UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE ON EXTREME EXTERNAL PHENOMENA

- - -

Nuclear Regulatory Commission
Room 1046
1717 H Street, N. W.
Washington, D. C.

Wednesday, June 4, 1980

The Committee met, pursuant to notice, at 8:30 a.m.

BEFORE:

DR. DAVID OKRENT, Presiding

DR. STEPHEN LAWROSKI

DR. DADE W. MOELLER

RICHARD P. SAVIO

HAROLD W. LEWIS

JESSE EBERSOLE

JEREMIAH J. RAY

W. LIPINSKI

ALSO PRESENT:

E. LUCO

J. MAXWELL

B. PAGE

DR. Z. ZUDANS

G. THOMPSON

M. D. TRIFUNAC

R. MATTSON

D. ROSS

ZECH

P R O C E E D I N G S

(8:30 a.m.)

DR. OKRENT: The meeting will now come to order. This is a meeting of the Advisory Committee on Reactor Safeguards, Subcommittee on Extreme External Phenomena. I am David Okrent, the subcommittee chairman.

Dr. Moeller from the ACRS is with me today, and we expect other members to be in later in the morning and the afternoon.

Also with us are several ACRS consultants, Messrs. Lipinski, Luco, Maxwell, Page, Zudans, Thompson and Trifunac.

The purpose of this meeting is to discuss matters relating to the seismic design of nuclear power plants, Task Action Plan A-40 and the NRC Research Program on Extreme External Phenomena.

The meeting is being conducted in accordance with the decision of the Federal Advisory Committee Acts and the Government and Sunshine Acts.

Dr. Richard Savio is designated the federal employee for the meeting.

The rules of participation in today's meeting is as announced. The notice for this meeting was previously published in the Federal Register of May 15, 1980 and May 21, 1980. A transcript of the meeting is being kept and made available -- -- Federal Register notice. It is requested that each speaker first identify himself, speak with sufficient clarity and volume so that he can be readily heard.

We have received no written statement and no request for time to make oral statements from members of the public.

We will now proceed with the meeting. We, as some of you may have noticed, have a rather long agenda. Mr. Savio's first estimate was we would run to 9:45. After I saw that, we chatted, and he now has a reply that shows it is running till 8:00 o'clock. I should note this was supposed to have been a two-day meeting in Los Angeles, which we had to change to Washington in order to save travel money for the NRC, although it may have cost more since we have so many California-based members and consultants for this subcommittee.

I am going to try very hard to stay within the agenda time, even if it means not finishing a topic because otherwise it is going to be a long day or even longer.

So I suspect what we may do is on some of the topics, at least, we may try to summarize them if that seems to be appropriate at the end of a topic rather than trying to wait to the end of the day when everybody is tired and anxious to go eat.

With that I will call on the representative of the NRC staff, I think, Mr. Zech.

MR. ZECH: Good morning, Dr. Okrent. I think that you have indications of a slightly revised schedule which we proposed from the agenda there. We have talked to Dick Savio with regard to it. In particular, we would like to start out if we could with item 1.c. and Howard Levin of the staff will have about an hour to an hour and fifteen minutes presentation. Then we will follow up with items 1.a. and 1.b. if that is okay with you.

DR. OKRENT: That is okay, but I would like to warn him at the beginning not to plan on an hour and fifteen minutes of presentation because that leaves no time for questions.

MR. ZECH: We will keep that in mind with the other items also then, yes, sir.

DR. OKRENT: All right.

MR. ZECH: After that, at 11:00 a.m. Bob Bier will be down and other members from Roger Mattson's division to talk with regard to the items that are listed on item number 2. And then the rest of the agenda is assigned with the staff, and we hope to keep with the schedule as you have indicated.

I don't have anything else to say unless you have any questions in general of the staff. If not, we will ask Howard to start if he would, please.

MR. LEVIN: Good morning. My name is Howard Levin. I am with the Systematic Evaluation Program Branch in the Division of Licensing.

This morning my presentation is about the Systematic Evaluation Program Seismic Review, and we are fortunate enough to have some of the members of our senior consulting team here today who will participate in the presentations.

The presentations will go as follows. I will attempt to provide an overview of the program which will be followed by Dr. Bill Hall from the University of Illinois, who will address the basis for our evaluation, the criteria, and provide an overview of the results of one completed review for the Dresden 2 facility.

That will be followed by Dr. Robert Kennedy from SMA who will address the structural reevaluation for Dresden 2. And then Dr. John Stevenson from Woodward-Clyde will address the mechanical and electrical reevaluation.

In my presentation I will provide a very brief summary of our philosophy in the SEP Seismic Review, provide an overview of our review procedures, think a little bit about the seismic hazard determination, a very important part of our program. But most of that will be addressed this afternoon during the A-40 presentations, the common elements of that. And provide a summary of our preliminary conclusions thus far in the reviews.

The last issue I have is in support of safety-related equipment involved in the category of preliminary conclusions, and as indicated previously (inaudible).

To refresh your memory, we have eleven facilities in the SEP. They are eleven of the oldest operating reactors in the country.

Ten of the facilities are located in the central U. S., the northeastern U. S., and one facility at San Onofre is located in southern California.

This slide summarizes some basic information about the facilities. The reason I put it up here is to show primarily the age, to give you a feeling for what the evolution design criteria might look like from plant to plant in the program. And I might point out that these plants received their construction permits from approximately 1956 to 1967, and during that period seismic design criteria evolved very significantly approaching standards which we would use today in the standard review plan.

Over to the righthand side of the page I have the column that is listed SEP seismic review group, and I will come back to that later. And what I am trying to do is differentiate the way we are looking at these different facilities. We have basically divided them up into two groups, and basically the older facilities fall in Group 2 and Group 1, and I will come back to that.

DR. MOELLER: Both plants at the cutoff point between Group 2 and Group 1 received their CD's in 1964. Was there a difference between February and December as far as PWR versus BWR or what?

MR. LEVIN: No, it is really not BWR versus PWR.

They may have received their construction permits there, but the primary reason a plant fell into Group 1 or Group 2 was the level at which seismic design was considered in the original licensing. And for one reason or another that seemed to be a logical breakpoint. The Haddam Neck plant was sort of the knee in seismic design criteria where it became more rigorous as it --

DR. MOELLER: Thank you.

