMITH
J. HOCH
D. BRAND
B. KENNEDY
S. SMITH
I. WIGHT

Statements From Members of the Public:

ALBERTA L. RICH
BRUCE CAMPBELL
JUDITH B. EVERED
SANDRA SILVER
STANLEY H. MENDES

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P R O C E E D I N G S

MR. OKRENT: Good morning.

The meeting will now come to order.

This is a meeting of the Advisory Committee on Reactor Safeguards, Subcommittees on Diablo Canyon and Extreme External Phenomena. I am David Okrent, Chairman of the Subcommittee on Extreme External Phenomena and Dr. Seiss is Chairman of the Diablo Canyon Subcommittee and he is seated on my far left.

Other ACRS members present today are Mr. Kerr, Mr. Carbon, Mr. Ebersole and Mr. Etherington. ACRS consultants in attendance are Mr. Luco, Mr. Maxwell, Mr. Thompson, Mr. Trifunac and Mr. Page.

Mr. McKinley and Mr. Savio are members of the ACRS staff.

I believe that Dr. Crouch will be participating in these discussions and will arrive later in the morning.

The purpose of this meeting is to discuss the matters described in Chairman Palladino's April 13, 1984 letter to the ACRS. A copy of this letter is attached to the agenda. Copies of the agenda are available at the doorway to this meeting room.

The meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act and the Government in the Sunshine Act. Mr. McKinley and

Mr. Savio are the designated federal officials for this meeting.

The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register on Wednesday, May 2, 1984.

A transcript of the meeting is being kept and will be made available as stated in the Federal Register notice.

It is requested that each speaker first identify himself or herself and speak with sufficient clarity and volume so that he or she can be readily heard.

We have not received any written statements from members of the public. We have received requests for time to make statements from Mr. Bruce Campbell, Ms. Silver and Ms. Evered. I would ask these persons to identify themselves, if they are now here, so that we can make arrangements for the scheduling of these presentations.

Are Messrs. Campbell, Silver or Evered here now?

(No response.)

Well, we will request again later for their presence.

I would ask that in the discussions today we try to remember the main points of the letter from Chairman Palladino and make sure that we develop as much information related to these points as is possible.

Let me ask the members of the subcommittee whether they have any comments on the proposed agenda? Might I ask whether there are members who will have to leave to get planes before the scheduled or anticipated or guessed adjournment time?

MR. KERR: What is that, about 10 p.m.?

(Laughter.)

MR. OKRENT: Well, I see it says 5 p.m, but I don't know at the moment that that is rigid and I was just wondering what your plans were.

MR. KERR: No problems.

MR. OKRENT: Okay. So we could run later if we need to. I couldn't believe it said 5 o'clock and I said to myself how could I have possibly okayed such a short meeting.

(Laughter.)

Mr. Savio advises me that we now also have received a request from Alberta Rich to make an oral statement and we have a written statement that he will hand out.

Well, I propose that we move directly into the second agenda item and be ahead of the agenda probably for the last time today.

Who is the spokesman for the staff?

MR. SCHIERLING: My name is Hans Schierling. I am the Licensing Project Manager for the NRC staff. I would like to give you a brief summary of where we are currently standing regarding the licensing activities as you requested.

As you know, on April 13, the Commission issued its order and memorandum, CLI-84-5, in which we instated the low-power license which became effective on April 19th after certain legal procedures were exhausted asking for stay of that order.

On April 18 the staff issued an order modifying the current low-power license and that modification consisted of issuing seven technical licensing conditions that would require the licensee to perform certain activities in the area of piping and supports before the staff would recommend issuance of a full-power license.

Also on April 18th the staff issued Amendment No. 9 to the low-power license and hat amendment included two additional licensed conditions. The first one was in accordance with a stipulation by ALAB 763 and also in accordance with the Commission's order that the licensee should perform certain additional jet impingement analyses that had to be completed prior to full power.

The second item was the subject that we are here together to discuss, namely, the formulation of a full-power licensed condition for a revalidation program of the seismic design basis for Diablo Canyon.

The Commission order, by the way, also had requested the staff to issue specific techincal specifications for the component cooling water system which the staff has previously done in Amendment No. 8.

As of this time there are a number of issues which in the opinion of the staff have to be resolved before issuance of a full-power license. Again, let me say these are No. 1, the piping and support issues that I mentioned earlier and, by the way, the staff is pursuing that resolution rather vigorously.

We have assigned approximately 15 people to bring these items to resolution. The staff is currently looking at the material that the licensee had provided regarding the jet impingement analyses. I think we will hear enough today about the seismic design basis revalidation program which again has to be resolved prior to full-power license, and by that I mean elements for a specific program will have to be provided to the Commission before they will vote on the full-power license issue.

MR. OKRENT: What was it that you were saying would have to be provided to the Commission just now, elements of what program?

MR. SCHIERLING: As you will recall, Dr. Okrent, the Commission voted I think on March 26th and 27th on a seismic design verification program. By the way, I forgot to mention on this subject that we had a meeting in early May and I assume that you were provided with summaries of that meeting. As stated in that meeting summary, the licensed condition that we currently have in the low-power license in Amendment 9 is as voted by the Commission.

MR. OKRENT: All right. I just wanted to make sure that it wasn't something else you were talking about.

MR. SCHIERLING: No. As you probably recall, the Commission on March 26th and 27th, at that meeting the staff had prepared four specific elements that would be included in that program, and I think these four specific elements, the Commission in the staff's opinion would like to hear about before they will vote on a full-power license. So the staff considers that this also has to be resolved prior to full power.

The staff never formally documented, although we did report to the Commission on a number of items that had remained open as of late March on the IDVP. The staff is currently in the preparation of preparing an SER supplement on that matter.

As you know, there are before the staff somewhere in the neighborhood of 500 allegations on Diablo Canyon. While most of these are duplicates, nevertheless we treat them as individual items.

Some of these were provided to the NRC as a basis for petition under 10 CFR 2206 and the staff will have to No. 1, address all those open items that have to be resolved prior to full power and, secondly, the staff will also have to respond to the GAP petition itself which is a petition to defer any further licensing action on Diablo Canyon.

There do remain a number of other items that the staff will have to address for a full-power consideration by the Commission and that includes items such as the shift adviser program and a staff evaluation of the staff at the plant during criticality and low-power testing.

As I mentioned earlier, the staff will document in one form or another all of its evaluations of these matters most likely in the form of SER supplements. Some of these are already in preparation and others have to wait until certain activities have been completed, in particular the piping and support activities currently underway.

Looking into the future, there is a

possibility, it is my understanding that there will be a congressional hearing on Diablo Canyon. We also expect that the Commission would like to hear from us before they will vote on issuance of a full-power license.

As you probably are aware, the low-power testing for Diablo Canyon Unit 1 I think was completed yesterday and the licensee informed us that the plant is ready for power ascension for which a full-power license would be required by June the 9th or somewhere thereabouts. That is with regard to plant readiness.

I think the licensee realizes that certain other licensing requirements have to be met prior to issuance of a full-power license and I think the licensee estimates at this time that it will be about mid-June or somewhere thereabouts that he expects to have a Commission vote.

The staff is working on a different schedule. It is our opinion that many of these activities cannot be completed until later in June and we are currently thinking of a Commission meeting maybe in late June or even early July.

This is in summary where we are standing right now regarding the licensing of Diablo Canyon Unit 1.

Is there anything you would like to add?

(No response.)

MR. SCHIERLING: Let me introduce here before we continue further the members of the staff and other people that are here today for the staff. Mr. Dick Vollmer, the Director from the Division of Engineering, Jim Knight, Assistant Director in the Division, we have here Bob Jackson, Steve Brocoum, Dick McMullen, Sandy Israel and Bob Rothman from the Geoscience Branch, except Sandy Israel from the Reliability and Risk Assessment Branch.

We also have here Steve Perkins from the USGS.

Is there anything you want to add at this time?

(No response.)

With that, Dr. Okrent ---

MR. OKRENT: Excuse me. Before you go on, as I listened to what you identified as issues, and particularly issues for this subcommittee meeting as distinct from some of the other things that you were addressing, it seemed to me that you bypassed item B on Chairman Palladino's memorandum to Mr. Ebersole of the ACRS which requested that the committee review testimony before the Commission on the recently received paper by Messrs. Crouch, Bachman and Shay.

I guess I am not quite clear what stance you think you are taking with regard to that part of the letter from Chairman Palladino.

MR. SCHIERLING: First of all, we will make a presentation on that subject today, and Jim Knight would like to elaborate on that.

MR. KNIGHT: The staff at the Commission meeting, and we would continue today to take a position that the Crouch paper is one might say a prime example of the body of information that should be considered in a program of the type that is being contemplated over the next two or three years for the review of the seismic design basis of Diablo Canyon.

In our view it is an integral part of such a program, along with, I am sure most people would agree, a very large body of information that has been developed over the years and more which will undoubtedly follow.

MR. OKRENT: Well, I am not trying to prejudge the outcome of the discussion because right now I have no basis for an opinion, but I myself don't just see it as another part of what you call the body of knowledge.

It seems to me that in this transcript the staff and the USGS offered some preliminary evaluations of the significance of this possible interpretation of geology, et cetera, in the area.

It is my understanding at least of Part B of Chairman Palladino's letter that we look at this interpretation to see that at least it is rather plausible or whatever one wants to say. It has been somewhat singled out, it would seem to me, and it is not just part of a body.

So I hope in fact that what we are going to hear today gives some detailed or as specific as one can reasons for the kind of general judgments that the staff and the USGS provided to the Commission in that transcript.

MR. KNIGHT: Yes, and that is fully our intention.

MR. OKRENT: Good.

Any other questions?

(No response.)

MR. OKRENT: Okay. Why don't you continue.

MR. BROCOUM: My name is Steve Brocoum of the Geology Section of the Geosciences Branch. We are passing out a handout here which will have the viewgraphs from the two presentations we will make today, the first one which is on the specific elements, our proposed specific elements on the Diablo Canyon licensed condition, and the second on the impact of the new information from the Crouch paper on Diablo Canyon. That will be later according to the schedule.

I will be making a presentation as will Dick McMullen, the geologist reviewer on Diablo Canyon and Bob Rothman who is the seismology reviewer on Diablo Canyon.

We will each make different parts of the presntation.

The first slide.

(Slide.)

The first slide just shows you some of the background leading up to the licensed condition.

First of all, there is an obligation of the NRC and the utilities to keep up with the latest information on the science and to update their analyses, especially when a new paper comes along that gives a different interpretation of the techtonics than one had before.

Second, there have been extensive investigations conducted offshore by the oil industry but also by government, seismic and geological investigations. Just to give you an idea of the extensiveness of these investigations I want to show one slide that is an advertisement that we recently saw in the Journal of Geophysics.

(Slide.)

This slide shows the seismic reflection lines that are available from one company only, that is a service company to the oil industry, and these are available if one wishes to purchase them, seismic reflection profiles off the shore of California. In the Santa Maria Basin alone there are over 10,000 miles of seismic reflection profiles available.

Mr. Crouch in his paper presented his on the basis of six seismic reflection profile lines, but he I think had knowledge of many others. But again, this shows you that there is an extensive amount of information available.

(Return to former slide.)

Thirdly, and we can go back to the other side, there, there was a letter from ACRS in 1978 recommending that the seismic design be re-evaluated in about 10 years. In our program that we are proposing, and that was '78, so about 1988, and the program we are proposing would complete this re-evaluation in about 1988.

Another point, point D, is that the Commissioners and many others have questioned the tau effect and there was extensive I think testimony last year on that effect alone.

Now in anticipation of all of this, in February of '84 the Division of Engineering management asked the Geosciences Branch to prepare a possible licensed condition, and on February 23rd we did send from Jackson to Knight a memo listing several options and recommending one particular option, and that would be point F on our next viewgraph.

(Slide.)

That was before we were even knowledgeable about the Crouch paper. On the 22nd of March we received information about the Crouch paper and subsequently we did prepare the proposed licensed condition. That proposed licensed condition was very similar to the option, the one we are suggesting today is very similiar to the option that we recommended on February 23rd.

As Hans already said, the Commission voted in favor of a licensed condition, the words being PG&E shall develop and implement a state of the art program to revalidate the seismic design basis used for Diablo Canyon. PG&E shall submit for NRC staff review and approval the proposed program plan and proposed schedule for implementation by January 30th, 1985. The program shall be completed and final reports submitted to the NRC by July 1st, 1988.

The Commission instructed the staff to in consultation with the utility, and we did have a meeting on May 8thm, and with ACRS, and that is the purpose I think of the meeting today, to specific elements of this licensed condition, and that is what we have and are presenting today. On April 13th the Commission made that paragraph a condition of the low-power license.

The staff suggests that the procedure for implementing the licensed condition be that PG&E prepare a proposed program and submit it to the NRC for review by the staff and take the lead in carrying out this program. Now is the normal mode of operation for most of our reviews.

The NRC and its advisers, which would include national labs, the USGS, Dr. David B. Slemmons and others, would review the proposed program, make comments and make suggestions and finally approve it. They would review the results of the program and they would at the same time during these three years conduct parallel investigations so that we would be able to properly advise PG&E and properly review their results.

(Slide.)

Now we have four specific elements. They are listed here as Conditions 1 and 2. I think the way we refer to them now is we have a licensed condition. So what is listed here as Condition would be specific element No. 1.

First of all, we want to update the information relating to geology, seismology and geophysics since the ASLB hearings in 1979.

Again, as I indicated by that one slide and based on new knowledge on seismology and strong ground motion and geophysics, we have about five years or more new information to consider. There might be some cases when we want to go back and re-evaluate pre-'79 data which may based on the new information be shown to have not been evaluated say correctly.

Finally, it is not the intent to require PG&E to collect new information. It is the intent for them to evaluate all the new information that is available. However, there might be some instances where they may have to go out and collect new information where there is no available data and it might be critical to the interpretation say of a new techtonic hypothesis.

The purposes of updating the geology, seismology and I should add geophysics is, first of all, to determine the character of the Hosgri Fault at depth. Is it in fact a thrust fault or is a strike slip fault, is it changing from a thrust fault to the south to a strike slip fault to the north, does it pass beneath the site, which is a possibility if it is a thrust fault, and if it does pass beneath the site, how close to the site does it pass?

As the interpretation of the Hosgri Fault may change, it may be desirable to relook based on all this new information at the length of the Hosgri Fault. I believe that the length was determined to be 140 kilometers in '79, but if it is a thrust fault instead of a strike slip fault, maybe the length parameter has to change and maybe all the new data will give us new information on the length.

We would also try to determine what the length from a single rupture of large earthquake might be and what the length of rupture might be. We would also hope to determine how recent the last movement was and how frequently it has moved say in the last several million years. That would be for recurrence information.

Finally, if the Hosgri Fault truly is a thrust fault, we would want to be sure that there are no significant thrust spays or faults near to the site. Most thrust faulted terrains consist of numerous listrick or faults with flatten at depth. So if the Hosgri is a thrust fault, there is a possibility that there may be faults elsewhere which may also be thrust faults that have been undetected or, if they have been detected, have been mischaracterized.

So that is the first specific element.

MR. OKRENT: Before you take that away, what does it mean to say confirm overall length?

MR. BROCOUM: Well, I think our position was or the determination of the ASLB was that 140 kilometers will be the length you will have to assume for making calculations for making magnitude calculations of possible earthquakes.

Now if it happens to be a thrust fault instead of a strike slip fault, we may have to look at that question of how long it is. I think that is what we mean there. I think if there is different techtonic interpretation, maybe we have to relook at the question of how long the fault is. That is what I am trying to get to.

MR. OKRENT: I agree that you might have to look at the length of the fault and you have said one would try to look just where it passed under the site, if it does, and so forth. I was just trying to understand the usage of the word "confirm" in several places.

MR. BROCOUM: Okay. I guess because we had determined it was 140 kilometers, we want to, you might say, redetermine what the length is in light of the new hypothesis. We can change the word to determine I guess. I don't myself have a strong feeling on confirm or determine, but we are starting with 140 kilometers since that is the number we had in the past.

MR. OKRENT: You first said that you had in mind that PG&E would be using available information although they might have to try to get some new information. Are you going to discuss that aspect of the thing in greater detail in some other part of your presentation?

MR. BROCOUM: Not specifically, but I could make a comment now.

MR. OKRENT: All right, would you.

MR. BROCOUM: Most of these seismic reflection profiles, which will probably be the best single tool for determining the geometry of the shape of the fault at depth, are offshore profiles. They don't go onshore or very close to shore. If you follow the Crouch paper, you can follow the fault back towards shore a certain distance. After that you don't know what the fault does.

So it is conceivable that PG&E may have to do a seismic reflection profiling into shore and on shore to determine the geometry and if the fault passes underneath the site and how close it comes underneath the site. That is the kind of new information. The data may not exist. So they may have to go and collect it. That is one of the examples.

MR. OKRENT: I was wondering if that is what you had in mind.

Mr. Kerr.

MR. KERR: How will whoever does this know when it is finished, by looking at the calendar or is there some defined end point that says the study is now complete?

MR. BROCOUM: I guess what I would have to say is when they have looked at a representative amount of the available data, and depending on how well the various kinds of data agree with each other, I think we could reach a judgment at that point if they have done enough work.

If they look at several lines of evidence and the results are very diverse, then we would probably require them to look at some more. But if they look at several lines of evidence and they all suggest that the fault is a thrust fault, for example, then that particular aspect of is it a thrust fault near Diablo Canyon may be answered. I think that is the way we normally would do that.

MR. KERR: I interpret that answer to mean they will be finished when the NRC staff determines they are finished.

MR. BROCOUM: That is generally the case in most of these studies I believe and in most of the investigations.

MR. KERR: I think it would be possible to tell someone what you wanted them to look for, and I don't see from what I have heard that they know what you want them to look for. I am certainly no seismic expert, but I would have difficulty knowing other than to go out there and look at existing evidence.

MR. BROCOUM: These are some of the things we are asking for, but we are suggesting that PG&E develop a detailed plan. It is their plant and they know more about it than we do. They develop a plan or a detailed proposal which we will then interacting with them review and comment on. So this is just meant to be an outline at this point.

MR. KERR: Suppose they concluded after an initial look that enough had been done. That would be a proposal that they might make since they know more about the plant than we do you tell me. What would be the staff response to such a proposal?

MR. JACKSON: I think you are touching on some items that we might discuss later on on the Crouch paper. I was just sitting here thinking that it may have been better for the agenda to do it backwards from the way we have it.

MR. KERR: I will wait.

MR. JACKSON: I think what it is is we essentially developed, based on the Commission's guidance some broad general conditions to be met. When we discussed these with the Commissioners they said that they really didn't want to have specific elements in there by us defining that at this point in time.

We have had one extensive meeting with the utility in which we discuss with them the kind of things we are looking for and we would be looking for feedback from them on those types of things they think need to be done. For instance, I think a reasonable option is that in certain areas some things do not need to be done, nothing additional needs to be done.

It is a judgment area and it is an interactive area. It depends on what has become available in different time frames and what you have available in different areas. We do not know all that right now.

I think to come back and touch on another point on the "confirm," as a staff we really feel that the seismic design basis and the knowledge we have of the plant vicinity in general is adequate to make the decisions we need to for the plant.

We are going to continue to learn new things, and even after this study is done, geologic sciences is not going to stop doing work offshore California or onshore California and there is going to be a continuing development of knowledge and data,and you are going to have to make some judgment as that data becomes available as to whether or not it is eroding in any way the previous judgments you have made on the site.

I think what we are looking at with this Crouch paper and any other paper that might be out there that we are not aware of is the fact that we have a sound and reasonable within the geologic framework design basis and that is where we came up with the word "confirm" going back in. We can play with semantics of what confirm means.

MR. KERR: Dr.Okrent asked and I would also ask, and it will perhaps be answered later on, for the operational significance of the word "confirm," that is how will they know when something is confirmed?

I recognize that you can't answer that very specifically, but it seems to me if you start the process that it would at least be well to have something in mind as to how one knows when something is confirmed.

MR. JACKSON: I understand what you are saying. There are some sort of review criteria by which you pass or fail. But I would comment, and I know we have argued about this before, in the geologic sciences that is a very difficult thing to decide on when you have meant some pass or fail. We don't have codes and other things.

MR. KERR: Don't you think one is more likely to find something if he has at least some idea of what he is looking for?

MR. JACKSON: Well, I think between ourselves and PG&E and the USGS we have a fairly good idea of what kinds of things need to be looked for and that needs to be put down. We haven't put that down in an itemized list at this point in time. That is what we would be doing with the utility between now and next January. We have had one meeting and we agreed that we would have essentially one more meeting with a wide range of experts in each different condition that has been recommended here because it is a different suite of professional experts that would have to have input into how you develop a program in those areas.

MR. BROCOUM: We are also going to have a senior adviser, and I will get to that later a little, but we are going to have a senior advisory review panel to help us in implementing this licensed condition.

Okay, that is the first specific element.

(Slide.)

Second, if the interpretation of the techtonic picture changes in the area of Diablo Canyon, we would have to re-evaluate the SSE, the magnitude of the SSE which on the Hosgri Fault or any other fault which was found to be the controlling fault, and it is of course the Hosgri Fault at the present, we would have to again look at fault length and the magnitude determined from fault length, from rupture length, from rate of slip, from, if determinable, maximum displacement during a single event, from historical seismicity, and there is a lot more data in the last five years, and from any other approaches that are available such as area of a fault plane.

In this aspect of the study the USGS and Dr. David B. Slemmons will be very much involved.

Again, the reasons being is that the new data in geology, geophysics and seismology may cause us to come up with a new techtonic picture of that part of offshore California and we will be using the latest available methods for determining the magnitude and the latest available regression analyses. The magnitude calculated from the thrust fault may be different than the magnitude calculated from a strike slip.

Now the next specific element concerns the revalidation of the ground motion at the site, and I think that Bob Rothman, who is the seismology reviewer on Diablo Canyon, will cover elements three and four.

MR. JACKSON: Do you want to take questions on this segment before you leave or you could come back afterwards.

MR. BROCOUM: Well, maybe Bob would be the best one here since he is a seismologist.

MR. JACKSON: Well, why don't you stay up there in case they have questions on the whole package.

(Slide.)

MR. ROTHMAN: I am Bob Rothman. I am the seismologist in the Geosciences Branch of NRR.

The third element of the licensing condition would be the re-evaluation of the vibratory ground motion at the site. Maybe I should explain a little background.

At the time of the Hosgri reanalysis of Diablo Canyon there was very little near-field strong ground motion data available from large earthquakes. Because of this, the spectrum used by Dr. Newmark for the Hosgri reanalysis was based on the Pacomia Dam record as recorded from the San Fernando earthquake of 1971. At that time this was the largest horizontal ground motion recorded from an earthquake.

Since that time there have been a number of strong ground motion recordings made near to the large earthquakes and the data base has expanded significantly. There has been a lot of work done on this data base, regression analysis for various aspects of faulting, site conditions and magnitude.

What we are looking at is a multiple element approach to this. We don't think we have enough confidence. Although the data base is larger than it was, it is not the type of data base that you can get unique answers with.

So what we are looking at it a multiple approach to this looking at several different ways of evaluating the ground motion, regression analysis of both horizontal and vertical spectra values for site specific conditions and those are based on the type of faulting on the Hosgri Fault, the distance from the fault to the site and the geological conditions at the site.

Site specific spectra, both vertical and horizontal developed in other measures. Developing a spectra shape and anchoring it to a peak acceleration is another way of doing it.

Numerical model studies used the most recent techniques and theoretical modeling to model the fault. The various sensitivities of these studies are rupture propagation rates, stress drop, orientation of the fault to the site, type of motion from the site and the site conditions to develop a theoretical basis for comparing your empirical data base.

Then the fourth element in this study would be a soil structure interaction type of analysis. This would possibly answer some of the questions that have been raised in the past about the tau effect, which we could call a foundation averaging effect. We have have the effect of embedment of the large nuclear power plant structure on the ground motion at the site.

The parallel effort by the staff would include work by the USGS in the analysis of a strong ground motion, a data regression analysis and also support probably from the national labs in reviewing this information.

MR. OKRENT: Excuse me. Before you take that away, in looking at some of the prior viewgraphs that have been used by the staff, when soil structure interaction effects was mentioned I think it said empirical or analytical indicating that one of those two approaches, for example, the empirical approach might suffice.

In your discussion today you didn't phrase it the same way. Am I missing something?

MR. ROTHMAN: No, I don't think so. We have not made a decision. We think probably a two-pronged attack might be the way to go. We have not made a decision on numerical or empirical data. It may be one, the other or both. We haven't reached that point yet.

Our position is that probably the best way to attack any one of these problems is to look at as many aspects as we can because we don't think any one particular way will answer all the questions.

MR. OKRENT: Is there good empirical data on this now?

MR. ROTHMAN: There is some empirical data from recent earthquakes. I don't know if you would say good. We have seen recordings from inside structures and free field from the same earthquake, nearby free field, which would tend to indicate that the recordings in the structures were lower, let us say. But these have to be evaluated as to whether you are seeing amplification outside the structure or a decrease in signal inside.

Somebody has to do an analysis on this and see just what the effects are, but there is no data available now than there was five or six years ago. Somebody has to take a hard look at some of this data.

MR. OKRENT: Is there near-field strong motion for a large thrust fault earthquake?

MR. ROTHMAN: Pacomia Dam.

MR. OKRENT: No, no, since.

MR. ROTHMAN: Well, we have the Coalinga earthquake as recorded in the Pleasant Valley pumping station and in the free field near the station. That was a thrust fault of about magnitude six and a half I believe.

MR. OKRENT: How near was that?

MR. ROTHMAN: I don't know the exact numbers, but less than 10 kilometers. The exact number I don't know. There are also a number of aftershocks from that earthquake that were recorded.

MR. OKRENT: Mr. Kerr.

MR. KERR: Under those requirements do I get the impression that the techniques for doing this and the methodology exists, but one must apply this now to Diablo, or do the techniques have to be developed as well?

MR. ROTHMAN: The techniques are more or less developed now. The data base has expanded. So you have to incorporate the new data into your regression and see how that affects the significance of your analsis.

MR. KERR: Developing new data into techniques doesn't accomplish anything if the technique is mature. So you are telling me that the technique is evolving?

MR. ROTHMAN: That is right. If you are looking at a regression analysis, as you get more near-field data, your coefficients may be changing in the analysis.

MR. KERR: So I think you are telling me that there will be technique development required as well as application of a technique to this specific plant.

MR. ROTHMAN: Right. Since these techniques have been evolving we have gotten more near-field data from thrust faults and strike slip faults which allows you to do a sensitivity study on fault type.

MR. KERR: So I could classify this as a research program as well as an analysis program, right?

MR. ROTHMAN: I wouldn't necessarily call it a research program.

MR. KERR: No, I was saying that I would.

MR. BROCOUM: At the Commission meeting there was some debate over the words "state of the art." At least one of the Commissioners was worried that we would be requiring the utility to come up with new techniques instead of using say tried and proven techniques.

I think what we would say here is we are trying to avoid requiring them to come up with new techniques, but maybe improving existing techniques as a function of the availability of new data. But I think it is the Commission's intent noc to require them to develop new techniques.

MR. OKRENT: I think it is fair to say that back in 1978 the utility was with the help of its consultants applying what were then relatively recent techniques to provide an estimate of the ground motion at the plant. I meant they did this of their own volition to try to support their case.

MR. SIESS: Excuse me. Somebody used the express tried and proved techniques. Could you give me an example of a technique that you think has been tried and proved and what kind of proof and proof of what, proof that it works?

MR. BROCOUM: As a geologist I will talk about when say you mapped around Diablo Canyon and you map the faults. Now geologists have been mapping them for a hundred years. A more geophysical type technique which has been greatly used by the oil industry is seismic reflection profiling of which you can see there is a lot of data. When the oil industry drills, they have to be able to identify what we call traps. So they have refined these seismic reflection techniques to a high degree. They are always of course being improved, but they are not fundamentally new techniques.

I think what the Commission was getting at is we should require them to invent, if you like, or to discover a new technique. Now the utility may do that on their own if they see something, but that is not I don't think the intent for us to require them to do that. They don't want a research program. They want a reverification or revalidation program.

MR. SIESS: Well, if the emphasis is on state of the art, it seems that you would not only not require them to develop a new technique, but you would not permit them to use a new technique that was not as you called it tried and proved.

MR. BROCOUM: But almost any technique used in the earth sciences is constantly being improved. I think if they have a ---

MR. SIESS: You didn't say improved. You said proved.

MR. BROCOUM: I was trying to paraphrase in that statement the intent of I think it was Commissioner Bernthal's statement. I can't quote him directly, but I think the Commissioners made that tried and proved. I wouldn't say that myself because the techniques in the earth sciences are always changing.

MR. JACKSON: Dr. Siess, I think the comment you mentioned is that we would require them only to use proven techniques. I would tend to disagree with that, and I think as a staff we would think that would not be totally appropriate.

I think that some of the greatest advances that we have had in the earth sciences have come as a result of the nuclear power plant work, I know in the earthquake fault trenching exercises and in most of the areas of ground motion.

I think the idea here is that we know that these techniques are constantly evolving. The area of soil structure interaction building effects is developing at a rapid rate. There were a number of recent meetings over the past year or two in which there is still a great deal of argument over what is true and what is not true in this area.

I think what we are trying to say as a staff is that we think what the responsible thing to do is to use the best techniques you have available. To use an example, the utility for San Onofre 2 and 3 implemented a numerical modeling study which was very innovative and used a good approach.

It had some problems with it, but looking at that data they acquired from that particular modeling study in concert with other knowledge that we had, not just by and of itself, but looking at it as it compared and contrasted with other data you had from your limited data set, it showed and gave the staff at least great confidence, and I believe the ACRS and the ASLB, that the judgments being made were were indeed sound.

I think the same philosophy goes here on the Diablo Canyon project. We are not telling them and didn't have envisioned to go out and advance the state of the art in soil structure interaction 10 years and then come back to us when that is proved, if we want to argue about those terms, and then go do that for the site. I think there will be a combination of both, using what is currently available and then if there is some area where you may need to look at new data and it comes in new, we as a staff, along with a peer review panel, we would try to look at the veracity of that work that was done.

This is a concept that is really important in the peer review panel. When we met with the utility a couple of weeks ago, we can to this issue on the soil structure building effects kind of problem. When we came to that we realized this is an area that would take an entire day or two just to decide on the kind of things you might want to look at and what might be done. I know you have Dr. Luco here and he might be able to offe: some comments on that also.

But I think that is what the philosophy is. I would not discourage them from advancing the state of the art as long as it is reasonable and we can consume it in a reasonable time frame.

MR. SIESS: When you mention new approaches and new techniques you are really thinking of things that might have been new in '78 but not used or things that have been developed since 1978. You are not really talking about things that might be developed between now and July 1st, 1988?

MR. JACKSON: I think that is correct.

MR. SIESS: Thank you. That is very helpful.

(Slide.)

MR. ROTHMAN: The fourth eleme.. of the condition will be an assessment of the significance of the conclusions form the estimation studies which have been done on the three preceding.

The purpose of this condition is to assess the significance of any differences, if there are any, between the existing seismic design basis and that inferred from the new information presented in the preceding three subelements.

Since PRA, probabilistic risk assessment, allows us to associate risk with different levels of ground motion, it can be put into the context of the impact of any changing geological information or seismological information on analytical assumptions, and it also serves as a tool for screening to identify so-called weak links where focused attention may bring about significant improvement in calculated risk and, if necesary, a limited deterministic analysis may be called upon to better define the seismic margins of specific systems in the plant.

MR. OKRENT: I didn't understand when I read it and I don't understand when I have heard it said just what it is you are trying to distinguish when you say you will do a seismic PRA, and I am thinking of what I read, it said using the capabilities determined by the SSE or some words like this, and then, if necessary, look beyond it to make estimates of capability.

Could you explain all of this a little bit?

MR. ROTHMAN: I think I can explain this somewhat. Currently we have a facility that has been built, and it is built to a certain seismic capability. We can call this the design or the analysis of the plant. We are now going to come up with a seismic hazard curve, whether it be a deterministically developed curve or a probabilistically developed curve or possibly both.

We want to then use this input information, the seismic hazard to the plant to evaluate the systems within the plant to see if there are any weak links. We have Sandy Israel here who is from the group that does probabilistic risk assessment work and I think he would like to say some words about this.

MR. ISRAEL: Dr. Okrent, I guess you are familiar obviously with the PRA's that have been done in the past on Indian Point, Zion, Limerick and what-have-you.

MR. OKRENT: Yes.

MR. ISRAEL: These PRA's have included assessment of the seismic events in a probabilistic framework. Basically there are several elements to doing a seismic PRA and let me go through those to bring everybody up to speed, including the geologists and seismologists.

(Laughter.)

From my standpoint the most important thing is the systems analysis rather than the seismic hazard curve. Basically what we are looking for is what are those combinations of basic events that could potentially cause core melt and/or off-site consequences.

In order to do this one has to develop some sort of plant familiarization to determine what causes the failure of the various systems of importance such as the auxiliary feedwater system, the electrical systems and the ECCS systems. This sort of requires plant walkthrough and some searching for potential common mode failures which are probably the most significant failures in association with seismic analyses because the seismic event has that potential.

I think it is important that when one starts looking at what fails the systems that one has to be concerned with the support systems that are needed to operate the auxiliary feedwater system and/or the ECCS or whatever have you, because failure of the support systems would indeed cause failure of the frontline systems needed to mitigate potential accidents.

The next aspect of this is to pull together what we will call event trees to determine what are those combinations of failures that actually do give you core melt. There may be several different types of events that result in core melt. I think one of the difficulties with it in the seismic area is that it is sort of an iterative process and in order to make it manageable one has to have a feel ahead of time as to what the potential weak links are in the plant in terms of seismic capacity and focus on those maybe more than in dealing with a lot of peripheral equipment which may have very high seismic capacity and which would add considerably to the effort required to prune all of the information.

But having pulled together the set of sequences that lead to core melt, obviously to get back into the seismic and the engineering area, one thing is obviously the seismic hazards curve and that was really addressed by the first two conditions that were talked to today.

It is sort of interesting that in terms of the seismic hazards curve that this type of re-evaluation is probably more in depth at Diablo Canyon than it has been or would be at other plants where we probably have done considerably less than what is being considered here in terms of seismic hazard.

You are also worried about the fragility. Having established those components, those sequences and those combination of components that lead to core melt, you are now concerned about the fragility of those components. A part of that analysis was condition 3 with the local site ground motion and soil structure interaction which affects fragility.

Having developed the fragilities obviously for the various components, everything is convoluted, the seismic hazard, the fragility curves and the set of sequence equations that lead to core melt, and you end up with coming up with the likelihood of core melt for a specific sequence or sets of sequences.

MR. OKRENT: I think this is helpful for those who don't know what goes into seismic PRA, but my question was really more specific in what the staff had written before, for example.

MR. JACKSON: What are you reading? We have written a lot of things in the past couple of months and I probably don't have it in front of me.

MR. OKRENT: It is a memo dated May 7th, 1984 is the on I happen to be looking at at the moment, signed by Robert Jackson.

(Laughter.)

MR. JACKSON: I vaguely remember it.

(Laughter.)

MR. OKRENT: There are no page numbers, unfortunately, a practice I abhor.

(Laughter.)

But there is, nevertheless, an item 4 after you go a few pages which says "PG&E shall assess the signficiance of conclusions drawn from seismic re-evaluation and revalidation studies on items 1, 2 and 3 utilizing the follow two elements.

"A. PG&E shall perform an up-to-date, realistic seismic probabilistic risk assessment, PRA, assuming the seismic capacity of the plant as it is actually constructed."

I am trying to understand that last clause and what you mean and why.

MR. ISRAEL: All right. What I visualize here is that we have a number of elements that deal with the front end, the seismic hazard curve end, if you will, and I presume that coming out of this review up front there may be several options in terms of what a seismiz hazard curve could be.

Certainly using a probabilistic framework one could look at the various options and run them through the calculations to see what impact they have on the core melt likelihoods, if you will. Then that would serve as one of the inputs to determine whether their concept of what is an adequate earthquake design dealing with whatever this new information is is appropriate or not.

Going a step further, suppose that at some point they say no, we think that the concept now of the seismic problems with the plant are sufficient and maybe something more should be done, and I think that is where the second area comes in from the seismic PRA.

We have narrowed our scope. We have identified potential weaknesses in the plant, and if something more is going to be done, presumably we would deal with those specific aspects that loom largest in the seismic risk.

MR. OKRENT: I am sorry. If you think you explained that clause, I missed it. I will read it again.

MR. JACKSON: Let me try, Dr. Okrent.

MR. OKRENT: All right.

MR. JACKSON: I still can't find where it is, but let me make a couple of comments though. If I can step back just one minute. I think the comments on the requirement for a PRA in general were to do two things.

One is when we deal with this question of what is the response spectrum for the site going to look like after we look at all this new data, whether it be lower, higher or the same, we then have to decide how we are going to deal with that four years down the line.

So in wrestling with that we are saying how can we deal with this. One way to deal with it is very straightforward and say we will deal with the problem of, and let's say we have an exceedence or a higher response spectrum in some frequency range, and we just require them to backfit to that level. That is the simplest thing, or reanalyze that level, and that is what we have done. That is the deterministic margins analysis.

So with wrestling with the question internally in the limited time frame we had, we said what other techniques might be available to be able to look at this also realizing that you are going to learn a lot of new things even before this three-year study or four-year study is done.

So one of the ways was to say how can we put this potential into some other perspective, and one way is by the PRA approach. The PRA approach basically indicates that we can then deal with the fact that we have some exceedences in some frequency range by putting that in a risk perspective, and that risk perspective, and it is the same argument that we made to you in I guess Sequoyah in '78, is if indeed that exceedence may be a factor of 2 in the order of 10 to the minus 3 or 10 to the minus 4 or something like that, it may not be worthwhile to do anything or it may be the same.

Now to go to the second part, the more specific aspects of the question, was that when you are doing PRA, the general PRA that has been done, you have some plant specific information. In Diablo Canyon, because of all the reanalysis that has been done, and this is what Sandy alluded to, you have a lot more specific equipment component systems information than you might have on some of these other plants.

I think the only intent of that statement, which I think you are reading into more than was intended, was that in doing this and trying to give advice to the utility, don't use off-the-shelf generic observations on some piping run, but use those that you know exist in the plant. That is all that was there, and I think that was the basic intent of the comment, or that was the basic intent of what is there.

MR. OKRENT: So the phrase "assuming the seismic capacity of the plant as it is actually constructed" means you should use plant specific fragilities in terms of what you actually have there?

MR. JACKSON: That is what was the intent, yes.

MR. OKRENT: And this is some probabilistic capacity and not a deterministic capacity going in for the calculations?

MR. JACKSON: That is correct, but that area still has to be worked out with the utility. We don't know how much falls into this category and how much unique data they have here compared to a Zion or another Limerick or whatever.

I think the thing we are saying is if you are going to do it, you might as well use the best knowledge that you have and that you know for this particular plant which has been looked at lot.

MR. SIESS: Bob, I am having trouble understanding the distinction between the PRA and the deterministic things that occur at two places. There is a deterministic estimate and there is a deterministic analysis. In what sense are you using deterministic?

MR. ROTHMAN: We are using deterministic in that sense based on empirical obtained data, seismic hazard data and theoretically developed say from a model study as opposed to probabilistic data which would be developed under a PRA.

MR. SIESS: The deterministic then applies to the seismic input and not to the structural response?

MR. ROTHMAN: A limited deterministic analysis can be used to better define the seismic margins. In that case that would be in the deterministic analysis of the plant looking at the design versus possibly any new information that comes from the deterministic evaluation of the seismic hazard.

MR. JACKSON: I think, Dr. Siess, that the two things are really separate. To go back to my earlier comment, we say look at it through a PRA and if the exceedences fall within some small risk band based on what you know and the degree of uncertainty you have in this plant.

Let's say the exceedences might be very high in some frequency range, high enough that you don't have confidence in the PRA results that you might have. You may want the utility to go back and look at that particular piping rung, let's say as an example, and look at that specifically, not using a range of possibilities, for instance, but using an actual deterministic analysis of what is really there and then upgrade it or backfit it as necessary.