MR. LEVIN: Okay, the primary objective of the SEP seismic review is to provide an overall safety assessment, and what we do is we attempt to use current licensing criteria as a guide and to help us make these decisions to make that overall safety assessment.

One of the objectives of the SEP was in fact to compare the current criteria. And I would say that that is a secondary objective in the review, which we feel that the primary objective is to make the overall judgment. And we do that just to see how far we might be backing off from current licensing criteria.

The review recognizes and attempts to deal with the inherent seismic resistance capabilities of these facilities, and some of these things are not rigorously accounted for in some review plan type review, and many are very difficult to quantify. And we are trying to get at this to provide as realistic an assessment as possible.

Turning to the second point, that we have attempted in the seismic hazard determination, which I will address later this afternoon in the A-40 presentation, to get a realistic assessment of what the hazards should be, simply because with these older facilities we just don't have the luxury of being overly conservation in that specification or in any criteria for that matter.

So we are doing a more medium centered evaluation to get at the real seismic margins of the plant.

An important concept is to make a determination if the plant meets the intent of current criteria. And I say this with respect to the general level of safety that current criteria dictates today as a packet.

We haven't in the SEP review attempted to go through point by point in the standard review plan and check off criteria that the plant meets, because it is our view that individual criteria don't really dictate the overall safety of the plant as specific criteria. There's counter-balancing effects.

And then last, an important concept is the backfitting concept in the SEP. And in accordance with Regulation 50.109, which says backfitting should be considered as substantial additional protection to the public health and safety, can be obtained. And we use that as a guide in making decisions whether to backfit or not.

DR. OKRENT: Do you have a quantitative measure for that?

MR. LEVIN: No. I think the closest that we come to that is possibly in the seismic input area, where -- and we can address this later -- where one, because we have taken from one approach a probabilistic point of view, and one can assess, let's say, the original design level versus in light of what we might think that falls as far as the hazard, and determine how significant the deviation might be and is it really warranted in upgrading or using a different seismic hazard.

We can address that later this afternoon.

DR. OKRENT: All right. Let me pose the kind of question I have in mind that I would appreciate hearing something on today. I read, for example, where at Lacrosse the staff estimate was there was a potential for liquefaction which might be in the range of 1000, 100,000 per year, and the applicants have other studies that indicated it might be less than one in 10,000 per year.

When I go to the subcommittee later this month, people are talking about one in 100,000 to one in a million per year for displacement, and it would help me to understand what that logic is if there is a logic, and if there is not a logic why there isn't a logic.

MR. LEVIN: Okay. There is not an explicit goal such as the 10 to the minus 6, 10 to the minus 7, goal that you might be trying to shoot for, that we are not quantitatively trying to do that. But in the example that you gave, in Lacrosse, I think the staff and the licensee have both concluded that the probability of liquefaction is of the order of 10 to the minus 4.

However, there are other things that you have to integrate into that. Given liquefaction with the probability of structural problems and then even proceeding past that, the probability of some unacceptable consequence, which could possibly get you to some risk level which might be acceptable. We just have not attempted to do that, and I am not sure that we could do it defensively.

DR. OKRENT: I will repeat. I am aware of a couple of cases where there are numbers that are being bandied about, and I would like to understand the basis for a decisionmaking where it is an estimate of numbers, and I would have to assume that in any of these things you have to provide some kind of numerical guidance if you can.

MR. LEVIN: Yes, I think you are correct, but what was attempted there was to see -- are you talking about something that has a probability of one in a hundred or one in a couple hundred thousand, that kind of discriminator? And as far as Lacrosse the perception was, and the reason was, that there was a show cause order issued on that plant, was it at the 10 to the minus 4, the kind of numbers which we all kind of agree with.

Didn't make us feel that comfortable. If it had been significantly lower probability than that, I think it may have been dismissed, because it was perceived that when you convolute the other factors in and structural margins, consequences, that you wouldn't get from a quali'tative sense the kind of level risk that we would be happy with. So therefore we would call for dewatering.

Jim, do you have anything to add to that.

MR. MAXWELL: I agree on that.

DR. OKRENT: I am not trying to argue which of these was right. I am trying to see if you have some semblance of uniformity in going from reactor to reactor when you decide that one of them should be designed for displacement or not and then another one should be designed for liquefaction and another one should be upgraded just for the vibratory motion, even though it doesn't have either of the first two problems. That is all.

MR. LEVIN: I guess at this time in the SEP we haven't attempted to quantitatively deal with that.

MR. ZUDANS: Before you go to the next slide, the goals, is the second goal, comparison to current criteria, really a tool to see whether the first goal is met rather than an independent goal?

MR. LEVIN: On a first level we attempt to use that as a basis for meeting the first one. Many times you just can't make it with current criteria and ve to go to other things. You may want to take into account, you know, in light of current knowledge, things such as allow ductility, damping, things like that.

I haven't gotten to it, but as far as priorities I will just briefly mention them. An assessment of the integrity of reactor coolant pressure boundary, safe shutdown systems, and engineered safety features, and that is the order of priority which we are looking at in the review.

DR. MOELLER: What determined the order of priorities?

MR. LEVIN: Primarily I guess, as far as the reactor coolant pressure boundary, the objective was accident mitigation. Don't cause an accident.

And the second item, safe shutfown, was your ability to in fact shut down given a seismic event.

And at the third level, your ability to deal with an accident should it occur.

DR. MOELLER: Okay, so it is a sequential thing. But I understand --

MR. LEVIN: But in effect all three of those items are going to be evaluated.

DR. MOELLER: Thank you.

MR. LEVIN: Okay, I come back to the Group 1, Group 2. And if you recall the Group 1 plants are the later vintage plants in the SEP program, the Group 2 the earlier. And we made a decision early in the program that we would attempt to review docket material, a licensee's file material, and make a judgment with respect to the early plant, simply because there is a significant data base developed and we thought we could do it.

On the Group 2 plants the data base was poor. In some cases seismic design wasn't explicitly addressed in the original licensing, and since there was very little that we could look at and make a judgment we basically put that on the licensee to come to the program to evaluate the seismic design of their plants.

Group 1 the staff and consultants are looking at, and Group 2, the licensees have that responsibility.

Two basic elements of our team are the group of people we refer to as the senior seismic review team and various seismic review teams which are organized for the review of the specific facility.

We look to the senior team to do the following, and I will just read off the viewgraph. Recommend review criteria and procedures. Demonstrate these procedures at one plant. And we will discuss that today.

The criteria that we are basically using in the SEP is NUREG/CR-0098, which was written by Newmark and Hall. And the senior seismic team developed a procedure for which to go through a plant and review it, and that has been their primary contribution. But in addition to that, and we have stumbled upon special problems of more than usual difficulty, we have gone to them for consultation.

The seismic review teams include members of the NRC staff and consultants, primarily from Lawrence Livermore and their subcontractors. And these people are the individuals who are responsible for the day-to-day review and documenting the results for evaluation.

DR. MOELLER: Excuse me. I guess I missed the point. You have the senior seismic review team. Is that above both Group 1 and Group 2?

MR. LEVIN: The senior seismic review team has, as I said, they have recommended criteria to us, and they demonstrated a review of procedure for one plant, and that was Dresden 2, which is a Group 1 plant.

Okay, and basically the procedure is one which we are applying just to later facilities, and the other facilities, the evaluations are being done by the licensees themselves. And they will --

DR. MOELLER: No, I guess what I am mixed up on is how do teams or Groups 1 and 2 relate to the senior seismic review team?

MR. LEVIN: Okay, Group 1 refers simply to a facility. The senior review team is a team of people which will perform the reviews on those facilities.

DR. MOELLER: So the senior seismic review team will look both at Group 1 and Group 2 plants?

MR. LEVIN: Generally speaking, they will complete a detailed review for one plant, and that is Dresden 2. For the other plants they will provide an overview of work which the seismic review team is doing. So we will document results, we will go to them for advice. They will review the results and give us feedback in those other reviews.

DR. MOELLER: Thank you.

MR. LEVIN: The Group 1 review approach goes as follows, and this was developed by the senior team. The first thing that we completed was, as I mentioned earlier, the criteria document, which is the NUREG by Dr. Newmark and Dr. Hall.

A group of systems engineers identified items which should be considered for seismic categorization. An extensive docket review was completed. We supplemented that with materials from the licensee, A-E, and NSSS files. We then followed that up with site visits.

The objective from this point was just to gain information about the facility and become familiar. After the site visit we requested additional information which came up as a result of the site visit. And from that point on we followed up with a detailed review, and in many cases that review involved confirmatory evaluations, primarily to update in a way of a more contemporary analysis work that was originally done by the licensees to help us in our judgments.

And part of that activity was to determine structural response, in-structure spectra, and in certain cases we audited certain equipment and piping systems which we felt were representative of the way other systems in general were treated at the plant.

And then we compared the performance criteria and documented results. And those results for Dresden are documented in a NUREG, 0891. And I believe most of you received that.

These are the Group 1 plants, and I have indicated here the schedule for completion. Basically we plan to have NUREG reports completed for all these plants by the end of this year.

MR. ZUDANS: Is any one of these complete already?

MR. LEVIN: Yes. NUREG 0891, which I believe --

MR. ZUDANS: Yes, what was the title of that?

MR. LEVIN: Can you help me, somebody that has it? I think it was just Seismic Review of the Dresden 2 Facility, something simple like that.

SPEAKER: Called Seismic Review of Dresden Nuclear Power Station, Unit 2, for the Systematic Evaluation Program, NUREG 0891.

MR. LEVIN: Thank you. This is our Group 2 review approach, and it is very similar to Group 1 except as you get past our docket review the monkey is on the licensee's back to implement a program. We are working with him to make sure that we are in agreement with his approach, and basically we will review his submittals after he has completed his evaluation.

This is the schedule which we are working. Basically we are talking about an 18-month evaluation program, and we anticipate we are to have input from the licensees by January 1, 1982, and our review will be complete by April, and that modifications will be complete by January 1, 1983 if they are required.

MR. ZUDANS: On Group 2 you do not have any screening analysis planned, unlike in Group 1? Is there some big reason for that?

MR. LEVIN: No. We will, okay? In particular, we plan to audit certain of their calculations via confirmatory evaluations primarily as an example in the piping area and in the mechanical area.

MR. ZUDANS: That sounds like you are screening evaluations and that sort of thing?

MR. LEVIN: Yes.

MR. ZUDANS: I see.

DR. OKRENT: If I estimate crudely, that is probably going to take you five or six years past the beginning of the SEP program.

MR. LEVIN: The 1983 date?

DR. OKRENT: Yes.

MR. LEVIN: Yes, the program started in October of 1977, so that is right.

DR. OKRENT: How do you judge whether there are some things that need more urgent attention?

MR. LEVIN: Okay. We have already stumbled upon a couple of them and have taken action. I will be addressing them in just a couple of minutes if you could --

DR. OKRENT: I had rather -- tell me how you do it other than stumbling.

MR. LEVIN: Okay. Basically it comes out of a review of the facility, a review of the docket. It comes out of site visits. I might add that one of the things I will be addressing in a minute or two is the issue of anchorage and support of electrical equipment, which was surfaced as a result of site visits to the plant, where we found that there was equipment that was unsupported and there were no other lateral load resisting mechanisms than friction.

So that was something that came out of a site visit. There were other things that have come up which we have taken action on which came out of just a paper review. But it is something that just has to fall out of the review procedure, and a judgment has to be made along the way whether something has to be taken care of immediately.

Lacrosse was an issue. Anchorage and support was an issue. At Yankee Rowe support of the steam generator and reactor internals is an issue, and that is being taken care of.

We can't do it any faster than we are reviewing it. So I am not sure of the thrust of your question, Dr. Okrent.

DR. OKRENT: Well, I will tell you then how I might go at it if I were trying to be responsive to my question. I think you have in fact a senior review group who together with a few other people as so needed could look at these plants, just from a distance say, I don't think they would have to go up to them, knowing the state of design, or the absence thereof, in the time period during which each was being designed and constructed, they could make some intelligent guesses as to where there might be weak spots and say maybe you should look at these in a preliminary way first to see if they violate some criteria, whatever it is. You know, there is something that we really don't want to let stay for three more years, or whatever is the time period you are talking about.