So it is like a second step in a decision process. You use one ---

MR. SIESS: But the deterministics apply there to the seismic input?

MR. JACKSON: Well, both I think, the analysis also, both the input and any analysis, but you wouldn't use a range of possibilities of fragility failure. In other words, you wouldn't have a distribution of failure, but you would have some picked point at which it would fail.

If you are using a PRA you would be looking at a distribution of potential failure levels. So I think there may be some decision point, and we just wanted to maintain that point, that you may have to drop back away from the probabilistic analysis at that point in time.

MR. SIESS: I just don't understand how you would make a decision based on those two kinds of studies. The current licensing process would be the second.

MR. JACKSON: I think the intent of this whole PRA, and this bothered the Commission a great deal when we went to them the first time, was that we were trying to supplant the existing deterministic regulation with PRA as a decisional tool.

I think there is no intent -- well, I shouldn't say no intent -- I guess there is no reason for us to conclude that we are trying to supplant the regulation. What we are saying is you use the seismic PRA and use the PRA in general to help you make the overall judgment on the adequacy of what you have learned, whatever that may be.

In the end you are really making a deterministic judgment, if you like, within the regulations which is what is required by the regulations, but you can use the PRA for insight in making that judgment. I think that is all we argued. Essentially we really argued with you all on this on Sequoyah back a few years ago and this is not greatly different than what we had proposed in Sequoyah.

What we argued there, if you recall, is that although we had some exceedences of the design spectra at some frequency ranges, that it all fell within a factor of two to four in the same order of magnitude and therefore we recommended no change was necessary. But in the end the ACRS requested that we go back and do "a deterministic analysis" to show that the equipment met the higher level.

MR. SIESS: Since you mentioned higher level, can I go to another item on that page "i" we are discussing. It is not on the slide. It is under 4(A). In the last line it says "Different ground motion levels up to and beyond the existing seismic design basis." Have you got any idea of how far beyond and how you would arrive at that?

MR. JACKSON: I think the general answer, and Sandy can comment further on it, is that when you do PRA you assume a wide range of possibilities. I think you are asking specifically whether it would be 2, 4 or 8 times the SSE or something like that?

MR. SIESS: Yes. I mean we have seen figures like 4 and 5 for eastern U. S. sites and I am wondering if you are thinking of 4 and 5 for the California site.

MR. ROTHMAN: Well, presumably the hazards curve will be whatever it has to be in order to encompass ---

MR. SIESS: Wait a minute. Start over. You lost me.

MR. ROTHMAN: The seismic hazard curve, which is the input function, which would be the frequency of having an earthquake of such a size would encompass the range necessary to general core melt frequencies.

MR. SIESS: So this would be based on the physical phenomena, and whatever the range is you get from the physical phenomena, from the geology seismology and not an arbitrary.

MR. JACKSON: Correct.

MR. SIESS: And not like Livermore did.

MR. JACKSON: I am not familiar with what Livermore did.

MR. SIESS: Okay.

MR. OKRENT: Dr. Trifunac, did you have a question?

MR. TRIFUNAC: Well, I have a question and a suggestion at the same time. As I listened to the discussion so far, and perhaps I should wait until later on, but we seemed to focus on a lot of details, is the fault a hundred kilometers long or is it 150 kilometers long. Whether you increase the seismic ground motion levels by a factor of two or whatever is one question, but how does that change the distribution functions and the outcome of calculations in the PRA is another.

It seems to me that a lot of effort and a lot of discussion is focusing on what goes on with input and we are just not looking at some of the uncertainties, the major uncertainties in what happens later on.

It might be worthwhile as we go along to ask the question is that going to make any difference? Let me give you an idea of what I am talking about.

We go to the probabilistic description of ground motion and then we put that into another little nasty calculation of response through the mean estimates and we don't do any distributions of response whatsoever, and then we put that into probabilistic calculations of fragilities, which are rough guesses at best in most cases, and then we come up with some distribution curve.

Now I have never seen anybody ask the question if we considered the dynamic response of the structure seriously in the probabilistic sense, perhaps the standard deviation of that is going to be so large that whether the ground motion is .5G or .75G doesn't make much difference.

I am not saying it is that way, but I see enormous emphasis on one item which happens to be earthquakes just because so many earthquakes took place recently and we just ignore a lot of other steps in the process.

So if we are going to do a re-evaluation, I am wondering whether it is wise to have somebody look at this and say well, perhaps we could have done a lot of these things 10 years if we really took a detailed analysis of all the procedures and tried to integrate everybody's work and not waste time on arguing whether this is thrust fault or a stike slip fault. Everybody is concerned whether it is a thrust fault or strike slip fault.

MR. JACKSON: I think the point you are raising is a very good one. When we went to the Commission the first time we essentially wanted to offer up the seismic PRA or PRA I guess in general as a first step. That did not receive great favor there at the meeting and it was because of the comment made earlier about the problem in the regulation we have is the deterministic regulation.

I think what you are saying is a very good observation. What it is saying is we ought to go at this a little bit backwards. We have a lot of uncertainty in the geology of the meaning of the fault, the estimation of magnitude and the estimation of the ground motion from that magnitude.

What you are saying by using probabilistic analysis of the plant is you can back out and see whether or not you know whether a six and a half or a seven and half is really important to the overall analysis or whether or not it is a strike slip or thrust fault.

I really think that is an important point, but I think the reality is, although that is a good way to go at it, but the reality is we do have a deterministic regulation to be met. So we can only use the PRA for insight.

But I think when it is done you could feed back through the system and say look, it does not matter too much. Once I know how my plant behaves, it may not matter too much whether the ground motion is in the vertical direction as one level or another level. Therefore, that reduces the burden on the seismologist and geologist to describe that very accurately which I have been trying to get to be done for about five years or so.

MR. TRIFUNAC: Well, I remember early in the 1970's when somebody came along and said this ought to be a seven and a half magnitude earthquake, and I hear today and I look at these slides which say well, we ought to reassess the fault length and we ought to reassess the geometry and does it come three kilometers from the site or does it comes six kilometers from the site.

It was clear 10 years ago that this was a near-field ground motion and it is clear today, and whether it is going to break for at kilometers or it is going to break at 20 kilometers, again it is not going to make any difference. We have pointed out 10 years ago that it shouldn't be seven and a half because of that proximity and we are still talking about the same thing.

I think it is only fair for the utility in this case for the NRC to reconsider this seriously and everybody to reconsider it seriously. You have a site close to the fault and the physics of the fault adjacent to the site is going to make some difference and we should focus on the parameters of the physical process that are relevant.

MR. JACKSON: We have Paul Smith here from Lawrence Livermore Laboratory and he wanted to make a couple of comments on what actually is done from the PRA.

MR.SMITH: Paul Smith, Lawrence Livermore Lab. I just wanted to make a comment that at least as I understand Dr. Trifunac, what he is asking to be done is actually done in PRA's, whether it is commercial or what we have done so far on the more detailed work on the SSMRP and will be also included in the simplified work on the SSMRP. That issue is in fact addressed explicitly and specifically as far as uncertainty in response.

MR. OKRENT: Let's see, if I understood one of his points, it was whether or not the response of the structure was done only in terms of the mean or probabilistically. Are you saying it is done probabilistically?

MR. SMITH: That is correct.

MR. OKRENT: Both in SSMRP and the shorter method?

MR. SMITH: Yes, it is. It is just a different technique that is used, but the issue is explicitly included in both techniques.

MR. TRIFUNAC: Has it been included in previous PRA'S?

MR. SMITH: To my knowledge, it has been

included in all of them.

MR. TRIFUNAC: It would be very helpful if somebody could point out how and where.

MR. SMITH: Well, if you would look in the fragility reports you will see factors which address response and there are betas associated with those factors and they do address specifically the issue that you raised. I mean whether you agree with them or not if of course another point as to their size and whatever, but of course in the SSMRP there is a lot more detailed work because of the research nature of the program.

MR. JACKSON: Which one was it? Which report was it?

MR. SMITH: All the fragility reports that I am aware of ---

MR. JACKSON: For Limerick or Zion.

MR. SMITH: Yes. The most recent one I have seen is Millstone, and it various from specific parts of the plant to another and you may or may not agree with what they have included, but the issue is included.

Yes, Dr. Siess.

MR. SIESS: There is a step between the seismic hazard and the component fragility and that is the analysis of the structure and the structure response. Is that considered a probabilistic variable in your SSMRP studies and in the other PRA's?

MR. SMITH: Yes.

MR. SIESS: The damping factors, the multilevel inputs and so forth, are those varied systematically over a range, or do you use the conventional assumptions?

MR. SMITH: A recognition that damping is not known precisely is included in all the analyses. Now how it is done is different depending on whether it was a research program as ours was compared to what I would call a commercial PRA, but that issue is addressed.

MR. SIESS: we are seeing data now that suggests that our conventional analysis say for piping has conservatisms on the order of 2 to 10. Was that sort of thing varied in commercial PRA's or were the conventional assumptions made, licensing assumptions made?

MR. SMITH: We have some results that go up to 90 on that. I think in general my feeling, and I am looking for evidence that supports this or refutes it, but I think in general as we learn more about this area, we will find that there has been basic conservatism put into the fragilities say and the means of the estimates of fragility and at the same time the uncertainties as not as large as they perhaps might be, the overall effect I don't know, but it may still lead to the conclusion that the results in the analyses are conservative.

In general I think the tendency has been, and I know from some of the work that we have done comparing say what we did on Zion with the SSMRP and what was done by the utility, that we simply believe that some of the fragilities, for example, as stated in the commercial PRA are too conservative.

MR. SIESS: I wasn't really talking about fragilities. I am thinking about the variations, the random variations in say floor response spectra. Now in SSMRP you put in a family of hazards which are distributed. You used a fragility which has got a distribution, but was there a similar variation in such things that would get from the hazard to the spectra?

MR. SMITH: Yes. That is explicitly included in a calculated manner in the calculations in the SSMRP which is one of the reasons for its detail of course. So that is explicitly in the calculation in a very in depth manner in that research effort.

We are simplifying that. We have another project which is just coming to conclusion now to attempt to take that information and translate it into more simple form that is yet adequate and it is more like what was done in a commercial PRA, but you will be able to track from the more detailed work to the simplified work and see where all those uncertainties feed into the process.

MR. TRIFUNAC: What is done, as I understand it, is that you have an input, and that input equates into SSRS equivalent, whether it is the measurement of the structure or the estimate for the four equal spectra, and that is multiplied by an expoential type factor to suggest some kind of distribution function and this is really what is done. I read that to be scaling in the mean, and I don't see any convolution there whatsoever. If I am misinformed, I would appreciate learning about it, but I have gone through some of these and I haven't seen it any place.

MR. JACKSON: Bob Kennedy is here.

MR. KENNEDY: Bob Kennedy, Structural Mechanical Associates. Having participated in 15 of these seismic PRA's, I can assure you that uncertainties in response of both the structures and uncertainties in response of the equipment relative to the structure are all explicitly included in these seismic PRA's. In particular, uncertainties in the ground response spectra shape is included, uncertainties in structural damping and the influence of structural damping on structural response is included, uncertainties in soil structure interaction is included, uncertainties in the amplification with depth between free ground surface and foundation level is included, uncertainties in both frequency content of the structure and uncertainties in mode shape of the structure are included.

So each of the parameters that is important in a deterministic structural response analysis in any seismic PRA is not just treated deterministically, but it is treated probabilistically. Probability distribution functions are established for those parameters and estimates of the variation of that parameter on overall structural repsonse is included.

Now these are included to different levels of depth in different PRA's, but every PRA I have seen in the last five years anyway has explicitly included uncertainties in response.

MR. TRIFUNAC: Yes, I agree with and I didn't mean to imply otherwise. The question is how they are included in there and whether they include them in such a way that the distributions, that the results on them are truly reflecting the convolution of the distribution of input and the distribution of output. I didn't mean to suggest that they are not included.

I meant to suggest that there is perhaps a question of now they are included and whether that procedure there is significantly important or not as far as trying to find whether the fault was 100 or 150 kilometers long. That is my point.

So that again the emphasis of my suggestion or question or comment, anyway you want it, is that rather than perhaps focusing on the specific geological issue, which may be very important in its own right, I am wondering whether we are taking a balanced view and asking the question are we really giving proper attention to all the steps in the process so that we are not overemphasizing one item and completely ignoring another one which might overwhelm the result in the end, i.e., what is the significance of the result.

I didn't mean to say that you are not looking at them. I am just wondering how do you do that.

MR. OKRENT: Is this particular topic going to enter naturally as part of any scheduled future discussion?

MR. JACKSON: No.

MR. OKRENT: Well then I will take other comments on this now.

Dr. Cornell, did you want to make a comment?

MR. CORNELL: Allen Cornell, consultant to PG&E. As a direct response to Dr. Trifunac's question as to how the convolutions are done, when you read the PRA studies you don't see them as convolution integrals. You see those uncertainties through several beta factors, and the betas get combined as some squares of betas which is reflecting the fact that these are treated, that the random variables are treated as log normals in which case the convolution can be done explicitly without any formal numerical integration.

Secondly, I think the more important comment that you are addressing here is that the results for the most part shown today in PRA's is that the uncertainty in dynamic response and uncertainly in fragility and so forth is relatively small compared to the uncertainty in the seismic hazard curves in terms of driving uncertainty in final results. Therefore, there does have to be a lot of emphasis on the uncertainty in the seismic hazard curves.

Now your point is well taken, except for the physics. It may well be that the seismic hazard curves are very insensitive to fault length because of the near-field problem. That needs to be resolved and I think we will find through the hazard analyses studies that are done that those kinds of questions will be resolved or will make themselves apparent.

MR. SMITH: One additional comment I would make on your question, Dr. Trifunac, is we recently got our results from the detailed work that we did on the SSMRP which are right along the line of the question you are asking, and it is also somewhat surprising.

We have concluded, and this is based on the very detailed work that we did, but we have concluded that the, and I have to try an state this carefully because I don't have viewgraphs, that if you just allow say uncertainty in a hazard to enter into the problem you will get one description say of uncertainty on core melt.

Now if you allow only uncertainty in fragility, and I use here the word fragility in the sense of including both the response uncertainty and local fragility uncertainty in the SSMRP detailed approach. So if you include only uncertainty on fragility, you will get another description of core melt probability and an uncertainty.

If you compare those two uncertainties, you will find that the uncertainty introduced by uncertainty in fragility is slightly more than what is introduced by the hazard for the analysis that we did.

It you state it another way, if you look at our curve that we published just to core melt probability, for example, you will see a certain distribution plotted there and you say where does that uncertainty come from, that total uncertainty.

It appears for the analysis that we did in the plant that it comes one-third from hazard or one-third from fragility, fragility used in this sense including both response and failure, and one-third from a coupling of the uncertainties in the analysis.

Now you can do more and more analyses to get more and more refined versions of various questions of the type you asked. Those are I think interesting results from the standpoint of future efforts on focusing where your concerns should be and where your review and meetings should focus on.

MR. JACKSON: Dr. Trifunac, maybe I could just ask for a little clarification because I may have stated what you said wrong. I get the message that what you are saying is rather than concentrate on determining what the fault looks like and where it goes, we should go back in and look at the structural capability in a probabilistic sense and then determine how significant it is that we know and what the faulting is doing and where it is? Is that an oversimplification of what you said?

MR. TRIFUNAC: Perhaps. I don't mean to suggest that one shouldn't find out what the fault is doing. But I hear that somebody has to make a decision here on what is or what is not done in so many years from now. It just strikes me that it doesn't seem to be a complete approach to just look at that specific subject.

That is one of many things, but we can go back 10 years a look at a lot of things that have been done here and have not been done. It seems to me that it is not worthwhile to put all the bets on just where the fault is. Perhaps this is the most visible problem, but it seems that the balanced approach which looks at all the uncertainties and all the missed and yet available states of the art, pardon the imprecision there, perhaps should be looked at if you want to come up to a decision. Otherwise, you have the decision already.

MR. JACKSON: Definitely is the intent here is to do that. That is why condition 4 was asked for. We realize exactly what you are saying to be the case. We may resolve what we know in the last few years about the faulting or the current interpretation of plate techtonics in California and that may change five years from now. We don't know.

That is why we added this as a category. In fact, this was the first one we put down and the other ones came afterwards and then we reversed the order after discussions with the Commission.

MR. OKRENT: I guess we had better proceed with your next viewgraph.

(Slide.)

MR. BROCOUM: Okay. The next viewgraph summarizes the parallel staff efforts which were some that we mentioned already.

For all the four specific elements the staff and its advisers of course will review the data or analyses or PRA supplied by PG&E.

In terms of element one, which would be the geological, seismological and geophysical update of the information, the staff, along with its advisers, may do some independent analysis. For example, the USGS as advisers to the staff may study some of their data offshore in the vicinity of Diablo Canyon and Dr. Slemmons may study some information about the geometry of the fault depth and so on. The advisers will most likely be the USGS and Dr. Slemmons for element No. 1.

For element No. 2, which is re-evaluation of the SSE, for the design earthquake, there might be an independent analysis of the SSE again by the staff and its advisers. Its advisers will be the USGS and Dr. Slemmons.

For condition 3, which is the ground motion, or element 3, there may be some independent evaluation in selected areas of the ground motion by the staff and its advisers which would include some of the national labs and the USGS.

Finally, for condition 4, which is the PRA, the advisers which will help the staff review the PRA will be the national labs and the USGS.

We are also intending to set up a senior advisory review panel or panels. We haven't decided yet whether to have one panel or several panels. Some of the possibilities that have been discussed is to have one overall panel which would consist of a geologist, a seismologist and a geophysicist to help advise the staff on this program.

There have been other discussions that maybe we should also have subsidiary panels such as a ground motion panel which would consist of seismologists which are well versed in historical and instrumental seismicity and in attenuation, but I don't think we have decided yet ourselves the exact composition and how many panels there will be.

MR. SIESS: How much of this parallel activity will actually be parallel to the licensee's activity; that is, completed within the next three years, and how much of it will have to be undertaken by the staff after the July 1, 1988 date?

MR. BROCOUM: We are intending to do a substantial amount of parallel activity. I don't know if I can give you numbers, but I think the intent is to work in an interactive mode and not to have the utility do their study for three years and then come back and give us a whole series of reports which we then begin to review.

MR. SIESS: Does that apply to the PRA?

MR. BROCOUM: I believe so, yes.

MR. SIESS: You are going to do a concurrent review of the PRA, not the Geosciences Branch.

MR. JACKSON: No, but the Geosciences Branch would have input into that in terms of the public.

MR. SIESS: Has the NRC staff ever done a concurrent review of a PRA?

MR. ISRAEL: No, I don't believe we have.

MR. SIESS: You don't believe you have.

MR. ISRAEL: We have to lag somewhat in order to have sufficient information to put on the table.

MR. SIESS: It seems to me that where the staff has reviewed commercial PRA's that it has taken at least one year and sometimes more than one year after it was completed for the staff to make a review of it.

MR. ISRAEL: We have completed reviews of commercial PRA's, at least of the draft reports, in six months.

MR. SIESS: In how long?

MR. ISRAEL: In six months, the draft report.

MR. JACKSON: I think the intent here was to have some interim decision as we go along that we don't give to the utility the mission to go do this and then say come back three years from now and then we will review it. I think the idea was we would meet as frequently as necessary to decide some of these difficult issues.

The idea is not just to offer this up and then we will see you in four years or three years, but the idea is to sit down and go through and try to address some of these questions that you have raised with them and with their consultants and with our own PRA review group to decide how you are going to proceed before you get three years down the line and have something that you could have changed say two years before.

MR. SIESS: And the items listed up there as independent would be concurrent?

MR. JACKSON: Those would be concurrent as much as possible. For instance, we already have a little bit of work going. We have recently funded or are in the middle of funding a contract to the USGS, to Dr. Algermissen's group to look at ground motion estimates in some of these areas. So I think that some of those independent assessments would come concurrently with what they have.

This is an issue that is of some degree of controversy with the Commission. During the Commission meeting that we had on this, the Commission raised the question of whether we as the staff should have the lead in doing all of this or whether PG&E should have the lead. I think at least one or two Commissioners raised the question.

It has been our position that we should apply the normal chain of work. As a staff we have limited personnel and resources and we can serve best in a review mode with enough parallel independent effort to be able to judge the objectivity of what the utility has been doing. That is essentially what the intent of the Commission's comments were to us.

MR. OKRENT: Dr. Thompson.

MR. THOMPSON: There is an enormous body of useful data in this offshore reflection material. Although everyone seems to agree that one needs to look as much as possible at the geometry of the fault and so on independent of whether that is going to affect the ground motion drastically, this data exists.

I haven't seen a plan or people identified who are going to make a really close critical study of all of that data, and I wonder how that is planned for.

Now we have the Crouch paper which has no seismic data but only line drawings of a few seismic lines in it. So that enormous source of information somehow needs to be tapped.

MR. BROCOUM: Yes. The last slide I showed represents the data from one company. So there are numerous companies and there may be much more data. I am not sure one can look at every single line, but I am sure that it would be prudent to look at a representative number of lines.

We feel that the utility should take the lead in going through that data and culling out the useful data. However, through the USGS and our own we I think plan to do an independent assessment of say critical lines, if you like, or particular lines that are very important to the interpretations that seem to be the ones that are most favored towards the end of the study.

I don't think we have the resources ourselves to be able to go through all these lines.

MR. THOMPSON: Well certainly not. What I am suggesting is that you need to identify people in your own organizatigon or in the USGS who can critically evaluate that in working with the utility.

MR. BROCOUM: I think the USGS would be a very key factor in reviewing the seismic reflection data.

MR. THOMPSON: Who are the people in the USGS?

MR. BROCOUM: I don't know if we are prepared to say at the moment, and I don't know if the people have been picked because at this moment we do not have a contract with for this with the USGS.

MR. JACKSON: I think, Dr. Thompson, what we had in mind is we had something similar to this at San Onofre 2 and 3 where we had offshore lines that needed to be interpreted. In that case we used Gary Green and there was an individual from CDMG, California Division of Mines and Geology who did a great deal of offshore interpretation of data, Gary Green, under the auspices of assisting us from the USGS.

I think the intent would be to go and have them do the same thing here. That is what we have always done with seismic reflection data. We don't have in-house capability, except for one individual in this area.

We don't want to overpromise in this area. This is an exceedingly expensive undertaking, as you know, and it could be tremendously burdonsome to PG&E. To buy these lines is very, very expensive and then you may never know that you have all the lines that could be available to some other company and companies may not release them period because of competitive advantage in drilling.

So I think you have to be realistic. This is one of the dilemmas of the so-called Crouch paper coming up on us like it did.

MR. THOMPSON: There may be some practical ways to go about. It may be that you could use a couple of different contractors and have them interpret the data. You don't have to have all of the data, but just perhaps a few representatives of it.

MR. BROCOUM: The slide I did show is data that is available for purchase, that particular slide that I showed earlier.

Secondly, I think it is important to make clear that we go with a problem to the USGS and they have the I think final say as to who of their people will work on a particular problem. I think that is the way it normally is. We can't say we want so and so or so and so to work on that problem. They like to pick their own people. We can suggest, but we cannot order them.

MR. JACKSON: That is in our memorandum of understanding with the USGS. I think it is really an important question in an area where you are going to get not only the reflection profiling, but I think we are aware that there is drill hole data available offshore which would either confirm or reject some of the seismic line information that is available. That also has a problem in availability.

MR. THOMPSON: Well, I think if you go about it right you will get a lot of cooperation from the contractors and the companies and some of the interpretation can come out of different sources independently and be checked against each other. It is certainly totally impractical to think of buying vast amounts of that data. It costs a great deal.

MR. OKRENT. Unless we are going to go into the oil business as well.

(Laughter.)

MR. JACKSON: Dr. Crouch will be here this afternoon or he may be here now and he may offer some comments on the reality of the cheap availability of these lines.

MR. BROCOUM: Any more questions?

(No response.)

(Slide.)

MR. BROCOUM: The final slide just summarizes progress reporting and scheduling.

Although we requested quarterly progress reports and semi-annual meetings in Bethesda and we even suggested I think in our more detailed explanation an annual meeting with the ACRS, the important thing is that rather than these exact dates that it be an interactive program, and I think that is a key word, an interactive program between the staff, its advisers and the utility so we work parallel so that by the time this program is finished we have a pretty good idea of what our, if you like, evaluation of it will be.

The schedule is for the utility to submit a proposed program by the end of January of 1985 and in the wording approved by the Commission the program would be completed by July 1st of 1988, althought there has been some discussion of changing that wording, to change the wording to three years from the date of approval by the staff to make sure that the utility gets three full years to undertake this program.

So that is the conclusion of this part of the presentation by the staff.

MR. JACKSON: On the progress reports and annual meetings also, I think that will be changed to say as necessary or something like that.

MR. OKRENT: Any questions at this time on the presentation? We can come back to these points later in the discussion if we wish.

(No response.)

MR. OKRENT: If not, I am going to suggest we take a 10-minute break and after the break I am going to ask the members of the public who wish to make comments to make their comments and, if they can fit their comments into five minutes, I would much appreciate it. If they need more time, would they tell Dr. Savio. I think we have four. So we will do that right after a 10-minute break.

(Recess.)

MR. OKRENT: The meeting will reconvene.

Will Alberta Rich please ---

VOICE: She just stepped out.

MR. OKRENT: Is Bruce Campbell here?

MR. CAMPBELL: Yes.

MR. OKRENT: Would you please then make your comments and we will get Alberta Rich when she returns.

MR. CAMPBELL: Good day. My name is Bruce Campbell. I am with End Nuclear Dumping in the Pacific from Los Angeles and other groups.

Would you like my address, too? My address is 614 Gretna Greenway, LA 90049.

It is questionable whether Diablo was originally built to withstand even a 6.5 to 6.75 quake. The basis for seismic design in involving shaving safety margins and mystically claiming the tau effect does not prove to me that Diablo's design is conservative. Thus, it does not pass and Diablo should not be operating.

PG&E admitted at a Waterboard Waste Discharge hearing that the alleged solid bedrock base of Diablo, which made it so different from the Imperial Valley setting for the October 15th, 1979 quake, PG&E admitted that that under Diablo is actually highly fractured siltstone and sandstone.

I will go into my written comments.

Chairman Salzman, the chair of the three-man Atomic Safety Licensing Appeals Board, which held the Diablo seismic hearings in the fall of 1980, was appointed to a federal judgeship by President Reagan shortly before he ruled that Diablo was seismically safe. This fact alone should disqualify the results of these farcical hearings which I attended in their entirety.

Dr. James Brune, Professor of Geophysics at UC San Diego does not see the IV-79 quake as an exampleior a design basis conservative quake ". . .in terms of stress drop, accelerations, velocities and spectrum relative to its Richter local magnitude.

He goes on "There are too few data for earthquakes of magnitude 6.5 to magnitude 7.5 to establish the rate of increase of average peak acceleration or spectrum of ground motion going from M-6.5 to M-7.5," though it is obvious that on the average they will be higher for a 7.5 quake.

There is confusion as Drs. Luco and Trifunac I believe still believe that there is not solid evidence indicating that one can assume that the ground acceleration will leap up and the magnitude will be a certain rate.

Near-field IV-79 data indicates that values of vertical acceleration can be considerably higher than two-thirds the values of horizontal acceleration. Since each new well recorded quake brings surprises, as Imperial Valley, quote from Brune ". . . statements that certain assumed peak accelerations are conservative are necessarily cast in doubt, whereas the negative statement that such accelerations have not been established as conservative remains true."

The ACRS's own Drs. Luco and Trifunac tend to agree with this statement even when raked over the coals in the fall of 1980 seismic hearings.

The two NRC Commissioners who disagreed with the full Commission's decision not to review the ASLAB seismic decision continue, ". . . the use of the so-called tau effect to permit a substantial across-the-board relaxation of the seismic standard applied to the plant, the Board's reasoning is utterly inadequate and is very likely wrong."

At best the Diablo construction permits assumed that the reactors could experience of peak of a 6.75 magnitude quake at a distance of 20 miles. The USGS predicts 7.5 as the maximum quake for the nearby Hosgri area, despite the fact that a 7.5 quake already occurred in the Hosgri fault zone west of Lompoc on November 4th, 1927.

The seismic evidentiary hearings concluded that Diablo could be redesigned to withstand a 7.5 quake at 5.8 kilometers on the Hosgri fault. Diablo's design is not conservative. Every advantage was taken of slack in safety margins left in the pre-Hosgri analysis. A larger damping value of 7 percent and not 5 percent was used in analyzing structures.

Credit was taken for actual as-built strength of materials rather than the usual minimum required strengths. So larger vibrations became tolerable. The redesign has already shaved safety margins to the extent permissible by regulation.

Page 8 of Gilinsky/Bradford's statement explains the ridiculous nature of the ASLAB's reasoning on the tau effect, and I may read that if I have time toward the end. The NRC's Office of Policy Evaluation put it this way. "Except for the judgment of Drs. Blume and Newmark, there is no evidence to demonstrate an ability to predict tau effect over a range of earthquake magnitudes, structural configurations and site conditions."

Also Newmark relied on the work of Dr. Uma Hara who was talking about a small odd shaped building not bearing any similarity to Diablo Canyon. Now Dr. Newmark is no longer with us and so can't be explaining his reasoning.

Since the basis of seismic design is questionable at best, the reverification was off to begin with. Teledyne has financial ties to PG&E and thus there was no independent design verification program.

Bechtel and PG&E cannot be trusted to do an seismic design review. Document control was virtually non-existent in that review. Can these people be trusted to carefully evaluate all new and older data and conclude accordingly?

The conclusion has been reached that Diablo must operate and PG&E must get their billions of dollars back.

The terms "confirm" and "verify" do not seem to allow the possibility that an honest assessment could determine that Diablo cannot be proven to be seismically safe.

Dr. Crouch's paper seems to indicate that the Hosgri is likely a thrusting fault and thus could result in greater ground accelerations than was conceived of back in the 1980 seismic hearings.

There was discussion of the length of the Hosgri fault. I believe that most geologists and seismologists agree that the Hosgri is just a southern component of the Hosgri, San Gregorio and San Simeon system which is the largest subsidiary of the San Andres fault.

Thus, you have to consider the other faults related to the Hosgri fault, plus the splay from the northwest, which was discussed in the October 1980 seismic hearings, the splay aimed at the plant even on a PG&E map. There must be a thorough seismic study between the shoreline and a mile offshore. It is likely that splay aimed at the nuke will link to an onshore fracture.

Thus, it seems obvious to me from that splay and testimony in 1980 and from extrapolating from Crouch's paper that part of the Hosgri fault, that the state's largest subsidiary of the San Andreas fault runs beneath the Diablo reactors.

Thus, PG&E obviously cannot be trusted to do a thorough seismic study. We must shut Diablo down and have a thorough a seismic study totally independent of PG&E, Bechtel, Teledyne and the NRC as possible.

Obviously Diablo is not conservatively designed and tried and proved in its conservatism.

MR. OKRENT: Thank you.

Any questions from the subcommittee?

(No response.)

MR. OKRENT: I guess I don't see any now.

Let's see, it wasn't clear to me. Mr. Mendes wanted to talk?

MR. SAVIO: Yes. He requested to be able to speak after Dr. Crouch and PG&E and finished.

MR. OKRENT: Later in the day. All right.

MR. CAMPBELL: Excuse me. I did turn in my packet. You probably already have Commissioner Gilinsky and Bradford's comments on that they thought the ASLAB seismic decision should have been reviewed. Also, I have Dr. Brune's paper. You probably have them somewhere, but I will give you one copy anyway.

MR. OKRENT: Thank you very much.

Alberta Rich, would you please give us your statement now.

MS. RICH: Hello.

My letter starts "Dear Sisters and Brothers," and at this point I have a problem because there are no women on the Board and I don't feel especially represented as a female and I am wondering why there aren't any women on the Board or as advisers or anywhere except for a few in the audience?

Is there anyone that can answer that for me?

MR. OKRENT: You would have to speak to the Nuclear Regulatory Commissioners. They make the appointments to the Advisory Committee on Reactor Safeguards.

MS. RICH: Are there any women in that group?

MR. OKRENT: There have been in the past. There are not currently now. I think that is not an issue for this particular subcommittee meeting and we are not going to discuss that in any way.

MS. RICH: Okay. I just want it registered as a concern of mine. I am female with grown children. I have daughter who is 19 whose boyfriend lives in Paso Robles which is very close to the plant and in all likelihood she will be moving there. I live in Santa Barbara which is downwind from the plant.

I would like to read my letter.

MR. OKRENT: Please do.

MS. RICH: It is my opinion that seismic re-evaluation needs to take place at Diablo Canyon nuclear power plant prior to considering granting a full-power license to PG&E.

From what I have been hearing this morning, unless I am really confused, it sounds like the re-evaluation is going to go on perhaps as the plant is already operating full power?

MR. OKRENT: This is what the regulatory staff is proposing.

MS. RICH: Okay. I find problems with that. It seems that it should happen before it would go on line.

The proper group to conduct a study on anything would obviously be one that did not benefit one way or the other financially or by favor from the results of this study.

So it would seem to me that PG&E would be the wrong people to conduct a study, and I am not too sure who the right people would be, if there are any.

I am attaching to this letter some concerns from San Luis Obispo citizens about the inadequacy of an emergency reponse plan in the San Luis Obispo county area and request that you consider these concerns in your deliberations.

I would like to read that sheet. This says "A few facts you should know about the county nuclear emergency response plan for Diablo Canyon. "

"The primary means of alerting the local population and emergency personnel to a nuclear emergency is by electrically powered sirens, telephones and local radio and TV stations. All of these are subject to failure in emergency situations.

"The county plan states that sheltering will be the protective action you will take if there is less than three to five hours for evacuation. Nevertheless, with very few exceptions, for example, two buildings at the California Men's Colony, buildings in the area, including the emergency operations center, cannot shelter you from nuclear radiation, i.e. alpha particles, beta particles and gamma radiation.

"Since there are few major roads, evacuation is uncertain. In the winter storms in 1983, for instance, each major road was partially or totally blocked, some several times. Even in ideal weather conditions stalled vehicles, accidents and cars running out of gas, et cetera, could prevent a speedy departure.

"A major inaccuracy in the county plan is that the time estimates for evacuation under the worst weather conditions, including flooding or fog, are only 20 percent greater than under the best conditions.

"Scarce attention has been paid to earthquakes in the plan. For example, building which would be used as shelters have never been structurally evaluated. The plan admits that evacuation could take ten hours or longer in the event of a severe earthquake. It will take a great deal more than 10 hours if roads and bridges are substantially damaged."

Last week in Santa Barbara we had a minor sulfuric acid spill on the freeway which blocked traffic tremendously. It was hours before semis and people were getting through town.

"Many of the standard operating procedures for town schools, et cetera, have little or not discussion of how to evacuate the carless population or the disabled population or the institutionalized population or the private school population.

"Evacuation is predicted on the notion that people in one area will leave while those nearby will calmly remain until told otherwise. The subject of individual or group behavior is never addressed in the county plan.

"For those who may be contaminated, local decontamination facilities are virtually nonexistent. Although wind direction is a crucial factor in a radioactive release, no extended study of wind direction at different altitudes throughout the area has ever been carried out.

"All county, state and federal employees may be conscripted in the event of a nuclear emergency. Very few know this and even fewer have received any training. A recent study of local school teachers showed one-third of them would leave their students in a nuclear emergency to be with their families.

"Despite official confidence in the emergency plan, a full-scale drill, including a major evacuation has never been held. Two lesser drills have been replete with major problems, many of which were not even mentioned in the official analysis."

I think this is pertinent because an earthquake could result in the need for an evacuation.

I spent some time talking with several workers from the Diablo plant in the last month while I was up in that area. One of the workers who is in a position of high responsibility is a victim of a great deal of stress. He is a Vietnam veteran and he talked about scraping his friends off of his body, and as far as I could tell hadn't dealt with that. He was very, very drunk when I was talking to him. And I talked to another person who had just gotten off of work and the first thing he did was buy a six pack of beer.

Now I have heard a lot of allegations that other people have made, but this is my personal experience with just two of the workers that I happened to talk to and I don't know what the percentage is. But I would consider that one of the most significant possible causes of failure of the system is related to personnel, is human failure. In an earthquake situation I wonder how they would be able to respond.

I just have one last thing to say, and that is that I pray that we all open ourselves to the spirit of truth and love and join together for the healing and transformation of this magnificent planet.

Thank you.

MR. OKRENT: Thank you, Ms. Rich.

(Applause from the audience.)

MR. KERR: Dr. Okrent, with your indulgence, I want to point out that I think the NRC has been concerned about trying to get women members on the committee and, indeed, each time there is a vacancy there is an advertisment which asks for nominations. I believe that in the last several vacancies that there have not been nominations of females. I think the Commission would welcome the nomination of qualified women for the committee.

MR. OKRENT: Thank you, Mr. Kerr.

Mrs. Evered I think is next on Dr. Savio's list.

MS. EVERED: I am Judith Evered of 6648 Delplia, Isla Vista.

I am here today to represent the Isla Vista Recreation and Park District. I represent them on environmental concerns.

We have a local government in Isla Vista and there are 13 to 14 thousand people who live in our area. We are 80 miles downwind and downcurrent from Diablo nuclear power plant. So we have been following proceedings for many years and have been increasingly alarmed at the prospects and in fact very disturbed that it went low power recently.

I myself have been to testimony, all the testimony from the Central Coast Waterboard and the State Waterboard on this question of a permit of putting radioactive water and poisons into the Pacific, and I have also been to NRC hearings and to three weeks of hearings in the San Luis Obispo Municipal Court where we had experts come from all over the country to assess the quality and the prospects of Diablo Canyon being on line.

I can tell you I am really, really worried about this. The biggest anxiety of so many people in San Luis Obispo, Santa Barbara and Ventura Counties is the fact that it is right within earthquake country. It is not just that the Hosgri is 2.5 miles from it. It is that there are many splays from this earthquake fault. There are 12 earthquake faults between that and the San Andreas, that is within 40 or 50 miles.

We are just talking here about probably risk assessment and talking about having full power before we fully know what that is going to do. Here the previous speakers just talked about how the workers are under stress and there was more testimony in the San Luis Obispo county about the marijuana and coke and beer that the workers consumed. I think they probably know they are doing a very difficult thing there and they have to escape the thought that perhaps they would be agents to third of California being obliterated if there were an earthquake there.

It is just like if you dropped a one megaton bomb on that plant or if there were an earthquake which split it, what would happen to his whole State? I cannot imagine why we are so passive about this, and I really expect that this committee will be able to persuade the NRC not to go any further with that fission process there until we can be absolutely certain that people in that area are protected. Otherwise, it will be on our consciences forever.

I am also very concerned that we have an independent review and I am totally amazed today to find that PG&E are going to do their own study and that is going to be evaluated, because in all of the testimony that I have heard PG&E didn't check themselves or assess themselves, in my opinion, as honestly as they might have in the past.

It was noted in the Central Coast Waterboard that in 1967 they came to an arrangement with Fish and Game, the State Department, to pay them money so that they wouldn't oppose the licensing and so on of Diablo Canyon, and that is all in the records. It is in the records of the testimony given.

There have been other times where PG&E would approach members of the local waterboard to approach their bosses to change their votes. I mean look at the history of PG&E putting nuclear reactors onto earthquake faults. In Bodega Bay there were six years of controversy before that was stopped, and PG&E didn't stop themselves. It was attorneys and the population from San Francisco that opposed it.

Then Point Arena and Humbolt Bay, earthquake faults. We just happen to have two plates along the California Coast which are separate and they move. So I mean it is ridiculous. As it has been said here today, worrying whether it is going to be thrust or slip or compression, it doesn't matter what it is, but it is inevitable.

I have much research here and I will be happy to give it to you because I can't at all cover it all. It says, you know, there is a 50/50 chance we will have a big earthquake in the next 10 years, and that is very high odds when people's lives are at stake.