See, I found that the staff, not your part of the staff, required B&W plants to shut down and fix certain things. They required a lot of plants to shut down and fix some piping, hangars, and so forth.

I am trying to understand is there some logic in the way the staff goes with these things, or is it just what you stumble on, to use your words.

MR. LEVIN: Well, I think the basic premise, before we started all this, was that for the period of time that we were evaluating SEP plants that continued operation was justified for the interim for a variety of reasons. Okay.

As I mentioned --

DR. OKRENT: I am not saying that they shouldn't operate.

MR. LEVIN: Okay.

DR. OKRENT: I am saying though that there is reason to be questioning about certain features of these plants there are some things you could accelerate, a first look at, and see whether your suspicions are correct or whether things are borderline, if things are borderline you certainly want to have a lot more information before you act precipitously.

MR. LEVIN: I think some of these things will come out as I get to the preliminary conclusions that we have come to. I personally believe that we have addressed the things that would fall into that category, things that bothered us. One of them was the support of electrical equipment.

As an example, I think people feel fairly confident based upon -- it is a fact that these plants are not going to meet current criteria. Okay? But I think that because of other indications, operating history in similar industrial facilities throughout the world -- like, as an example, in the structural area -- that we don't feel that that is an area that is something which we should drop everything and immediately go look at.

We did feel that about the support of electrical equipment. In the piping area I think we feel very comfortable about the later facilities. There is no question that on the three or four oldest facilities that they are going to be throwing pipe hangers in that plant. But all indications are that plants designed to similar criteria, B-31 requirements without seismic provisions in 1955 timeframe, have ridden through earthquakes.

So we feel that in this interim, in that area as an example, that we can proceed with a review in a methodical way and not react to that. There have been certain things we have reacted to -- the support issue, the Yankee Rowe heavy equipment problem, the fact that it wasn't, the steam generator wasn't supported. There was questions about the reactor support at Lacrosse, et cetera.

So I feel that as far as initial screening we have accomplished what you -- and we can get into this a little bit further in detail.

MR. ZUDANS: Can I continue on the same subject?

MR. LEVIN: Sure.

MR. ZUDANS: Now if I look at your review approach, actually you have, there seems to be at least, a goal in the first top way to everything you could lay your hands on, the complete docket review.

MR. LEVIN: That is correct.

MR. ZUDANS: Now is it then to be assumed that you have gone through a complete docket review for all the plants in both groups?

MR. LEVIN: That is true.

MR. ZUDANS: And you also have made site visits to?

MR. LEVIN: All of the later facilities and some of the earliest ones.

MR. ZUDANS: And of course you have looked at the information from licensees, A-E's, and -- -- already. So if there was something glaring, then you looked from a point of view of an expert's understanding of what you are looking at, you might already have discovered. So maybe it is not as bad as it might appear?

MR. LEVIN: I don't believe it is. I agree with that assessment.

MR. ZUDANS: But nevertheless, you cannot rely on just stumbling on. You really have to --

MR. LEVIN: Well, it is much more systematic than that.

MR. ZUDANS: Yes.

MR. LEVIN: I think that may have been a poor choice of words.

DR. MOELLER: I don't know the extent of the modifications that would be required, but if I read your schedule correctly you allow one year in which to make all of the modifications. Would the plant be shut down that year, or can these modifications be made in a few months? Of course we don't what they are.

MR. LEVIN: I think in some cases the plants will have to be shut down. I don't think in any case we are talking about shutting them down for an entire year.

As an example, in the Yankee Rowe modifications, if you are talking supporting the steam generator, providing upper lateral restraint, the plant is going to have to be shut down.

But there are other modifications that are related to the support of the reactor internals -- I don't know how many of you are familiar, but the reactor internals sit on concrete legs, which will be stiffened and reinforced, which are external to the reactor, and I believe that could be done during operation.

So it is a mixed bag.

DR. MOELLER: Thank you.

MR. LEVIN: This slide summarizes briefly some of our accomplishments. As Dr. Zudans pointed out, we have completed docket reviews for all the plants. We have completed the review of Dresden 2, as documented in the NUREG.

Structural confirmatory reanalysis has been completed for Ginna, Palisades, and Oyster Creek. The Millstone confirmatory analysis is in progress. That analysis includes looking at structural response and in-structure spectra.

Equipment qualification reviews are nearing completion for the other Group 1 plants. We are working with the licensees to develop a mutually agreed upon program for their reviews for the Group 2 plants. We are currently involved in a review of the San Onofre 1 seismic reevaluation program results for Phase 1.

We have reviewed the Lacrosse liquefaction issue. We have devoted a significant amount of time to the 1E equipment support issue which led to an I&E information notice which I will address in a few minutes.

We have just recently, we are coming to completion of the site specific response factor project, and that we will address this afternoon.

This slide summarizes what might be called our SEP lessons learned. In the structural area, as I indicated, at this point in time we believe that the Group 1 plants will be shown to be adequate and that the Group 2 plants will require some upgrading.

In the mechanical and piping area the Group 1 plants will be adequate with some exceptions. And substantial upgrading will be required of the Group 2 plants, and that is primarily in providing pipe restraints and things of that nature, because there were three or four plants where the piping was basically on a design for dead weight and temperature and pressure and essentially no inertial input.

We feel that the largest problem that we have come across in the review is that in the electrical area, and the problem is in my view 75 percent a documentation problem and that very little information exists as far as functional qualification of the equipment.

addition to that, as I mentioned, the anchorage and support of electrical equipment has become an issue, and that has been taken care of. And I will address that right now.

MR. ZUDANS: You are upgrading the requirements from a structural point of view always based on current day criteria or some -- -- criteria?

MR. LEVIN: No, we have generally utilized the NUREG by Drs. Newmark and Hall which recognize -- and Dr. Hall will be giving a presentation next. But there are in light of current knowledge certain things that we can take advantage of such as increased stamping and ductility

MR. ZUDANS: -- -- as compared to official criteria?

MR. LEVIN: That is right.

MR. ZUDANS: Is what you will propose for the future, right?