Otherwise, I have research here on the fact that children are affected more and that work by the concerned scientists, and I also have a map here of Diablo Canyon, and I will be putting it in. It is from the USGS. It shows disturbed terrain on each side of Diablo Canyon Cove, which means probably seeing that it is in the direction of the splays from the Hosgri, there is the plant and just within yards is a fault.

The other thing I might say about the early history of PG&E is when they were trenching there in 1967 they came across faults, disturbed terrain. So they said stop the trenching. We may be finding things out which will cause controversy.

So I think you only have to go into the history of this and present it to the NRC, and I am quite sure that they will be rational and say no full power until this study is complete and until we know that California is safe.

Thank you.

(Applause from the audience.)

MR. OKRENT: Ms. Evered, I am going to depart from my usual practice and offer a couple of comments and maybe make a request in connection with your presentation.

If you have something that supports the statement which you made that there is a 50/50 chance of a severe earthquake, and I assume you mean by Diablo Canyon, occurring within the next ten years, I wish you would submit the material because it would be relevant certainly. The estimates that I have seen have been much smaller. So I would like to see the particular information.

Certainly the question of who should do what part of whatever studies are contemplated is one of the things that the Commissioners themselves have raised. So that will be thought about by the ACRS as part of its consideration of this.

I might note that my own knowledge of the history of Bodega Bay is that it was Dr. Richard Done who was then the Director of Regulation of the U. S. Atomic Energy Commission who took the position on behalf of the Atomic Energy Commission that that plant should not be built at that site, and that was the reason that PG&E did not move forward.

Now there were certainly people from the area who objected, but I think one should note that in fact the Director of Regulation of the U. S. Atomic Energy Commission took that specific position and that was the thing that concluded, if you will, further work there.

I was very curious about a comment you made that if there were an accident at Diablo Canyon that a third of California would be obliterated. I don't know of any basis for such a comment. It goes completely in an opposite direction to what I have learned about the potential effects of nuclear power accidents.

Do you have scientific documentation for that?

MS. EVERED: Yes.

MR. OKRENT: Again, I would like to see it because at the moment is stretches my credibility by a rather large factor. I just have to note that.

MS. EVERED: Okay. Well, my reference people on that are Helen Coldacot. "Stop The Nuclear Madness" was one of her very brief books. Also it was a professor of nuclear physics, Misheo Cacoo, from New York State University. He was an expert witness one day in June of 1982, and this will be in the Municipal Court records at San Luis Obispo.

Also, the fact that in Russia, and it is not very generally known, that a place called Kiskum had an accident in I think it was 1957, and hundreds of square miles of country were just torn apart and now they can't be used. This only came out after the Secret Information Act enabled us to get the information. It was thought to be stored waste products.

So, you know, you have got the troubles at every end.

MR. OKRENT: Well, I have read some of the literature on the last thing you have mentioned. I don't believe that the earlier references you gave include what I would call a technical examination of the possible effects of an accident. A third of California is a lot more than a hundred square miles obviously. So I think you might want to in fact re-examine the validity of the people whose expert opinion you are quoting.

In that particular case I for one would be skeptical. I am frequently skeptical of what the NRC or the utilities tell me. In this case I am skeptical of the particular number you got from other sources. I just wanted to note that for the record and for you.

MS. EVERED: I would think if radiation were on the ground it would be unusable and that nobody would want to go there. They let off balloons at Diablo Canyon and they went as far as North Carolina. We are pretty certain that radiation travels all the way around the world. So in a sense it is the planet we are talking about and not just a third of California.

MR. OKRENT: Well, I will leave the discussion at that.

Thank you.

MS. EVERED: Thank you.

MR. OKRENT: Ms. Silver, who I vaguely remember from San Luis Obispo.

MS. SILVER: I am Sandy Silver. I am with the San Luis Obispo Mothers For Peace, an intervenor in the Diablo Canyon proceedings for over ten years.

I am very happy to address this group again. I have personally had the good fortune of meeting several of the members on this committee.

Dr. Okrent, not only did we meet in San Luis Obispo, but it was 10 years ago at UCLA that we first met. I have always appreciated the questions that you have asked at these ACRS meetings. I have personally been on tours at the Diablo Canyon facilities when PG&E would begrudging point to an instrumentation setup saying that oh, yes, that was one of Okrent's big deals so he decided to put it in, and I appreciate very much your concern on that matter.

And also, Dr. Okrent, I didn't realize that it was not common for the ACRS to address comments to the participants of the public. I think that that perhaps is a poor procedure. I think it is much healthier is there were give and take from any person who is before this group.

Obviously in the scientific field questions are very healthy and the search for honest answers is equally important. I have a very deep respect for scientists. My husband in fact is a physics professor at Cal Poly. But having dealt with the murky NRC proceedings over the past ten years, I have been exposed to scientists who have, in my opinion, performed in a very questionable manner.

In 1967 the then Chairman of the ACRS used information provided by scientists who had been hired by PG&E to conclude that "The Committee believes that the questions related to seismic design have been resolved satisfactorily." The scientist of course chairing the ACRS in 1967 was Nunzio Palladino, the current Chairman of the NRC and the person who asked this committee last month to expedite your proceedings on the Diablo Canyon case.

That matter came up when a worker had made allegations on small bore piping and had his allegations substantiated by an NRC inspector. As you may remember, this caused quite a furor before the hearing with the Commissioners and that is why they asked you at the full ACRS meeting to look into this.

Although this committee had that very worker here to testify before it, not one question was asked by any one member of that worker.

In addition, Drs. Axtmann, Ebersole and Okrent pointed out there there was a great "bulk of material" to review, and many documents had arrived only the night before the meeting. Yet in a record-breaking time you all were able to take all the documents and all the testimony and write to the ex-ACRS Chairman giving your approval for a low-power license, and with astonishing gullibility you wrote that you understood that "Allegations such as those made by Mr. Stokes will be investigated and appropriately considered by the NRC staff." I don't know the basis of that statement, frankly, gentlemen.

In 1969 Hoskins and Griffith discovered the Hosgri Fault, but because these scientists were under contract with an oil company they withheld from the scientific community and from the public at large any of that information because of "proprietary" information.

The withholding of such information, knowing that two giant nuclear power plants were being built two and a half miles from that earthquake fault, may be good business, but it makes a mockery of scientific discovery and frankly it is just plain immoral.

One can't help but wonder about the current state of information regarding the Hosgri Fault. What other proprietary information is being withheld from this committee and from the public who lives next to that fault.

We are now in 1984 not dealing with the plants being constructed, but rather with a fully constructed nuclear power plant which has gone critical and which has been allowed to operate at five percent power. If more information is forthcoming, and it certainly seems that it is forthcoming, there will be new information, wouldn't it be scientifically more prudent to get all of the data before allowing this ill-fated plant to operate at any power?

Let's take another scientific topic of a rather questionable nature, the infamous tau effect. At a meeting held last year between the ACRS and the Commission Commission Gilinsky said that he "did not think that there was a scientific or engineering backup to that," referring to the tau effect. Dr. Siess replied "that is obvious."

Commissioner Gilinsky later said "it doesn't seem to be something you could write down or reproduce or even in fact the staff." Again Dr. Siess replied, "No, he is dead," meaning Dr. Nathan Newmark. "And I doubt if he could explain it to you if he were here. He frankly had problems explaining to other technical people the basis for his conclusions, it was Newmark's judgment."

Gentlemen, many of you are university professors. If a very bright student came up to you during an exam and said, you know, I know the correct answers, but I just don't know how to explain it. Would you give that student an "A"? Or to put it another way, if Dr. Nathan Newmark were a woman and she came up to you and said, I don't have any scientific basis for proving the tau effect, but based on my intuition I think it is correct.

Yes, the tau effect has caused concerns among scientists. Commissioner Bernthal came up with his own suggestion. At that same meeting he said "It seems to me it is fine if we want to make a laboratory. In fact, I would urge that we do that at the Diablo Canyon site to be focused for research in this area."

How would you feel if your children or your grandchildren were guinea pigs in an experiment go prove the validity of an intuition?

You on the ACRS must share the responsibility for nurturing such callous thinking among scientists for in 1978 you wrote to the Commission advising that Diablo Canyon could go on line despite all the unanswered questions on seismic design because, among other reasons, it was in a low population zone.

My children and my friends, they are expendable to all of you.

And now here we are again with yet another proposal to do something to rationalize and analyze away the original very basic problem of this plant. Stated quite simply, it was sited in the wrong place. It was designed and constructed using the wrong seismic criteria.

You sit here today trying to decide what kind of a seismic study should be done by 1988 and whether or not PG&E should once again hire scientists to do the study. By 1988 if things go as PG&E and the NRC plan, we will be dealing with not one but two fully operational giant nuclear power plants.

Both plants have been designed and constructed in the identical fashion. They have the same weaknesses. If there were to be the postulated 7.5 magnitude earthquake on the Hosgri and the phantom tau effect proves to be non-existent, we could face two LOCAs. Can you imagine the chaos in the control room? Can you imagine the destruction of a 7.5 earthquake? Can you imagine the thousands of people trying to evacuate? To be sure you would be able to gather lots of scientific data.

You may dismiss this whole discussion as an emotional outbreak from some poor woman who is overly concerned about her children, but I base all of my arguments on the same facts that you have had. I have no vested interests in this plant.

As scientists your principal duty is to see that experiments are conducted in a controlled environment, one which doesn't endanger the lives of your fellow human beings.

I feel compelled to remind you as scientists your deliberations should be completely devoid of any considerations having to do with the cash flow problems of a giant corporation.

I would appreciate and I would be happy to answer any questions that you might have or any comments that you might have.

MR. OKRENT: Well, let me make one observation. The Committee prepares its letters as a committee and the Chairman merely signs it on behalf of the committee. So back in 1967, if that is when in fact the letter was first written, if Mr. Palladino signed the letter, it was not that he wrote the letter himself on behalf of the committee. I think you should know that that is the procedure.

MS. SILVER: Would he have been in any of the deliberations?

MR. OKRENT: He would have participated as one of 10 or 15.

MS. SILVER: He was a participant.

MR. OKRENT: Yes, but he is not the sole author is what I would like you to know.

MS. SILVER: I understand that.

MR. OKRENT: Are there any other questions.

MR. KERR: I would only comment that we are certainly concerned about the effects of earthquakes on people in California. Indeed, were I to consider moving to California I would be quite concerned about earthquakes and not just the effect on nuclear plants. It has been our concern to try to see that earthquake effects on nuclear plants are taken into consideration.

It would be my guess that the damage to nuclear plants is likely to be in the case of a severe earthquake much less than the damage to many other large facilities, including dams, for example, which might flood and cause a very significant number of casualties. So we share your concern about the effect of earthquakes.

MS. SILVER: Mr. Kerr, if I might address a comment to you. Actually in reviewing for this committee I checked in my notes of 1975 and one of the things I said was that as a Californian I am concerned about earthquakes, but that is my own choice. But to have a nuclear power plant, as I said here, that was designed and built and they didn't know about the Hosgri Fault, that is the problem. They didn't design it for that fault.

So what they have done over the years is trying to retrofit it, and that is a very difficult job. In doing the retrofitting they did the blueprint errors.

You have to realize that there is no credibility felt for PG&E and the NRC has almost none, a little bit more than almost zero because of the patchwork that has been done on the Diablo Canyon plant.

You cannot admit to error and that is big problem. It is a very big problem. And one of the reasons I feel, and most of the people feel that you cannot admit to error is because you have a $5.1 billion plant and you cannot allow PG&E to lose that investment and that should not be in consideration in these proceedings.

You are scientists and you have to find out that did they do it correctly or didn't they. And in our opinion, excuse me, it is not only an opinion. I think that most of you who are here could testify to the fact that the Mothers For Peace have put on one of the best technical arguments ever seen by the NRC on the issue of seismic safety. We have hand world renowned experts come and testify.

This isn't an opinion. It is an honest differing c. views. Unfortunately, an NRC proceeding is the wrong place to exchange views because there is an adversary role that should not ever be allowed. In other words, we are looked upon as the enemy and, unfortunately, I think by the ACRS, some members may look upon the public or intervenors in particular as "the enemy," the other side. We are not. We are not at all.

By the way, in again reviewing my notes, I believe it was a Mr. White who made the comment, and I shouldn't have used the name, I can check my notes, but he had said that he was so pleased to serve as a consultant to the ACRS because he attended a meeting with PG&E and the NRC and was so pleased to see that they had worked so well together that he really couldn't tell which was the NRC and PG&E. That is exactly what we are worried about. The NRC is supposed to be protecting us, and they have abdicated that role.

MR. OKRENT: I am going to have to thank you, Ms. Silver. I should just note that the ACRS appreciates receiving comments related to the safety of any plant it is reviewing and there have been occasions in the past when such comments have had a rather profound effect on the deliberations. I will just leave it at that.

(Applause from the audience.)

MR. OKRENT: I think this completes the public comments at this time. We will have one additional one later because it has been so requested.

We will go back then to the agenda and I believe the next item is entitled comments from NRC working group on seismic design margins.

MR. JACKSON: In looking at the agenda, I wasn't absolutely certain why this sat in the Diablo Canyon meeting.

MR. OKRENT: I am not quite sure either, but I see it here.

MR. JACKSON: Since you asked we will give you something anyway.

MR. OKRENT: In fact, you could tell us is there an NRC working group that is working?

MR. JACKSON: Yes, basically.

(Laughter.)

(Slide.)

MR. JACKSON: I didn't bring the memoradum, but a working group internal in NRC has been developed composed of a number of people and two co-chairmen, myself and Jim Richardson from the Office of Regulatory

Research. We are co-chairman a group which includes the Branch Chief for the Systematic Evaluation Program, the Branch Chief for Probabilistic Risk Assessment, Ashok Thadani and several individuals from the Office of Research who deal both with probabilistic methods and deterministic studies, Leon Baritan and I think Mr. Kenny Alley.

We have also had several meetings internally and it led to the request that we develop an outside assistance working group of technical experts, an expert panel it is called here, but we are still arguing about what that group ought to be called. A number of them are here in the audience and I have a slide later on which will indicate who they are.

There was a meeting that took place of this expert panel several weeks ago. I was not in attendance at the meeting. I have a few comments that I can note as to what came out of that meeting and I hope that Allen Cornell or Bob Kennedy will correct me where I am wrong.

The general schedule that has been developed -- and I must add, too. There will be a meeting of this expert panel group and the internal working group on June 11th. The idea there is to get together and try to work out exactly what the charter of the working group is in general and what we should be trying to achieve.

There is still some degree of difference of opinion on exactly what needs to be done and we are trying to go under the guidance of the ACRS on this.

Essentially the working group in May/June will meet both separately, which they have already one, and in June, and based on that joint meeting will develop a progress report to the EDO, the Executive Director's Office. The attempt is still being made to develop a first draft of a plan to address the ACRS concerns in August of '84. That still may be somewhat optimistic.

We then plan to schedule a meeting in September of '84, I guess it is the August 30th/September 30th time frame. Then in October '84 an EPRI, Electric Power Research Institute and NRC workshop on seismic margins is planned so that there is some interaction of knowledge that has been gained in the margins area from EPRI.

EPRI based on the discussion we had in the External Events Subcommittee meeting in December in San Francisco has been looking into with the industry what they should be doing in terms of the margins area. Mr. Rubel Thomas was there and you had some discussions with him at that point in time. The goal now is to establish a plan by December of '84.

(Slide.)

The general status is that an NRC working group has been established and the membership tries to reflect an across-the-board use of both deterministic margins to failure and margins to code and margins using probabilistic techniques.

Some more mundane things are noted here just to keep you informed. The expert panel met on May 4th. They defined a charter and I have read the charter. It is not totally clear yet, and I think rather than give that to you here, I think there should be time for the internal group and the expert panel to meet and discuss it before providing it to you. The NRC working group is now reviewing it and, as I said, will meet on June 11th.

(Slide.)

The working group is chaired by Bob Budnitz and Bob Kennedy from SMA, Allen Cornell from St nford, Jack Reed from Jack Benjamin Associates who is in the probabilistic risk area. Paul Amico was added based on our previous discussions with you about adding someone who was an expert in the systems area, and after a number of discussions took place, Dr. Amico was added. I am not familiar with him personally. And Bill Hall from the University of Illinois was added.

(Slide.)

Now Chairman Budnitz wrote us a memorandum trying to outline what had been done in the meeting that took place among them, and I thought there were a few conclusions that they reached, some consensus conclusions as they are termed here, but I know from past experience that representing consensus conclusions is often dangerous. So I think he tried to itemize the kind of insights that had been gained.

A number of items were noted. Essentially several important conclusions emerged in the form of consensus opinion, and these set down are approximately as interpreted by the chairman. I will just try to paraphrase them briefly as it might relate to the discussion here and future discussions.

Related to PRA, their general comment was that the most important recent insights we have gained into plant capability have come from PRA, probabilistic risk assessment.

The second general conclusion they reached is that validation is needed of the models and data that has been used to make the probabilistic risk assessments, and I don't think that is news. That is something that has been known.

The third item I think I should read out because it seems to be reasonably carefully worded. It says "The panel agreed that the ensemble of new plant study with PRA, those designed in the period after about

1973 to '75 when the seismic design approach became significantly more standardized, seem to emerge as somewhat more robust against earthquake threats than the ensemble of older plant study.

"Specifically the number and type of ideosyncracies found in the PRA's of new plants are fewer and less troublesome. The earthquake levels at which they are appear are generally higher and the overall thoroughness of the seismic designs seem to be generally better."

Now I think this is an important point to touch on. In some of our discussions in the working group, we have tried to deal with the generic, and I know you don't like that term, but the generic grouping of what we can say about PRA and margins in general.

It does seem that there will be some time period of plants before that time period that would have to be looked at almost on an individual basis or a subgrouping basis. For those after some time period, which they have defined here roughly as the '73 time frame, could possibly be looked at as a single group.

They also go into some key insights that they think have come out of the PRA, and again I will read just to be accurate in what they have said.

"If one assumes that the uncertain assumptions and data underlying the current seismic PRA's are in fact correct, then the panel tentatively agreed that the following key insignts would emerge from the existing seismic PRA literature."

The first one is there would be a confidence that an earthquake at about the size of the SSE would have a very low probability of compromising plant safety.

The second item is there would be high confidence that earthquakes at about twice the size of the SSE would have only a low probability of compromising plant safety. I guess you have to determine what very low means versus low. I think we have argued in the community about this before.

At three times the SSE, for earthquakes of a size three or more times the SSE it is difficult to generalize from the PRA literature as to whether an actual threat is posed to the plant study.

They go on to itemize some more things.

The next one is earthquakes of a size four to six times the SSE or greater have been found in almost all PRA studies to pose a defininte threat to plant safety and there is little controversy about this conclusion.

Then the final item is that for plants studied with PRA to date the specific safety compromises that result from earthquakes tend to be different from one plant to the next, sufficiently so that it is difficult to draw general conclusions concerning which aspects of plant design or operation are most vulnerable to earthquakes.

Again, I think if you have specific comments on that, I would need help from the group that participated in developing it to defend it.

There is again a meeting in September arranged with the ACRS, and I am sure an earlier one could be arranged if you wanted to explore these things further.

I am not sure the NRC working group internally would agree or disagree with all of these conclusions. That will have to take place in June.

MR. OKRENT: I think you need a pessimist on your panel.

(Laughter.)

MR. JACKSON: That may be true.

MR. EBERSOLE: Would you clarify a term you use here. You said earthquake of a size.

MR. JACKSON: Of a given size I think or a given magnitude. A given magnitude is what I am sure they are referring to.

MR. SIESS: Do they really think in magnitude or PGA?

MR. JACKSON: I think that probably PGA. All of us think in terms of PGA, but that relates back to the magnitude or design spectra.

That is really all I had prepared, unless you have specific questions. I think it might be more appropriate to discuss this with a larger segment of the working group and after we have had an opportunity to meet with the consultants and discuss some of these conclusions, but I thought I would share what we had with you at this point in time.

MR. EBERSOLE: Of the group that you identified up there, which of those would be responsible for identifying what I guess I will call the Q list?

MR. JACKSON: I really don't know.

Paul Smith, Paul, do you know which one would identify the Q list?

MR. SMITH: I am sorry, I don't understand the question.

MR. EBERSOLE: Well, it is the critical component list.

MR. JACKSON: Who identifies the critical component list of this expert panel?

MR. SMITH: The critical component list?

MR. EBERSOLE: Yes.

MR. SMITH: Well, that is certainly not ~ activity that is contemplated at this point for this panel.

MR. EBERSOLE: Well, ultimately your seismic margins have to be addressed to the individual components that reflect the integral safety picture, and I am merely asking which of those individuals will act in the capacity of identifying and confirming the adequacy of the Q list?

MS. SILVER: Well, let me just summarize the situation as I know it from another viewpoint and perhaps provide a perspective that will answer your question.

I am from Lawrence Liver Laboratory of course and we are assisting in this effort and generally we are assisting the chairman in carrying out the function of this panel.

I think where they are now is attempting to -- well, first, one point I think, Bob, from the summary that you didn't make. I think one question was asked of the panel, is something necessary to be done and is it necessary to do something on the issue of seismic design margin.

Although the issue was not explicitly addressed at the meeting, I believe it is fair to say that there are two panel members here who can contradict me if I am saying it wrong. I believe it is fair to say that based on the discussions that went on that they concluded that something was necessary. It was necessary to do something. Now what that something is is of course not defined at this point, but I think that was an important conclusion in that meeting because that was a question asked, should something be done.

The second thing is I think at this point what the panel is attempting to do is to identify after you after you go with seismic design margins, to break that down into more specific regulatory needs. Now that is not necessarily regulatory criteria as such, but just breaking that issue down a little bit in a more refined way, and that is where they are at this point and that is not yet done.

When that is done and people can look at it, including yourselves, as to what those more specific needs are, then the step is to take those and to develop the required research or other tasks, cooperation with industry or whatever and however it works out and develop them in more detail as to what that means in this process of addressing the seismic design margins.

Now whether or not a Q list appears as a task then is unknown at this point and it is a possibility, but I don't know of any basis to say yes or no to the question. It is certainly not barred in my understanding of how the panel and the subsequent effort by Lawrence Livermore is to proceed, but it is not certainly in there either.

MR. JACKSON: I think the real honest statement is we have just begun to get rolling at one meeting of the expert panel. We have not even had an interaction between the expert panel and the NRC internal group. We are working on that and I think it may be a little bit premature to be making a great statement of where we are going and what we need.

The specific answer to your question is we in selecting the expert panel and asking Livermore to assist us, we hope that this represents about a reasonable cross-section of people knowledgeable about plants and PRA and margins in general. So the need for lists like that might come out of such interaction. And we have our own internal people and Thadani from the PRA group should also comment on it.

MR. ETHERINGTON: When you mention the increasing probability of damage with increasing size, is that given a particular size or does it take into account the lower probability of the bigger size as well?

MR. JACKSON: Al?

MR. CORNELL: Given the size.

MR. JACKSON: Given the size, yes.

MR. OKRENT: I see a hand. Who is that?

MR. JACKSON: Bob Kennedy.

MR. KENNEDY: I am Bob Kennedy, SMB. That letter that was read was the chairman's interpretation of what we said. Something is left out of that letter. That letter clearly applies to the PRA's that have been in relatively low seismic areas. It would be a mistake to say that those conclusions would also apply in a high seismic area where you would have to go to three times the SSE to have significant probabilities of damage.

Those conclusions are clearly from low seismic plants because that is where most of the seismic PRA's have been done.

MR. SMITH: That is correct. I remember that point was brought up at the meeting and I don't recall whether or not that caveat is in Dr. Budnitz's summary or not.

MR. JACKSON: And, indeed, the margins program is essentially aimed at the eastern plants in general because that is where most of them are, I guess as many as plants as possible, but most of the ACRS letters that have come forth on the need for margins analysis have been on the plants recently, the near-term OL's that have been recently done in the last two years. So I am glad you mentioned that.

MR. OKRENT: That is because we were reviewing those those plants.

MR. JACKSON: Okay. That is fine.

MR. OKRENT: I am serious in my suggestion that you ask yourself whether you are getting a sufficiently pessimistic or skeptical look. The conclusion about very low probability at SSE, for example, if it doesn't consider the potential for things existing like was present at Oconee 1, a rupture which could lead to internal flooding, could be caused by the SSE, or other things of this sort.

It seems to me to be built on assumptions that may or may not be valid and there are other things one could put into that list.

MR. SMITH: Dr. Okrent, if you would recall what Bob read, it was prefaced with a statement that said assuming the validity of the, or something along those lines, and this specific issue came up between myself and Dr. Budnitz. I saw the earlier draft of those summaries and I stressed exactly that point and I believe that is why those words are there.

MR. OKRENT: If I assume all plants are built the way they are designed and they all have the margins the staff says are there and these hazard curves are highly conservative and so forth, of course, you know, but what we have seen in fact is a variety of situations that depart from each of the assumptions I just gave.

MR. JACKSON: I think it is a question we will raise with the group and have to discuss. I think we had some of the same concerns from reading the document.

MR. OKRENT: Let's see, according to the agenda we are now up to comments by PG&E. I assume they relate primarily to things on Diablo Canyon rather than the seismic margins program.

MR. HOCH: Thank you, Dr. Okrent.

I am John Hoch, PG&E's Project Manager for Diablo Canyon.

I would like to introduce to the ACRS Subcommittee, I guess more properly Subcommittees, PG&E's Vice President of Engineering, Donald A. Brand sitting on my right. Don has overall responsibility for all of PG&E's activities related to the Diablo Canyon seismic licensing condition and he will lead our discussion today on the items on your agenda.

MR. BRAND: Thank you, John, and good morning.

We have reviewed the NRC staff's proposals for the seismic licensing condition for Diablo Canyon that is dated May 7th, 1984 and we have advised the staff that we concur with its contents.

We met with the staff on May 8th to discuss these proposals in detail. I would like to make the following observations regarding the long-term seismic program for Diablo.

One, we view the seismic program for Diablo as a means of updating our seismic studies to take into account all seismic data that has become available since 1978. We will provide a comprehensive program description to the NRC staff in final form by January 30th, 1985. As such, we will be factoring those comments from this committee and from the NRC staff and consultants coming out of today's meeting as well.

We anticipate meeting with the NRC staff two or three times between now and January 30th to discuss the formulation of our plan. By doing so we hope to obtain full staff input into the preparation of that plan.

We plan to make extensive use of consultants who have worked previously on Diablo Canyon geologic and seismic studies. In addition, we expect to retain additional geotechnical consultants in many of the major areas, such as geology, earthquake magnitude, ground motions, et cetera.

We expect to employ a system of peer review in many of these major areas to strength our program, and we understand that this system has been used successfully by other utilities.

Our long-term seismic program for Diablo will include in-depth studies in offshore and onshore geology, seismology, earthquake magnitude, ground motion and soil structure interaction.

These studies will incorporate the results of recent onshore work, both within the petroleum industry and by others, and we will review our existing interpretations in light of this recent data. If changes in interpretation are required, these will be made and consequences assessed.

We intend to perform a full-scope PRA which adequately addresses both seismic and non-seismic initiators. This PRA will be up to date, realistic, plant specific and will adequately represent uncertainties.

We have with us today Dr. Allen Cornell of Standford Univerisity and Dr. Robert Kennedy of Structural Mechanics Associates, both of whom will be assisting us in this effort.

Dr. Cornell has a few general words to say on the subject of hazard curve development and then I will call on Dr. Kennedy for a few general words on fragility curve development.

Dr. Cornell.

DR. CORNELL: I am Professor Cornell, a consultant to PG&E. I want to state simply that the seismic hazard analysis that we can anticipate for the Diablo Canyon study will certainly be of the most advanced that current practice permits, and in particular of course will address and incorporate the information that is developed in other parts of the program with respect to ground motion estimation and other statistics.

In addition, it will certainly direct itself to the uncertainty question, that is the alternatives in hypotheses that may exist with respect to the nature of the faulting or the nature of the ground motion prediction models and so on. This is in the form of the major improvements in seismic hazard estimation procedures that haveen developed in the last several years and applied at other plants with other seismic PRA's.

In addition, an element that may be considered is the use of so-called historic methods to supplement the seismic hazard analyses in the intent to verify those analyses where direct empirical data permit that.

I think that finishes my comments.

Thank you.

MR. OKRENT: Could I ask were you planning to use subjective waiving of different hypotheses as a way of coming up with some median hazard curve with a probability distribution around it? I have seen that done on some of the PRA's. Is this what you referred to by recent techniques?

DR. CORNELL: To my knowledge, that has been done on all of the seismic PRA's, but the particular plans for this study have not been developed.

MR. OKRENT: It leaves quite a bit of the answer in the hands of whoever is doing the weighting.

DR. CORNELL: My experience is, and we have presented several studies to you and to the staff in the past, that that is normally not the question.

Secondly, other studies recently have done the weighting not by having one individual apply the weights which to be sure represent his estimates of the professional community's opinions and not necessarily his own. But rather than having a single individual it will involve the use of an expert panel to develop weights and then it becomes a multi process, a multi-person process as opposed to one.

MR. BRAND: Dr. Okrent, if I could add to the answer to your question. We will between now and the end of next January be putting together the flesh on the skeleton of this program and we will in this intervening six to seven-month period be developing a more specific answer with more detail to your particular question.

MR. KENNEDY: Bob Kennedy, Structural Mechanics Associates.

Basically Mr. Cornell has talked about the hazard curve development. The other part of the program that the plans are being formulated now for is the development of the fragility curve and the incorporation of uncertainties in response parameters and therefore uncertainties in response of the structures and equipment.

The approach that is intended to be used is very plant specific wherever possible. In other words, fragility estimations will be made based upon detailed walkdown of the plant and based upon the use of the most sophisticated deterministic analyses that exist for the plant.

These deterministic analyses that exist will be modified to account for the effect of parameter variation, of probabilistic distributions on damping and soil structure interaction effects, et cetera.

In addition, existing and possibly some new failure capacity analyses will also be used in developing these fragility curves.

So for the civil structures and the passive equipment failure modes, certainly these will be based upon plant specific fragility. For active fragility modes, as you are well aware, must of the data base for fragilities is a generic data base and that data base will have to be relied on for a lot of the active failure modes, but it will be a PRA fragility study of a type similar or slightly beyond what has been done on other commercial PRA's that have been looked at by the ACRS staff and submitted to the NRC.

MR. OKRENT: In some of the utility sponsored seismic PRA's there was of necessity a limited sampling of things like penetrations and so forth, and that is just one example.

Do you contemplate about the same sort of thing here and, if not, in what way would it differ?

MR. KENNEDY: That is getting into the level of detail that basically has not been resolved. I think it is clear that it will have to be a limited sampling rather than a complete sampling because of the impossibilities of incorporating it into a seismic PRA in absolute complete sampling.

I suspect the sample size will be somewhat larger on Diablo Canyon than on most of the other commercial PRA's, but it certain would still be a limited sampling.

MR. OKRENT: It will be of interest to see how you choose your sample for penetrations and for other things and why it is adequate in your opinion, but I am not looking for an answer now.

MR. KENNEDY: Your comments are duly noted.

MR. OKRENT: Any questions?

Dr. Ebersole.

MR. EBERSOLE: I just wanted to ask in the sampling process would you be having a look at the primary loop PORV's and running a seismic analysis of that?

MR. KENNEDY: That is a question that really depends on what is placed into the fault trees and event trees. I guess what you mean by PORV's is power relief valves?

MR. EBERSOLE: Yes.

MR. KENNEDY: Such valves would normally appear in a PRA. If your concern is a failure of those valves due to structural failures as a result of the seismic event, that would certainly generally be included. If the concern is some type of a degradation of those valves as a result of the seismic event, gradual degradation is an area that has been very difficult to incorporate into seismic PRA's and I would question whether it would be in the PRA. I guess I would have to know a little bit what the level of what the area of concern is before I could answer what would likely be in and not be in.

MR. EBERSOLE: Well, I will tell you the reason I mentioned it. The PRV's offer an escape route and are another mode of cooling and yet they are not normally qualified in the seismic context.

MR. BRAND: If I may beg off, I would rather defer until we present our plan in more detail to the more specifically answer that question.

MR. OKRENT: I think it would be useful to take the PORV as one example of a lot of non-seismically qualified equipment that enters into what are called internal initiators and PRA's and may have to be considered in a very different sense here.

MR. BRAND: To the best of my knowledge, our PORV's are seismically qualified already.

MR. OKRENT: Well, that is good for them, but there will be other things.

MR. BRAND: That is only the beginning, I am sure.

MR. OKRENT: That is I think the general message you should get from the question.

I guess that covers your presentation here, does it, or are there more points?

MR. BRAND: That is the extent of our formal presentation on this particular phase of the program.

MR. SIESS: Question.

MR. OKRENT: Dr. Siess.

MR. SIESS: This may be none of my business, but I was wondering how PG&E is going about this. Are you going to manage it yourself and have a number of different consultants in the different areas, or are you going to have one group carry out this whole thing? You have had a couple of different consultants speak and I was just wondering how you were going about it.

MR. BRAND: We have talked amongst ourselves about this and as well we have had some additional information from staff in our meeting with them earlier this month. They have recommended as well that we talk with other utilities and gain the benefit of their experience.

MR. SIESS: That is on the PRA, isn't it?

MR. BRAND: No, excuse me. This is on the broader seismic program. Before we come back with a specific recommendation and plan, we intended to do just what we have been asked to do in terms of discussing that with others and then going from there, unless I misunderstood your question.

MR. SIESS: At this point you think you understand what the staff wants? You had one meeting with them or more than one?

MR. BRAND: We have had one formal meeting with them and we thought we had a very good dialogue. I am sure there are things in their minds that we are not yet aware of and we will be having subsequent meetings to assure a full exchange of views so that the plan that we report next January will conform with their expectations.

MR. SIESS: Now on the basis of what you know now, you are satisfied that you can complete this effort in two and a half years? That is what you got after the January 31 date to July 1 I guess.

MR. BRAND: As I understand the schedule, we are to submit a plan and the staff is to review and approve that plan. There was the idea originally that we were to complete our program by I believe July of 1988. At the same time staff has introduced an alternate schedule of three years after formal approval of our plan and I think that question will still require a more final answer.

With regard to your earlier question, I would want there to be no misunderstanding with regard to our overall seismic research plans. PG&E intends to manage that plan and that program.

MR. SIESS: I understood you have the final responsibility. I was wondering if you were going to be managing multiple consultants or doing what you might be doing in-house?

MR. BRAND: We have discussed both ways. There are advantages and disadvantages of both ways. We have yet to make a final decision on that.

MR. SIESS: That will be part of your plan?

MR. BRAND: Oh, yes, sir, most assuredly.

MR. SIESS: Thank you.

MR. JACKSON: I might add, Dr. Siess, that the schedule motivation, it came from the Commission and was directly related to the ACRS letter of 1978 as being 10 years. I guess you had some magical number in mind when you selected 10 years, but that is really where the motivation for 1988 came from.

MR. SIESS: I can't speak for the whole committee, but 10 years seems like a nice round number.

(Laughter.)

MR. SIESS: I am not sure that six years later as much progress has been made as we might have expected at that time. Three years seems a little optimistic to me, but along the lines of Dr. Kerr's question earlier, it at least tells you when you are going to be finished.

MR. OKRENT: I wonder if PG&E has any thoughts on the questions that have been raised concerning who should do this study. Commissioner Bernthal, for one, if I recall, raised such a question, and we have heard some of the members of the public raise the question in a different way.

I would be interested to hear any comments you might have on that and also, assuming that you are thinking in terms of PG&E directing the study as we have been talking about just in the last few minutes, whether you see any mechanisms that would provide what I will call a truly independent review aside from whatever review the NRC itself might be giving it with its consultants?

MR. BRAND: With regard to your first question, we believe the staff proposal is a reasonable one in terms of placing we in primary responsibility in terms of managing and carrying out a program that we propose and that staff approves.

At the same time, we are cognizant of Commissioner Berntnal's views on this subject and recognize the appropriateness of having the NRC staff manage a parallel effort as well.

All things being equal, we would believe that our study will be objective, and at the same time we recognize that there are other views regarding that and we feel that the staff's proposal of doing independent work addresses those views.

As well, we will be having a peer review panel giving still further objective input regarding our performance of our program.

With regard to your second question, I don't have I think further views to really give you on that right at this time I believe.

MR. OKRENT: Any other questions for PG&E?

(No response.)

Well, let's see, timewise we seem to be in the middle of the agenda item which is called general discussion and ACRS consultants' comments. I guess this gives me an excuse to see whether any of our consultants would like to provide any comments at this time on the principal subject of this morning's discussion which is the proposed seismic re-evaluation approach?

(No response.)

MR. OKRENT: The silence is deafening.

(Laughter.)

MR. PAGE: Mr. Chairman.

MR. OKRENT: Yes, sir.

MR. PAGE: As an ACRS consultant I would like to postpone most of my comments until after we have heard from Dr. Crouch. However, in the meantime, my first reaction to the conditions that are proposed by the staff with respect to geologic and seismic studies is a favorable reaction. I think that their proposed conditions are logical and reasonable. We may have other remarks later in the day.

MR. OKRENT: Well, I am not surprised that you think you may have things of more direct interest in the afternoon's discussion.

Are there any comments that the ACRS members want to make at this time?

(No response.)

MR. OKRENT: Well, I am going to propose that we begin an early lunch in a few minutes. I will assume that we can take an hour for lunch and be back here whenever we break, is that right, Dr. Savio?

MR. SAVIO: Yes, that is correct.

MR. OKRENT: Since my watch says it is nearly 12:20, I will be magnanimous and say we will be back at at 1:20 and we will begin with the paper by Mr. Crouch at that time.

The meeting is recessed.

(Whereupon, at 12:18 p.m., the ACRS subcommittees recessed, to reconvene at 1:20 p.m., the same day.)

AFTERNOON SESSION

(1:25 p.m.)

MR. OKRENT: The meeting will reconvene.

Dr. Crouch.

MR. CROUCH: I would like to preface my talk by saying that I was invited here by Dr. Savio to give a presentation of a paper that has recently been published by myself, Dr. Steve Bachman and John Shay from Nekton, Incorporated.

We have been asked to present some of our ideas and conclusions about this paper, and I come here before the committee not as an advocate or either being on the pro or con side of Diablo Canyon, but to objectively give some of our views on what we think are some of the major tectonics aspects of California.

They do have somewhat indicate implications to Diablo, as I understand it. We are not qualified to speculate nor to suggest what those implications might be.

So with that I would like to first briefly present the interpretations and conclusions given on our paper. The paper is entitled "Post-Miocene Compressional Tectonics Along The Central California Margin." It was published very recently at the San Diago meeting of the APGS&PM Pacific Section. It was published in a special volume that was edited by myself and Dr. Steve Bachman.

Secondly, I would like to discuss what we believe is the true character of the Hosgri and possible general misconceptions and perhaps misinterpretations concerning the Hosgri itself.

Thirdly, perhaps from some of the discussions this morning I thought I might address the availability of proprietary data that has been collected in the vicinity of Diablo that might be useful to the re-evaluation effort that people have discussed in the meeting this morning.

I would also like to discuss problems with present geological interpretations given that the Hosgri is predominantly a thrust rather than the previously assumed strike-slip fault interpretation.

If I could have the first slide.

(Slide.)

I will try to point out just briefly what some of the objectives are in terms of our interpretation. I show here three different fault orientations. On the left a strike-slip fault block in which the right-hand block is moving laterally or horizontally past the left-hand block.

On the right, which is called a reverse or thrust fault, depending on the exact angle of the fault itself with the horizontal plane, it shows the right-hand block moving upward and over the left-hand block.

In the bottom of the slide it shows an oblique fault which has both vertical and horizontal displacement.

Now the basic premise of our paper and the argument that is of concern regarding the Hosgri fault is that it has previously been interpreted as a strike-slip fault, that is the block on the left, and we believe that it is more appropriately interpreted as either a reverse or thrust fault or perhaps an oblique right slip fault, and I will talk about this more as I go through the paper and interpretations of that.

If I could have the next slide, please.

(Slide.)

MR. PAGE: Before you go on, you might mention that the original thought was a vertical fault.

MR. CROUCH: Yes. You might run it back one.

(Previous slide displayed again.)

Originally associated with most strike-slip interpretations the Hosgri and a number of other faults like the San Andreas are generally regarded as being vertical kinds of faults. It is shows here as inclined to the angle of the fault in map view. But in many cases, espacialy in offshore seismic data, generally a strike-slip fault is interpreted to be in the cut the vertical plane.

The next slide, please.

(Slide.)

This is a map of Central California extending from San Francisco to the north down to the Santa Barbara Channel, what we call the Transverse Ranges, the Western Transverse Ranges at the south. Santa Barbara is located about here and then San Francisco up in here and Monterey Bay shown here.

We have shown on this slide the major faults from the San Andreas fault westward in the central coast ranges. These are interpreted as the Rinconada fault and then the fault of concern to most of you, the San Gregorio, San Simeon, Hosgri fault system.

Also in our paper we discuss what is called the North Channel Slope fault by people from the USGS, namely, Bob Yerkes and others who in 1980 published a paper describing some of the first motion studies and some of their interpretations based on mainly well log data that there is a major thrust that bounded must of the North Channel.

Anyway, we concentrated on this North Channel Slope fault and the Hosgri fault because both had been disputed. The North Channel Slope fault has been disputed as to whether or not it even existed and the Hosgri fault had been accepted by many, many workers as being a wrench-style fault or strike-slip type fault.

The next slide, please.

(Slide.)

This is a slide showing the coverage of some of our data. It doesn't show all the data I have used in this interpretation and it doesn't show a lot of the data that I have worked with over the last four or five years throughout the Santa Barbara Channel and Santa Maria Basin. But it does show the basic grid that Nekton had collected, and this was proprietary data that was sold to the industry to evaluate some of the OCS leases for OCS-73, OCS-53 and OCS-68.

The coverage is basically from the City of Santa Barbara, along the coast from Santa Barbara out around Pt. Conception and Pt. Arguello. It covers the state leases that were supposed to be coming up for sale sometime off Pt. Conception and Pt. Arguello here, and then to the north our most recent set of data collected with regard to OCS-73.

Now we also collected back in 1980 a multifold Sparker survey in which I have done quite a bit of interpretation across the entire Santa Maria Basin, and that is not shown on the track lines here.

First of all, I would like to show you our line drawing interpretations of SB-1, which is off Capitan here, line PC-1 which bisects Pt. Arguello and Pt.

Conception and then we will go to the north and I will discuss some of the lines that we have across the Hosgri fault zone.

If I could have the next slide, please.

(Slide.)

This is line SB-1. It is trans-northeast/southwest and is essentially a dip line across the northern Santa Barbara Channel. By that I mean that it is crossing the structural grain which is generally east/west in the Channel so that we are getting as close to a cross-section of the general Northern Channel as one might get with seismic data.

We have somewhat conservative in our interpretation of the amount of offset along this fault, but basically I wanted to show you some of these lines because we think the character that we are seeing along this North Channel Slope fault is very much like the character we are seeing in places like the Santa Maria Basin.

Primarily I point out that a number of these thrusts that we interpret, and there are three major thrusts interpreted here. This one right here is what we call the North Channel Slope fault and it always tends to be the biggest player along the North Channel.

If you will notice, there are two aspects to these. One is that the tend to flatten and sole towards the northeast and, secondly, they don't cut the sea floor, at least according to our interpretation and the data we have.

Now of interest here is that Yerkes and a number of other people have done first motion studies out here that suggest that many of these faults are active. That is to say, we feel from our work in the offshore that seismic lines do not provide a reasonable estimate of activity on these faults. And this is true not only offshore, and I will show you some slides later that suggest that onshore it has been shown quite conclusively that some of these thrust faults tend to die out and up dip.

So the monitoring of the faults and fault activity using seismic reflection data may be very misleading. For example, we think that a lot of the upper section here, we think the North Channel Slope fault is indeed active and that it is causing the existence of the North Channel Slope fault which is Yerkes' basic premise and we agree with that premise, but we think that the faulting is essentially flexing the upper more ductile part of the section and we don't pick it up on our seismic until we get down deeper into more lithofied rocks.

That is at least one interpretation. Some of the other possible interpretations are, and we see this onshore, that some of these faults may actually steepen and then flaten out again to surface. So they actually turn into bedding plane faults which are almost impossible for us to interpret on seismic records. That is, as they come up shallow, they actually roll over and flatten out into bedding planes. So they don't show us any specific offset.

The next slide, please.

(Slide.)

This is the line off Pt. Conception, line PC-1, again running northeast/southwest and across the general northwest trending structural grain we see at Pt. Conception. Again we see flatten of the thrust we believe and in fact Arguello field, which is very close to this line, we think is largely set up due to pliocene thrusting and the Arguello field itself is the anticline that lies above that thrust. And that has been similarly interpreted by Cheveron and a number of oil companies as well.

Of interest off Pt. Conception and Pt. Arguello is that the general trend of thrusting as compared to the Santa Barbara Channel is much more northerly than it is in the channel. It is as if thrusting is basically wrapping right around Pt. Conception, and I will talk more about that later.

The next slide, please.

(Slide.)

This is from some more detailed work that I have done in the northeastern Santa Maria Basin. It covers an area that is south of Diablo. In fact, Nekton doesn't have any data off Diablo. Most of our data stops about two or three miles south of Diablo. The reason for that is because we were covering the tracks that were offered during OCS-73. It is not because we weren't interested in the other areas. We simply were collecting data in areas we thought would be of interest to the oil industry.

Anyway, this map represents a compilation of work that I have done over the last three or four years in the offshore Santa Maria Basin. It doesn't cover the entire basin. It only goes out to what we call the offshore Lompoc fault. So it covers about the inner one-third of the basin, the offshore.

Further to the west are a number of other structures that are very similar to this and we pointed this out in our paper that Dave McCulloch and others from the USGS and Hoskins and Griffin early on showed very similar sorts of interpretations of what we show, and that is that faults and folds in the offshore Santa Maria Basin tend to be very parallel to each other.

We think this is very significant in terms of how you interpret the faulting because many of the sediments in the offshore Santa Maria Basin are quite ductile, quite easily folded and we believe that if there was major wrench faulting that has been proposed, that is on the order of say 80 kilometers or more, as Clarence Hall and others have interpreted and people have assumed, then there ought to be en echelon style folding that has been shown to exist from clay cake model experiments and other things along sheer zones.

So our argument here is that basically because these faults and folds are parallel to each other, they are showing more of a perpendicular sort of compression rather than some kind of oblique transpression or convergence due to wrench style faulting. Now I will talk a little more about how one might sort those two out because in some cases it is very difficult.

Anyway, the lines that we show in our paper and I will show here today cover a segment of the Hosgri that is northwest of Pt. Sal. I show three lines, or four lines, three that are across the fault in a somewhat perpendicular mannner and one that is parallel to the fault itself. We think in both cases they give us a pretty good idea and a pretty good handle on the thrust nature of the fault zone.

The fault zone itself is roughly three to five kilometers wide and this has been shown by a number of people as far back as Hoskin and Griffin and Holly Wagner from the USGS. So the general area of deformation associated with the Hosgri is quite a wide zone.

The next slide, please.

(Slide.)

I show this SM-1 as the first line to the south there. One of the best lines we have, that Nekton has anyway in the offshore Santa Maria Basin where the asymmetry of folding and the association of folding with faulting is very clearly seen on a profile. We can also very clearly see I think flat line reflectors that cross-cut the other trends or reflectors that follow the fault zone itself, and those are shown in the little lines below the fault zone and above it that give us a pretty good handle on where the fault trace really is.

The asymmetry of folding we see is very characteristic of thrust and fold belts and not as nearly characteristic of strike-slip or wrench style type areas. In wrench style type areas or strike-slip areas most of the folding tends to be symmetrical and developed above a vertical fault zone that then tends to branch out at the surface into what they call flower structures, and this is not the character of faulting we see anywhere in the offshore Santa Maria Basin.

Now at about two kilometers there I show a disturb zone in the upper part of the section and that is what has been mapped as the Hosgri proper or what we call the Hosgri proper. That is what has been depicted on most maps and most people's interpretation as the Hosgri fault itself.

I think the thing to note here is that if our interpretation is correct, then the actual depiction of the Hosgri itself is probably quite inaccurate as shown on most published maps. And just where one wants to depict the Hosgri alone in a diagram such as this is I think difficult to do.

The next slide, please.

(Slide.)

Again, this is the Hosgri proper as mapped in most published reports. Again, we show reflectors that cross-cut and show essentially two thrust zones. One we think may come up and be associated with that part of it and one we think is perhaps a stronger player here that is outboard of it.

We have also shown here and in the previous profile the Top Sisquoc. Now some of the misconceptions about the age of the Hosgri I will get into in a minute, but basically the big inconformity here that we see in the offshore basin and the time of most deformation is associated with the Top Sisquoc horizon. That is things tend to have stopped in terms of folding and faulting for the most part at the Top Sisquoc. That is where most of the deformation seems to at least quiet down in the basin. We don't necessarily think that that means that there is no activity out there.

At the Top Sisquoc is what we call the Base of Foxin actually, or the base of the upper pliocene. It is really the big break we see in the offshore. It is not in the middle miocene and what-not as people have sometimes discussed in the literature. It is quite young and it is very much in tune with what people have talked about in the onshore such as Woodram and Bramat in the onshore Santa Maria Basin. They discuss most of the deformation in the onshore as being pliocene and younger and we agree with that very strongly.

The little circle here shows -- well, I will show you in a minute -- that is where we pick up the faults on the cross-line which is a strike line parallel to the Hosgri. Now notice that that is on the order of one and a half kilometers east of the Hosgri proper as mapped by most people.

The next slide, please.

This is that line and it is a strike line parallel to the Hosgri fault zone itself and again one and a half kilometers east of the Hosgri as mapped by most people and again the Top Sisquoc. You notice the overlying section is folded in a manner that is cross-cut by these thrust faults.

This is very similar to the kinds of ways that they identify thrusts in the overthrust belt and other places across the United States with the co-corp lines. It is to not only have a dip line that shows some of the offset, but also strike lines that give you cross-cutting reflectors, that is cross-cutting to the overlying folding that will give you some kind of idea of what the dip of your fault plane might be.

The next slide, please.

(Slide.)

One last line that is at the northern end of the diagram just again to show you some of the relationships along the Hosgri itself. At three kilometers is the Hosgri as has been mapped by most people. Again, a fairly major fault to the west of it that has an asymmetric fold associated with it.

You notice that here, and I will show you in general how this works later, but in a number of these slides you will notice that I have shown faulting that is occurring behind the trust faults. In some cases these are normal faults, that is the one side is down with respect to the other side, and in the other cases they may be thrust faults or reverse faults that are fairly steep reverse faults. Here shown as a reverse fault is the westernmost block reverse faulted over this easternmost block.

In many cases we find in our seismic data that these are the faults that really show up on the shallower penetration data rather than the actual thrust faults themselves. Again, we don't know how one resolves the age of thrusting from some of that.

We think some of this faulting is actually due to the instability of the overthrust block, at least the normal faulting. In other words, it becomes unstable as it is trust out over the other section and you get release of the block downward. In some cases as it is being thrust, this block gets pushed upward and propagates closer to the surface.

The next slide, please.

(Slide.)

Just so we don't forget the onshore, this is a slide that just shows some of the Santa Maria Basin onshore. We were just looking at an area that is right about in here. In the onshore most of these fields are produced from well-known structures that have a number of welds drilled through them and in most cases they find that these are set up by reverse and thrust faulting.

In fact, the Casmalia, Orcutt and Four Deer fields that I show here are all bounded by a fairly major thrust. In some cases they have drilled wells, for example, near Casmalia that spudded in lower miocene rocks and went through 7,000 feet of volcanic and back into Monterey rocks beneath the thrust itself.

Of interest to many of the oil companies that are working onshore now is the fact that they have been drilling subthrust plays along the Orcutt-Casmalia trend which are now producing 30 to 35 gravity oil as opposed to the more usual 10 to 15 gravity in the overthrust block.

Now there have been a number of recent fairly good seismic records shot in the onshore Santa Maria Basin and the people from the oil industry have told me that they see very much the same sort of thrust that we see in the offshore, that is flattening at depth, and that, indeed, instead of being high angle reverse faults, many of these faults flatten at depth and become major thrust faults.

(Slide.)

In the next slide I will show you a cross-section. If I can go back for just a second.

(Previous slide shown.)

I will show you a cross-section of the Orcutt field. This cross-section trends northeast/southwest and so it crosses the general structure of the Orcutt field and crosses what they call the Orcutt Frontal fault which runs from the four Deer around through Orcutt and up through Casmalia.

The next slide, please.

(Slide.)

Again, you will notice that there is a considerable amount of well control to set up the general style and nature of this cross-section, and it is this lower section, the rollover, the Monterey at 8,000 feet or so that is producing some very exciting wells in the onshore Santa Maria Basin within the last year to year and a half and now is the site of a considerable amount of activity by a number of oil companies in drilling these subthrust plays. We think a very similar relationship can be seen in the offshore.

The next slide, please.

This is a map of again the Central Coast Ranges west of the San Andreas fault. It is tilted a little bit from what you are normally looking at so we could orient all these faults into our diagram.

But basically what we did when we did this study and we could see these compressional type features offshore, we went back to the older literature and looked at some of the work even as far back as in the early 1900's and especially in the 1930's or so. People like Reed and Holister, well know California geologists, wrote classic papers showing that there is a major amount of convergence and compression occurring in the coast ranges.

One of the people sitting on the committee here, Dr. Ben Page from Stanford has also been very instrumental in pointing out a tremendous number of reverse thrust compressional type features in the central coast ranges. So in our paper to point out that what we are seeing offshore can be seen onshore as well, we put together this diagram.

Now the point to be made here is that the people onshore are at somewhat of a disadvantage today because in the offshore we have such high quality seismic data now that we can see what is happening in the subsurface. That is now true when you are tramping over the mountains onshore. You look at a fault and you if you noticed in the previous diagrams, many of those thrust faults we showed tend to steepen at the near surface. So that if they were exposed, uplifted and exposed onshore to erosion that a geologist walking across the mountains would see the high angle reverse nature perhaps of the fault but wouldn't have any way of interpreting the flattening at depth that we see.

So we feel like the seismic data that we have we can actually now suggest to some of the people that have spent a lot of time onshore just what the character of some of these faults might be.

Along with that, a number of these faults onshore, such as in the southern sector, for example, and in this area near the Quiama Basin, and the Quiama Basin is a fairly well explored oil producing basin, and a number of those thrusts have been well established by drill hole information, most major thrusting that has essentially pushed in the sides of that basin very much like what we see in the offshore Santa Maria Basin.

To the north near Pt. Sur, in that area, in what is called the San Lucia Ranges people such as Compton from Stanford have suggested in the past from their studies that major shortening has occurred in a trend perpendicular to the San Andreas, and that comes from detailed studies of mainly granitic rocks that Compton worked on.

So we feel like there is an adequate amount of data to suggest that what we are seeing is more of a regional extent rather than a localized effect.

Let me have the next slide, please.

(Slide.)

This is a diagram showing the trends of the fold axis in the San Maria to Coalinga area and you can put a other similar diagram throughout a good portion of the coast ranges to show that many of the fold axis as well as the faults in the previous diagram are not en echelon on the San Andreas, but they are parallel to it or close to being parallel to it.

Now if wrench tectonics was the primary control here, we would expect to find most of these fold axis and most of the subsidiary faults at some angle to the San Andreas or at least at some angle to each other.

For example, if the Hosgri were another splay of the San Andreas as has been proposed, then we would expect to see en echelon folds and faults merging with the Hosgri rather than being parallel to it, and that tends to be true along other faults as well that cut the coast ranges. Many of them have fold axes that parallel to them rather than en echelon to them. Here they very rarely diverge from on the order of 10 degrees to the general trend of the San Andreas.

The next slide, please.

(Slide.)

Well, to sort of summarize what we believe is a sort of major compressional aspect to the offshore and onshore part of the California margin, this slide shows the vector resolution of the Pacific North American plates with the San Andreas fault system itself.

The point here is that the Pacific plate has been fairly well shown to be moving in a direction, as I have shown here by this vector at about 56 millimeters per year. The San Andreas, it this vector here represents the San Andreas motion, and it is parallel, if you notice, to the strike ot the fault itselt, and its magnitude is about 37 millimeters per year. That has been worked out by a number ot people to a reasonable degree we feel.

So that if one resolves the overall plate motion with the San Andreas motion, then you have to come to a fairly strong conclusion that in order to close this vector loop, as most engineers would do say, we have to have some component of convergence across the margin.

In other words, the San Andreas is not acting as a simple sheer or not taking up the plate motion that has been worked out for the Pacific North American plate. So you have got to have some other component to account for that.

(Slide.)

Along with that there should be a residual amount of strike-slip faulting outside the San Andreas. We take this diagram, and I realize for some of you that are not geologists or geophysicists this may be a little complicated, but it essentially resolves the North

American Pacific plate motion over the last 5.5 million years. We take that age because that is the time of the opening of the modern gulf and it is when we have the best data as to what the activity on the modern San Andreas really is.

We show here the general strike and magnitude of the San Andreas vector shown in Brown. I won't talk about this lower part of the diagram. It accounts for the variability of opening in the basin range which is a part of this whole thing.

If you want a more detailed and technical description of that, I suggest you read a paper that was handed out to the committee members by Tom Jordan and Bernard Minster in which they describe in a very mathematical and rigorous way the overall vector solutions here.

Anyway, we take the general strike and magnitude of the San Andreas fault shown in brown and because that doesn't match this strike and magnitude of the North American Pacific plate motion shown here, then we add to that the residual strike-slip and the residual convergence, the strike-slip shown here, and you can see that according to the amount of basin range opening, one assumes you can get different magnitudes of strike-slip and then different magnitudes of convergence.

But in any case, no matter what you assume in terms of the amount of opening of the basin range, you are still left with the conclusion that you have to have some kind of convergence across the margin simply because the San Andreas and the North American Pacific plate motion do not match. So the San Andreas is not a pure sheer.

So our estimate of strike-slip versus convergence is shown on the right and we favor something on the average solution here. We feel like the minimum amount of 28 kilometers of convergence matches some of the minimum amount we can estimate from trying to do balanced cross-sections in the offshore and from some of the data that has been interpreted in the onshore, that is over a distance of about 200 kilometers on the margin we calculate roughly 17 to 20 percent shortening during the last five million years say and that equates to something on the order of say 34 to 40 kilometers of shortening in the last five million years.

That perhaps could be equivalent or even greater than the amount of residual strike-slip faulting, that is the strike-slip faulting outside the San Andreas proper.

The next slide, please.

(Slide.)

Well, if you have that much shortening across a margin such as this, and if it is active, then you are left with the next conclusion that we show in our block diagram as a DeComont model that it seems quite plausible to us that we have to have some kind of attachment zone. That is to say that the Pacific plate is virtually still going underneath the margin some place and that the true, what we call the plate boundary between the Pacific North American plate isn't the San Andreas fault, but is somewhere east of that, and that is supported by some studies by Bob Yates and Hadley and Canamorie from Cal Tech and some other people as well, and perhaps could also account for some of the strike-slip faulting in the first motion studies we see in regions such as the Mohave and regions such as the southeastern part of the Sierras.

So that one might expect, if we are correct here, that a future San Andreas could possibly cut say perhaps the San Juaquine Valley in that area, and to bring it closer to home perhaps some of the Calivaris faulting and Hayward faulting is also a result of this continuing convergence of the underlying plate here.

Anyway, we think that this diagram takes care of a number of perplexing problems we have had with California geology, not the least of which is the fact that the Hosgri and a number of northwest trending faults that are north of the east/west trending Transverse Ranges suddenly end and die out as they merge with the Transverse Ranges.

This has been a real dilemma for people like Clarence Hall and a number of others that have tried to tie the right-slip faulting of the Hosgri into the east/west trending Transverse Ranges. Now their choices for this has been to try and make these east/west trending faults, to try and make them pre-existing right lateral faults and then have a later history of left lateral faulting.

We think it is quite difficult to establish this earlier period of right lateral faulting and to change the general nature of this fault from one to the other to suit a general sort of model.

(Slide.)

I think in the next slide we show just a simple diagram of this idea that the Hosgri fault, and I am sorry about this slide. It was one my draftsman put together before I had a chance to review it. I have been gone for the last three days. It is not Santa Maria Basin. This is the general southern coast ranges.

Anyway, it still shows what I want to show, and that is if we interpret the Hosgri as a thrust fault and if that merges with some of the more recent interpretations of what they call the Santa Ynez River

Valley fault, Sylvester and Darrow, Sylvester being from the UCSP, and that there is active left lateral motion on what is called the Santa Ynez River fault that merges with the Santa Ynez fault and the east/west trending Transverse Ranges, then I think it is a very simple solution to move the block here towards the west and set up thrusting on faults such as the Hosgri.

The same is true as shown here for things like the Big Pine fault and the westward push and some of the reverse faulting we see on the Hildreth-Camuesa fault onshore. So there is a whole number of northwest trending faults that merge with these east/west trending faults that can be set up very nicely by having left lateral faulting going to thrusting.

This is not unlike that that was discussed by people such as Johns, Dick Wellingham that was consulting for PG&E and a number of others that have suggested that basically as blocks move around what we call the big bend of the San Andreas, it is right here, that generally as the faulting, the strike-slip faulting continues around big bend those blocks get pushed westward and set up the component compression we see along the Hosgri fault.

The next slide, please.

(Slide.)

Well, let me just summarize briefly some of the evidence for thrust faulting, and then I would like to go into what I think are a few perhaps misinterpretions or misconceptions about some of the things we wrote.

First of all, we think we can see flattening of faults on seismic reflection data that suggests thrust faulting, and the overlying asymmetry of folds tends to support that idea as well as the parallelism of folds and faults in map view.

Associated with that are a number of first motion studies, especially in areas like Pt. Sal, San Luchea Bank and others that also suggest that there may be even perhaps pure thrust motion along at least the southern segment of the Hosgri fault.

This is something, as you know, that Savage and Prescott argued was the case for the 1927 7.5 earthquake that has caused so much stir on the Hosgri. We agree with Savage and Prescott's interpretation that we think that that indeed probably was a thrust type earthquake.

The next slide, please.

(Slide.)

One misconception I think I found from doing this study that a number of people have is that a thrust fault has to be a wiggly line and that it can't be straight. I would like you remind you people that in the areas like the overthrust belt and in a number of other areas around the world where well-known thrust faults exist that on a regional sort of scale in many cases these appear to be quite straight types of faults.

In the overthrust belt in Wyoming and Utah this is just showing you some of the general well-known thrust faults. This scale down here goes from zero to 30 kilometers. So we are showing on the order of 90 kilometers and you can trace some much further than that that have a generally a very straight trace to them.

So the argument that the Hosgri/San Gregoria system is a straight trace and therefore doesn't have thrusting to me is not a very good one.

The next slide, please.

(Slide.)

Secondly, what I alluded to before was that we are seeing sort of thrust faulting in which above the thrust itself we quite characteristically see reverse faulting and normal faulting and sometimes that is what is often described and see on the shallower penetration records and not the thrust itself.

This bring up another point. If you consider the Hosgri as being a thrust fault rather than a strike slip fault, then I would say if you believe that, you need to throw away the trace of the fault as shown in published maps because it assumes that the Hosgri is a straight trace.

It is basically assumed that this part of the fault, that is right here and right here, represents the true Hosgri itself. If one were to tie across the Hosgri, assuming it is a thrust, then you get quite a different picture that it isn't indeed straight in a very detailed sense.

There is no data available right now or there has been no interpretation done which has tried to tie one horizon to the other across the Hosgri itself. They have only mapped a linear part of the zone. In fact, in many cases, as we state in our paper, we think as mapped the Hosgri in many cases has been mapped as what we think are mainly gas distrubed sediments in the shallow part of the section that in some cases occur above these normal and reverse faults above the main thrusting itself.

The next slide, please.

(Slide.)

This is a slide across the Ventura Avenue field from a paper by Bob Yates. It is an area that has a sort of characteristic style to it in terms of thrusts coming out near the surface and then dying out in the shallower part of the sections.

Here some of the ash layers have been dated essentially setting up this fault as being quite young on the order of .6 million years ago and yet doesn't cut much of the section that is above the overlying anticline or overlying fold simply because it is going up and flexing the upper part of the section and not cutting it.

We think this is very true in many of the lines we have seen in the Santa Barbara Channel and it is very true in many of the lines we have seen in the Santa Maria Basin.

Again, the point is we don't believe that seismic sections, seismic reflection lines are reasonable ways to date earthquake activity.

That is the last slide.

In conclusion we would just like to point out that the Hosgri as we have mapped we think is quite different than has been depicted in published reports. There have been a number of statements given recently as to implications of the age of the Hosgri as we have reported and the overall magnitude of faulting on the Hosgri.

I might just point out that we also believe that the tie of the Hosgri to the San Simeon/San Gregorio also should be questioned considerably because if you map and if you look at the Hosgri as we see it today, at least we interpret it, those ties become quite tenuous. In fact, as you go north into the San Gregoria fault, for example, the earthquake solutions do indeed tend to become more right lateral type of faulting.

Whereas if you look at the Hosgri, the southern segment, then you tend to get more pure thrust motions. At least that is the gist I get from reading published papers on that.

The actual Hosgri trace itself may indeed not coincide at all with faults such as the San Simeon, and San Simeon onshore may indeed be some of these subsidiary reverse faults and thrust faults above the Hosgri zone of thrusting itself.

So one has to consider, if you will, a bit of the dogma that went into interpreting the Hosgri strike-slip fault and the joining of that segment of the fault to faults such as an San Simeon and San Gregorio. We think that that whole system needs to be reconsidered or at least questioned as being interpreted as one major strike-slip fault system.

I might also add that the basis for strike-slip displacement on the Hosgri I think is rendered mute by recent drilling in the offshore. I have been invovled in a number of wells in the offshore which suggest that a lot of the basis for the offset of stratigraphy, the basis that was used to establish the 80 kilometers of offset on the Hosgri from Pt. Sal to Sam Simeon has been considerably I think questioned b[illegible] recent drilling of industry that has occurr[illegible] [illegible] [illegible] [illegible]a southwest of Pt. Sal and west of the Hosgri as mapped.

Thank you very muc[illegible].

Do you want me to stay for questions?

MR. OKRENT: Yes, would you, please. Let's see what questions there may be.

MR. SIESS: You were going to comment on the availability of proprietary data.

MR. CROUCH: Yes, I was. I am sorry. I was going to talk a little bit about the availability of data. The surveys that I have used and showed you on the slide are data that was collected by Nekton and sold to the oil companies or oil industry primarily. That data is sold for, or I think those three surveys that amount to on the order of $170,000. So they are not inexpensive.

There have been a number of surveys done in the last two to three years by companies such as Western Geophysical, GSI and a bunch of companies that do seismic reflection profiling and sell it to industry, and I would suggest that perhaps the geologists and geophysicists that work with those data, I don't think, as has been depicted earlier, I do not think they are immoral.

I might say that Hoskin and Griffith, for example, published all of their information in 1971, just two years after that data was collected. That is about the right kind of time to even do an interpretation of the data you have. At the time Hoskin and Griffith published that data, they simply mapped a fault. They had no idea it was a strike-slip fault as later was interpreted as such anyway. They simply showed a fault, and I think they did the scientific community a great privilege and a great service by publishing some of that proprietary information. We have all learned a great deal from their work.

So I think in many cases at least some of the consultants could at least view the kinds of data that are available in the offshore now. They are quite considerably better than the data that were used in the early work that established some of the Hosgri faulting and depiction of the Hosgri itself. We know considerably more now than we did back in the mid to late 70's.

MR. OKRENT: Dr. Trifunac.

MR. TRIFUNAC: If you went and basically gathered all the literature information and what you could map in the field, could you account for this convergence in relative size to the slip or just looking at the geological offsets on the faults and the folds?

MR. CROUCH: I think that would be very difficult because you are looking at only one part of the fault itself that is exposed at the surface. Now people like Compton and other people such as Gene Fritsche from I think the University of Northridge have made estimates based on their interpretations of regional sorts of -- well, maybe I should say local areas. Compton in the San Luchia Ranges made an estimate of 12 percent shortening across in a northeast/southwest sense. Fritsche argued for on the order of 20 percent shortening based on looking at thrust faults and deformation in the Monterey rocks and the Ozina fault area at the southern end of the coast ranges.

We have made similar, on the order of 15 to 20 percent shortening estimates using our data form the offshore San Maria Basin.

Now how one gets at that any more rigorously, especially seeing as how if our interpretation is correct that these may flatten out at depths of greater than several kilometers, as you know, the data dimished considerably at that depth, the data available to make an interpretation.

MR. TRIFUNAC: Well, I didn't mean that detailed data, but the sort of thing I was referring to perhaps is best illustrated by some of the work that was done in Southern California, for example, Andrews, where he just glanced through geologic literature and he didn't do any field work and he simply tried to assess the degree of slip on a particular fold system, the logic being that if you go across the entire system it has to come up to about five or six centimeters a year and it does.

MR. CROUCH: Well, that is basically what we have done. We have suggested that if the Pacific North American plate motion vector is correct and the estimate of the San Andreas motion is correct, then we are left with an absolute minimum of 30 kilometers of shortening across the margin in the last five million years. I don't know how to get at that problem any better than that.

MR. TRIFUNAC: But then all these leftovers, that is the difference between 37 and say 56 a year, are attributable to the whole region and not just the particular fault.

MR. CROUCH: The basin range opening, that is correct. Now the people that are working on this problem in a much more rigorous mathematical way are Mr. Bernard Minster and Tom Jordan. In fact, when we published our volume we invited them to give a paper in our volume because we thought it was so important to this overall picture and you might want to read that paper. I think it is an excellent paper.

MR. TRIFUNAC: I have. Thank you.

MR. OKRENT: Dr. Page.

MR. PAGE: Could I ask a couple of questions and make some comments.

I think Dr. Crouch gave a very excellent report on an excellent piece of work and I for one have confidence in nearly all of his observations.

As he point out, his view of the Hosgri fault today is quite different from our image of past years, and so different in fact that one wonders whether there ever was such a fault we imagined or if there was and if there still is in the north what becomes of that fault as it passes southward toward the Diablo Canyon power plant.

In the same volume in which Dr. Crouch's paper was published there is another paper concerning the northern part of this supposed continuous San Gregoria/Hosgri fault zone which purportedly there is 100 to 150 kilometers of strike-slip on the zone. If that exists, it has to be disposed of somehow as one goes southward.

I want to ask Dr. Crouch whether there is any chance whatsoever that there are two fault zones, one like our former image and another like the one represented in his profiles? For example, do the seismic profiles approach the shore very closely and, second, is there the barest chance that there might be a high angle fault zone between the ends of the profiles and the shoreline?

MR. CROUCH: I have thought about all those things, Ben. The paper which we referred to that suggests there is on the order of 150 kilometers of right lateral motion on the Hosgri, which also appeared in our volume, I will point out several things.

Again, it is interesting to note that one accepts that argument as being correct, then one has to deny the 80 kilometers of offset that has been established for the Hosgri. That is, if you make that one continuous fault zone, it again negates the entire argument that Clarance Hall and others have made for the southern segment unless you tie it into some other fault.

Secondly, the authors that wrote that paper again I think, unfortunately, are at some disadvantage because they do not have access to offshore seismic data to look at what the stratigraphy and structure is west of the area they are talking about.

As you know, the Hosgri and San Gregorio just clip the shoreline in a few places, and basically the offset that has been established is based on the interpretation of different stratigraphies across that fault.

The point that we make in our paper and I am so astounded by in a number of places that strike-slip faulting has been argued is that there is a tremendous amount of stratographic difference across a thrust fault. That is, if one were to approach the interpretation with say a thrust fault bias, I can't help but wonder whether or not one could come up with very much the same kind of story that these authors do to make a stike-slip fault.

Now that is not to say that they are not correct, because I haven't studied that area in particular. I just bring those up as possible questions that we began to ask about the interpretations with regard to the Hosgri, and one might also start raising those questions in other areas that have been thought to be well established such as say the Rinconada fault.

The other point that you can make is that from an awful lot of seismic data we have looked at, from the time of the volcanics, and there are volcanics that underlie the Monterey section, and they indicate to us anyway some kind of activity that formed some of the earlier parts of this basin or at least some kind of perhaps disruptive activity. I would like to prefer that that is related to perhaps extension.

But, anyway, since the time of the volcanics up to the time of what we call the near top miocene horizon in our paper, which is roughly on the order of five million years ago, we see very little evidence of major activity in the offshore along the Hosgri zone and we don't see any evidence of major deformation.

So that the only time that we can have major strike-slip faulting going on we feel is either during or before the volcanics occurred or after the Monterey was deposited and sometime about the miocene/pliocene boundary. That is pretty much what Livering and Bramlet argue for the onshore Santa Maria Basin, and except for there, they are seeing some growth in the miocene in some of the Monterey rocks, but again they tend to be more shoreward oceanograpically anyway than we are.

So the whole timing problem, or the timing that they want to move that fault, the San Gregoria, I think is a real dilemma for us, and especially if you tie it to the Hosgri. Now if you want to bring it into some of the faults that cut near Monterey Bay, such as suggested by Gary Green and others and tie it into the Rinconada or something else, I am not prepared to argue those points with you. But I would certainly argue that I think it is virtually impossible to tie it into what we call the Hosgri in the offshore Santa Maria Basin.

MR. PAGE: How about my other question about the possibility of a fault zone between the ends of your seismic profiles and the shoreline?

MR. CROUCH: We cannot because we do not have data there. We cannot rule out that possibility. If that was the case, it would make the kinds of faulting we are seeing a giant sort of flower structure associated with wrench tectonics.

If that were the case, there are several things that bother me about that. One is we see this parallel folding and faulting and thrust faulting across a very wide area. In other words, we can follow it all the way from the Hosgri clear out to the western part of the basin, the Santa Maria Basin where we can see some beautiful thrust setup folds and then on out into the San Luchia Bank where our very well constrained first motion study suggests there is pure thrusting going on.

Then, as you know, Ben, some of the work that you have done suggests that there is thrusting even in the old paleotrench at the base of the slope.

So if indeed these were flower structures, we find it hard to imagine that we are getting that kind of feature over such a wide area. Characteristically along strike-slip fault zones we find that those types of features are restricted to within two or three kilometers, such as people have shown in the Salten Sea area that indeed you get thrusting along the strike-slip fault zone, but it is a very narrow zone compared to the kind of relationship we are seeing here.

So that if it is related to some vertical fault just out of the area that we can see, which again I find it hard to believe, but if it were to be that case, it represents a whole new structural style that nobody has ever recognized before.

MR. PAGE: Thank you. Now I think it is worth noting that probably -- well, it is highly unlikely that there could be a great deal of strike slip on the faults that you represent. There could be some, but it seems to me that it would be mechanically very inefficient to have 10, 20 or 80 kilometers of strike-slip on those gently inclined grid plates, which is another reason for wondering whether or not there is a real connection between the system of faults you have represented with the faults with greater displacement farther north.

I note that in your profile, SM-1 I believe it was, which is about 26 kilometers from Diablo Canyon, that you had something like 1300 meters of dip slip separation at the Top Sisquoc formation that would be say one and a third kilometers of dip slip which is a fair amount.

But farther north closer to Diablo Canyon in your profile SM-4 it showed something like 150 meters of dip slip separation, which is likely the largest component of slip on that fault or at least it would approach the largest component of slip on that fault and it is very modest.

MR. CROUCH: I might point out, Ben, that when we put this paper together and we showed some of those horizons, we didn't expect quite as much scrutiny as we are getting.

(Laughter.)

We didn't pay as much attention perhaps in retrospect that we might have, especially since that time that I wrote that paper, I have been involved in several more wells and I have a little bit better control.

Again the amount of offset is based on how well you can tie from one side of the fault to the other and it is not always that easy. In some cases we show what is called the Top Sisquoc really represents in some cases on the eastern side of the fault we know as a very dramatic erosion surface. So that really the section is the base of the Foxin which may be time transgressive in part and it may be thickening in different areas so that it becomes somewhat difficult for us to get really -- in other words, we haven't addressed or tried to address necessarily the exact amount of vertical offset.

I do know one company, for example, that recently processed quite a spectacular line across the Hosgri fault and used a great deal of detail and could very clearly see on the order of seven to eight thousand feet of dip slip separation across the Hosgri out on the basement horizon, and that essentially was overthrusting and they are in fact planning on drilling a subthrust play along that fault.

MR. PAGE: Thank you. May I ask one final question. What do you think the resolving power of your seismic reflections are with respect to possible offsets of the seafloor?

MR. CROUCH: The data we are using again does not resolve the offsets that have been noted in previous work by Wagner and others where they argue that there is seafloor offset along the Hosgri.

Our data are such that we are looking at basically the deeper parts of this zone and we didn't try and address the shallower part, and I think we said that in our paper. Basically where we see it on our data, perhaps one could interpret it a little different than it has been interpreted in the past in that some cases there are gas bubbles above this zone of thrusting and we think that gas is basically related to, it is thermgenic gas related to trapping of the underlying Monterey formation, some of the leakage of that formation up into the section.

When you use high resolution records with say a half a second of penetration, you see the distrubed gas zone, especially in an area where you assume you have a major strike-slip fault, and then you naturally say well, there is the fault zone.

So we wanted to point that out, that perhaps in some cases people are looking at the wrong player. Now the difficulty here is that if you use high resolution data to try to and establish some of the younger age of the fault, then you miss the kinds of things we are seeing down deeper.

So basically you have to use both. You have to combine the shallow penetration records with the deeper penetration records so that you can get an overall picture of what the entire fault zone is really doing.

MR. OKRENT: Dr. Thompson.

MR. THOMPSON: I am very much impressed with the quality of your offshore seismic data, but in looking at the broad picture it seems to me that there are still some at least cautions, and this is something of a plea for verification. One might say to begin with that it is pretty hard to see vertical faults in seismic reflection data and that it is particularly hard to establish the amount of strike-slip offset from reflection data. I realize in talking to you that you have used drill data and other data to arrive at some of your conclusions.

A second thing that seems still a bit hanging to me, if we remember your vector diagram which has the San Andreas displacement and direction on it, and also a vector which I believe is taken from my work in the basin range, and then it is assumed that the rest of the closure must be a convergence perpendicular to the San Andreas fault.

But if one looks at the San Gregorio direction, that is not parallel to the San Andreas fault. Yet the Hosgri and Calivaris faults, for example, have quite a good direction that you need for closing that vector triangle. So that if there does happen to be 150 kilometers of strike-slip on the San Gregorio, that would provide an anternative way to help close that triangle.

Perhaps that summarizes enough for now, but there are similar questions like that.

MR. CROUCH: I agree with you that that is a way to possibly view it, especially the San Gregorio. The San Gregorio, interestingly enough, tends to have more oblique slip type solutions generated along it, at least from the published literature anyway and some of the recent work done by people such as Jerry Eaton at the USGS.

So the dilemma for me is again to tie the San Gregorio, if it indeed is a major right slip fault, to tie to the kinds of features we are seeing on the Hosgri. That is the dilemma that I have, and we didn't concentrate on trying to interpret the San Gregorio per se. But to say that perhaps, you know, if you want to tie the two together, then we would favor this sort of argument, especially as you go around big bend and then you are primarily influenced by the defection strike of the San Andreas. So you set up a thrusting sort of situation.

But then as you go northward and you change the general strike of the San Andreas, then you change into perhaps an oblique slip type situation and perhaps as the San Gregorio itself changes strike you could then accommodate a more pure strike slip situation.

The point, nevertheless, is that we don't feel one could simply tie in the San Gregorio/San Simeon/Hosgri fault in the way that has been done and make any adequate sense from the data we have.

MR. THOMPSON: Just to reinforce some of the things you have said, the stress directions in Califoria tend to be rather uniform. So, as you say, it is easy to understand large thrusting in the big bend area of the San Andreas or in the east/west part of the Santa Barbara region. But one would certain expect sheet stresses on the Hosgri direction rather than ---

MR. CROUCH: I might add one further point that is of interest to us from the work we have done, and that is that, you know, big bend is supposedly, according to most literature, have been in existence for at least the last five million years ever since the opening of the modern gulf.