MR. LEVIN: We are proposing it for use in reevaluation of the older operating reactors.

Okay, the problem of anchorage and support of electrical equipment came out of our site visits to the six later SEP facilities. And basically what we found from one piece of equipment to the next, there was a lack of uniformity in the way that, let's say, identical pieces of equipment on the same floor might be supported. Some equipment lacked positive anchoring, and by that I mean that there is no anchor bolts, welds, or anything attaching it to the floor or the wall. And essentially the only lateral load carrying mechanism was friction.

It appeared that many of the anchorages were, in some cases, afterthoughts in the field. They were engineered. There are tac welds and things of that nature that were used. Just from our judgment they didn't involve engineering.

Another issue that came out of that is the support of internally attached equipment -- control room panels, instrument racks, motor control centers, things of that nature, cabinet type equipment -- where there were in many cases long cantilevered pieces of equipment attached internally which may have only been -- may not have been supported adequately. And we are trying to look into that in detail, and that is a real bear.

And then the last issue was the potential interaction of nonseismically designed equipment with seismic Category 1 equipment. And in that I refer to loose items that may be around block and tackle, dollies, gas bottles, and in addition anything else in the plant, nonseismic ductwork, stairways, anything that could become loose and dislodged and fall and damage seismic Category 1 equipment.

Here are examples of equipment which we and the licensees found not to have positive anchorage. And it varied across the plants. I don't think we can make a general statement saying that the older plants were worse or that the newer plants are better.

In my opinion it varied with the constructor, and we found that most of the problem exists from the time that there is information got on the drawings, that somehow between, in the procurement phase, of the installation phase, something happened, and the equipment didn't always get installed the way the designer had planned.

January 1st of this year the staff took action and sent letters to each of the SEP facilities requesting that they do the following. Number one, inspect the facility, all safety-related electrical equipment within 60 days and then report back.

Two, evaluate the adequacy of the anchorage and support systems.

Three, correct these deficiencies, if any, by September 1, 1980.

The first step was primarily designed to identify any equipment which was not supported at all. And the nine months of the evaluation phase was primarily focused at making evaluation of the supports in general. Are they adequate, even if, you know for those cases where they exist?

&1
end T. 2
Burrell

Short-term resolution of the problem has been that all equipment that was identified as not having positive anchorage has been fixed or will be fixed prior to startup from current outages. The only exception to this is the Big Rock Point Plant where they will be taking action to make corrections this September during their outage.

A long-term resolution is that the licensees are in the process of evaluating the adequacy of these supports, and we are maintaining a target date of September 1st. This is not a very good picture, but I hope it can give you a feeling for some of the things we have found.

This is a battery rack, pretty much standard for this vintage plant which you see throughout the SEP, one which through our evaluations we didn't like too much. We found that these wooden battens are a potential problem, and maybe in this next slide I can give you an indication of what racks look like today.

The first one is another picture of a rack that might be existing at an SEP plant. This is a rack which standard review criteria might dictate today, and that would be acceptable to staff.

A lower rack is one similar to the first, which has been modified by a licensee because they have determined that they didn't like the way the first one looked. They said, look, we are going to take some interim action until we can do a more detailed evaluation, and they provided some more substantial bracing for that rack.

Here is another indication of an interim action that was taken by a licensee for a motor control center which lacked positive anchorage, and here what was done for an interim until anchorage can be engineered was an anchor bolt was driven into a wall and a support brace was provided at the top to keep the unit from overturning. That was considered to be an interim action. That licensee is now currently working to engineer a proper support.

That is all I have as far as my presentation. Dr. Hall now will provide a discussion of the basis for our reevaluation, the criteria we are using, and provide a more detailed overview of what our results were addressing.

DR. OKRENT: Could I ask a point of information? There was a memorandum dated November 16, 1979, or a letter, from Lawrence White to U.S. NRC, attention Howard Levin, which transmitted their estimates of the return periods. Excuse me. The NMI earthquakes at ten eastern sites or nine eastern sites for the 200, 1000-year and 4000-year return periods.

Is that considered to be a reasonable estimate of the state of affairs?

MR. LEVIN: The numbers which you see on that page?

DR. OKRENT: Yes.

MR. LEVIN: One thing I would like to point out, and maybe we can do it now. We will adjust this in detail this afternoon. We have utilized Lawrence Livermore and ten of their subcontractors to provide some detailed sensitivity studies in this area. It is a multifaceted approach which is probablistic and deterministic. They provided us with just thousands of spectra and have sensitivities all over the place.

They haven't really made a recommendation in the sense that we say you shall use this in design. Basically, the staff reviewed the sensitivity studies and the various analysis are completed, and we made a decision. This initial decision will be discussed this afternoon.

So I wouldn't interpret those as numbers which we are recommending, necessarily -- I don't know the paper you have there -- but that we are recommending in the SEP. You will hear that this afternoon.

Leon, do you want to address that?

SPEAKER: Dr. Okrent, that letter is a correction of a misprint that appeared in an August 1979 publication. Since then we have done a lot of additional studies. That does not represent the state of the art presentation. It was a correction of a misprint that appeared in one of the original studies that was done.

DR. OKRENT: I see. Do you have your own estimate of what the 200, 1000 or 4000-year period earthquakes are for these sites?

MR LEVIN: I think later on this afternoon we will present you with how we view this thing at this time.

DR. OKRENT: All right.

MR. RIEDER: Dr. Okrent, gentlemen, I will make my comments brief. I am William Hall, consultant to the NRC through N.M. Newmark Consulting Engineering Services, and a professor of civil engineering at the University of Illinois at Urbana Champagne.

I have two viewgraphs. These viewgraphs are really only to remind me what to say. You have in front of you or will have in front of you momentarily a copy of a writeup that Nate Newmark and I prepared trying to put forward some of the bases for the SEP reevaluation process.

I will confine my comments specifically, in the interest of time -- you can read the document at your leisure -- to two principal points. I want to address first the business of general philosophy behind the way we went at the program, in line with some of Dr. Okrent's questions. Secondly, I want to home in slightly on the Dresden-2 evaluation.

This will be followed immediately by presentations by Dr. Stevenson and Dr. Kennedy, with the details -- and I think perhaps in the interest of asking questions, it might be wise, Dave, to wait a minute or two until you hear that part, as I think there are some questions that are going to come forward here.