So if one takes that into consideration, then you have essentially a set of conditions whereby if you bring the area west of the San Andreas back down, then you set up a condition. What we are seeing now is modern activity and what we suppose might have happened in the past, and I think it very nicely explains why some of the structures, say, for example, in the Santa Maria Basin, which we think are a little bit older than the Hosgri proper, we think the Hosgri cuts these, and it sets of a condition whereby perhaps other areas west of the present San Andreas and now north of the big bend were once opposite big bend and had thrusting set up say in the direction of what we now see off Pt. Conception and Pt. Arguello, which is again more westerly than say the Hosgri.

So that we have basically through time a whole set of different structures going in somewhat different directions that are cross-cutting each other and so it makes for a very difficult interpretive job to sort those out, because it is only happening in the last five million years. You know the difficulty of trying to age date faults that are within five million years of each other and it is not easy.

So some of these faults like, for example, what we call the Orcutt-Casmalia fault zone, which we think is a primary thrust, strikes into the Hosgri itself, and it has been argued by some people that it is right lateral and then joins with the Hosgri.

Well, we would argue that that fault was primarily formed when the Santa Maria Basin onshore was opposite big bend and then it has the proper direction for that kind of force, and then later the Hosgri and these other faults we are seeing in the offshore were operating across some of those trends and we think the seismic data bear that out pretty strongly. We think we can see an age difference in some of those relationships.

I might further add that I have looked at seismic data all the way up the margin and we think that we can see similar thrusts and fold type arrangements in areas as far north as Pt. Arena Basin, but they appear to be in older sections that we a . see in the offsore Santa Maria Basin. So we set up a way of producing thrust and fold type arrangements over quite a span of time.

MR. OKRENT: Dr. Maxwell.

MR. MAXWELL: I would like to take you to that slide in which you showed the DeComa being the rather interesting and potentially important region.

MR. CROUCH: That is my most speculative slide, I might add.

(Laughter.)

MR. MAXWELL: I don't know whether you have seen it or not, but could you show that?

(Slide.)

MR. MAXWELL: I note that your courage failed you when it came to the San Andreas and that you carry that down through that zone as a vertical line.

MR. CROUCH: No, we don't.

MR. MAXWELL: Do you stop it?

MR. CROUCH: What we show there, and maybe you couldn't see it very well, but what we show at the San Andreas is the depth limit of earthquakes, and that, as you probably know, is on the order of less than 15 kilometers. In fact, that forms to me a very strong part of our whole picture. That is true not only on the San Andreas, but it is also true, according to Gothrup anyway, throughout the Central Coast Ranges. I know of nobody that has found any earthquake solutions at depths deeper than 15 kilometers.

Our reason for picking that particular boundary was in part due to the depth limit of earthquakes. We feel like since there are no earthquakes generated at about that level that essentially we are sliding along some kind of a seismic zone.

We argue that if one looks at the solutions of refraction data in the coast ranges and what-not, which shows the Moho generally a depth of about say 23 kilometers, then you assume a normal oceanographic crustal thickness of about 10 kilometers. Then we are left with the top of the old oceananic slab at roughly say 13 kilometers.

I am especially intrigued by looking at some of the rocks say on Pt. Sal. Some of the very highly altered upper part of the otheolite is primarily serpentine and very highly altered metavolcanics that we think would provide a considerably good glide surface to do all this on. So that is our basic premise.

We also add into this diagram a few other observations by other people such as a strong set of reflectors beneath the Gavlin Range at about nine kilometers, and also, as you know, Carl Wentworth and others' work in from the Coalinga quake which argues for major thrusting in an area that was previously considered as being folded and faulted by strike-slip faulting and now they argue that there is major thrusting occuring east of the San Andreas.

So we feel like the San Andreas is not the boundary we really want to hang our hat on. We at least have to go further than the Coalinga area and just how far one wants to go to the east is an intriguing question.

MR. MAXWELL: If I remember correctly, Tom McEverly had a line across the San Andreas further north and thought he could detect a surface passing right under the San Andreas at about 12 kilometers. Is my recollection correct on that?

MR. CROUCH: I think so, yes.

MR. MAXWELL: Well, in the south, further south than we are here, the Parkfield line that Cocarp shot, it is rather lousy data, but if you are biased you can see discontinuities on the two sides to get down to about 12 kilometers and then something going across about at that depth also.

MR. CROUCH: Well, I have looked at some of the Cocarp lines there and I am spoiled by the data that I looked at. So I am reluctant to use that data. My feeling is it really requires some new lines, and we have suggested to Cocarp to come out and shoot this area again and that is being considered. Another group called Calcrust may also be considering it. But it begs the question of whether or not we can see this zone at depth.

We think there are at least some hints of that anyway in places, although it is certainly not by any means well established.

MR. MAXWELL: I think it is very wise not to publish that data.

(Laughter.)

MR. MAXWELL: One other thing. Back on the slide on which you showed the trace of the Hosgri and the onshore faults. I think it is a few more back from this.

(Previous slide shown.)

I don't have any trouble philosophically with your straight thrust fault for the Hosgri, but the onshore pattern looks like sort of the series of faults like one would expect in a thrusting sequence. In fact, in one of your cross-sections you also show at least two branches of this Hosgri. I wonder if the Hosgri itself, if we knew better, wouldn't look more like some of that onshore rather than a straight line?

MR. CROUCH: Well, indeed, I tried to make the point and I have sort of tried to depict the Hosgri as shown by published literature rather than depart from that in this diagram. But I have done detailed mapping on the Hosgri where I have had enough control to have some pretty strong confidence, at least in small portions, as to my ties both on the upside and the downside of the Hosgri, and in those interpretations we do indeed get quite a bit more curvature on the Hosgri than is shown.

But again, if you assume it is a strike-slip fault with a vertical trace and you use high resolution records, you will indeed get a very straight trace. In fact, the gas zone we mentioned extends in one place for on the order of two to three miles in a very linear map view and it doesn't coincide with the thrusting at all and that has been mapped as the Hosgri trace.

MR. MAXWELL: How deep were you able to follow the Hosgri?

MR. CROUCH: Well, we see it soling out in a number of places within the basement of the offshore Santa Maria Basin. Well, let's put it this way. We see a whole number of faults across the entire basin soling out at what we think is an ocheolite sequence in an older piece of oceanic crust at the basement, and generally speaking that is on the order of mainly about 8,000 feet that it tends to start soling out, and that seems to be the general nature of what we are seeing, although again, as we go east from the Hosgri itself we don't have the data to really show I think in a conclusive way just exactly wht the dip of that fault zone might be. It could indeed steepen, as you go east of the zone, steepen somewhat and we just really don't have the data to say.

We do have enough strike lines, I might add, and I showed you one. We have other strike lines that pick up that zone to about I think as far east as about three kilometers east of the zone, three to five kilometers where we are still seeing the fault within the upper three seconds.

MR. MAXWELL: So you really can't trace it down to your Decomont.

MR. CROUCH: No, we can't. I simply assume that that had to do that in order to accommodate that much shortening across the margin. There had to be some kind of master sole, let's say, to this whole system if we assume there is that much shortening and if those onshore faults indeed do flatten at depth as well. But we have no line whatsoever that shows a tie from the surface down to that Decomont, no.

MR. MAXWELL: Thank you.

MR. CROUCH: I wish I did though.

(Laughter.)

MR. PAGE: Mr. Chairman, may I make another comment. I have been trying to recollect what I have seen at San Simeon where a fault zone, which we formerly thought was the Hosgri fault zone, intersects the shore and there are some relations there that still make it difficult to connect the faults there with the ones that have just been described by Dr. Crouch.

For one thing there is a mismatch of rocks on the two sides of the fault at San Simeon such that the rocks on the left side are not seen on land on the right side, and the Lasvea formation which appears on the left side is quite different from the Lasvea formation near the Town of San Luis Obispo on the east side. This would seem to require probably some strike slip, perhaps a good deal.

Further, some of the scares at San Simeon face landward, face eastward as though the ocean side had risen, and there is displaced marine terrace material, as you know with the west side up.

Now if those faults were reverse faults, the side that is up in actuality is the side that should be down in terms of reverse faulting. The west side is stratographically displaced downward. All these things make it hard to hook on that zone to a zone that picture in your offshore profiles.

So there is some rather drastic discontinuity in our fault zones offshore that I guess will have to be resolved. I think that is one of the big problems that has arisen as a result of Dr. Crouch's paper.

MR. CROUCH: I might make several comments regarding that, Ben.

First of all, the basis for offset of the Hosgri itself is that the rocks near Pt. Sal, and I will point that out on the map here, right here, the rocks there have been suggested to be again repeated at Sam Simeon west of the Hosgri, and indeed the rocks are quite different west of the Hosgri zone as mapped at San Simeon.

They are quite different than they are on the east side.

I think one of the major difficulties that people had again has been the lack of data. If you will notice, along the entire San Gregorio system the fault comes onshore in basically three places, very small regions of the overall fault zone. In fact, estimate about 85 fault zone is under the ocean.

So that people such as Clarence Hall who argue for 80 kilometers of offset on the Hosgri have simply had to say that west of Pt. Sal they have had to make an assumption about what the rocks are. Since that time there has been a number of wells drilled by oil companies since OCS-53 that have penetrated the entire section southwest of Pt. Sal and west of the Hosgri as has been mapped.

Again those data are proprietary, but I believe for purposes such as this that information could be discussed or looked at from the individual oil companies that have drilled those wells.

But I can say just for the record here that indeed the rocks southwest of Pt. Sal and west of the Hosgri proper are very similar to some of the section we see on the shore in the Pt. Sal region and that any offset we see in stratigraphy in areas such as San Simeon could also be very easily a result of thrusting, and especially if we are seeing thrusting on the order that we think we are seeing it in the offshore on the order of several kilometers or more and we can then juxtapose quite different sections in small areas such as San Simeon.

I might also add that we don't believe, at least from my work, I don't even think the Hosgri is represented at San Simeon. I think it goes around San Simeon Point and that the faults exposed at San Simeon are actually subsidiary faults to what I would consider the major portion of that zone.

Thirdly, although we don't show it, we sort of stress the northwest over southeast direction of thrusting. In the offshore Santa Maria Basin almost as frequently we see southwest over northeast type style thrusting back in the other direction. So that thrusting such as that you described at San Simeon could very well be thrusting in the opposite direction.

In other words, because there are conversions across the margins and horizontal compression doesn't mean we have to have a preferential direction towards the thrusting, although we seem to get mainly northeast dipping thrusts, but we see an awful lot of thrusts that are dipping towards the southwest and that are also parallel to the entire system. It is a very common aspect.

For example, some of the structures just on the west of the Hosgri have a remarkable thrusting back in the other direction.

Does that answer your question?

MR. PAGE: I don't think we can answer it at this point.

(Laughter.)

MR. CROUCH: I don't think so either.

MR. OKRENT: Dr. Thompson.

MR. THOMPSON: Does any of your controlled seismic or otherwise go far enough west of the Hosgri to preclude the possibility that the zone of large strike slip, if it be such as the San Gregorio, is present out there to the west perhaps where you have your old paleosubduction line?

MR. CROUCH: No. I think you will find this true of almost all proprietary seismic data that has been shot by industry. Basically the western margin of the offshore Santa Maria Basin is considered to be approximately the San Lucnia Bank for economic reasons. So there are very few, in fact I know of no lines that go all the way out perhaps by some company such as Gulf who may have run some regional lines perhaps that continue westward. But I know of no industry lines that continue out over the old paleotrench zone myself, but there may indeed be some that exist, but there won't be the kind of detailed data that we are looking at here. There will be more say single, widely spaced regional sort of lines.

MR. THOMPSON: It does seem to me that that is a candidate location for possible large strike slip.

MR. CROUCH: Well, I know myself, and I have looked at the trench further to the south and Dave McCullough and Ben have discussed some of the aspects of the trench in this area, and I have yet to see anything that would suggest to me that there are major strikes of faulting associated with continental margin, that is, there is no en echelon folding and faulting in the trench sediments themselves that I have seen.

I looked very hard for it in an area off of the Vorland called the Patent Escarpment and the trench at the base of that. Unless you want to do it on the escarpment itself it is very difficult to do. Of course, the escarpment is almost unresolvable because of the very steep angle there.

MR. THOMPSON: It may be hard to get evidence, but I threw it out as certainly a possible place to put it through.

Finally, one other comment. Having been very much interested in this zone that is 7 to 15 kilometers deep and perhaps with our paper on the Gavlin block that you are referring to with the subhorizontal reflectors, that is all over the place and I think it probably represents a brittle to ductile transition or a decoupling zone of some kind. The Cocorp lines across the complementary fault of the San Andreas to the south show a very good reflector of 10 kilometers going right across there. So these look like they are upper plates that are sliding on a zone of a seismic detachment down there, and the only thing I would question about it is the suggestion on your diagram that it is all moving in a nice regular diretion out toward the coast.

I think it is more likely just a zone of adjustment and decoupling of all of these upper blocks from the lower part.

MR. CROUCH: I would tend to agree with you. We simply were trying to set up sort of general view of decoupling rather than really pay attention to the nitty-grit y details of the whole thing. Again, in retrospect, the idea of some of these thrusts we are seeing where we are getting at least thrusts dipping towards the southwest suggest that we are looking at horizontal shortening in a general way. But again, the majority of thrusts that I see tend to dip back towards the northeast.

MR. THOMPSON: The heat flow evidence is an additional strong line of evidence that such a zone exists.

MR. OKRENT: I am going to suggest that we take about a 15-minute break now. Dr. Crouch has been standing quite a while. When we come back we will see if there are further questions for him, or we will take sections of what was on the agenda that relate to what I will call the geology and geophysics before we get into what this all means and so forth.

So if either the staff or USGS or PG&E have comments on what I will call the geology, we will take them next. So think about that during the break.

Okay, 15 minutes or so and no many more.

(Recess.)

MR. OKRENT: If we can, we will reconvene.

(Pause.)

Well, let me, while we are waiting for one or two people to return to the meeting room, ask if the NRC staff has comments on the geological aspects that we just heard.

MR. JACKSON: We spoke with PG&E and basically we have no quarrel with most of the things that Dr. Crouch presented. PG&E did have some interpretations of the geology. We thought what we would do is hold our presentation, which mainly goes to the question that you posed to us about say some bounding implications of what we have seen, and I think we would like to focus in that direction.

MR. OKRENT: That is what the subcommittee members want to hear from you and we are holding that for the moment then.

MR. JACKSON: I hope you are not disappointed.

MR. OKRENT: I might ask PG&E to then at this time tell us what comments they have on the geology, and I hope that Dr. Savio can tear the media away from Dr. Crouch.

MR. BRAND: Thank you, Dr. Okrent.

By way of background PG&E first became aware of this paper in mid-March when it was brought to our attention by one of our consultants, Mr. Hamilton. At this time the paper was a preprint of a paper that had been prepared for presentation to the American Association of Petroleum Geologists at their meeting last month.

As we have heard this afternoon, the authors feel that certain geologic interpretations in the offshore area may need to be revised to show a higher contribution from compressional tectonics than previously thought.

As with any new information, it is difficult to determine the acceptance of these findings until there has been substantial peer review, and that process is underway even this afternoon.

Nevertheless, we believe the Crouch

interpretation is generally consistent with the interpretations reviewed previously in connection with the Diablo Canyon licensing studies and that these new interpretations do not adversely affect the safety of the plant.

With me here today is Mr. Doug Hamilton of Earth Science Associates and Dr. Stewart Smith. Both Mr. Hamilton and Dr. Smith have previously consulted on Diablo Canyon and they will be making a further presentation right here and I would ask Mr. Hamilton to begin.

MR. OKRENT: Fine.

MR. HAMILTON: Thank you, Dr. Okrent.

I will take the liberty, if I may, of largely reading my commentary which was prepared on behalf of PG&E relating to the paper "Post-Miocene Compressional Tectonics Along the Central California Margin" by Drs. Crouch, Backman and Shay.

PG&E has been asked to provide comments on the significance of the article "Post-Miocene Compressional Tectonics Along the Central California Margin" by Crouch, Bachman and Shay" relative to existing assessments of the seismic capability of the seismic fault in the region of the Diablo Canyon power plant.

In order to do this, it is necesary to briefly review the geologic interpretation that provided one of the bases for the findings of the ASLB hearings of 1978-79 leading to the Board's recomendation that the plant be licensed.

As of the 1978 ASLB hearing for Diablo Canyon Units 1 and 2, my firm, Earth Sciences Associates, had developed a detailed geologic map of the coastal region between Pt. Arguello on the south and San Sameon on the north and had also proposed a regional tectonic framework that seemed to provide a tectonic rationale for the conditions observed in the coastal study area.

(Slide.)

The geologic map is reproduced in part in Slide 1 showing Figure 35 from the direct testimony developed by R. J. Jahns and myself for the 1978 ASLB hearing. I will return to a simplified version of this map later on in this discussion.

The geologic map is reproduced in part. Let me first just outline the major features on this map which was part of the direct testimony for the ASLB hearing.

This map covers a portion of the Hosgri fault as we had mapped it from Pt. Sal up to north of Esterio Bay. The coastline is shown by a line that I have just indicated. The Diablo Canyon power plant is located at this point on the coastline and the fault as we had mapped it is indicated by this zone of various faults, both buried concealed faults and faults interpreted as being near the surface.

The major rock units that were identified at that time included the quite young sediments that fill parts of the basin colored in yellow here, an older section corresponding generally to lower pliocene and miocene rocks shown in blue and extending both in the offshore and in the onshore, and then the basement rock sequence which is shown in darker colors of purple near Pt. Sal and in dark green along the coastline south of the power plant and offshore from it and across the end of faults near San Luis Obispo and inland.

The next slide, please.

(Slide.)

The major faults and directions of crustal movement that define the regional tectonic framework around Diablo Canyon were shown on this map which was developed by R. J. Jahns for the 1975 FSAR supplement and presented as Figure 8 in our direct testimony at the 1978 hearings.

Note the coastal region with the Hosgriand related faults as these are shown as this line of faults. The arrows indicate the pattern of crustal movement with progressively increasing crustal extension occurring north of the Garlock and northeast of the San Andreas big bend.

That is in this area here where the crust appears to be extending from east of the Sierra and creating a deflection in the San Andreas recognized as the big bend and the zone of compression existing in the crust west of the big bend indicated by this arrow here and apparently creating a particularly high area of compression north of the Transverse Ranges along the transition as the Southern Coast Ranges and offshore basin faults splay into the Western Transverse Ranges faults.

(Slide.)

For the purpose of relating the previous work done for Diablo Canyon licensing to the study by Crouch and others, it is useful to first refer to their map of the Southern Coast Ranges. Their map which you have seen previously, and which I will reproduce, covers roughly the central area of this map here.

The next slide, please.

(Slide.)

This is an uncolored version of the same map that you were shown during Dr. Crouch's talk. Note the Hosgri and offshore Lompoc faults. The Hosgri is shown as this line of faults and the offshore Lompoc is the one farther west of it offshore from Perisma Point.

These are mapped essenially as on early maps, including those submitted in 1975 and 1978 as part of the

Diablo Canyon licensing material.

To go beyond the comparison showing that the regional fault pattern and tectonic interpretation given in Crouch and others is generally similar to the ones previously submitted in connection with the Diablo Canyon licensing studies, we have go on to make an initial review of specific elements of the Crouch and others' interpretation of the configuration of the Hosgri fault in the subsurface as opposed to our previous understanding of the Hosgri zone between Estero Bay and its apparent southerly termination south of Pt. Sal.

The next slide, please.

(Slide.)

Our review focused especially on the following items.

First, on the location of the Hosgri fault between Estero Bay and Pt. Sal.

Secondly, the pattern at the surface and at depth of the faults that make up the Hosgri zone.

Third, the evidence for age and quaternary expressions of tectonism along the Hosgri zone, and these items then I will discuss in order.

First, the location of the Hosgri fault between Estero Bay and Pt. Sal.

For the purpose of illustrating the major points of this review, I have prepared a simplified version of the geological map of the coastal region I showed earlier.

On this map I have shown the pattern of faults as we presented it in 1978. The faults are indicated by the dark lines here and the Hosgri zone in particular is the line I have indicated, and also the locations of key seismic reflection lines pertinent to the present discussion. These include the three lines across the Hosgri zone corresponding to Figures 5, 6 and 8 in Crouch and others. These are as lines SM-1, SM-2 and SM-4 and five lines across the zone at points within and northward from the part of the zone that was illustrated by Crouch and others, and these are indicated as the lines in light blue identified with names from the vendor of those lines which is Consolidated Geotechnics.

One of these lines, as you can see, ties with Crouch's line SM-4 and the others lie successively north, including an area opposite the power plant site.

Now a key element in our review has been the establishment of a correlation between the features illustrated by Crouch and others and features recognizable on the lines in our existing data base. This has been done through a comparison of Crouch and others drawing of their line SM-4, this line, and our line C76-584, this line.

The major fault and stratigraphic features identified in the line SM-4 drawing, which you have seen previously today, can be recognized on the CGT line, although I would not claim that they could be interpreted in detail on that line, especially in its unmigrated form without the Crouch and others drawing for a guide.

In any case, this comparison establishes a location and what we would call the signature of the Hosgri and allows us to search for it on successive CGT lines across the zone. The result of this exercise is shown on the plot on the same fault map on the next slide.

(Slide.)

On this plot each colored interval along a seismic line track corresponds to the interval over the front of the east-side block over the Hosgri. The colored intervals are those intervals shown in orange on Crouch's lines and on the CGT lines.

This front everywhere underlies the steeply dipping faults of small, apparently mostly normal displacement that were plotted during our original study in 1974.

Thus, our review seems to indicate that the geographic location of the Hosgri fault between Estero Bay and the vicinity of Pt. Sal is essentially as shown on our existing map.

Now Dr. Crouch's map did not extend in detailed form much north of is northernmost illustrative line SM-4. I think if it had we would see that his map of the Hosgri would come up essentially like this and tie back in to the main zone that we have mapped. It is this interval here that he described as the main Hosgri zone where the gas charged sediments were shown as a fault, but the pattern then seems to wrap back into the zone as mapped offshore from the power plant site area and northward into Estero Bay.

A second item of our review then is the pattern at the surface and at depth of the faults that make up the Hosgri zone.

With regard to the second item, the pattern at the surface and at depth of the faults that make up the Hosgri zone, we again turn to a comparison between the Crouch and others line drawings and the features visible on both single and multi-channel lines in our presently available data base.

(Slide.)

On the Crouch and others examples we see steeply dipping faults of small displacement from around one second nearly to the surface. This would be the faults in this area, those up in this area and the faults in the upper part of the sections in each of the three cross-lines that he showed as illustrations. And we see also in the line drawings well defined reverse and thrust faults between about one and two seconds depth. These are the faults then that bend over and go to shallower dips and extend down dip from the near surface zone of steeply dipping faults.

In comparing this patter with the various examples of seismic data illustrated in the Diablo Canyon licensing proceedings FSAR and direct testimony, we find the records to show a similar pattern of steeply dipping faults, which typically are clearly defined only in about the upper second of record in the single channel records.

In the multi-channel records a reverse fault is evident, as illustrated in Figures 36 and 37 of our 1978 direct testimony.

Figure 37, which is this line, is a direct reproduction of CGT line C76-80 which is opposite the power plant site area. On this the high-angle faults in the upper second are shown rather indistinctly in this general area and the reverse fault is shown in this unmigrated section as this feature here.

(Slide)

The next slide was Figure 36 of the direct testimony and that is a one-to-one cross-section based on a migrated version of a proprietary CDP line which was filed as a supplement to the FSAR Appendix E in 1975. And on this line again we show the high-angle faults in the near surface and the reverse faults at depth line out seaward from those essentially in the same fashion as are shown in the more detailed line drawings that Crouch and others present.

Now with regard to the question of the dip in the Hosgri, we draw on three elements of data.

First, the line drawings of seismic lines across the Hosgri zone provided in Crouch and others.

Second, the cross-section baed on the migrated lines opposte Diablo Canyon as seen in this illustration here, and here we are talking about this particular line.

Third, the location and the orientation of depth of focal mechanisms of earthquakes that are reported as recorded in the region during the last several years.

The next slide, please.

(Slide.)

The three line drawings in Crouch and others show dips on the farthest down-dip indication of the Hosgri of about 17, 20 and 26 degrees simply measured with a protractor on their lines. That would be from this line, this line and this line going from south to north.

The latter two drawings though are from lines that are oriented about 45 degrees to the strike of the

Hosgri zone and so record apparent rather than true dips. protractor, would be about 26 and 35 degrees, and those are the numbers that are indicated on this drawing here, 17 degrees, 26 degrees and 35 degrees.

This shows the Hosgri steepening from 17 to 35 degrees dip over a distance of about 10 kilometer going north. Nineteen kilometers north of line SM-4 along the Hosgri trend, our cross-section based on the migrated CDP line, which is this dark blue line here, showed the buried reverse fault present below about one kilometer depth to dip 40 degrees northeast. A down-dip projection at that angle would place the fault plane 10 kilometers beneath the plant site and it would pass at a minimum distance, which is normal to the fault plane that is, of about six and a half kilometers from the plant site.

Now a further line of evidence regarding the likely configuration of the Hosgri fault at depth may be inferred from the data of earthquake records. Earthquakes have been recorded well enough to permit location, depth and focal mechanism studies at least in three areas pertinent to this study as well as at other locations further north along the San Gregorio/Hosgri trend.

The characteristics of several of these earthquakes will be described in a forthcoming article by Jerry Eaton of the USGS. Three earthquakes are discussed in the USGS open file report 81-44 by Lindh and others. These are cited here.

The southernmost earthquake which occurred in May 1980 and was centered a few kilometers west of Pt. Sal, or in about this area here, had a focus at about nine kilometers depth and a mechanism interpreted as probably involving southwestward thrust or reverse movement on a fault plane dipping 25 to 35 degrees northeast. Such a fault would lie well beneath the Hosgri, but might be parallel to it and would be subject and would project toward the surface somewhere west of the Hosgri, that is the movement was on a fault dipping into the plane of the map something as I am indicating with the pointer and projecting into the surface somewhere out in this area.

The second earthquake was reported as occurring virtually on the trace of the Hosgri zone in Estero Bay approximately at this location at a depth of 2.8 kilometers. The mechanism was analyzed as strike slip or normal but not reverse. Such an earthquake could have originated as a minor normal or right oblique normal adjustment on a steeply dipping break in the upper part of the Hosgri zone. If this was the case, however, it would indicate a steep dip to at least 2.8 kilometers depth, assuming accuracy of the analysis on that earthquake.

The third area of recorded earthquakes lies north and northwest of San Simeon, some 70 kilometers north of Diablo Canyon. There earthquakes have occurred at depths of around seven kilometers with focal mechanisms analyzed as indicating reverse to right oblique reverse movement on fault planes dipping 45 to 55 degrees east or northeast. the fault planes indicated by the focal mechanisms project upward toward faults of the San Simeon zone that are mapped at the surface.

Taken altogether, the evidence of earthquake focal mechanisms suggests that:

1. Fault planes probably associated with the Hosgri zone extend down dip to depths of at least seven to nine kilomters, approximately at the angle of dip they assume between one and two kilometers of depth.

2. The Hosgri zone steepens progressively from near Pt. Sal northward to near San Simeon, and motion along it changes from nearly pure reverse on the south to right oblique reverse, or that is reverse with a significant component of right lateral strike slip, near San Simeon.

The August 1980 earthquake in Estero Bay may indicate that some component of right lateral movement occurs along the zone at a point roughly midway between Pt. Sal and San Simeon.

Now the third point of our review was concerned with evidence for age and quarternary expressions of techtonism along the Hosgri zone.

With regard to the third item we received, evidence for age and late quaternary expressions of tectonism along the Hosgri zone, I would like to first return to the line drawings presented by presented by Crouch and others.

(Slide.)

The most northerly of these drawings represents the cumulative late neogene displacement along the Hosgri proper as little more than 100 meters, that being the offset of this horizon at both the Top Sisquoc and the late Miocene or near Top Miocene. If you take the scaling of those drawings, you come up with a figure that is not too much more than 100 meters implying a very low average rate of slip during something approaching the last five million years.

Farther south the Top Sisquoc horizon, representing a time of perhaps in the order of two and a half or three million years, is displaced about 100 meters, but an underlying near Top Miocene horizon was displaced some 1200 meters or I think perhaps Dr. Page measured it at 1300 meters.

Since the deeper older horizon is somewhat greater than five million years in age, an early higher rate of movement is indicated for this part of the Hosgri.

All three of the Crouch and others lines show the Hosgri and other related faults dying out upsection before reaching the present sea floor, but the authors caution that other lines of evidence, including uplift and earthquakes, suggest continued activity along the zone.

The locations where there is reasonably good evidence for post Wisconsinian, our latest quaternary fault displacement, or other deformation near or at the sea floor are indicated on our fault map in Slide 12.

(Slide.)

These indictations are shown on a further overlay of redish lines overlaying the faults or other features and they are located at this point north of Pt. Boushon and along a length zone of the Hosgri fault opposite Pt. Sal and north of Purisima Point, a single point on what we call the Purisima trend, and both the surface offsets and distinct surface upworking on the offshore Lompoc structure.

The northerly of these features on the Hosgri north of Pt. Boushon is a landward side down-step of about one and a half meters height and less than one kilometer strike length in the sea floor along a fault bonding the seaward side of a graben within the Hosgri zone and

Estero Bay; a zone of subsea floor west down displacements of as much as two a half meters that extends into but not through the post-Wisconsinian surficial deposits ove rthe Hosgri zone opposite Pt. Sal, that is this longer zone shown here; a single step in the base of the surficial section over the Purisima zone, this feature; and the prominent upwarp and adjacent minor surface displacments of the offshore Lompoc zone, here.

Overall, late quaternary deformation is scattered and of small magnitude along the zone much north of Pt. Sal. It is fairly widespread and more extensively developed along faults and some associated folds opposite Pt. Sal and Purisima Point.

The limited extent of the late quaternary deformation along the Hosgri zone much north of Pt. Sal suggests that this part of the zone has not been characterized by earthquakes large enough to create recognizable surface displacements or warping beyond the one east-down step recognized to date, and possibly others like it, but not yet identified. Those would include some similar features that have been suggested in Holly Wagner's earlier interpretation.

By way of comparison, the magnitude six and a half San Fernando earthquake, which was oblique thrust mechanism, was accompanied by recognizable surface scraping over a strike distance of about 15 kilometers with maximum scrap heights of about one meter.

I wil now summarize our initial review of the date and interpretations in Crouch and others as follows:

First, the Hosgri fault zone, if interpreted according to the form indicated in the illustrations in Crouch and others, lies generally beneath the zone of faults identified as the Hosgri fault zone and shown on maps previously submitted in connection with the Diablo Canyon licensing proceeding.

Second, the dip of the Hosgri, as it is characterized in Crouch and others, steepens northward from 17 degrees to 35 degrees among the examples they show. Our previous interpretation showed the reverse fault that apparently corresponds to this fault opposite Diablo Canyon to dip eastward at about 40 degrees.

Data from earthquake focal mechanisms appear consonant with an interpretation that the Hosgri continues down dip at about the same angle it assumes below about one kilometer depth, and this continuation extends at least into the seismogenic depth zone typical of Southern California, or Central California, which would be in the order in this case of six to nine kilometers.

A down dip projection of the reverse fault recognized opposite the plant site would pass beneath the plant at about 10 kilometers depth.

Our third element of our review is summarized to say that expressions of late quaternary movement associated with the Hosgri fault suggest that earthquakes large enough to create extensive surface displacements have not characterized the part of the zone between Pt. Sal and Estero Bay for at least the last 10,000 to 15,000 years, the age of the sea floor and the shallow sea floor covering that area.

Fourth, the interpretation presented in Crouch and others is generally consistent with data and interpretations submitted and reviewed previously in connection with the licensing studies for Diablo Canyon.

The concept of large magnitude earthquakes originating during geologically recent time, either through many tons of kilometers or through large-scale thrust movements alonf the Hosgri zone in the region of Diablo Canyon, however, does not appear consistent with the available geologic data, including the interpretation presented in Crouch and others in 1984.

That concludes the commentary that we have prepared on the Crouch and others paper.

MR. OKRENT: Dr. Maxwell.

MR. MAXWELL: Are there copies of his presentation available?

MR. OKRENT: Well, there is going to be a transcript of the subcommittee's meeting and we can certainly have a copy of this section sent to you. That is no problem. I don't know whether Dr. Hamilton has copies.

MR. HAMILTON: We can prepare copies of the slides and prepare copies of the text is requested.

MR. MAXWELL: We would prefer the text.

MR. OKRENT: Dr. Kerr.

MR. KERR: Is it your position or that of the licensee that the Hosgri fault is considered a capable fault as it is used in regulatory parlance?

MR. HAMILTON: I believe it would be so considered.

MR. OKRENT: Are there other questions for Dr Hamilton?

(No response.)

MR. OKRENT: Would you mind repeating that last conclusion. I was temporarily diverted trying to write a note down, just the very last conclusion.

MR. HAMILTON: Yes. That conclusion was as follows. The interpretation presented in Crouch and others is generally consistent with data and interpretations submitted and reviewed previously in connection with the licensing studies for Diablo Canyon.

The concept of large magnitude earthquakes originating during geologically recent times either through many tens of kilometers rupture or through large-scale thrust movements along the Hosgri fault in the region of Diablo Canyon, however, does not appear consistent with the available geologic data, including the interpretation presented in Crouch and others 1984.

MR. OKRENT: Thank you.

MR. KERR: Then in the language of a non-seismologist that means it is capable but not very?

MR. HAMILTON: I think that is a good way to phrase it, yes.

(Laughter.)

MR. HAMILTON: That is, I think the evidence if fairly clear that there are earthquakes up to some level which does not create extensive surface deformation, especially given Dr. Crouch's interpretation which would imply which would imply some vertical warping or maybe the development of secondary features such as the one you see in Estero Bay.

The Hosgri fault does seem to be capable of producing scattered features like that, but it does not appear to have produced any consistent pattern of them north of Pt. Sal and in the area that the study has been concentrated in.

MR. OKRENT: Dr. Page.

MR. PAGE: Doug, could you repeat the hypercentral depths that were reported in the USGS open file or paper that you cited? I think it was Jerry Eaton's paper.

MR. HAMILTON: Yes. Well, I am trying to not cite Jerry Eaton's paper because it is not yet released. The hypercentral depth that has been determined for the Pt. Sal earthquake was about nine kilometers. I believe that Rob Cockerham takes a figure of about 8.9 kilometers and Jerry Eaton a little bit more, but it centers around nine kilometers.

The small event that was picked up in 1980 in Estero Bay was solved by Lindt and others as a depth of 2.8 kilometers. In their analysis of that they indicted that although the data were not really good enough to permit a complete reliable solution, that they felt that a reverse mechanism was precluded and that either a strike-slip or a normal solution was permitted.

The earthquakes north of San Simeon were indicated I believe as being around 7 kilometers in depth.

MR. OKRENT: Any other questions for Dr. Hamilton at this time?

(No response.)

MR. OKRENT: If not, let's go on to Dr. Smith. I think we were told he is next.

MR. SMITH: I am pleased to see the focus of interest in seismology shift back towards the underlying scientific issues.

The questions raised by Dr. Crouch provide a much needed opportunity to ventilate the seismological framework or the seismological dogma under which we have been working and open this framework up to re-examination.

The framework I speak of is the set of geologic and seismic assumptions which have been imposed upon PG&E. It differed significantly from the recommendations made to PG&E by their consultants, and in many ways this framework seems to me anyway to contradict the available scientific evidence.

Briefly, the framework I am talking about contained the following assumptions.

The Hosgri fault is a major active strike-slip fault 140 kilometers in length and possibly connected through the San Gregorio fault to the San Andreas and it functions as a secondary plate boundary.

Two, the Hosgri may have had significant horizontal strike slip motion in late quaternary time because such horizontal motion might have gone undetected in the marine seismic reflection profiles available.

Three, the historic earthquake for the Hosgri fault must be taken as the magnitude 7.3 1973 Lompoc earthquake.

I hope that all of these assumptions will be carefully reconsidered in the light of new data and developments in earth sciences and, in fact, what is happening here today is an excellent example that illustrates this process is already well underway.

Dr. Crouch's picture of the Hosgri is certainly more consistent with my view of the offshore fault situation than is the "plate boundary viewpoint" which has been imposed upon us.

The primacy of vertical slip on the Hosgri and other offshore faults called for by Dr. Crouch essentially rules out the regular occurrence of large earthquakes like 1927 on the Hosgri over the past few tens of thousands of years.

Finally, this is not the last tectonic interpretation we will hear about for coastal California. No geologic hypothesis can ever be considered to be absolutely complete or correct, nor need it be so considered since the link between observed geology and future earthquakes can never be exact.

In May of 1976 I made the following statement to this same committee. "The basic suppositions that we used in specifying the earthquake potential for this region essentially included in them the possibility of finding out something new about the earth in the years to come." And I think that that is as true today as it was ten years ago. "I doubt very much that we have the final picture about the structure of the earth in this vicinity."

I would essentially hold that view some eight years later today.

Thank you.

MR. KERR: I am glad to know that seismologists can also talk in terms of tens of years. I had gotten the impression earlier that anything smaller than a half million years was inconsequential.

(Laughter.)

MR. SMITH: That was in tens of thousands of years.

(Laughter.)

MR. KERR: I am referring to the eight and ten years ago when you talked to the committee.

(Laughter.)

MR. OKRENT: Okay. I think that covers the PG&E presentation.

MR. BRAND: Yes, it does, Dr. Okrent.

MR. OKRENT: I wonder if USGS has any comments on the geological/seismological aspects.

MR. PERKINS: I am Dave Perkins with the

Geological Survey.

The basis on which the USGS is here today is largely as an observer because we have not formally been charged with acting as reviewers in this matter, but rather are being asked to be kept up to date by attending these meetings and preparing what we would propose to do in the future when charged.

There has been some advice given to the Commission by Jim Devine largely on the basis of a brief review of the paper and his extensive experience in the previous review.

I am reminded that there was a great deal of reluctance amongst fellow geologists several years ago to connect up various pieces of the Hosgri and other associated faults, but they felt constrained to do so when the situation of review arose in order to provide some conservatism.

So I am sure that they are going to be greatly relieved that this is no longer a requirement and that is possible to consider much shorter segments of this Hosgri fault system as perhaps characteristic of the earthquakes and I am sure there is going to be some revision in the method by which maximum magnitudes will be assumed.

The Survey's position is likely to develop out of an entire tectonic re-evaluation I think guided perhaps by this Crouch paper to a certain extent. There are some limitations in the extent to which the Crouch hypothesis is based upon a direction for the North American plate. If this direction is changed by 15 degrees, much of the convergence on the minimum estimate of convergence will disappear.

So there are a number of things that have to be nailed down perhaps more firmly before we can make moment estimates on the magnitudes and distribute these moments across what kind of area over which we can assume this new tectonic takes place.

The ground rules are likely to change because of the manner in which regressions are being done for ground motions and the new information and the tectonic analogues which will be used for models for these ground motions.

I don't have any further remarks to make at this time,except to be very pleased that all the information is already being developed.

Thank you.

MR. OKRENT: Thank you.

Does the staff want to add anything of a geological or seismological nature?

MR. JACKSON: No. I think in the prepared presentation we had it mixed a little bit and I prefer to stay with that prepared presentation.

MR. OKRENT: All right. Why don't we go to your prepared presentation.

MR. JACKSON: I would like to make a couple of comments first before we begin.

We took the approach of looking at potential implications and I think that that includes presenting some things that I don't think that we would believe would be true in terms of a final licensing position, but in terms of providing the ACRS and the consultants with a range of possible implications, I think we tried to approach it from that point of view.

MR. OKRENT: Thank you for doing that.