Howard Levin has given a good history of the SEP program. I shall not dwell on this at any length at all. As a member of the Senior Seismic Review Team, we are extremely appreciative of the support effort provided by the NRC staff and the Livermore staff in getting this program under way.

Perhaps the most important thing I have to say today centers around this item at the bottom of the first page pertaining to the general philosophy and approach pertaining to the review process.

Needless to say, it was with some effort that we decided on what level to approach this particular reevaluation. It is recognized by most of us, I think, that with the earlier plants as they were designed and constructed, the criteria that were used were different than what we use today. In some cases these criteria are less rigorous, and in some cases they are more rigorous than things that we might wish to use today.

In the case of nuclear facilities, of course, we realize that there are certain safety functions that just have to work. On the other hand, that doesn't mean that these have to work in the absence of inelastic behavior to a limited degree, although in some cases there are some systems that obviously have to remain elastic from a design point of view in the sense of insuring the kind of adequacy that we would desire.

Thus, the attention was to the system in the sense that we are looking at an entire system, hoping that we can convince ourselves that it is able to achieve and maintain afterwards the safe shutdown condition.

Thus, in line with what Howard has said and what we were struggling with, it should be apparent that this review is somewhat different than we might go at a new licensing operation for a brand new plant today. We tried to focus in on the pertinent items that we thought were really of significance from the safety point of view.

This involves some assessment on a broad range of the safety issues.

From the very beginning we never did envision that this was going to be one study based upon demonstrating compliance with specific criteria as reflected in the standard review plan today or the regulatory guides. But we did utilize the current licensing criteria with respect to the level of design they dictate. we used this as a baseline from which to measure the relative safety margins, and the key word here is "relative."

By this I mean that we use this as a basis for drawing our judgments. More than that, it gave us a reflection of the intent that we were after in looking at this. Now, I am going to elaborate a little bit on these statements that are here in writing, to get back to Dr. Okrent's question.

I think you can see from what Howard has said and from what I have said here briefly that to get started on this program was difficult in the sense of deciding to what depth to carry out these studies. At first Nate and I had envisioned that we would -- we worked on the 0098 document that has been referred to, giving some of the criteria that we thought would be applicable with regard to an existing facility. We thought that it would be possible, perhaps, to come in here and do a rather -- I won't call it quick , but a rather rapid oversight review of the facility in such a manner as to look from a system point of view at the important essential elements, draw some judgments, and go from there.

As we got into the process, there is a balance of information you have to have on which to make these assessments. It became obvious that to draw some of the judgments that we wanted to make, it was necessary to go into further detail, into looking at the systems, analyzing some of the systems and so on.

On the other hand, at the other extreme, we want to be very forthright here and make it clear that we were not out to redesign the facility. This was not possible and was not the intent, and would be impossible with the manpower we had, the economic ability and so on.

So it is a middle ground here with regard to the level of effort that should be expended. The Dresden 2 Plant was a learning experience in this particular sense, in trying to delve into the depth that we should go, and perhaps this is where the Senior Seismic Review Team played the major role, in trying to set a base for the level of this investigation.

It centered around two things again, as Howard said. One was the assessment of the integrity of the reactor coolant pressure boundary, and an evaluation of essential structure systems and components to safely shut down the plant and to maintain the removal of residual heat.

The review process I think Howard has covered. It is one of "look see," digging into the documentation. I think it is fair to say that we uncovered, and John Stevenson will address this a little bit, a lot of documentation with regard to equipment and so on, which perhaps wasn't as obviously available as one might think, and then making the anlalysis and studies that one sees fit to arrive at a judgment.

The evaluation itself, of course, is largely judgmental. It is one of trying to be sure that you have looked at representative systems, and by this inferring perhaps what the other systems were like, and raising the types of questions that will have to be addressed in more detail by the owners in the sense of getting into the plant and looking at things and checking that they do meet the same sorts of criteria.

So much for the first part. Now, I don't know if there are any questions at this particular moment. There probably are. We can move ahead.

With regard to the Dresden 2 evaluation, I will just paraphrase this extremely briefly because you are going to hear in detail from Bob Kennedy and John Stevenson. We first had to make a decision on what the hazard was that we wanted to go ahead with the reevaluation in the case of Dresden 2. We were quite aware, of course, that the site-specific spectra studies were under way.

In order to move ahead, the decision was made to use the 0.2 g zero period peak horizontal ground acceleration as a norm against which to look at things. It is possible that the site-specific study will show that this anchor point might be slightly high, wh[illegible], of course, an additional margin if it turns out to be [illegible] way.

Nonetheless, we decided on the basis of our initial look-see in this particular case to move with that number to go ahead because we had to have something to operate against. We went in and looked at the structures in the sense of the as-constructed plant as much as we could get at it. We made some analyses, found some things which Bob Kennedy is going to tell you about.

We look at such things as, of course, the analysis models that were used, the combinations of the loadings, the force and deformation patterns, and arrived at some decisions about the structures.

Perhaps more importantly in some respects with regard to problems that one might expect is that of the equipment and distribution systems. It is a little harder to get at. We both conducted a physical inspection of some of the systems that we could see readily, used these to draw some conclusions in the sense of looking at representative sampling of the resistance of these particular items, and identified a number of things that have to be looked at further. I will leave this to John Stevenson to describe to you in more detail.

Page 8 of the writeup we reproduced out of the NUREG report the general conclusions. The recommendation and conclusion chapter is a long one in the report. This is the very last section of that particular chapter. We pointed out that with regard to the structures and structural elements, in general we found them in our estimation adequate to resist this 0.2 g zero period ground acceleration control spectra base, with one or two possible exceptions. Bob is going to address this and these were identified in the report.

We had some observations on the piping, particularly with regard to the as-built piping supports, and some suggestions that should be followed there by the owner. Then in the mechanical and electrical equipment field, we had a number of items which we examined and tried to draw some judgments about the margins of resistance against damage.

Some of these were predicated on items that are listed there with dots, in the sense of trying to insure that the equipment has engineered anchorage, which means that it isn't just tacked down but somebody does study what the overturning forces might be and so forth, and really designs the anchorage; that there be additional reconnaisance of the plant to identify and upgrade some of the things that have to be examined.