MR. BROCOUM: We summarized most of our evaluation in the May 21 memo from Bob Jackson to Jim Knight. I understand that a copy of that was distributed and it was missing page 5. Dick Savio has a complete copy now. We apologize if it is our fault.

MR. JACKSON: It had page numbers on it though.

MR. BROCOUM: It does at least have page numbers, that is correct.

(Laughter.)

(Slide.)

MR. BROCOUM: We are going to just try to break it down to three parts, the significance of the new findings of the Crouch paper, the impact of the new findings and what re. lts we hope to get from the proposed elements to the licensed condition.

I should also point out that when we received the Crouch paper we shortly thereafter had a conference call between the staff and Dr. Crouch and Dick McMullen, the geology review on Diablo Canyon, went to the meeting in April, the Pacific Section AEPG meeting, American Association of Petroleum Geologists meeting to hear the Crouch paper and to visit with Dr. Crouch and to hear of course the other papers. There was a whole symposium on California coastal geology.

MR. KERR: Would you be willing to summarize briefly what the staff considers the new findings to be before you tell us what their significance is?

MR. BROCOUM: Yes, I am going to do that with the next slide.

(Slide.)

The next slide refers to the signifiance of the new findings.

First of all, the Hosgri fault may be a thrust fault dipping towards the east/northeast and could pass beneath the site closer than the 5.8 kilometers previously assumed. How far to the north is most of Dr. Crouch's data.

Maybe we could have the map next, and we will go back to that slide.

MR. JACKSON: I think, Steve, you still need to answer his question. You are talking about significance and he is saying what are the new findings.

Dr. Kerr, in light of the previous presentations ---

MR. BROCOUM: I think the major new finding is that the Hosgri fault may have a larger thrust component that we thought previously and the geometry of the fault is such that the fault may dip and pass closer to the site than was previously thought. Those I think would have a major impact on the Diablo Canyon site.

MR. KERR: From the picture that Dr. Crouch showed, almost cartoon in his first slide, I got the impression that a strike-slip fault does not have to be vertical and, hence, it seems to me that it isn't just the fact that it is a thrust fault that leads you to this conclusion that it might pass closer to the plant than previously ---

MR. BROCOUM: No. It is the apparent interpretation that Dr. Crouch showed of his seismic reflection profiles. I understand the previous interpretation showed it to be a vertical fault and there seems to be some debate exactly what was being interpreted in the previous seismic reflection profiles.

The ones that Dr. Crouch showed shows that the fault does dip to the east and does seem to shallow as it gets deeper, and it seems to be shallowing and becoming relatively flat at about two and a half kilometers or so at least by our interpretation of it.

MR. KERR: Thank you.

MR. BROCOUM: I just want to make one point at this point. The closest line, SM-4, is approximately 15 kilometers south of the site. So most of his data is south of the site.

Now Doug Hamilton mentioned several times that there is a paper Jerry Eaton. We have seen the paper, but it has not been open filed yet by the USGS, but we are allowed to give a very brief summary.

A brief summary of that paper is that he looked at six earthquakes, three of them south of Pt. Sal, these are earthquakes since 1976, and those three earthquakes more or less occuring near the coast there have as a four-point solution thrust, almost pure thrust solutions which the preferred solution fits the interpretation that is shown by Dr. Crouch. In other words, the northeast plate is thrusting to the southeast over the southeast side of the fault.

The next earthquake further north that he looked at was near San Simeon, and that earthquake shows oblique motion. The preferred solution shows right oblique motion.

Then he had two other earthquakes north of San Simeon which show almost pure strike slip. The preferred solution fits in very well with the faulting up there, right lateral strike slip with the four points striking as the geology suggests.

That paper we have been promised will be open filed very shortly.

Okay, can we go back to the slide?

MR. OKRENT: Excuse me. While you are talking about new findings, I thought Dr. Perkins had suggested that one possible new finding would be that one would no longer have a, or might no longer have a good basis for postulating that the Hosgri tied on to a long fault north of the Hosgri. Is that correct?

MR. BROCOUM: That is correct. I think Dr. Page made the same point a little earlier.

MR. OKRENT: And the staff doesn't disagree with this?

MR. BROCOUM: No, we don't disagree with that and that is one of the reasons we want to reconfirm, or whatever the word we used was, the length of the Hosgri fault.

MR. KERR: Excuse me. What is it you would be reconfirming?

MR. BROCOUM: Well, I just used that word again because I think we used it earlier. We would want to determine the length of the Hosgri fault.

MR. JACKSON: I think I could add an additional comment maybe. It is very hard to summarize very easily, but I think in the review we have done we have recognized that there are a number of tradeoffs if you take as fact the interpretations of the paper and assume that they were different interpretations than we made at the time the licensing decisions were made by whichever party, USGS or the staff or the utility.

There is a tradeoff. If you have a strike-slip fault which has long continuity, then you are going to get a certain magnitude earthquake.

If you now make the assumption indeed that the thrust mechanism is more dominant, then you can still get an equivalent magnitude earthquake for a shorter rupture length of a thrust fault.

So essentially just saying the fault is shorter would not necessarily reduce the magnitude. The known data of relating magnitude to fault type is somewhat different depending on the type of fault.

MR. ETHERINGTON: Does that mean more energy per unit area for a thrust fault?

MR. JACKSON: I think that is reasonable, yes.

MR. OKRENT: Dr. Maxwell.

MR. MAXWELL: I think your question, Mr. Ethrington, the answer is not quite what you said, Bob, because it is the area of movement that is involved.

MR. JACKSON: I am sorry.

MR. MAXWELL: In a thrust plane the area of motion that actually moves can be relatively large as compared to movement on a plane like so.

I wanted to ask why you kept the 7.5 earthquake in your discussion here and whether there was any basis for it?

MR. JACKSON: That is the design basis earthquake that has been used and we haven't gone back and tried to look at different fault lengths and reinterpret the magnitude at this point in time. We just looked at a range of possibilities.

MR. BROCOUM: We think if the Hosgri fault does actually, if its dip does flatten with depth, a most conservative case would be that the fault would pass under the site at about two and a half kilometers and we will present some numbers of what that means in terms of acceleration a little later.

Secondly, "B", the character of the ground motion may be different. Normally ground motion due to thrust earthquakes is normally higher than ground motion due to strike slip. So that has to be looked at.

Thirdly, the geologic data we think indicates a low amount of, if it is truely a thrust fault, a low amount of tectonic movement which can mean a lower earthquake recurrence interval.

Maybe the words here "no sea floor offset" is too strong, but a relatively low sea floor offset. I think we were talking about 150 meters or so of offset in the Sisquc, which is at least two millions years old.

If you turn that around and you had a very high rate of movement, say you had one meter per thousand years, you would be expecting two thousand years of offset.

Point D, the analysis of several recent earthquakes, such as Jerry Eaton's and such as that open file report mentioned by Doug Hamilton, seems to indicate that it varies from thrusting on the south to more or less strike slip north of Sam Simeon.

Finally, the faults that were mapped on the site, we don't believe changed the conclusion which was reached at the time that they are not capable. That conclusion was based in large measure on the fact that marine terraces 80 to 120 thousand years old were not offset.

If these faults were indeed thrusts rather than say strike slip, or whatever they were interpreted to be then, the offsets would be easier to see.

However, we have to take into account that if the Hosgri off Diablo Canyon is a thrust fault, the possibility of other faults nearer to the site which may be splays off the Hosgri or splays off the Decomont has been postulated.

The next slide.

(Slide.)

The next slide is going to discuss the impact of the new findings. I think we are going to have Dick McMullen discuss this as he is the reviewer on this site.

MR. MCMULLEN: As was stated earlier, it is our position that after a preliminary review of Dr. Crouch and others paper we see no reason at this time to change our previous position on the seismic design basis of the plant.

The following are a few of the reasons why. I notice that some of them are a little bit repetitious, however in a little bit different context here.

As has been said several times, Dr. Crouch's paper is of very high quality, but his theories and theses need to be looked at in the overall context of our evolving knowledge about the tectonics of California.

There are other theories. In fact, as Dr. Page brought out, there are still those who believe there is a substantial amount of strike-slip movement, and that is just an example that this paper needs to be evaluated in the context of that paper and others that will be written in the near future.

We know it appears that somewhere between the San Gregorio fault zone and Pt. Sal the tectonic regime changes from predominantly a thrust mechanism to a strike slip and that needs to be looked into relative to the site.

The character of the thrust faults at depth need to be determined, the depth beneath the site and whether they flatten out at two a half kilometers or do they ramp on down deeper and do they join a sole fault at depth. The existence of a sole fault is unknown.

In a sense the compressional aspects of faulting was considered during the licensing activities in that the Lompoc earthquake in 1927 was selected as the Hosgri earthquake, and that is considered by most people to have been a reverse mechanism earthquake.

MR. OKRENT: Excuse me. I wonder if that statement is a complete statement in the sense that you really didn't postulate a 7.3 as a thrust fault or a reverse fault occurring right at the Diablo Canyon site and tried to examine the motion that might result from it, did you?

MR. McMULLEN: No, that is true.

MR. JACKSON: I think part of the problem or a lot of it is that we were not involved when those decisions were made. But a clear input to the process was the use of the Pacomia Dam record by the USGS and Dr. Newmark to reach that conclusion that that was a thrust type event.

MR. OKRENT: Well, I have heard a lot of the process. In the first place, as we well know, the Lompoc had a different impact and it was used as a way of benchmarking, if you will, what might occur off the coast in that vicinity. Whatever the cause, I think USGS says well, if we can get a 7.3 there why can't we get it on the Hosgri off of Diablo Canyon. But I just want to say I think that particular statement is a little bit incomplete with regard to what was really done with the Lompoc earthquake.

In other words, I am saying compressional aspects may have been mentioned here and there, but everyone was talking strike slip with a certain magnitude.

MR. McMULLEN: The thrust of the Pacomia record was used as a matter of conservatism.

MR. OKRENT: It was sort of used. Again, one has to be careful to say it was used, too.

MR. KERR: Suppose that the compressional aspects had been considered, what would have been the difference?

MR. OKRENT: Well, we may get to that in some of the next slides. I don't know. Let's see.

MR. JACKSON: I think in the next presentation the ground motion will get into the different G values you might get out of particular mechanisms.

MR. SIESS: That is as of now or as of 1978?

MR. JACKSON: As of now.

MR. MCMULLEN: As was stated earlier, the geologic evidence in the offshore data appeared to indicate a low recurrence interval of a large earthquake.

The onshore faults at the site were mapped and shown to be not capable, and that is not really expected to be changed now. However, it was looked at at that time as being within a rich fault type tectonic regime, and in that type of regime you don't usually look for a thrust fault parallel to the wrench faulting.

So this needs to be considered again in light of the thrust type mechanism.

MR. BROCOUM: I just want to make a correction of something I said before. I said that the most conservative approach would be for the Hosgri fault to pass two and a half kilometers beneath the site.

I don't think anybody of us really believes it will be that close because if you look at other well-known thrust faults, these faults tend to ramp deeper and go into a sole, if you like, a sole thrust, and the model that Crouch showed suggests that it would be 10 kilometers or so deep. So I think that truly that two and a half kilometers would be an extremely conservative number.

(Slide.)

Next we have the impact on the ground motion.

The ground motion estimates using data from the Crouch paper might be higher than previously assumed. They could be lower. To get the correct or better ground motion estimates you need to know the correct or a better magnitude estimate.

You have to have an estimate of the ratio of strike slip to thrust movement and you have to have a good estimate of the distance of the fault to the site.

I think at this point Bob Rothman is going to present some very preliminary numbers.

MR. ROTHMAN: I was asked to make some bounding estimates on the ground motion based on some assumptions that we could make on the location of the fault and the regression analyses that have been performed to estimate ground motion.

The problem is that what we want to look at is the relationship between strike-slip faulting and stress faulting. About the only person that has done any regression analysis which has included this capability in the analysis for fairly recently obtained data was Dr. Ken Campbell who is presently working for the U. S. Geological Survey. He published a paper in 1983 which was a summary of is work for the preceding three or four years in which he looked at the effect on ground motion of various parameters.

So we have used his work to look at some of these effects.

(Slide.)

You will see at the top it says the Hosgri reanalysis was based on a magnitude of seven and a half strike-slip earthquake at a distance of 5.8 kilometers and a free-field peak ground acceleration of three-quarters of a G.

Using Campbell's 1983 relationships for free field estimates, and this is not taking into account any kinds of effects, but free field estimates for a magnitude of seven and a half strike slip at 5.8. We have average and 84 percentile ground motion numbers there.

You can also see we have done it for a

magnitude six and a half thrust fault at 5.8 which is the furthest distance that we can allow the earthquake to go because of the fact that we have a fault trace at that distance and for the assumption of it passing at two and a half kilometers below the site.

We have also done this for magnitude seven and seven and a half earthquake. So these are the ground motion estimates for peak horizontal ground motion acceleration using this regression analysis.

MR. SIESS: Excuse me. As I recall, there used to be some difference between the peak horizontal ground acceleration that the USGS came up with and something called an effective peak ground acceleration.

MR. ROTHMAN: This is not effective. This is peak ground acceleration free field.

MR. SIESS: And the .75 up in line 2 is the same thing?

MR. ROTHMAN: That is right, the same.

MR. SIESS: Okay. That is a USGS estimate at the top?

MR. ROTHMAN: The top is the numbers we used in the original ---

MR. SIESS: I know that, but would that correspond to the USGS?

MR. ROTHMAN: That .75G was used as the peak ground motion number to anchor the spectrum.

MR. SIESS: And I would compare that then with Campbell's first line?

MR. ROTHMAN: Yes, that is right, with Campbell's first line and probably with the 84 percentile as in Campbell's first line.

MR. SIESS: Well, it would be higher than 84.

MR. ROTHMAN: We were also asked to look at the vertical ground motion that would be possible due to a thrust fault near the plant, and I would like to make some comments.

Campbell has done some regression analyses on the vertical for thrust faults and for strike-slip faults. We have had some comments on this work. The data set for the vertical ground motion is dominated by data from deep soft soil sites.

There has been some postulation that the vertical ground motion may be site dependent, the peak acceleration may be site dependent and that soft soil sites may show higher vertical ground motion for the same magnitude than does rock sites, which would be the Diablo Canyon site. It would be a rock site.

In support of this we have a modeling study which was done for the staff and it is published in NUREG CR-3102. It was not performed to look at this, but performed to look at some other factors in ground motion.

You can look at a ratio from this work, the ratio of the vertical ground motion as postulated for soil sites and for rock sites, and it does appear from this study at least, the modeling study, that there is a higher vertical ground motion in the near field from soft soil sites than there are for rock sites.

With this caveat in mind, I would like to show you some of the estimates that we have obtained using Campbell's regression analysis.

MR. KNIGHT: Before you take that off, perhaps an element of clarification. When Dr. Newmark worked with the Pacomia Dam record, many would argue that actually the free field anchor point for the peak acceleration -- the peak acceleration at least off of the Pacomia Dam record, if I remember correctly, was like one and a quarter G or something of this type. The whole business of effective acceleration and exactly how you define it has been a matter of some controversy throughout this whole business.

I just wanted to have that clarified so we didn't have some confusion in the record as to whether or not that .75G would be considered directly analogous to the first line.

MR. ROTHMAN: I am sorry. I misinterpreted the question.

(Slide.)

On the next slide we will look at some of the estimates of vertical ground motion.

Here is a comparison using the regression analysis for vertical ground motion, a strike slip and a thrust fault at 5.8 kilometers from the magnitude 7.5 earthquake. You see that the thrust fault is predicted to give a higher vertical acceleration.

The next slide, please.

(Slide.)

MR. KERR: Excuse me. You said that was taking into account th? rock foundation?

MR. ROTHMAN: No, we have not taken anything into account. Campbell's relationship is based primarily on data collected at steep soil sites and we have no way of factoring that out. An argument has been made by other people that soil sites do amplify the vertical ground motion. So these predictions may be higher than you would find on a rock site.

This is a ratio of the vertical to the horizontal for strike slip and for thrust. You can see that the ratios are approximately the same indicating that both the horizontal and the vertical for thrust faults are obviously higher than the horizontal and vertical for strike-slip faults.

The next slide, please.

MR. OKRENT: Excuse me. What am I seeing on this slide again?

MR. ROTHMAN: What I have done here is I have taken the peak vertical acceleration and divided it by the peak horizontal acceleration for the exact same conditions, strike slip and thrust fault for the same distance to see if there is any difference in the ratio at least between vertical and horizontal on the ground motion.

MR. OKRENT: All right. Although Campbell finds that these are similar, he also finds that this ratio is larger than one.

MR. ROTHMAN: That is right.

MR. OKRENT: Whereas the design was less than one.

MR. ROTHMAN: That is right. You will remember the caveat. I suggested that this data is from soil sites, and it has been argued that the vertical are amplified at soil sites when compared to the horizontal and that you wouldn't expect this at a rock site necessarily, although they may be higher than two-thirds in the near field.

MR. OKRENT: I was going to ask you whether you expected about a factor of two difference between a rock and a soil site from your theoretical analysis?

MR. ROTHMAN: From the theoretical analysis it would actually be a factor of almost three difference in the rock to soil site at distances of about six kilometers. There is also a distance dependent effect besides a site condition effect. At a distance of about six kilometers it takes almost a factor of three.

MR. TRIFUNAC: Can you be more specific as to who argues that. Is that a paper?

MR. ROTHMAN: It is NUREG CR-3102 and it was done by Sierra Geophysics under contract to the Office of Nuclear Reactor Regulations to look at a different problem, but you can back this information out of it. We were looking at the effect of ruptured depth on ground motion, but it was done for several different types of geology comparing the ratios of soils to rock.

MR. TRIFUNAC: This is based on observations?

MR. ROTHMAN: No. This is based on theoretical modeling studies done by Randy Absell.

MR. OKRENT: We will get you a copy of the report.

MR. ROTHMAN: I have a copy here if you would like to have it.

MR. SIESS: Excuse me. This bothers me. It seems to me that we have been collecting safe shutdown earthquakes for a number of years at sites all over the country, some of which I know are soft sites because we need to worry about liquefaction, and we have been using roughly two thirds for the horizontal, and now somebody comes along and tells me it is almost twice that. What am I supposed to believe?

MR. ROTHMAN: Well, this is based on, it is for the near field, these ratios. At distances on the order of greater than 10 kilometers you do see ratios of two-thirds.

MR. SIESS: All near field.

MR. ROTHMAN: Well, let me even qualify it further, near field for earthquakes in the magnitude range of five and a half or six or greater. So when we talk about eastern U. S. sites it is a completely different situation. We are talking less than 10 kilometers for relatively large earthquakes.

May I have the next slide, please.

(Slide.)

Here we have looked at four and a half kilometers which would be the closest approach and a very conservative estimate. We have calculated an average for horizontal acceleration, 84 percentile, peak horizontal acceleration, average vertical acceleration, 84 percentile vertical acceleration and the ratio of the two.

This concludes the calculations that we made on this. I might add that we currently have in the process, in fact I signed the statement of work two days ago, there is a contract being generated with the USGS for Dr. Campbell to continue his work.

His work was based on data from the period 1980 and earlier. We now have more data in larger earthquakes in the last four years, and he is going to continue working on this under an NRC contract hopefully and possibly get some better insight on the actual site characteristics affecting the vertical ground motion and also spectral ordinances and not just acceleration levels.

MR. OKRENT: Questions?

MR. JACKSON: I don't know if we have attached enough caveats to putting numbers up like this. I think it points out some difficulties in using peak acceleration per se in defining ground motion, and you know as a staff we have tried over the past few years to go to other methods of better estimating ground motion design spectra.

So I think we have put these up in order to give you some feeling for what possibilities exist. It was based on the very limited existing data base.

MR. OKRENT: Did the staff have any other comments in the general matter of the possible impact on the design basis ground motion?

MR. BROCOUM: We have a little more of the presentation.

MR. OKRENT: All right.

MR. BROCOUM: We were going to just address how our proposed elements will help answer these questions.

(Slide.)

MR. KERR: May I ask one question, and maybe you are going to address this, but if you had to guess at this point in the process of re-evaluating the appropriate magnitude, would you guess it is going to be bigger, smaller, about the same or is it too early to judge or to guess?

MR. BROCOUM: That is a very touch call.

MR. JACKSON: I think though that we have discussed that a lot internally. I think there are a number of elements that might make it go higher and there are a number of elements that might make it go lower. I think generally we think it might come out the same or a little bit lower.

That would be, you know, asking us to look three or four years into the future and I think we just need to wait and see.

MR. OKRENT: Excuse me. When you said same, did you mean earthquake magnitude or acceleration?

MR. JACKSON: Earthquake magnitude would probably be about the same or a little less. The ground motion estimates, I think a great deal more has been done in that area over the past few years and I think that area would be much more difficult to predict in terms of exactly what will happen.

MR. CARBON: Are you going to get into the discussion soon on how long it will take to pin some of this down?

MR. BROCOUM: We are proposing that the applicant undertake a three-year program. I suspect after about a year we will have a much better idea, for example, probably of the Hosgri fault and perhaps its length. I mean that, it seems to me, will be one of the first things that will have to be looked at since we are talking about a different tectonic picture than we thought in the past.

MR. JACKSON: I think if you are referring to what we are currently proposing, what we are saying is the site as we see it and everything we know is being designed for a magnitude .75G close-in, large magnitude event. We see a lot of things that would indicate to us that we can move ahead with what we now know in terms of the existing basis and that this kind of information is the kind of thing we had envisioned be done in the longer term program.

I didn't get the context of your question. I think that the total knowledge on the tectonics of

California will be many, many years in forthcoming. So I think we have to make some judgments at this point in time and that is what we have tried to do. Is that the point you are trying to get at?

MR. CARBON: No. The context of my question was we have a new theory here and it may increase or it may decrease the effect on Diablo Canyon. When is it going to be that you are able to pin this down somewhat closer or are we going to have a big new uncertainty here?

MR. BROCOUM: I think there will be uncertainty for a period of time. Let me summarize why we are suggesting that we go ahead, and that is really basically the second page of the cover letter of that May 21st memo. We gave four reasons, and I will just summarize them.

The first was of course that this is a new hypothesis. It hasn't been extensively discussed in the scientific community. It will be, I am sure, in the next year or twp and the applicant and we will be looking at it. I don't think we can always jump everytime a new hypothesis comes along. I think it is a very well documented one, and I don't mean to belittle his hypothesis.

Secondly, to some degree or another during the licensing activities of Diablo Canyon, and I know this bothers Dr. Okrent, but the compressional aspect of faulting was included. To what degree seems to be a little uncertain, and part of the program is to determine what degree of thrust versus strike slip should be considered.

The third is that we assumed a magnitude of 7.5 in the near field. So the plant is designed for a near-field large event.

Fourth, all on-shore faults in the immediate vicinity of the plant have been shown to be non-capable and we don't believe that any of the new information changes that previous conclusion.

So for those reasons we suggested that the licensing activities go forward and not stop the licensing activities while this study was done.

MR. ETHERINGTON: Do you see the possibility of it being increased in the vertical component?

MR. BROCOUM: I don't know. Bob Rothman gave you all the caveats and the problems and we have a contract with Dr. Campbell to study all of these things. I think we are doing the prudent thing. I think Diablo Canyon in that area has such complex tectonics and seismology that I think even over the lifetime of the plant there will be constantly new information coming along.

This three-year program is at least designed to accommodate the new information that we have now and we will have say in the next year or two. We think it is a very prudent program and a very prudent way to go and that is how we designed the program.

MR. SIESS: Have you got any idea how you are going to factor this kind of information and Campbell's information into your PRA? It seems to me it is rather essential and that is why you are doing the PRA.

MR. JACKSON: It clearly would be a contribution to the hazards function that you would have to work out, all of these types of observations.

MR. SIESS: It seems to me like it is the dominant contribution and you can forget about all the others.

MR. JACKSON: Okay. I think that goes to the question you were asking this morning of how far would you go in terms of what accelerations would you consider as inputs into a probabilistic risk assessment, and the data from the Campbell curve and other regressions that might be available would be one of the factors that would go into that development.

MR. SIESS: Does Campbell's work give you any idea on the recurrence interval?

MR. ROTHMAN: No. It is strictly based on empirical data from all over the world.

MR. SIESS: So it increases the uncertainty in the magnitude but won't help you much on the recurrence interval.

MR. ROTHMAN: The assumptions on the recurrence rate on the faults and the size of the earthquakes will have to be weighted in a probabilistic manner and incorporated into that curve with continuation curves such as Campbell's. There may be several continuation curves used in the probabilistic determination of the hazard curve and then the various continuation curves will be weighted also. The people that perform this hazard analysis will be the ones that will weight them. I think Dr. Cornell talked about that a little bit this morning.

(Slide.)

MR. BROCOUM: The final side kind of shows ---

MR. OKRENT: Excuse me. Before you leave this one ---

MR. BROCOUM: This slide here. I am not going to leave that. I was going to talk about it.

MR. OKRENT: All right, continue.

MR. BROCOUM: I am sorry. Points "A" through I guess "I" we have already discussed. I think it is obvious why these things have to be determined in terms of giving us a better understanding of either the tectonics, the magnitude of the SSE or the ground motion.

Point J is provide analysis of more recent near-field records. There have been several recordings in recent years from earthquakes.

Point K is to provide a modeling study of the rupture mechanics and I guess propagation.

Point L is to provide a soil structure interation analysis which would take in the various factors.

The next slide.

MR. KERR: Excuse me. Before you leave that one, if you are now leaving it, I want to try to understand what I thought I understood this morning to be a statement that one did not expect PG&E to have to do research or to dig up new information, but rather to analyze existing information.

MR. BROCOUM: Yes, that is the general intent.

MR. KERR: If so, and let's take "F" for example, determined length of rupture during a single earthquake, how do they get that from existing information?

MR. BROCOUM: Well, by analyzing more recent earthquake information or past earthquake information or by studing seismic reflection profiles or by whatever other modeling or theoretical arguments I guess.

MR. KERR: You suggest they could do that without any new information being developed?

MR. BROCOUM: If possible.

MR. KERR: I am not trying to pin you down. I am just trying to get some idea. That looks like a rather formidable list of tasks, but it puzzles me to think that they can get all that information from existing data.

MR. JACKSON: I think as we said this morning, and it may have been list in the discussion, is that there are a number of elements where additional data would have to be provided.

In this particular one you picked point "F." You might be able to utilize the kind of arguments that Doug Hamilton was discussing earlier of making observations of connections and the like, and that may be what you might utilize there. So in this particular case you may not need to get new data.

But I think it is obvious from the discussion that you may have to fill in areas where you don't have information.

(Slide.)d

MR. BROCOUM: The final two points that we have also discussed earlier, which was assess significance in and any difference between the new findings and existing seismic design basis and, finally, provide limited deterministic analysis that can be used to better define specific seismic margins. I think that is the weak links thing that we occasionally talk about.

That concludes the staff's presentation.

MR. OKRENT: Dr. Kerr.

MR. KERR: In earlier questions as to why you feel that nothing significant needs to be done right away about licensing conditions, I found my own reaction to your response to be somewhat negative. What I had hoped you would say and what seems to me is the case from what I have heard today is that you don't really think on the basis of a preliminary analysis that the results of this paper will change things very much.

Am I correct in that assumption?

MR. BROCOUM: Yes, you are basically correct. I guess there is no guarantee, but that is our best judgment. That is why we were recommending that we go forward.

MR. KERR: Well, of course there is no guarantee, but on the basis of your best judgment you don't think that the eventual analysis is going to change, for example, or at least it is not going to make the magnitude that you consider appropriate to be very much larger or the ground level acceleration very much larger. Is that correct?

MR. BROCOUM: Yes, that is correct. Various things will change, and my feeling is they will probably all average out at the end and not be too different at the end.

MR. KERR: But if you really at this point felt that the magnitude of the earthquake was going to be 50 percent greater, you wouldn't be taking the attitude you are now taking, would you?

MR. BROCOUM: That is correct.

MR. JACKSON: I think some of the way we have come across in presenting it may be overly negative and it may be a little bit defensive on our part in that we do feel that a licensed condition is necessary. So in order to essentially require that a licensed condition be done we are trying to point out things that I guess come across more negatively.

We made an overall judgment for the Commission meeting on the low-power license, and in that meeting we also discussed extensively internally and with the USGS these potential outcomes.

I think the way I would characterize it is that our overall judgment is that based on Dr. Crouch's paper, and we really wanted to emphasize that there are a lot of papers available. When we went to the Commission we tried to indicate there are a number of balancing effects here. There are some things that may indicate that ground motion magnitude may be lower and some that could indicate higher. You could make extremely conservative assumptions of proximity of the plant. But overall our judgment is that the design basis and the magnitude as determined is adequate right now.

MR. ETHERINGTON: I think it was said that you expect the magnitude to be about the same. I would have thought that in a thrust rupture there would be more energy available on the overthrust part than on the underthrust part. Is that right or not?

MR. JACKSON: I think the comment we made earlier is that the smaller length of a thrust fault could have an equivalent magnitude with a longer length of a strike slip fault.

MR. ETHERINGTON: Yes, but I am distinguishing between the two sides of the fault. Wouldn't you expect a difference in a thrust fault?

MR. BROCOUM: In the resulting ground motion?

MR. ETHERINGTON: Yes.

MR. BROCOUM: You are talking about the up-thrown side versus the down-thrown side?

MR. ETHERINGTON: Yes.

MR. KERR: Bob, can you make a comment on that?

MR. ROTHMAN: I don't know if we have anything to agree with that or disagree with that. I don't think the information is available.

MR. ETHERINGTON: Well, I am just talking about simple geometry. The under part has got no place to go and the upper part is free. So you would expect it to move more.

MR. ROTHMAN: It would depend on the radiation pattern of the energy itself, the way the energy is radiating. You may have focusing up and down. I don't think it is a simple question that you can answer.

MR. ETHERINGTON: It is not just displacement then.

MR. OKRENT: Can I ask the staff the following question. Since, if I understand it correctly, Dr. Crouch was assisted considerably in arriving at his concept of what the situation may be with regard to faulting in the vicinity of Diablo Canyon, particularly the Hosgri fault, by proprietary information, information not in the literature, how are you going to judge the importance to what you are proposing be done or what needs to be done of getting whatever constitutes the necessary access for these purposes to such information?

MR. BROCOUM: I guess the primary type of data that would be proprietary would be seismic reflection profile lines, well data, geophysical well data, those would be the two major I think types of proprietary information. Seismic data is available since it is mostly collected by the USGS.

So those kind of data are the ones we really have to worry about, but that is true anywhere in the United States.

MR. JACKSON: I think that getting to the point, essentially what we do is in the licensed condition where we are essentially requiring the applicant to provide a representative set of all available data, proprietary and not to be evaluated in future interpretations, the staff has the ability and the ACRS to handle data under proprietary cover that can be submitted and looked at under, you know, proprietary rules and regulations where the staff can see it and the ACRS can see and the consultants to the staff can see it. I guess that any relevant party under the ground rules can look at the proprietary data.

The more difficult question is how can you be sure that you have looked at all of the available proprietary data, and that would be essentilally saying as a staff can you assure me that you have looked at all the lines. I don't think that could ever be guaranteed.

MR. OKRENT: I don't think one is talking about all lines. One wants to have some confidence that he knows enough about the relevant information that there is not something in fact that is known that contradicts a position taken in either direction very significantly.

I remember a recent example. A few years ago where we went on one site to the same company that was involved and there was proprietary data in one part of the company that the other part of the company did not know. We saw part of that and then later on in the interpretation say the other part of it which changed the overall interpretations for a site in the Pacific Northwest. All you can do is request and try to do the best you can to get a representative data set.

MR. BROCOUM: Also, if you have the proper experts, you know, if you have somebody that is expert on off-shore California, they will be aware of the type of data. They may not be knowledgeable about all of it, but they will be aware of the type of data that is available. So where you might go to try to seek it, I think that having the proper people is very important.

MR. CARBON: But you can go to an organization totally unconnected with the nuclear field and require proprietary data.

MR. JACKSON: No.

MR. CARBON: You cannot.

MR. JACKSON: No.

MR. SIESS: What do you mean by available, that somebody is willing to sell?

MR. JACKSON: That is correct.

MR. SIESS: That is what available means.

MR. BROCOUM: Some of the major oil companies are willing to, also having worked for one once, are willing to let agencies look at it, but they may cut out very important parts that are prospects.

MR. SIESS: Incidentally, you did use the word relevant in what you submitted to us in writing.

MR. JACKSON: I believe so.

MR. SIESS: You did.

MR. JACKSON: Dr. Crouch may be able to comment a little better. It is a very competitive field out there in terms of drilling oil wells and it gives a competitive advantage to different companies to have the information out there in the public record.

MR. CROUCH: I think one advantage you have now is that most of the sales in this area have taken place. In fact, I was recently at a meeting in San Antonio where Chevron discussed some of the particulars of the Arguello field, for example. It is not something they would have done a year ago or when they first discussed some of the aspects of the Hosgri.

So I think that you would find that information would flow a lot freer now than it would have say a year ago before the OCS-73 sale. I can't speak for the various oil companies or for Nekton itself, but my guess is that at least they in many cases would be willing to let you look at it to determine whether or not you agree with some of our interpretive work.

MR. JACKSON: I think we would look also to the USGS very extensively as an agency to help in this area and they should be aware of a great deal of information.

MR. OKRENT: Are there other questions of the staff?

(No response.)

MR. OKRENT: Well, thank you. We may get back to you yet.

I think the committee will be interested in hearing from our consultants such comments as you think you can make at this time, and recall there are two general subjects that we talked about. One was the general studies, including a PRA, but in particular other kinds of seismic studies aside from what might be done in a PRA. That was this morning's topic.

And this afternoon I suppose you might put it as does the new information on the geology, et cetera, of this part of the coastline pose a significant likelihood of a significant increase in whatever was the prior design basis. I think if you have an opinion on that question that you are willing to give at this time, I certainly would be interested in hearing it.

I think we will also be interested in getting something from you in writing prior to the June full committee meeting, but if there are some things you can tell us now, I think it would be helpful.

Do I have any volunteers?

(No response.)

MR. OKRENT: Would you like to have a 15-minute break?

A 15-minute break.

(Recess.)

MR. OKRENT: The meeting will resume.

Before Mr. Mendes begins, I should note I overlooked the fact that PG&G was asked to split their comments and they earlier commented on the Crouch paper and they still have some comments on the impact of this on the design basis ground motion. So we will take that for about 15 minutes right after our next speaker who is Mr. Mendes.

Mr. Mendes, please.

MR. MENDES: Yes. My name is Stanley Mendes and I am a structural engineer in Santa Barbara, California.

I have given you a two-page prepared presentation. I don't see any reason to read it because it is in the record.

I will just summarize with one of the

paragraphs in it that I think will give you the tone of my concern, and that says that over the years there has been an intense manipulation of the licensing process by both PG&E, NRC and staff.

They have demonstrated a pervasive bias against public scrutiny and constructed the plant based upon inadequate, offshore geologic investigations. Why? Very simple. No completed nuclear power plant has ever been denied a license.

What I would like to do is just very briefly go through a little bit of the history that I suspect will be new to certain members of the Board, but I think it is very relevant because we have been listening and I have listened to everything that you have. I have been here from the beginning. I have listened to comments on the state of the art, the state of the art of seismology, geology, new findings and these kinds of things.

As a professional engineer I understand fully what that means. While in a sense my comments may seem rambling, I think you will find that they are very much on target. For example, I hear talk of regression analysis to make probabilistic references to earthquakes, ground motion and that type of thing.

Let me start where I think we should start. The most important element to my mind and to the minds of most scientists is to relate theory to actual observations.

USGS Circular 672 states very clearly that a magnitude 7.5 earthquake that there have been no strong motion records closeer than 25 miles. So when we are talking, and what you have heard here today, you here people talking about magnitude 7 or 7.5 earthquakes, in terms of actual known ground motion it has never been recorded. It is theory.

I will read from a document which I will reference. "The geology of the Diablo Canyon site has been studied in depth. It is unlikely that any further studies, however desirable scientifically, would reveal any information of greater significance than that which has already been considered in the design of the plant." September 4th, 1970, Phillip Crane, counsel for PG&E, and this was the proposed finding of fact and conclusion of law after the Diablo Canyon hearings a few months earlier.

The second document. The reference on this document is memorandum and order on June 14th, 1971, Atomic Safety and Licensing Appeal Board, A. G. Wells, Chairman, Mr. Buck and Dr. Quarreles. Essentially this was a decision not to reopen the hearings, the just recently completed construction license hearings on Diablo Canyon, Unit 2.

It states, "It is also asserted" -- and that is by PG&E prior to this which I will read -- "that any attempt to relate the San Fernando earthquake to the Diablo Canyon site was virtually meaningless since the high ground accelerations recorded during the San Fernando earthquake, the principal factual data asserted as significant by the Conference, then intervenors, were associated with the kind of faulting not present near Diablo Canyon" -- and at that time the kind of faulting was thrust fault -- "occurred in a structural province fundamentally different from that in which the Diablo Canyon site lies and were recorded at points where the conditions of ground materials and topography were quite unlike those at the Diablo Canyon plant."

That is the end of the quote and I will furnish you the documents.

A little background for that. Shortly after the San Fernando earthquake as a consultant to intervenors we attempted to reopen the just recently concluded construction license hearings to give input on what had taken place in the San Fernando earthquake, the thrust faulting, the Pacoima Dam record.

The first answer of Pacific Gas and Electric to the motion for reconsideration, which basically was to reopen the hearings, signatured by Phillip Crane and dated July 10th, 1971: "Orderly administrative practice requires there be a finality to administrative action. During the approximately three-year period between issuance of the construction permit on Unit 1 and the operating license there can be expected to be a number of developments as the final design of the unit is perfected. The state of the art advances and new data are available."

Does it sound familiar? The San Fernando earthquake of 1971 is one such development.

"These developments properly are matters for study by the applicant and also by the AEC staff in its final review of the application prior to the issuance of an operating license or as a part of its continuing post-license surveillance."

It sounds to me like we are into something on post-license surveillance.

"Any such development which has influence on the design of the facility can be analyzed in the final safety analysis report or as a post-license change under 10 CFR 50.59. The public hearing record should not be continuously reopened to consider these developments. This would constitute an abuse of the hearing process."

PG&E made a supplemental reply to that request to reopen the hearings dated July 28th, 1971.

In part: "The high ground accelerations recorded during the San Fernando earthquake" -- and I intersperse, were associated with the kind of trust faulting not present near Diablo Canyon -- "occurred in a structural province fundamentally different from that in which Diablo Canyon lies and were recorded at points where the conditions of ground materials and topography are quite unlike those at Diablo Canyon plant."

Even more important: "No large fault of any kind exists beneath or near the Diablo Canyon site."

Moving on where he is summarizing: "In other words, if the Diablo Canyon earthquake design criteria were being prepared today after the San Fernando earthquake and in view of all other knowledge that has been acquired since the criteria was developed, no changes from the criteria actually employed would be necessary to assure a safe shutdown of the plant. Thus, there is no reason to reopen this matter to reconsider the effect of the San Fernando earthquake on the design of the units since the design is adequate."

On April 18th. 1971, two studies were let that included the effects of the San Fernando earthquake. WASH-1254, Recomendations For Shape of Earthquake Response Criteria prepared by John A. Blume, then consultant for PG&E and eventually published in February 1973 and, No. 2, WASH-1255, A Study of Vertical and Horizontal Earthquake Spectra prepared by Nathan Newmark,a consulting engineer, and eventually published April of 1973.

The foregoing studies were utilized about December 1973 as a basis for new AEC design criteria for all new nuclear plants, and in the reanalysis of the facility when the Hosgri fault was officially recognized, they included, these studies included selected ecellograph records from the 1971 San Fernando earthquake and, in particular, the Pacoima Dam record and the Golden Gate Park record from San Francisco of 1957 which was utilized in what was then termed Earthquake "D" and this was a nearby assumption.

It was an assumption of an earthquake centered six miles under the plant, a magnitude six and three-quarter earthquake. The assumption was that it was tearing downward and no surface break, and the numbers that were used and assigned to that kind of an earthquake, my recollection is, was as a design basis earthquake, .2G, and by inference as a safe shutdown or now safe shutdown, .4G.