Then at the end we have a statement about the functional capability of equipment. This is a harder thing to get a handle on, something that has to be taken into account in terms of drawing an assessment as to the ability of these things not only to resist it from a physical point of view, but can the equipment perform its function during and after the earthquake. To the extent that we could, we tried to look at this from a judgmental point of view.

I am going to stop here and let these two gentlemen follow here and give you some of the precise details, and then I think at that particular point in time it would be appropriate to ask some more detailed questions. I think there will be some.

DR. OKRENT: I have a general questions. In arriving at conclusions that with certain reservations, Dresden 2 would be all right, is this something that represents, then, a best estimate, 99 percent confidence on your part? Where would you place your level of assurance in making your statement about its capability?

DR. HALL: Well, it is certainly a best estimate, to answer your first question, a best estimate in the sense of being capable to go through the hazard and function properly.

As far as putting an actual exceedance limit on it, we did not look at it with that kind of a number bound in mind. But I would say, frankly, that in line with work that Nate and I have done over the years in this particular case, I certainly think that on the assumption that attention is given to the items that we have directed attention to in the report, that this thing should come close to being in line with -- let's use a number like a median plus one sigma or something like this, in that particular range. I don't think there is any question about it.

Now, that isn't 99 percent, Dr. Okrent, but it has got a high level of assurance that I think we are okay, put it that way. I know you would like to have a number, but we didn't quite look at it that way.

DR. OKRENT: I find it important to know what yardstick an adviser or consultant is using in making a statement. I can remember once an ACRS consultant advising us that a certain design basis on one of the Great Lakes was okay, and I incidentally asked him what he had in mind. He said the 100-year value. People in those days used to be thinking about million year values, to put it in perspective, and yet, without asking, one doesn't know. So I am just trying to get to that.

DR. HALL: Let me elaborate on this just one more second, and then I think Bob will probably add to this, and John. Howard alluded to the fact that we were looking here more at a mean-centered type of thing with regard to our look-see. On the other hand, as much as we could, I think all of us were looking at the uncertainty picture and sensitivity, and Howard alluded to this, too, in the sense of finding the basis, at least mentally, for drawing some of these judgments.

So again, it is elusive in the sense of putting numbers on it, but I think we have been very, very conscious in the examination of documents and in our analyses to look at this matter of sensitivity; and Howard made some point of this. This is a large part of arriving at your judgmental basis. It is a general statement.

Let's go on. I think maybe this will come into focus when you see what is happening.

DR. OKRENT: If I can make two other comments. One is I think we all feel earthquakes are very improbable in the eastern United States. On the other hand, it was only a few years ago when we were talking about a construction permit for a reactor, either somewhere in Oregon or Washington, where people were talking about the design for volcanic ash, eight inches or something, and it seemed so far-fetched, I am sure. The applicant and the staff both felt uncomfortable.

MR. JACKSON: If you read the affidavits of the witnesses, which we have done, the comments, it reads like a work of fiction. But reading it now in hindsight, it looks like it was a very good staff review.

DR. OKRENT: Yes, indeed. One other thing. At some point in the relatively near future, it seems to me, before the end of the SEP program, the staff ought to be able to tell itself and the ACRS and the public what level of safety it is seeking with regard to the seismic designs of the SEP plants and why. I think you have to somehow quantify it with a ban. I don't think you can wave your hands, myself. I think that will lead the whole thing into unsettled situations, not only for me.

MR. KNIGHT: If I may, Dr. Okrent, I am Jim Knight. This discussion came up earlier. I certainly agree, entirely. One of the things that we have to do and do a better job at is developing the tools that allow us to take those next steps to get beyond the likelihood of the hazard and go into the performance of the equipment, the performance of the structure so that we can come to a bottom line.

That at the moment strikes me, at least, as our weakest point.

DR. OKRENT: I am afraid you are going to have concluded the review and decided what you need before you know why, that's all.

DR. ZUDANS: Maybe the "why" will crystallize at that point.

DR. OKRENT: It may, but it may not have been the "why" on which they have a defensible position.

DR. KENNEDY: My name is Bob Kennedy. I am one of the members of the Senior Seismic Review Team. I want to give a brief overview of the procedures that were used to reevaluate the seismic adequacies of the structures in the systematic evaluation program.

This review of the structures and of the equipment -- I am going to concentrate on the str :ures -- was conducted to enable the NRC staff to focus on critical areas when they went back to licensees to ask for more detailed evaluations.

In other words, the level of work that we did on the Senior Seismic Review Team was what I would call a scoping level of work, to define the general seismic adequacy of the structures and the equipment. It was not a detailed evaluation of individual structures and equipment but was more of a scoping type effort to look for problem areas.

The original seismic design criteria used on these older plants was very much different than currently accepted criteria. I think we are all aware of that. The real basic question is whether plants designed to the seismic design criteria that existed at the time the plant was designed -- in other words, criteria that existed in the early 1960s, generally -- whether plant structures designed to that criteria are seismically adequate based upon our current knowledge.

Now, I am going to concentrate in my discussion on Dresden 2 and 3 because that is the facility that we have finished the evaluation on. It is a Group I plant. We have also looked at other Group I plants. I think the general conclusions I am going to give for Dresden 2 and 3 are generally true for the Group I plants.

I am not prepared to really discuss the seismic adequacy of the Group II plants.

"Dresden 2 and 3 is a BWR Mark I. Basically this is a plan view. It consists of two reactor buildings tied together and then integrally tied into a much larger turbine building. Now, this integral tie into the much larger turbine building is an area that caused us some concern in the original design of plants of this vintage, torsional vibrations and the effects of torsion due to these inner ties of buildings was generally neglected in the original design.

So this was an area that we did feel we had to do a considerable look at was the effect of three-dimensional vibrations due to torsion, due to the inner ties between buildings, because that was something that, at that vintage, was typically not considered.

This is an elevation view. You see the reactor building tied to the turbine building and one of the critical areas was this inner tie at the intersection of the operating floor of the turbine building with the reactor building, again because of forces at that inner tie that results from torsion due to the different vibrations of the different buildings being ignored in the original design.