Okay that is where we were state of the art, and I am very close to being finished.

I got awfully curious about this. I didn't understand initially what was going on, but then it finally dawned on me. So I got more than curious and I did some personal investigation.

I have spoken personally with Mr. Ernest Hoskins, author of the Hoskins and Griffith report. I will include a copy of a news article that came to my attention that made me curious in which Mr. Hoskins made certain references to the fact that a Dr. Milton Dobbrin, geophysics professor at the University of Houston who was working in 1971 as a consultant to Bechtel and Bechtel in turn was acting as a consultant to PG&E for the proposed nuclear power plant at Pt. Arena.

And in the course of the debate about Pt. Arena, that PG&E eventually abandoned, it says, Dobbrin became aware that PG&E had a nuclear power plant started at Diablo Canyon. He suggested they better take a look at the article that Hoskins had written, and Mr. Hoskins said that was 1971.

I have talked with Holly Wagner of the United States Geological Survey and Mr. Wagner independently confirmed that Dr. Dobbrin had made the same comments to him.

For Dr. Crouch I have a suggestion. He was concerned with seismicity that might be related to the offshore faults. Well, according to a letter from Levy Gossick, Operations Officer, NRC, to Congressman Morris K. Udall on March 31st, 1977, the United States Geological Survey at the request of NRC monitored the area offshore of Diablo Canyon, and that was in the spring and summer of 1973 and prior to submission of the final safety analysis report by PG&E.

I am going to just give a reference, and you will receive copies of it, but it happens to be a Director briefing to Rousch. This is for your information. Of importance in the document is not necessarily the content, but the manner in which it is written and you will understand from it that there is a very close, ongoing relationship between staff and PG&E. And when they go hand in hand together, then I suspect that it is something that is very self-serving.

I have heard the discussion today on the probabilistic risk analysis, and I would ask why is it coming into being now when under date of January 12th, 1976, and it is entitled "Program To Establish Basis To License Diablo Canyon." This is the NRC staff.

One of the documents indicates "Concurrently form a task force to review the current status in an attempt to determine if a probabilistic basis can be established to license Unit 1 for an interim period of operation while the other reviews are being conducted."

To the same subject on a probabilistic analysis in 1976 under date of November 24th from R. B. Hoffman, Geology Section, memorandum for H. R. Denton, Director, in which he proposes a number of studies might be made.

He indicates "Finally, consideration should be given to a probabilistic approach. In such an approach all event sizes would be considered to have a finite probability of occurring and their locations would be probabilistic value also. It is possible using methodologies which are reasonably easily developed to compute a spectrum that would have a uniform probability of being exceeded in the lifetime of the plant."

Again, I am asking why now? These things have always been possibilities.

The next to the last document. July 14th, 1978. This is to Joseph Hendrie and it is the report on Diablo Canyon, Nuclear Power Stations 1 and 2 by this Board as it was constituted. This may be the Atomic Safety and Licensing Board. One of the points that was brought up and particularly emphasized: "It is evidence from the foregoing that the design basis and criteria utilized in the seismic re-evaluation of the Diablo Canyon Station for the postulated Hosgri are in certain cases less conservative than those that would be used for an original design. The committee believes, however, that these are offsetting factors that lead to the acceptance of these bases and criteria for an already completed plant," and I emphasize "already completed plant."

They include: "(1) The fact that the committee's consultants believe that the choice of the magnitude, seven and a half, for the postulated Hosgri event is relatively more conservative than the values considered acceptable for other plants.

"(2) Because to the extent and depth of the staff's review of the applicant's seismic re-evaluation, the likelihood of an undetected error in the seismic analysis or design is greatly reduced," and that is July 14th, 1978, about three years before a PG&E engineer found some transposition of plans and piping diagrams.

I guess what I am saying here is question this much more deeply than the presentations that you have heard as technical presentations. There is a lot to be learned, but I have seen a total flip-flop from a position in 1971 to what seems to be taking place now.

As I stated in my document here, it my recommendations that you question PG&E and NRC staff under oath to determine what sensitivity studies have already been made, what are the tentative findings, when were they made and by whom, I cannot believe that this is coming up on the basis of a paper, the preprints of which came into being apparently in March of this year.

Thank you, and I will try to answer any questions or touch on any portion of this that you would like.

MR. OKRENT: Do you have any questions for Mr. Mendes?

(No response.)

MR. OKRENT: Thank you.

We will go on now to the presentation by PG&E, and I do have 15 minutes allocated for it and no more.

MR. BRAND: Thank you, Dr. Okrent.

I would like to ask Mr. Larry Wight of Terra who will be making our presentation.

MR. WIGHT: Thank you. My name is Larry Wight from Terra Corporation. Dr. Okrent, I will require a lot less than 15 minutes.

I would basically just like to briefly try to put Dr. Rothman's bounding cases of ground motion into a context in two way, if I could.

Firstly, and perhaps very important, I think it is important to note that the benchmark that we should compare those numbers against is the USGS Circular 672 predictions for free field ground motion which corresponded to 1.15G as a peak reading.

And according to Circular 672, Dr. Newmark used that number in conjunction with the San Fernando Pacoima Dam record to develop a design spectrum. This has been characterized as effective acceleration, but I think it is very important to note that the largest acceleration recorded at that time was carried into the design criteria and it was on a thrust fault.

The second point that I would like to make concerns the results of Dr. Campbell. Dr. Campbell generated essentially all of those results while he was working directly with me at Terra. He left Terra last year and joined the USGS. But I would like to comment briefly on the useability of those results, in particular their application to a thrust fault environment.

The study was put together as a generic study to look at ground motion in general and not for a specific site and it attempted to distinguish between strike slip and thrust almost as an aside, I would have to say. Less than one-third of the data came from thrust faults and the predominant amount of data in the data base came from strike slip. And the data recording stations for all types of earthquakes were on a variety of site conditions, on the average generally not applicable to Diablo Canyon.

So my point is in the second regard that the results from which those numbers were derived was a generic result based only in part on thrust type earthquakes.

Any questions?

(No response.)

MR. OKRENT: I don't see any. Thank you.

Well, let's see, I will assume Mr. Maxwell and Mr. Trifunac have the most the time since one leaves tomorrow and one lives in Los Angeles.

(Laughter.)

MR. TRIFUNAC: I have to give these people a ride to the airport.

(Laughter.)

MR. OKRENT: Anyway, why don't we ask Mr. Page what comments he might care to make at this time.

MR. PAGE: I have no further comments at the present time.

MR. OKRENT: Can we expect something from you prior to June 13?

MR. PAGE: I will try.

MR. OKRENT: You are not going to be going out of town immediately tomorrow or something like that?

MR. PAGE: Tomorrow, yes, I am.

(Laughter.)

MR. OKRENT: I see. Well, write on the plane or something, will you, please.

MR. PAGE: All right.

MR. OKRENT: Mr. Thompson.

MR. THOMPSON: I think you asked for comments in two general categories and one was general studies. My recommendations along the line of general studies would be of course to examine the relevant reflection seismic sections and evaluate those.

Second, to reconsider the onshore faults in the light of the possibility of an underlying thrust. I think that this might be potentially the biggest problem.

Third, the design consequences of what one finds from these first two points that needs to be looked at.

Fourth, to critically review and evaluate the regional tectonic questions. Now they may not be so relevant to the plant itself, but I think we can learn something from them that would feel more confident about the whole picture if we had some of that data. For example, does or does not the San Gregorio fault have a big displacement and, if so, what happens to the displacment to the south.

On the second question ---

MR. OKRENT: Excuse me. Could I just understand what you meant by the onshore faults?

MR. THOMPSON: There are faults which have been mapped in the seacliff I believe which do not displace coastal terrace and were judged not to be capable, but I think probably that should be looked at again.

MR. OKRENT: Thank you. I wanted to be sure I understood what the point was.

MR. THOMPSON: Your second question, Dave, I believe was does the information on the geology along this part of the coastline pose a signficiant likelihood of requiring an increase in the design or some such question, and I can't really answer that right now. My guess is probably not, but I think it really has to have a hard look first.

MR. OKRENT: Again, if you have some things to add to this that you can prior to June 14, please do.

By the way, we haven't yet addressed this question, but we would like very much for as many of you as can come to the full committee meeting. I have to check with the subcommittee members on that issue yet. We have to think about just what the nature of that meeting will be.

In any event, Dr. Maxwell.

MR. MAXWELL: Well, of course largely with what George said, but I would like to point out or just emphasize again the rather commonly found north/south stress field which dominates California and point out that this must integrate the interaction between the North American and Pacific plates. There would be no other way for it to exist.

This suggests that the thrust faulting which one finds along the trend and the Transverse Ranges, which indeed results in rather large earthquakes such as the San Fernando and the Lompoc earthquake, is not to be expected, I would think, in the much differently trending northwesterly structures which on the basis of Dr. Crouch's work almost certainly are primarily also thrust faults.

It seems to me that it is highly improbable that these will give rise to the very large earthquakes that one finds in the more east/west trend to the south.

There is also the probability or a possibility, which I think should be looked into, that much of the motion that one sees on the type of structures that Dr. Crouch was mapping is by creep rather than by sharp breakages which gives rise to earthquakes.

I don't know whether creep tests have been made anywhere across these structures. I doubt it since they outcrop in such few places and apparently are rather hard to study where they are found. But everything I see seems to indicate that the new data would be less restrictive or would indicate that the 7.5 of SSE presently used is very, very conservative indeed.

MR. OKRENT: Could I ask with regard to your comment concerning the direction in which the thrust faults trend, the Lompoc fits that category?

MR. MAXWELL: Yes, apparently so. I have been asking George about it and it seems to be essentially parallel to the fault on which the San Fernando quake occurred, along the front of the Transverse Ranges. I am not expert in these areas.

MR. CROUCH: I don't think that is really the case. The offshore Lompoc fault has a northwesterly trend very similar to the same trend as the Hosgri.

Now that gets back to the whole problem, as many of you know, that people have relocated that 7.5 quake. Hanks suggested that it could be as far as somewhere near Pt. Conception which indeed could give it a different orientation. It is fairly nebulous at this moment in terms of its location.

MR. THOMPSON: There was a very great debate about this at the time and Gothrup was on one side of it and Hanks and others had a quite different view.

The thing that was most persuasive to me at the time was the offshore seismic reflection work which showed very considerable displacements at the bottom or down in that region. Those were mapped out pretty well and they convinced me at least that this was associated with a Transverse Range trend and not with the Hosgri trend. So that was the basis of my discussion with John here earlier.Now that is going back pretty far in memory.

MR. OKRENT: Let's see, let's try Dr. Trifunac and save Dr. Luco for last.

MR. TRIFUNAC: I have confusing pessimistic remarks. I am wondering what will happen in 1988 when another paper is published.

(Laughter.)

It seems to me that progress in this particular case cannot be made within the existing framework. I think what is necessary is to significantly raise the physical quality of the analysis.

What do I mean by this? Very briefly, I mean an overall analysis going into a non-linear range if something is to be done.

MR. CARBON: Would you repeat that last statement, Mike?

MR. TRIFUNAC: Yes. I said what I mean by this very generally and primitively speaking without going into details, I mean an engineering analysis going into a non-linear range of response.

Irrespective of how much conclusive evidence can be gathered on the geology of the area, I don't believe the geological and seismological investigations will be conclusive because we cannot predict what a given fault will do in the future.

I am not sure I understand what is going to happen in the next two or three years. I am very confused there, but if I could speculate what it seems to me would be useful to do in the next couple of years, it would be to review the overall analysis very carefully and not input data and input considerations which one way or another are presently going into the same black boxes that they have been going into for a long time.

It seems to me what is needed is a highly advanced, independent engineering analysis group that must have a broad, and I am underlying a "broad understanding" of all aspects of the problem, a group that understands what is the consequence of a geological input parameter to a dynamic response to a particular point.

Finally, I have a question, and I understand that Dr. Luco may talk about this more. I would like to know whether there are some recordings inside containment and outside, i.e., free field or analogue environments, during the last five years or six or seven years or so forth on strong motion instruments that were installed at the site at various stages of development.

I would like to suggest that if there are such recordings, that some of the answers to our uncertainties might lie in the analysis of that data, even though it is small, but we certainly would be able to calibrate such estimates as damping frequencies, the famous tau effect, soil structure interaction and so forth.

This is all I have.

MR. OKRENT: You are not volunteering any particular comment on the second question.

MR. TRIFUNAC: I am sorry, which one was the second question?

MR. OKRENT: Whether the new geologic information is likely to lead to a significantly larger design basis.

MR. TRIFUNAC: I will be very pessimistic about this as well in the sense that even if it is conclusive, I don't see what can be done about it because there is already a certain type of information for ten years that hasn't been used in the way it could have been used.

I mean let's be practical about it. Somebody comes along and says this is definitely the case and everybody agrees that this is the way the fault looks like and this is the type of motion of in the fault, and what are you going to do with it?

I mean I could see a whole sequence of very serious analyses where somebody asks the question well, is the plant on the downthrusted or upthrusted side of the fault? I think Mr. Etherington asked a question like that a short while ago, and is the ground motion going to be different on one side or the other. Well, of course, it may be very different and can you tell this from the presently available recordings? No. Can you tell this from some synthetic analysis? Perhaps yes. But then those are going to be questionable, too, to some people.

I have serious doubts that any conclusive geological investigation will swing the pendulum on way or another.

MR. OKRENT: Dr. Luco.

MR. LUCO: I would like to reinforce Dr. Trifunac's suggestion that strong motion data recordings within the plant be used as part of these studies.

I understand that a number of strong motion records have been obtained for perhaps four or five earthquakes within the plant. Although it is very likely that these are small amplitude excitation, still a significant amount of information could be obtained. We could learn about interaction effects, we could learn about embedment effects, it could be possible to calibrate analytical methods to solve the interaction problem, one could calibrate techniques as to structural response, low amplitude estimates of damping could be obtained and so.

So I strongly suggest that this study should be conducted. I would also suggest that this paper should be made available to various researchers so that independent studies could be performed.

We have to keep in mind that these are small amplitude vibrations, but still much could be learned.

In general the proposed study taking about three years to complete appears to me to be excessively long. It seems that we would repeat essentially what has been done in the last six years.

My recommendation would be to use a relatively high value for the input to the plant, conduct an inelastic analysis and establish that even with this relatively high motion that safety can be maintained. I would think that that type of study would not take three years to undertake.

Also, I would not be as sensitive to changes as new information is being found about changes in fault mechanisms or the position of faults and so on if one considers a sufficiently high input motion to start with.

Those would be my comments.

MR. OKRENT: I wonder if I could ask PG&E, do records exist for earthquakes inside and outside containment at Diablo Canyon?

MR. BRAND: Yes. We do have information from our seismic instrumentation system. The staff has requested this information from us in our meeting that we had only two weeks ago with them and we agreed and will be supplying that information.

MR. JACKSON: I might add on that that I think you are really getting into the elements of what a program would contain. I think we strongly inferred to PG&E that in any future studies in the soil structure interaction and building effects area, those records should be looked at very carefully in terms of future analysis and understanding and I think they agreed to begin doing that.

MR. OKRENT: Let me ask the subcommittee members if they have any questions for our consultants?

MR. CARBON: I do to Dr. Luco. If you answered our second question, I missed it. Do you have any comment on that?

MR. LUCO: Using as a basis for discussion the accelerations that have been presented by NRC for the thrust fault of different magnitudes, these numbers are not significantly different from what I have considered before.

So in my mind I don't see a significant change, but perhaps it reinforces a little bit the difference between what was actually used for design and what could happen under some very rare events. The accelerations that are actually used are significantly low I think than the ones appearing in this paper.

MR. OKRENT: Any other questions for our consultants at this time?

(No response.)

MR. OKRENT: Could I try the following out on the subcommittee. With regard to the full committee meeting, first, I am assuming we don't need a second subcommittee meeting. There isn't information we have requested that we would like to see the day before the full committee meeting. We had a short subcommittee meeting as a possibility, but I assuming that that is not a necessity unless I hear different.

It seems to me that it will be somehow relevant for the full committee to have a somewhat detailed discussion of both subjects and that they should not rely on a report from the subcommittee alone on either subject. This is my assumption, unless I hear different from you.

Let me ask, Dr. Savio, how much time was originally allowed for?

MR. SAVIO: Two hours.

MR. OKRENT: I wasn't there when that estimate was made.

(Laughter.)

I would suggest you work with Mr. Fraley to make for a longer time like three and a half hours.

I think we somehow need to have the essence of the paper by Mr. Crouch presented to the committee, but I am not sure of his availability then. It might that if he is unavailable that the staff would volunteer to give a very objective summary, but I will let Dr. Savio check with him.

I suspect that the full committee would appreciate it if as many of the consultants as are available came to the full committee meeting because there will be many others of the committee who have not had a chance to hear this.

Any differences with my guess as to how we should proceed?

Please feel free.

MR. CARBON: I would only add to it that I think the other members of the committee would probably find it useful and helpful if they received transcripts of the consultants' comments of today because there may be different thoughts conveyed there than what will end up in the letters which they will be preparing. Some comments may be left out, for example.

MR. SIESS: You mean all the comments or just those on this last round?

MR. CARBON: Just those on this last round.

MR. SIESS: Even I would like to have those.

MR. CARBON: I would, too.

MR. OKRENT: Any other comments from the subcomittees?

(No response.)

MR. OKRENT: I will have to talk with Dr. Siess and Dr. Savio with regard to a specific agenda. So you will hear from us later as far as the staff and PG&E are concerned.

Are there any additional comments the staff wants to make at this time?

MR. KNIGHT: No, sir.

MR. OKRENT: Any additional comments PG&E would like to make?

MR. BRAND: I don't believe so, Dr. Okrent.

MR. OKRENT: Well, in that case I guess we can adjourn this meeting and I also do so.

Thank you all.

(Whereupon, at 6:16 p.m., the meeting was adjourned.)

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CERTIFICATE OF PROCEEDINGS

This is to certify that the attached proceedings before the NRC COMMISSION

In the matter of: ACPS-Subcommittee on Extreme External Phenomena

Date of Proceeding: 24 May 1983

Place of Proceeding: Los Angeles, Calf.

were held as herein appears, and that this is the original transcript for the file of the Commission.

Mary C. Simons

Official Reporter - Typed

Mary C. Simons

Official Reporter - Signature

#1

STAFF PRESENTATION ON
SPECIFIC ELEMENTS OF DIABLO CANYON LICENSE CONDITION
AND
IMPACT ON DESIGN BASIS GROUND MOTION OF THE NEW INFORMATION ON FAULTING NEAR DIABLO CANYON

BEFORE THE

DIABLO CANYON AND EXTREME EXTERNAL PHENOMENA SUBCOMMITTEE MEETINGS

MAY 24, 1984

I. INTRODUCTION & BACKGROUND

A. OBLIGATION OF NRC & UTILITIES TO KEEP UP WITH SCIENCE AND UPDATE ANALYSES

B. EXTENSIVE INVESTIGATIONS CONDUCTED OFFSHORE BY THE OIL INDUSTRY SINCE 1978.

C. ACRS 14 JULY 1978 LETTER RECOMMENDING SEISMIC DESIGN REEVALUATION IN 10 YEARS

D. 1983 - COMMISSIONERS QUESTIONED TAU EFFECT

E. EARLY 1984 - FEBRUARY, DE MANAGEMENT, IN ANTICIPATION OF THE NEED TO TAKE SOME KIND OF ACTION ON THE ABOVE ACTIVITIES, REQUESTED GSB TO RECOMMEND TASKS TO UPDATE AND REASSESS THE SEISMIC DESIGN OF DIABLO CANYON

F. INITIAL CONDITIONS TO THE OL FOR THE SEISMIC REEVALUATION OF THE PLANT WERE DEVELOPED

G. CROUCH ET AL PAPER (NOTIFIED BY PG&E ON 22 MARCH 84) INTERPRETS COASTAL FAULTS AS BEING PREDOMINANTLY THRUST-NOT STRIKE-SLIP

H. PROPOSED LICENSE CONDITION DEVELOPED

1. COMMISSION VOTED IN FAVOR OF A GENERAL LICENSE CONDITION MARCH 27, 1984, WITH THE SPECIFIC ELEMENTS DEVELOPED IN CONSULTATION WITH THE UTILITY AND ACRS

"PG&E SHALL DEVELOP AND IMPLEMENT A STATE-OF-THE-ART PROGRAM TO REVALIDATE THE SEISMIC DESIGN BASES USED FOR DIABLO CANYON. PG&E SHALL SUBMIT FOR NRC STAFF REVIEW AND APPROVAL THE PROPOSED PROGRAM PLAN AND PROPOSED SCHEDULE FOR IMPLEMENTATION BY JANUARY 30, 1985. THE PROGRAM SHALL BE COMPLETED AND FINAL REPORT SUBMITTED TO THE NRC BY JULY 1, 1988."

2. COMMISSION MADE THE PARAGRAPH A CONDITION OF THE LOW POWER LICENSE ON APRIL 13, 1984

II. LICENSE SEISMIC CONDITIONS

A. PROCEDURE

1. PGE&E
   - A. PREPARE PROPOSED PROGRAM AND SUBMIT TO NRC
   - B. TAKE LEAD IN CARRYING OUT PROGRAM

2. NRC STAFF AND ADVISORS (NAT'L LABS, USGS & DR. SLEMMONS)
   - A. REVIEW & APPROVE OF PG&E PROGRAM
   - B. REVIEW RESULTS OF PROGRAM
   - C. PERFORM AN INDEPENDENT PROGRAM

B. CONDITION 1 - UPDATE GEOLOGY & SEISMOLOGY

1. REQUIREMENTS

A. EVALUATE POST-1979 ASLB HEARING INFORMATION

B. REEVALUATE SELECTED PRE-1979 DATA THAT MAY BE NEEDED TO FILL IN GAPS IN THE NEW DATA, USING NEW REPROCESSING TECHNIQUES

2. PURPOSES

A. CONFIRM CHARACTER OF HOSGRI AT DEPTH

B. CONFIRM OVERALL LENGTH OF HOSGRI IN LIGHT OF THRUSTING HYPOTHESIS

C. CONFIRM RECENCY OF LAST MOVEMENT & DETERMINE RECURRENCE

D. CONFIRM THAT THERE ARE NO SIGNIFICANT THRUST SPLAYS CLOSER TO SITE

C. CONDITION 2 - REEVALUATE THE SSE

1. REQUIREMENTS - MAGNITUDE OF SSE
   - A. FAULT LENGTH
   - B. RUPTURE LENGTH
   - C. SLIP RATE
   - D. MAXIMUM DISPLACEMENT FROM SINGLE EVENT
   - E. HISTORICAL SEISMICITY
   - F. OTHER APPROACHES SUCH AS AREA OF FAULT PLANE TO ESTIMATE MAGNITUDE

2. REASONS
   - A. NEW DATA ON GEOLOGY AND TECTONICS OF COASTAL CALIFORNIA THAT MUST BE TAKEN INTO ACCOUNT
   - B. NEW TECHNIQUES FOR ESTIMATING MAGNITUDE FROM GEOLOGICAL RECORD
     - (1) LENGTH OF FAULT
     - (2) LENGTH OF RUPTURE DURING SINGLE EARTHQUAKE
     - (3) SLIP RATE
     - (4) MAXIMUM DISPLACEMENT FROM SINGLE EARTHQUAKE
     - (5) AREA OF RUPTURE SURFACE DURING EARTHQUAKE

D. CONDITION 3 - REVALIDATE GROUND MOTION AT THE SITE

1. REQUIREMENTS

   A. REGRESSION ANALYSIS - HORIZ. & VERT. SPECTRAL VALUES FOR SITE SPECIFIC CONDITIONS
   B. SITE SPECIFIC SPECTRA (VERT. & HORIZ.)
   C. EARTHQUAKE NUMERICAL MODELLING STUDY USING MODERN TECHNIQUES
   D. SOIL-STRUCTURE INTERACTION EFFECTS

2. REASONS

   A. MORE RECENT NEAR-FIELD RECORDINGS THAT SHOULD BE TAKEN INTO ACCOUNT
   B. MODELLING STUDY ALLOWS FOR SENSITIVITY STUDY
   C. SOIL-STRUCTURE INTERACTION ANALYSIS TO EVALUATE THE EFFECT OF STRUCTURES ON THE GROUND MOTION

E. CONDITION 4 - ASSESS THE SIGNIFICANCE OF THE RESULTS OF CONDITIONS 1, 2 & 3 WITH RESPECT TO DESIGN & CONSTRUCTION

1. REQUIREMENTS
   A. SEISMIC PRA
   B. IF NECESSARY - DETERMINISTIC ESTIMATES OF SEISMIC CAPABILITY OF SELECTED STRUCTURES SYSTEMS, OR COMPONENTS

2. REASONS
   A. ASSESS SIGNIFICANCE OF ANY DIFFERENCES BETWEEN EXISTING SEISM. DESIGN BASIS AND THAT RESULTING FROM PREVIOUS 3 CONDITIONS
   B. LIMITED DETERMINISTIC ANALYSIS CAN BE USED TO BETTER DEFINE SPECIFIC SEISMIC MARGINS

III. PARALLEL STAFF EFFORTS

A. CONDITION 1

1. REVIEW DATA PROVIDED BY PG&E
2. SOME ANALYSIS OF INDEPENDENTLY ACQUIRED DATA
3. USGS AND DR. SLEMMONS, ADVISORS

B. CONDITION 2

1. REVIEW OF PG&E ANALYSES
2. INDEPENDENT ASSESSMENT OF SSE MAGN.
3. ADVISORS - USGS & DR. SLEMMONS

C. CONDITION 3

1. REVIEW PG&E ANALYSIS
2. ADVISORS - NATIONAL LAB's & USGS

D. CONDITION 4

1. REVIEW PG&E'S PRA
2. ADVISORS - NAT'L LABS & USGS

E. SENIOR ADVISORY REVIEW PANEL, OR PANELS, TO REVIEW RESULTS

IV. PROGRESS REPORTING AND SCHEDULING

A. PROGRESS REPORTS

1. QUARTERLY PROGRESS REPORTS
2. SEMI-ANNUAL MEETINGS IN BETHESDA

B. SCHEDULE

1. PG&E SUBMIT PROPOSED PROGRAM - JAN 30, 1985
2. PROGRAM COMPLETED AND FINAL REPORT SUBMITTED 3 YEARS AFTER APPROVAL BY THE NRC STAFF

NRC STAFF POSITION AS TO THE IMPACT ON DESIGN BASIS GROUND MOTION OF THE NEW INFORMATION ON FAULTING NEAR DIABLO CANYON

I. SIGNIFICANCE OF NEW FINDINGS

II. IMPACT OF NEW FINDINGS

III. WHAT THE RESULTS OF STUDIES UNDERTAKEN IN RESPONSE TO THE LICENSING CONDITIONS ARE EXPECTED TO ACCOMPLISH

I. SIGNIFICANCE OF NEW FINDINGS

A. HOSGRI MAY BE THRUST FAULT DIPPING ENE AND COULD PASS BENEATH SITE CLOSER THAN 5.8 KM PREVIOUSLY ASSUMED

B. CHARACTER OF GROUND MOTION MAY BE DIFFERENT (THRUST VS. STRIKE-SLIP COULD BE HIGHER, LOWER OR NO DIFFERENT)

C. GEOLOGICAL DATA INDICATE LOW EARTHQUAKE RECURRENCE INTERVAL

1. NO SEA FLOOR OFFSET
2. SISQUOC FORMATION (PLIOCENE) NOT OFFSET
   (2 MILLION YEARS OLD)

D. ANALYSIS OF SEVERAL RECENT EARTHQUAKES SHOW THRUST MECHANISMS - CURRENT ACTIVITY ON THRUST FAULTS

E. MAPPED SITE FAULTS NOT CAPABLE - BUT IF FAULT GEOMETRY IS DETERMINED TO BE DIFFERENT, ADDITIONAL CONFIRMATION OF LOCATION MAY BE PRUDENT

II. IMPACT OF NEW FINDINGS

BASED ON OUR PRELIMINARY REVIEW OF THE CROUCH ET. AL. PAPER, THERE IS NO IMMEDIATE REASON TO MODIFY PREVIOUS CONCLUSIONS ON SEISMIC DESIGN BASES

A. TECTONIC IMPACT

1. THE ARTICLE IS OF HIGH QUALITY, BUT ITS THEORIES AND BASIC THESIS NEED TO BE REVIEWED IN THE TOTAL CONTEXT OF CALIFORNIA TECTONICS

   A. NOT CLEAR WHERE NORTH OF POINT SAL, MAJOR DEFORMATION MODE CHANGES FROM THRUST TO STRIKE-SLIP
   B. CHARACTER OF THRUST FAULTS AT DEPTH AND NEARER TO THE SITE ARE UNKNOWN
   C. EXISTENCE OF A SOLE THRUST UNKNOWN

2. COMPRESSIONAL ASPECTS OF FAULTING WAS CONSIDERED DURING LICENSING ACTIVITIES (1927 LOMPOC EARTHQUAKE)

3. GEOLOGICAL DATA INDICATE LOW EARTHQUAKE RECURRENCE INTERVAL

B. IMPACT ON MAPPED SITE FAULTS

1. ONSHORE FAULTS IN IMMEDIATE PROXIMITY TO SITE HAVE BEEN SHOWN TO BE NON-CAPABLE. THE NEW INFORMATION IS NOT EXPECTED TO CHANGE THAT, BUT IT MAY BE PRUDENT TO REEXAMINE THE DATA IN LIGHT OF A PREDOMINANTLY THRUST SENSE OF MOVEMENT

C. IMPACT ON GROUND MOTIONS

1. GROUND MOTION ESTIMATES USING DATA ON WHICH PAPER IS BASED MAY BE HIGHER THAN PREVIOUSLY ASSUMED, HOWEVER, THEY COULD ALSO LEAD TO LOWER ESTIMATES OF MAGNITUDE AND GROUND MOTION

2. A NEAR FIELD, LARGE EARTHQUAKE HAS BEEN CONSIDERED IN THE DESIGN

D. LICENSING CONDITIONS - CONSIDERATION OF THE NEW INFORMATION IN THIS ARTICLE AND ALL OTHER CURRENT AND NEAR-FUTURE DATA WILL BE INCLUDED IN THE LICENSING CONDITIONS

III. WE EXPECT THE RESULTS OF STUDIES UNDERTAKEN IN RESPONSE TO THE LICENSING CONDITIONS WILL:

A. INDICATE THE CHARACTERISTICS OF FAULTS OF HOSGRI FAULT ZONE AT DEPTH AND DETERMINE MINIMUM DISTANCE FROM SITE

B. SHOW WHETHER OR NOT THERE ARE OTHER FAULTS IN THE SITE VICINITY - FOR EXAMPLE, UNDERLYING THE HEADLANDS

C. DETERMINE RATIO OF STRIKE SLIP TO THRUST MOVEMENT

D. DETERMINE RECENCY OF MOVEMENT

E. DETERMINE OVERALL THRUST RUPTURE LENGTH OF HOSGRI

F. DETERMINE LENGTH OF RUPTURE DURING SINGLE EARTHQUAKE

G. PROVIDE INFORMATION ON SLIP RATE

H. DETERMINE MAXIMUM DISPLACEMENT DURING SINGLE EARTHQUAKE

I. DETERMINE AREA OF RUPTURE SURFACE DURING EARTHQUAKE

J. PROVIDE ANALYSIS OF MORE RECENT NEAR-FIELD RECORDINGS

K. PROVIDE MODELLING STUDY W/SENSITIVITY STUDY

L. PROVIDE SOIL-STRUCTURE INTERACTION ANALYSIS

M. ASSESS THE SIGNIFICANCE OF ANY DIFFERENCES BETWEEN NEW FINDINGS AND EXISTING SEISMIC DESIGN BASIS

N. PROVIDE LIMITED DETERMINISTIC ANALYSIS THAT CAN BE USED TO BETTER DEFINE SPECIFIC SEISMIC MARGINS

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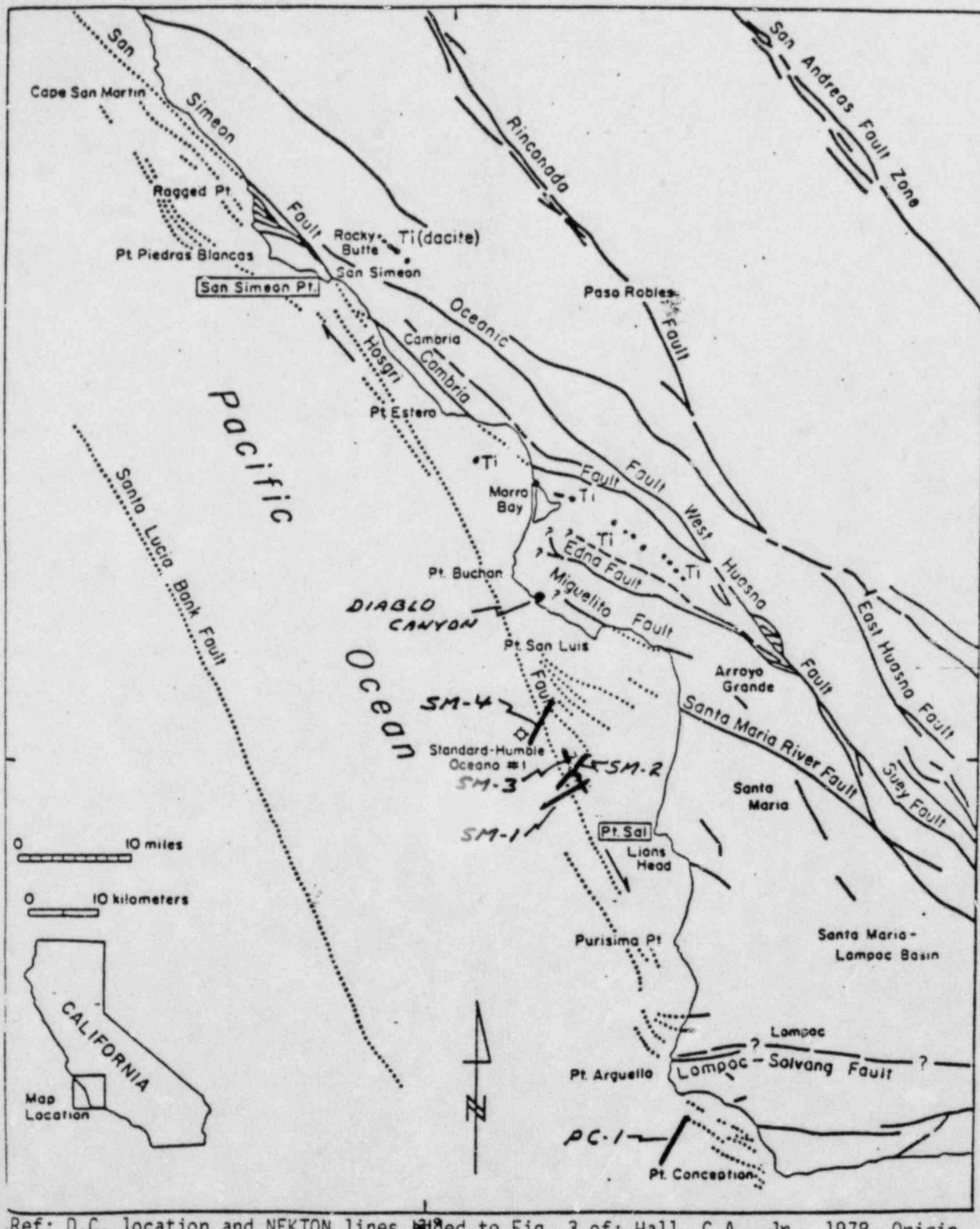
10,797 miles
Santa Barbara
2,205 miles
Point Conception (Anacapa)
890 miles
Channel Islands
1,566 miles
Los Angeles
Outer Banks
2,118 miles
San Nicholas
3,343 miles
San Clemente
734 miles
San Diego
San Diego
Southern California



Ref: D.C. location and NEKTON lines added to Fig. 3 of: Hall, C.A., Jr., 1978, Origin and Development of the Lompoc-Santa Maria Pull-Apart Basin and Its Relation to the San Simeon-Hosgri Strike-Slip Fault, Western California: in CDMG S.R. 137.

USING CAMPBELL (1983) FOR A MAGNITUDE 7.5 AT 5.8 KM

FAULT TYPE	AVERAGE PVA(G)	84% PVA(G)
STRIKE SLIP	0.51	0.77
THRUST	0.73	1.09

USING CAMPBELL (1983) FOR A MAGNITUDE 7.5 AT 2.5 KM

THRUST FAULT

AVERAGE	PHA	0.74G
84%	PHA	1.08G
AVERAGE	PVA	1.1G
84%	PVA	1.6G
RATIO	PVA/PHA	1.5

USING CAMPBELL (1983) FOR A MAGNITUDE 7.5 AT 5.8 KM

FAULT TYPE	PVA/PHA
STRIKE SLIP	1.21
THRUST	1.23

HOSGRI REANALYSIS BASED ON MAGNITUDE 7.5 STRIKE SLIP EARTHQUAKE AT 5.8 KM. FREE FIELD PGA 0.75G

USING CAMPBELL (1983) FREEFIELD ESTIMATES

MAG.	FAULT	DIST.	AVERAGE PGA (G)	84% PGA(G)
7.5	STRIKE SLIP	5.8	0.42	0.61
6.5	THRUST	2.5	0.64	0.93
6.5	THRUST	5.8	0.44	0.65
7.0	THRUST	2.5	0.69	1.02
7.0	THRUST	5.8	0.52	0.75
7.5	THRUST	2.5	0.74	1.08
7.5	THRUST	5.8	0.59	0.86

UNITED STATES
NUCLEAR REGULATORY COMMISSION
WASHINGTON, D. C. 20555

MAY 21 1984

MEMORANDUM FOR: James P. Knight, Assistant Director for Components & Structures Engineering
Division of Engineering

FROM: Robert E. Jackson, Chief
Geosciences Branch
Division of Engineering

SUBJECT: PRELIMINARY SUMMARY AND EVALUATION OF ARTICLE CONTAINING NEW INFORMATION OR INTERPRETATIONS OF FAULTS IN THE NEAR OFFSHORE OF CENTRAL COASTAL CALIFORNIA (INCLUDING THE HOSGRI FAULT NEAR DIABLO CANYON)

On March 22, 1984, representatives of Pacific Gas and Electric Company (PG&E) informed the Geosciences Branch that they had received a preprint of an article entitled, "Post-Miocene Compressional Tectonics Along the Central California Margin", by J. K. Crouch and others, of Nekton, Incorporated. This paper was presented at the annual meeting of the Pacific Section of the American Association of Petroleum Geologists in April, 1984. It is published in "Tectonics and Sedimentation along the California Margin," J. K. Crouch and S. B. Bachman, Editors, Pacific Section of the Society of Economic Paleontologists and Mineralogists, 1984.

We have completed a preliminary review of the article, had discussions with the principal author of the paper, PG&E and their consultants and brief discussions with the U. S. Geological Survey. We have attached our preliminary assessment of the potential effects that the interpretations contained in the paper could have on the seismic licensing aspects of Diablo Canyon.

The primary potential effect stems from the observation that the faults offshore may be more compressional in nature (one side of the fault moving up and over the other side) than previously understood, which was dominantly strike-slip (opposite sides of the fault moving past each other). The observations in the paper indicate that the Hosgri fault could bend over at depth and project beneath the site, possibly in closer proximity than the 5.8 kilometers now used for the Hosgri earthquake distance.

Based on our preliminary review we see no immediate basis for modifying our previous conclusions regarding the seismic design bases at Diablo Canyon. We base this conclusion on the following observations:

1. The paper has just become available and, although in our judgement it appears to be a quality publication, the theories and basic thesis contained in it need to be reviewed in the total context of our evolving knowledge of California tectonics. In the geologic and seismic area, many new hypotheses evolve and are often modified substantially with time. It would, therefore, be premature at this time to assume all information contained and inferences drawn as established fact or fully accepted theory.

2. During licensing activities for Diablo Canyon, this compressional aspect of faulting was included to varying degrees in the specification and validation of the ground motion, and although new ground motion estimates using information in the paper may be higher than previously assumed, the observations could also lead to lower estimates of both magnitude and ground motion.

3. It is currently assumed that a magnitude 7.5 event can occur 5.8 kilometers from the site with a resulting .75g ground motion. This facility, therefore, already considers a near field event in its design.

4. All onshore faults in immediate proximity to the site have been shown to be non-capable and this new information would not change that conclusion.

Finally, we have recommended and the Commission made it a condition of the Low Power License that a validiation of the seismic design bases be provided by July 1, 1988. This condition has been required to assure that all new information is evaluated by the most modern techniques. Therefore, all new information, such as that contained in this paper, would be incorporated into that analysis. In view of this license condition, we recommend that licensing action proceed and the new information be factored into the future validiation effort.

Robert E. Jackson

Robert E. Jackson, Chief
Geosciences Branch
Division of Engineering

Attachment:
As stated

cc: See next page

cc: w/attachment
T. Novak
R. Jackson
G. Knighton
A. Thadani
G. Lear
S. Brocoum
L. Reiter
P. Quo
H. Scherling
D. Gupta
R. Rothman
R. McMullen
H. Polk
L. Chandler
T. Algermissen, USGS

PRELIMINARY SUMMARY AND EVALUATION OF
ARTICLE CONTAINING NEW INFORMATION OR INTERPRETATIONS
OF COASTAL CENTRAL CALIFORNIA
INCLUDING THE HOSGRI FAULT SYSTEM
NEAR DIABLO CANYON

An article entitled, "Post-Miocene Compressional Tectonics Along the Central California Margin", by James Crouch, Steven B. Bachman, and John T. Shay was made available to the NRC staff on March 22, 1984, by the Pacific Gas & Electric Company (PG&E) utility. The major thesis of this paper is that most of the fault systems west of the San Andreas Fault in Central California, including the Hosgri Fault system, may not consist of vertical planar fault surfaces in which the movement is strike-slip (i.e., the major component of movement is parallel and horizontal along the fault trace), but may have fault surfaces which curve and flatten out at depth toward the northeast. The authors suggest that all these faults, including the Hosgri fault system, are predominantly thrust faults which have resulted from movement in which the overlying blocks to the northeast have slid up and over the underlying blocks to the southwest. This interpretation could indicate that the Hosgri fault system, which passes on the seafloor about 5.8 kilometers (km) to the west of the site, may curve and flatten at depth, and may be extrapolated to pass beneath the plant site at distances less than 5.8 km. The seismic reflection lines also indicate to the authors that no movement has occurred on the Hosgri fault since late Pliocene time, over 2 million years ago. However, continued earthquake activity and fault plane solution determinations for the region, which are generally compatible with the thrust fault interpretation, indicate that earthquakes can probably still be associated with these faults. The new information that suggests this apparent different interpretation of the Hosgri fault system consists of several high resolution seismic reflection profiles which have been processed by state-of-the-art techniques to migrate subsurface structures to their correct geographic locations. These seismic reflection profiles were taken offshore from a few miles south of Point Buchon, near Diablo Canyon south to about Capitan in the Santa Barbara channel. Although interpretation of only a few lines were shown in the preprint, hundreds of similar proprietary lines exist within the oil industry, and reportedly show similar thrust faults. Based on discussions with one of the authors, additional proprietary information, including seismic reflection lines and well data, also influenced their scientific determinations.

With regard to their thesis of compressional tectonics onshore west of the San Andreas fault, the authors cite numerous scientific publications which when considered in their entirety, suggest to them that tectonic movement to the southwest on northeast dipping thrust faults is the major mode of deformation in that region. Evidence that the authors cite includes geometry of the folds, parallelism of the faults and folds, fault plane solutions of recent earthquakes, and plate tectonic motions reported by previous investigators. The paper appears to be well thought out, written and documented.

There are several possible implications of this hypothesis on the seismic design of Diablo Canyon.

1. The Hosgr' fault system, which was used as the controlling structure in determining the magnitude of the controlling earthquake in the seismic reanalysis of Diablo Canyon may approach the plant closer than the 5.8 km stated in the SER.
2. Faults in the vicinity of the site may be splays of the Hosgri fault system if it is interpreted to pass under the site.
3. The ground motion resulting from the controlling earthquake on a thrust fault under the site may be different than that used in the SER. Also, as the geometry of the fault may be different (gently dipping) than that assumed by the applicant (vertical), the magnitude of the controlling earthquake calculated from a thrust fault may differ from the magnitude 7.5 that was assumed by the U. S. Geological Survey and NRC staff.

The interpretation of the seismic reflection profiles suggests that the Hosgri fault system begins curving toward the shore at depths of about 2 1/2 km. Data further to the Northeast (nearer Diablo Canyon) is not presented in the article. If the fault is assumed to have a horizontal attitude as it is extrapolated to the northeast of the seismic reflection lines, based upon this most conservative estimate, it could pass under Diablo Canyon at a depth of about 2 1/2 kms. If the interpretations and extrapolations are indeed correct, the faulting would most likely pass beneath the site at a depth considerably greater than about 2 1/2 kms. Observations of well-studied overthrust belts elsewhere suggest that thrust faults continue increasing in depth and eventually flatten out along a common fault referred to as the sole fault at the base of the system of thrust faults, which is usually much deeper than 2 1/2 kms. The model postulated by the authors shows the sole thrust to be 10-20 kms deep. There is, however, no definitive data in the article, at this time to determine if there is a sole thrust or to determine its depth beneath the site.

During site validation investigations of the Diablo Canyon site in the middle to late 1960's, several faults were found. These faults were evaluated by mapping and age dating as they were exposed along the seacliff adjacent to the site, in a network of trenches dug across the site, and in the plant excavations during construction.

Most of the faults range in offset from a few inches to several feet, are discontinuous and disappear into folds in the rock. There are several larger faults in the seacliff area with displacements on the order of tens of feet.

The ages of last movement along these faults were bracketed between 15 million years, which is the approximate age of the rocks that are cut by actions, no clear distinction was made as to which type of fault is assumed. In the most recent Appeal Board hearing, however, most of the reasoning presented assumed strike-slip faulting.

In assessing the level of ground motion from a reverse fault, several key input parameters would need to be known including:

1. The correct estimation of the earthquake magnitude. The compressional regime may require assuming an earthquake of a magnitude either higher, lower or the same as the 7.5 currently used. This may not be much different since one of the prime bases for choosing magnitude 7.5 was the occurrence of the 1927 magnitude 7.3 Lompoc earthquake which is believed by many to have occurred on a reverse fault.

2. The ratio of strike-slip to reverse or thrust motion.

3. The distance from the fault to the site. This is dependent on the extent to which the Hosgri flattens out as it proceeds eastward. If it steepens rapidly the fault could occur at a depth much greater than 2 1/2 km beneath the plant.

The estimation of near-field ground motion from a large earthquake is a difficult task frought with a large amount of uncertainty. A good deal of extrapolation and expert judgement is still required to make estimates at the magnitudes and distances needed for Diablo Canyon. The range of results assuming different fault types and distances undoubtedly will exhibit extensive overlap. For example, using a relationship by Campbell (1983) which does take these factors into account indicates that the 0.75g peak free field ground acceleration already assumed for the Diablo Canyon site would be a median estimate for the "worst case" (a reverse or thrust fault at about 2.5 km) magnitude 7.5 earthquake and a somewhat greater than mean plus one sigma estimate for the magnitude 7.5 earthquake on a strike-slip fault at 5.8 km distant from the site. These free field accelerations do not take into account reductions for buildings and/or embedment effects, spectral amplifications or the significance of the thrust motion assumption on the vertical acceleration determination. Other investigators and/or techniques could possibly yield higher or lower estimates. It is apparent, however, that the existing design basis will accommodate large near field earthquakes of different types at different distances. Uncertainty exists, and will continue to exist, however, as to defining the precise level of conservatism for each different scenario.

Reference

Crouch, James K., Steven B. Bachman, and John T. Shay, "Post-Miocene Compressional Tectonics Along the Central California Margin," in Crouch, J.K., and Bachman, S.B., Eds., 1984, Tectonics and Sedimentation Along the California Margin: Pacific Section S.E.P.M., Vol. 38, p. 37-54.

Chairman Salzman, the chair of the 3 man ASLAB (which held Diablo seismic hearings in fall 1980) was appointed to a federal judgeship by President Reagan shortly before he ruled that Diablo was seismically safe. This fact alone should disqualify the results of these farcical hearings which I attended in their entirety.

In the minority full NRC view about decision not to review the ASLAB decision, Commissioners Gilinsky and Bradford point out that "The Appeal Board decision deals with whether the basis of the seismic design, as formulated by the applicant and approved by the NRC staff and Licensing Board, are adequate."

Dr. James Brune, Prof. of Geophysics at UCSD, does not see the IV-79 quake as an example for a design basis conservative quake "in terms of stress drop, accelerations, velocities, and spectrum, relative to its Richter local magnitude. He goes on,"There are too few data for earthquakes of M=6.5 to M=7.5 to establish the rate of increase of average peak acceleration or spectrum of ground motion going from M=6.5 to M=7.5," though its obvious that on the average they'll be higher for a 7.5 quake.

Near-field IV-79 data indicates that values of vertical acceleration can be considerably higher than two/thirds of values of horizontal acceleration. Since each new well-recorded quake brings surprises (as IV-79), "statements that certain assumed peak accelerations are 'conservative' are necessarily cast in doubt, whereas the negative statement, that such accelerations have not been established as conservative, remains true." The ACRS' own consultants, Drs. Luco and Trifunac, tended to agree with this statement even when raked over the coals in the fall 1980 seismic hearings.

The NRC 2 Commissioners continue, the "use of the so-called 'tau effect' to permit a substantial across-the-board relaxation of the seismic standard applied to the plant, the Board's reasoning is utterly inadequate and is very likely wrong." At best, the Diablo construction permits assumed that the reactors could experience a peak of a 6.75 m quake at distance of 20 miles. The USGS predicts 7.5 as a maximum quake for the nearby Hosgri area, despite the fact of a 7.5 quake west of Lompoc in the Hosgri Fault zone on Nov. 4, 1927. The seismic evidentiary hearings concluded that Diablo should be redesigned to withstand a 7.5 quake at 5.8 km on the Hosgri fault.

Diablo's design is not conservative. Every advantage was taken of slack in safety margins left in the pre-Hosgri analysis; a larger damping value of 7% not 5% was used in analyzing structures. Credit was taken for actual 'as built' strength of materials (rather than the usual minimum required strengths) so larger vibrations became tolerable. The redesign has already shaved safety margins to the extent permissible by regulations.

Page 8 of Gilinsky-Bradford's statement explains the ridiculous nature of the ASLAB's reasoning on the "tau effect." The NRC's Office of Policy Evaluation put it this way,"Except for the judgment of Drs. Blume and Newmark, there is no evidence to demonstrate an ability to predict tau effects over a range of earthquake magnitudes, structural configurations, and site conditions."

SUBMITTED TO A.C.R.S. BY BRUCE CAMPBELL representing END NUCLEAR DUMPING in the PACIFIC
614 GRETNA GREEN WAY, LOS ANGELES, CA 90049

The DIABLO NUCLEAR REACTORS and the HOSGRI FAULT

MASS SUICIDE POTENTIAL

The HOSGRI - San Simeon - San Gregorio is one continuous fault system. It is the LARGEST SUBSIDIARY of the SAN ANDREAS FAULT, extending from coastal Santa Barbara County to NW of San Francisco, where it goes into the San Andreas Fault off coastal Marin County. The San Andreas itself is 48 miles from Diablo Canyon. The huge subsidiary which includes the HOSGRI is deemed CAPABLE of an 8.0 Richter scale QUAKE.

History of Quake Activity

Most recorded quake activity has shaken the southern Hosgri region, relating to Hosgri's linkup with the 2000 km. (1250 mi.) Murray Fracture Zone; this M.F. Zone extends from near Hawaii to the North American continent in Santa Barbara County. (Both the Hosgri and the San Andreas faults bend radically eastward where the Murray Fracture Zone dissects the edge of the continent.)

1. A 7.5 QUAKE occurred in the HOSGRI zone by Lompoc on November 4, 1927.
2. There were a series of 40 quakes with oceanic epicenters near the SW Hosgri region (one registering 5.8, with many surpassing 4.0) in 1969. These formed a line (parallel to the more south-easterly Arguello Deep Sea Canyon) 120 miles long and 20 miles wide, pointing at nearly a right angle to the Hosgri Fault in the direction of the Diablo Canyon reactors.
3. A 4.6 quake occurred on the Hosgri Fault in late May 1980.

Diablo's Placement and the Hosgri Fault

1. The main part of the Hosgri is 5.8 km. (about 3.5 mi.) out to sea from the Diablo nuclear reactors.
2. A branch of the Hosgri is 3.8 km. (about 2.3 mi.) from Diablo (according to Bucks and Buchanan of the U.S. Geological Survey).
3. A Hosgri splay to the NW is aimed at Diablo (even on a Pacific Gas & Electric map). A study, never undertaken or planned, could link this splay to an onshore fracture, which would mean that the quake energy from the Hosgri would be focused beneath the reactors.

Pacific Gas and Electric, Bechtel, the NRC, or any group related to the nuclear industry simply cannot be trusted to do an adequate job of examining either the seismic location or the magnitude of vibrations which Diablo's buildings and pipes can withstand. In the seismic design review, paperwork was sloppy at best, no document control; and criminal at worst, mass destruction and likely falsification of documents as well. PG&E cannot be trusted to do a study when more than a $5 billion investment is on the fault-line. An intensive study must be done between the Diablo reactor area all the way out to the 5.8 km distant main Hosgri, including the area between the shoreline and a mile offshore. It is very likely that the splay from the northwest even on a PG&E map discussed in the fall 1980 seismic hearings links up with an on-shore fracture, proving that a portion of the 2nd largest fault system in the state, the Hosgri, goes beneath the Diablo reactors themselves. Extrapolations from recent Crouch research indicates a similar conclusion.

May 24, 1984

Nuclear Regulatory Commission
Advisory Committee on Reactor Safeguards
Attn: Dr. Richard Savio

Dear Sisters and Brothers,

It is my opinion that seismic re-evaluation needs to take place at Diablo Canyon Nuclear Power Plant prior to considering granting a full power license to P.G. & E. The proper group to conduct a study on anything would obviously be one that did not benefit one way or the other (financially or by favor) from the results of the study.

I am attaching to this letter some concerns from San Luis Obispo citizens about the inadiquacy of an emergency response plan in the San Luis Obispo county area and request that you consider those concerns in your deliberations.

I spent some time talking to two workers from the Diablo plant while I was in the San Luis area last month. One of the workers who is in a position of high responsibility is a victim of a great deal of stress from earlier life experiences and seemed to be managing that stress with alcohol. The other one I talked to had just gotten off of work and right away bought a six pack of beer and prodeeded to down it. This person was scheduled to go back to work that evening (it was morning) for a 12 hour shift. I wonder how these people will be able to respond in an emergency situation(ie.earthquake).

I pray that we all open ourselves to the spirit of truth and love and join together for the healing and transformation of this magnificient plant.

With love,

Alberta L. Rich

Alberta L. Rich
6519 Seville #5
Isla Vista, CA 93117

A FEW FACTS YOU SHOULD KNOW ABOUT THE COUNTY NUCLEAR EMERGENCY RESPONSE PLAN FOR DIABLO CANYON

- The primary means of ALERTING the local population and emergency personnel to a nuclear emergency is by electrically-powered sirens, telephones, and local radio and TV stations. All of these are subject to failure in emergency situations;
- The County Plan states that SHELTERING will be the protective action you will take if there is less than 3 to 5 hours for evacuation. Nevertheless, with very few exceptions (e.g., two buildings at the California Men's Colony), buildings in the area, including the Emergency Operations Center, cannot shelter you from nuclear radiation (i.e., alpha particles, beta particles and gamma radiation);
- Since there are few major roads, EVACUATION is uncertain. In the winter storms of 1983, for instance, each major road was partly or totally blocked, some several times. Even in ideal weather conditions, stalled vehicles, accidents, cars running out of gas, etc., could prevent a speedy departure;
- A major inaccuracy in the County Plan is that the time estimates for EVACUATION under the worst weather conditions (including flooding or fog) are only 20% greater than under the best conditions;
- Scarce attention has been paid to EARTHQUAKES in the Plan (e.g., buildings which would be used as shelters have never been structurally evaluated). The Plan admits that evacuation could take 10 hours or longer in the event of a severe earthquake; it will take a great deal more than 10 hours if roads and bridges are substantially damaged;
- Many of the Standard Operating Procedures for towns, schools, etc. have little or no discussion of how to EVACUATE the carless population, or the disabled population, or the institutionalized population, or the private school population;
- EVACUATION is predicated on the notion that people in one area will leave while those nearby will calmly remain until told otherwise. The subject of individual or group behavior is never addressed in the County Plan;
- For those who may be contaminated, local DECONTAMINATION facilities are virtually non-existent;
- Although WIND DIRECTION is a crucial factor in a radioactive release, no extended study of wind direction at different altitudes throughout the area has ever been carried out;
- All city, county, state and federal employees may be CONSCRIPTED in the event of a nuclear emergency. Very few know this and even fewer have received any training (a recent study of local school teachers showed one-third of them would leave their students in a nuclear emergency to be with their families);
- Despite official confidence in the Emergency Plan, a full-scale DRILL (including a major evacuation) has never been held. Two lesser drills have been replete with major problems, many of which were not even mentioned in the official analysis.

WHAT YOU CAN DO:

1. Write the following congressional offices and demand that a hearing be held now on the County Emergency Response Plan:

   Representative Morris Udall
   Chair
   House Interior & Insular Affairs Comm.
   1324 Longworth HOB
   Washington, D.C. 20515

   Representative Leon Panetta
   339 Cannon HOB
   Washington, D.C. 20515

   Representative William Thomas
   324 Cannon HOB
   Washington, D.C. 20515

   Representative Edward Markey
   205 Cannon HOB
   Washington, D.C. 20515

2. Write - or call - the members of the San Luis Obispo Board of Supervisors and demand they rescind approval of the Emergency Response Plan until the above problems are remedied.

   Supervisors:
   Jerry Diefenderfer (Chair)
   Ruth Brackett
   William Coy
   Jeff Jorgensen
   Kurt Kupper

   County Government Center
   San Luis Obispo, CA 93408
   (805) 549-5450

3. Ask candidates for the Board of Supervisors for their position on the Emergency Response Plan and be prepared to follow up your query if necessary.

DIABLO CANYON IS SUPPOSED TO OPERATE FOR THE NEXT 30-40 YEARS. IF YOU BELIEVE THE PRESENT EMERGENCY PLAN ENDANGERS YOU AND YOUR FAMILY'S SAFETY AND WELL-BEING, WRITE OR CALL TODAY. THERE ARE NO MORE TOMORROWS.

Contacts:
Dr. Richard Kranzdorf
(805) 546-2842 (805) 544-3399

Dr. Barbara Stanford
(805) 549-9042

San Luis Obispo Citizens for an Effective Emergency Plan

STANLEY H. MENDES, INC.
STRUCTURAL ENGINEER
1226½ STATE ST. SUITE 7
SANTA BARBARA, CALIF. 93101
PHONE (805) 962-9870

16

This statement is presented to a subcommittee of the Advisory Commission on Reactor Safeguards, NRC, meeting in Avila, California, on May 21, 1976.

Gentlemen:

Thank you for the invitation to make a presentation. I sincerely hope that my being here will result in beneficial changes in the procedures presently followed by the NRC, formerly the AEC.

I believe that undiscovered earthquake hazards very likely exist at the Diablo Canyon nuclear power facility and at other nuclear power facilities constructed in California and elsewhere. These earthquake hazards may represent a serious threat to the health, safety, and welfare of millions of people.

My belief is based upon thirty years of experience with the design and construction of buildings and related structures to resist the effects of earthquake forces. My belief is based upon an intimate working knowledge of all facets of design, planning, and construction procedures and practices. My belief is a result of numerous on-site investigations of earthquake damaged structures and reviews of hundreds of existing buildings for earthquake related hazards. My belief results from personal experiences wherein I have witnessed the AEC and staff in concert with Pacific Gas & Electric Company engage in "coverup" activities and "stonewalling" attempts to exclude adverse testimony as to the earthquake safety of the Diablo Canyon facilities.

Following are the fundamental reasons why I believe undiscovered earthquake related hazards exist at the Diablo Canyon and other existing nuclear power facilities.

1. Present Nuclear Regulatory Commission reviews of the design and construction of nuclear facilities are inadequate, because only design criteria and procedures are reviewed. No in-depth reviews are made of the design results, construction plans and actual construction. Public school buildings and hospitals receive greater in-depth, independent reviews of earthquake safety provisions by the State of California than do nuclear power facilities.

2. Investigations of recent damaging earthquakes such as Alaska, 1964, and San Fernando, 1971, have conclusively proven that important basic earthquake design criteria previously used was based upon incorrect assumptions and inadequate knowledge about earthquake forces.

STANLEY H. MENDES
STRUCTURAL ENGINEER

 May 21, 1976

3. The present state of the art in the fields of geology, soils engineering, seismology, and various engineering specialties is such that substantial human and technical errors are possible and not at all unusual.

4. The advocate type proceedings which the NRC conducts in a semi-judicial atmosphere are not conducive to determining scientific or technical truths. Open and candid discussion among informed persons is the best way to determine scientific truth. Any such public discussion would undoubtedly reveal the many unknown factors and inadequacies which surround earthquake-resistant design and construction.

5. Citizen participation in so-called public hearings is manipulated by the NRC and permitted at such times as is convenient for the NRC and the utility company constructing the nuclear facilities.

6. Present technology is seriously limited by inadequate knowledge as to how structures really respond to earthquake forces.

Gentlemen, I would welcome a frank and candid discussion of my beliefs. I am open to any proof you may have that I am wrong!

Respectfully submitted,

Stanley H Mendes

Stanley H. Mendes, Structural Engineer

STANLEY H. MENDES, INC.
STRUCTURAL ENGINEER
3757 STATE STREET SUITE 20:
SANTA BARBARA, CALIFORNIA 93105

PHONE (805) 682-2599

May 24, 1984

visory Committee on Reactor Safeguards, NRC
eting in Los Angeles, California, May 24, 1984

en:

er consideration by this group is a proposed license on that would require Pacific Gas & Electric Company a seismic study to reevaluate the Diablo Canyon nuclear plant design basis.

u are being asked to recommend reevaluation due to discoveries that the nearby thrust fault may extend neath the plant.

Y POSITION IS VERY SIMPLE; IF THERE IS ANY REASONABLE ABOUT THE PRESENT DESIGN BASIS, THEN THE PLANT SHOULD E LICENSED AND OPERATED. ALL APPROPRIATE STUDIES SHOULD ADE PRIOR TO LICENSING, AND AS A MATTER OF FACT, SHOULD BEEN MADE PRIOR TO CONSTRUCTION.

I have carefully monitored the Diablo Canyon proceedings fourteen years. I have reviewed thousands of related uments of Record and off the record.

Over the years, there has been intense manipulation of licensing process by both PG&E, the NRC and staff. y have demonstrated a pervasive bias against public scrutiny d constructed the plant based upon inadequate offshore ologic investigations. Why? Very simple, no completed clear power plant has ever been denied a license to operate.

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STATE OF CALIFORNIA

SENATE COMMITTEE ON PUBLIC UTILITIES, TRANSIT AND ENERGY

Hearing on

NUCLEAR POWER PLANTS INITIATIVE

PUBLIC TESTIMONY

March 23, 1976

Sacramento, California

COMMITTEE MEMBERS

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Senator Nate Holden, Vice Chairman
Senator Lou Cusanovich
Senator Ralph Dills
Senator James Mills
Senator John Nejedly
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JO ANN VANICEK
COMMITTEE SECRETARY

ROOM 2035, STATE CAPITOL
SACRAMENTO, CALIFORNIA 95814
TELEPHONE: 445-9764

SENATE COMMITTEE ON PUBLIC UTILITIES, TRANSIT AND ENERGY

ALFRED E. ALQUIST
CHAIRMAN

AGENDA

HEARING ON THE NUCLEAR POWER PLANTS INITIATIVE
PUBLIC TESTIMONY

March 23, 1976
Room 4203, State Capitol
1:30 p.m.

WITNESSES:

MR. ROBERT MORETTI, Commissioner
State Energy Resources Conservation and Development Commission

*MR. RICHARD HUBBARD and **MR. GREGORY MINOR
*Former Manager of Quality Assurance
**Former Manager of Advanced Control and Instrumentation
(General Electric Company)

MR. IVAN STUART, Manager of Licensing and Safety
Nuclear Energy Division, General Electric Company

DR. ALAN D. PASTERNAK, Commissioner
State Energy Resources Conservation and Development Commission

DR. LEONARD KUNIN, Economist
Regional Energy Analysis Program, Lawrence Berkeley Laboratory, University of California

MR. KERMIT SMITH, Co-Chairperson
No on 15 Committee

MR. PATRICK MASON, Research Director
California Labor Federation (AFL-CIO)

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March 23, 1976

The Honorable Alfred E. Alquist, Chairman
and Members of
Senate Committee on Public Utilities, Transit and Energy
State Capitol Building
Sacramento, California 95814

Gentlemen:

My purpose in appearing before this committee is, hopefully, to make you concerned enough to investigate and determine, first hand, how the Nuclear Regulatory Commission (formerly the Atomic Energy Commission) really functions to supposedly provide effective earthquake safety regulation of the construction of nuclear power facilities. If you will really dig in and investigate, you will likely open up the biggest can of worms this state has seen in a long time.

I hope to convince this committee that the Nuclear Power Plant Initiative, as written, has true merit, that it is long overdue and much needed, and that you should willingly accept responsibility for determining that adequate safety provisions are incorporated into the design and construction of nuclear power facilities in California.

A proliferation of nuclear power facilities has been and is in process before <u>proven</u> earthquake safety provisions have been developed. The San Fernando earthquake of 1971 clearly demonstrated to all knowledgeable persons that there is still much to be learned before we can construct totally earthquake-proof facilities. There is still plenty of room for human and technical errors in the various disciplines needed to construct nuclear power facilities. I seriously doubt that the State of the Art is sufficiently advanced to produce the relatively risk-free facilities to which the people of California are entitled. The people should know the truth and be able to influence their destinies with respect to the use of nuclear power. The serious questions which can be raised about the adequacy of existing and proposed new plants should be discussed openly and candidly <u>in public</u>.

As a licensed Civil and Structural Engineer in California, my entire professional career of nearly thirty years has been devoted to the design of buildings and related structures to withstand the effects of damaging earthquakes. I am quite familiar with earthquake resistant design and have personally inspected and studied numerous earthquake-damaged structures. I know most of the strengths and weaknesses of my profession. Experiences during the past few years have given me some insight as to how the Nuclear Regulatory Commission really functions. Frankly speaking, their system scares the hell out of me. Here's how Big Brother really operates!

1. In the language of our times, I have personally witnessed the AEC engage in "coverup" activities and abuse their lawful powers in "stonewalling" attempts to exclude probable adverse testimony about the earthquake safety of nuclear plants. This was done in concert with Pacific Gas and Electric Company at the Diablo Canyon Nuclear Power Facilities near San Luis Obispo.

   The AEC and Pacific Gas and Electric Company have continued to construct the Diablo Nuclear Power Plant facilities for the last five years with full knowledge that the <u>basic design criteria</u> for the earthquake safety provisions of the facilities are incorrect. Why?

2.a) Public school buildings and hospitals receive greater in-depth, independent reviews of their earthquake safety provisions than do nuclear power plants constructed in California. Why?

b) No in-depth detailed reviews of earthquake safety provisions are made by NRC of design calculations and construction drawings to determine if errors have been made. Why not?

c) No in-depth detailed reviews were made by qualified staff of PG&E of the basic earthquake design criteria for the Diablo Nuclear Power Plant facilities. Why not?

3. Citizen participation in so-called public hearings is permitted at such times as is convenient for the NRC and the utility company constructing the nuclear facilities. These hearings are charades which exclude meaningful citizen participation.

4. The NRC conducts advocate type proceedings, including "discovery" procedures, in a semi-judicial atmosphere which by its very nature is not really conducive to determining scientific or technical truths. Open and candid discussion conducted in public among informed persons is the best way to determine scientific truth. This method also permits lay persons to better understand the limitations of the State of the Art.

5. The present State of the Art in the fields of geology, soils engineering, seismology and various engineering specialties is such that substantial human and technical errors are possible and not at all unusual. NRC procedures oftentimes belatedly discover substantial errors.

6. Nuclear power plants constructed as little as fifteen years ago, in accordance with knowledge then available, very possibly will not provide the necessary earthquake safety features which are required today. What is being done to review and update existing facilities?

In the interest of public health, safety, and welfare, I ask this committee to seek the truth, to continue to investigate and determine how the present system of safety regulation of nuclear power plants really works, to determine what inadequacies exist and to attempt to remedy the situation. The Nuclear Initiative is a giant step in the solution to a tremendous problem.

DISCUSSION

My discussion will be structured so as to give background information and reasons for my six (6) previous statements.

1. Nearly five years ago, several attempts were made to reopen AEC hearings on the Diablo Canyon facilities based upon new information available immediately after the San Fernando earthquake of 1971. I was consultant to Scenic Shoreline Preservation, Inc., a recognized intervener in the hearings. The AEC legal staff and Pacific Gas & Electric Company legal staff prepared briefs which said, in effect, "there's nothing new to be learned from the San Fernando earthquake" and "we used the best and latest techniques of analysis and design; therefore, there's nothing to worry about." This was all "attorney talk" unsubstantiated by the licensed Civil Engineers responsible for the design of the Diablo Nuclear Power facilities. On the basis of these representations, the Atomic Energy Commission refused to reopen the public hearings for new testimony or to permit additional cross examination of the designers of the facilities. Why?

   At that time, knowledgeable geologists, seismologists and engineers knew full well that the San Fernando events clearly demonstrated the incorrectness of many of the basic criteria and assumptions commonly made in earthquake resistant design. It was a whole new ball game!

An attempt was later made in 1972 to introduce my testimony. At that time, public hearings were held to determine whether construction should be allowed to continue pending preparation of the Environmental Impact Report. By specific Order of the Atomic Energy Commission, I was precluded from testifying. Why?

After the draft Environmental Impact Report was prepared, public hearings were held for comments. Even though the EIR included specific sections on geology, seismology, and earthquake design, I was not permitted to testify. Again, this was by specific Order of the AEC. Why?

I believe I was improperly and illegally excluded from giving testimony and participating in cross examination of the designers of the Diablo facilities because PG&E and AEC knew that the basic earthquake design criteria for the facilities was incorrect and they feared public exposure of the fact. These tactics bought them time to analyze and learn from the San Fernando experience and perhaps to determine on what basis the facilities as constructed might be accepted.

An investigation on the part of this committee will determine that even now, formal and informal discussions are taking place between PG&E and the NRC regarding adequacy of the basic earthquake design criteria for the facilities. The design is questionable because a previously unrecognized major active earthquake fault was discovered about 1972 only three miles offshore. In addition, the San Fernando earthquake of 1971 proved conclusively that ground (rock) accelerations more than three times that for which the plant was designed are possible.

During the past five years, the construction of the Diablo facilities has gone full speed ahead. The tactics of PG&E and the AEC were quite obvious; get the facilities constructed so it will be much more difficult to deny an operating permit when one billion dollars have been invested. Big money talks!

In an affidavit prepared in 1972, dated January 23, 1973, for the Diablo Canyon public hearings, I stated, based upon my investigations, "I doubt that sufficiently detailed physical explorations of the offshore fault systems have been made." This has since been proven true. Also, the basic earthquake design criteria included "--maximum rock accelerations at the site are estimated to be: -- --

Earthquake D . . . 0.20g." My comment was,

> "Again, an extremely important element related to design involves a matter of assumption and judgment and does not reflect the accelerograph record of Pacoima Dam. That accelerograph record shows numerous peaks between 0.50g and 0.70g. This record indicates <u>considerably higher accelerations</u> and for a <u>much longer period of time</u> than the above estimates. These higher accelerations occurred over a period of time of 3 to 4 times longer than the Golden Gate Park, San Francisco, 1957 record which was utilized to design for Earthquake D."

Only in January of this year has the NRC asked for justification of design based upon ground accelerations of in excess of 0.50G. This comes rather late in the game, because the operating license hearings are scheduled for June of this year. It appears that for construction to have been allowed to continue to completion, the PG&E and the NRC must have already reached a mutually agreeable understanding.

If by chance the Diablo Canyon facilities are <u>not</u> given an operating permit by the NRC, what will happen to the one billion dollar investment of PG&E? It is my understanding, based upon present Public Utilities Commission policy that the investment is <u>risk capital</u> and may not be charged to utility customers by way of rate increases.

It is my sincere hope that the present NRC proceedings questioning the earthquake safety features of the Diablo Canyon facilities are honest and forthright so as to ultimately reveal the truth. Not being privileged to sit in on the "informal" discussions between PG&E and NRC, I just don't know.

2.a,b,c) Detailed <u>independent</u> reviews are made by the State Office of Architecture & Construction for all public school buildings and hospitals which are to be constructed in California. These reviews include a check of criteria, method, and procedures. They also make a detailed check of the results of the design, including verifying that plans correctly and completely agree with design assumptions and results. In addition, independent field inspections are made to assure compliance with approved plans and specifications.

The Office of Architecture and Construction procedures contrast greatly with the NRC procedures. The NRC does not make a detailed check of analysis, design calculations and construction plans. They only "--check criteria, method and procedures." On February 18, 1975 at San

Luis Obispo, California, Mr. Larry Shao, of the Structural Engineering staff of NRC made the following statements when asked by a commission member of the Advisory Commission on Reactor Safeguards about the checking procedures of the Diablo Canyon nuclear power plant design:

> "We don't check detailed results. We only check criteria, method, and procedures. Do you know how long it would take to check a detailed analysis? It would take about four or five years." -- -- "In order to check detailed answers, I would need a staff of a thousand people to do that."

From such a procedure, it is clear and apparent to experienced engineers that human errors and mistakes will have to all be discovered by the designers of nuclear facilities. Let's have a close look at how PG&E designers of the Diablo Canyon facilities made an in house check. The seismological evaluation of the Diablo Canyon site is contained in the Preliminary Safety Analysis Report (PSAR) and set forth in reports dated January 9, 1967, and May 28, 1968. There are no significant differences in the Final SAR (FSAR) published only a couple of years ago. Under date of July 18, 1975, representatives of PG&E responded as follows, under penalty of perjury, to several significant questions contained in Interrogatories by San Luis Obispo Mothers for Peace dated June 19, 1975.

"37. Name the person or persons responsible for the review of the "Seismic Evaluation of the Diablo Canyon Site" prepared by Hugo Benioff and transmitted to Mr. Gordon V. Richards under dates of January 9, 1967, and May 28, 1968.

Response

This document was submitted in connection with PGandE's applications for construction permits for the two Diablo Units. It was reviewed by PGandE personnel, AEC Staff personnel, the Advisory Committee on Reactor Safeguards, the Atomic Safety and Licensing Boards, and various consultants to each. PGandE is unable to name specific individuals responsible for this review."

"38. State the nature and extent of the review which was made, including the number of man hours spent by each person or persons involved in the review referred to in question No. 37.

Response

PGandE does not have records of the time spent by its personnel in reviewing reports of its consultants and obviously does not have that information for members of other organizations."

"39. Name the person or persons responsible for the review of the "Recommended Earthquake Design Criteria for the Nuclear Power Plant -- Unit 2, Diablo Canyon Site" transmitted to Mr. Gordon Richards from John

Blume and Associates, Engineers, under date of June 24, 1968. Said report is dated June 1968.

Response

See response to Interrogatory 37."

The significance of the responses are tremendous. PG&E can't name one single person on their staff who reviewed the basic earthquake design criteria for the Diablo facilities. Certainly the criteria is important enough to have it reviewed by the best qualified persons on PG&E's staff! Yet no one knows who reviewed it nor how much time they spent reviewing it. I honestly question <u>if</u> it was reviewed at all.

One main point I wish to make is that if the basic earthquake design criteria are incorrect, then it logically follows that the earthquake safety provisions of the facilities are likely to be inadequate. One can be reasonably sure that the plant was not materially "overdesigned" -- not with PG&E's money at stake.

3. As previously set forth in the discussion of item No. 1, I have on three separate occasions been excluded from giving testimony at public hearings by specific Order of the AEC. Indications are that I probably will be permitted to testify at the June 1976 licensing hearings. This comes

a bit late! It will be a cold day in hell before I will dignify those hearings by participating.

4. The primary aim of advocate type proceedings is to sway a third party to a particular point of view. In the process, facts detrimental to a point of view are almost never brought to light, except by the opposition. One does not harm one's own case! The name of the game is win --- winning is everything.

In contrast to advocate type proceedings are those normally followed by scientifically trained persons. Here, a premise is set forth and examined for merit. The pros and cons are discussed by all parties. Facts become facts when they are mutually accepted. The entire purpose is to determine the truth -- not to win. How vastly different are the statements made by "experts" when they are part of a round table discussion among colleagues as compared to "expert testimony" during advocate type proceedings.

5. Practicing professionals in the fields of geology, soils engineering, seismology and various engineering specialties will inform you, if asked, of personal experiences wherein substantial errors have been made. Don't expect many of these persons to volunteer to come before you and furnish

such information. Errors and omissions are usually reserved for discussion in private committees and conferences, but not before a Senate committee -- unless asked, that is.

How adequate are NRC procedures which allowed the Diablo Canyon facilities to become over one-half constructed before discovery of an active offshore fault capable of generating a Magnitude 7.5 earthquake with ground accelerations on the order of 0.70G to 0.80G?

What if the San Fernando earthquake of 1971 hadn't happened? We would be sitting here in ignorant bliss believing that maximum ground accelerations would never likely exceed 0.50G. The Pacoima Dam record produced peak accelerations of 1.25G!

6. I don't believe any knowledgeable person would be so foolish as to say that the professions haven't learned a great deal about earthquake resistant design during the past fifteen years. There has been an explosion of knowledge which is still going on. Most earthquake design concepts are based upon assumptions, many of which have yet to be proven by performance during damaging earthquakes. Only during damaging earthquakes do we get a clearer picture of the adequacy of our design procedures.

It is fair to say that the San Fernando earthquake of 1971 clearly showed there is one hell of a lot to be learned.

While much attention since then has been focused on the well-known hazard of older unreinforced masonry buildings, what has the NRC done to review and update the earthquake safety provisions of older nuclear power facilities? This is a problem that I'm certain the NRC would not wish to have exposed publicly, but it is one which they should face up to as soon as possible. This committee should concern itself with the adequacy of existing nuclear facilities in California.

RECOMMENDATIONS

It is recommended:

1. This committee should investigate the Diablo Canyon nuclear power plant proceedings and fully inform the Legislature of your findings regarding their propriety and the degree of confidence you have as to whether proper earthquake safety provisions have been made.

2. The Legislature should take action to assure that an independent review is made of the earthquake safety provisions of nuclear power facilities which presently exist and those which are to be constructed in California in the future.

3. The Legislature should conduct public conferences and public hearings to solicit open and candid discussion among interested and informed persons to determine the following:

   a) whether the State of the Art is sufficiently advanced in the fields of geology, soils engineering, seismology, and earthquake engineering so as to permit the design and construction of nuclear power facilities without substantial risk to the health, safety, and welfare of the people who live in California.

b) the degree of risk which accompanies design and construction of nuclear power facilities which are subjected to the forces and effects of earthquakes.

c) the consequences of a nuclear disaster which may accompany natural disasters such as earthquakes.

4. The Legislature should inform the electorate of the findings from the foregoing recommended conferences and hearings and allow them to participate in reaching a decision as to whether and/or under what conditions nuclear power facilities are to be constructed and operated in California. A decision should also be made as to whether and under what conditions existing nuclear power facilities should be continued in use.

Respectfully submitted,

Stanley H Mendes

Stanley H. Mendes