This viewgraph simply compares the design criteria that was originally used with the re-evaluation criteria that we used. Basically, the plant was designed for a 0.2G Housner spectra. . The re-evaluation list conducted for a

0.2G Reg Guide 1.60 spectra.

Now, the spectra associated with Reg. Guide 1.60 tends to be, throughout most of the frequency range, considerably higher than the Housner spectra. The Housner spectra was originally intended to be a median center spectra. The Reg. Guide spectra was intended to be a median plus 1 standard deviation spectra.

Why was this chosen? It was chosen because at the time we did the structural evaluation the site-specific spectra for this site were not yet developed, and it was the senior seismic review team's opinion that the site-specific spectra would be enveloped, conservatively enveloped, by this spectra that we re-evaluated for.

So we think this is a source of conservatism in the re-evaluation and I think this afternoon the site specific spectra will be talked about so you can make your own judgments at that time.

The next area of considerable difference, plant Dresden 2 and 3, in the original dynamic analy s were analyzed using much lower damping levels, typically, that are in the Reg Guides currently and even lower than what was judged by the senior seismic review teams as proper damping levels to be used in the re-evaluation program are given in NUREG-0098 which was originally developed by Drs. Newmark and Hall.

Basically, this briefly compares most of the structure being concrete. In Dresden 2 and 3, 5 percent damping was used in the original design an^lyses. Reg Guide would suggest 7 percent. The NUREG uses the 7 percent as a lower bound damping estimate and a 10 percent value as sort of a median damping level.

Throughout the review effort, the senior seismic review team concentrated on using what were judged to be median damping levels. So the work that we've done is based on the use of a 10 percent damping level for concrete as . compared to the original design of 5 percent, plus the use of a Reg Guide 1.60 spectra, as opposed to the original design Housner spectra.

This gives a rough idea of the impact. Typically you would compare a Housner .5 percent damp spectra with a Reg Guide 1.603 percent spectra to be consistent with the philosophy that the review team has used, or alternately a Housner 2 percent damp spectra compared to a Reg. Guide 7 percent damp spectra.

I think you can see that generally the response factor used in the review was somewhat higher than the response factor used in the original design, but not excessively so.

The review wanted to look at, in the original design of plants in this vintage, typically they used absolute sum combination of one horizontal and one vertical component of an earthquake. Throughout the review, we used SRSS combination of all three components.

Original design, the structures are held elastic. Throughout the review, we did allow a limited amount of inelastic behavior, judged by the seismic review team to be acceptable levels for structures. In other words, if we calculated or estimated small amounts of inelastic behavior, considered the structures to be acceptable.

Basically, the levels of inelastic behavior that we consider to be acceptable throughout this review on structures was a level of 1.3, a ductility factor of 1.3, inelastic deformation 30 percent of the elastic.

We were very concerned about the effect of torsion which was ignored in the original design. We also wanted to be sure that for the re-evaluation of the equipment that we used a floor spectra approach, or an instructure spectra approach for all instructure spectra that were calculated from the dynamic characteristics of the structure, where in the original design, what was typically used for most of this -- and this is typical for most of these SEC plants of the group one level -- is they used an amplified ground response spectra. They simply scaled the ground response spectra up to the acceleration of the floor level.

They didn't change the frequency characteristics of the ground response factor. Or, in some cases, they used floor spectra from other plants.

Now, as I said, it was a scoping type evaluation. The procedure that was used is the following.

First of all, using models of relatively simple structural models, but that included the effects of torsion, included what we judged to be the important dynamic characteristics of the structure, using these models we did a re-anaysis of the basic lateral load carrying systems.

In the re-analysis, we used the criteria I earlier showed. We then compared the peak forces, shears and moments from the re-analysis at various levels throughout the system with those that were originally used in the design, in the original seismic design.

If the forces and moments that we got from our re-analysis were less than those used in the original design, we judged those structures to be adequate, i.e., we did not go back in and look at the details of the original design, as long as we had lower forces than were originally used.

Secondly, if the forces that we calculated exceeded the original design forces by less than a factor of 1.25, we judged the structures to be adequate. Now, the choice of the 1.25 number is based upon this ductility factor of 1.3.

If those forces and moments did not exceed the original design forces by more than 1.25 -- and this is in the report -- we can justify that the ductility factor would have had to have been less than 1.3. Now, in fact, that is a fairly conservative scoping type approach because it ignores the possibility that there was reserve capacity in the original design.

But it does admit to some possibility of inelastic behavior if the forces were over the original design.

Now, any places where the forces exceeded the original design forces by more than 1.25, we did go back and do a careful look at the design of the structures. We went back and we judged whether these structures, by looking at them carefully, had sufficient capacity to take those forces with a ductility factor of 1.13 or less.

DR. ZUDANS: Could I ask a quick question?

DR. KENNEDY: Of course.

DR. ZUDANS: If the forces or moments exceeded, or didn't exceed 1.25, you concluded there was some inelastic deformation, therefore, you didn't worry about --

DR. KENNEDY: That there could have been some. We don't conclude there was, but that there could have been.

DR. ZUDANS: Wouldn't you have to have looked at the motions, displacement, the relative possibility of interference?

DR. KENNEDY: We did look at that. Anyplace where we did have any possibility of inelastic behavior, we did look at the relative displacement. Now, we're talking of very smally, inelastic behavior when we are talking ductilitiles of 1.3.

DR. ZUDANS: All right.

DR. KENNEDY: So we did do limited stress analysis re-analysis in those cases where we had critical elements in which the load ratio exceeded this 1.25 factor. Then we generated also floor spectra, as I indicated, for the use in the evaluation of equipment.

This is a representative plot. There's lots of such plots, but it's representative. It shows the overturning moments at various levels in these buildings from the north-south excitation. What's called JAB was the original design values as calculated by the John Blume organization.

The other values are calculated using the damping levels I already talked about and for .2G Reg Guide spectra.

The other thing to point out here is in the original design analyses, some very conservative analytical approaches were used. The way the modes were supraimposed, the way the displacement effects were accounted for, led to some fairly high calculated overturning moments and shears as opposed to what you would get from dynamic analysis.

So that, in this particular area, and this was representative of a lot of cases, the forces that we calculated in members actually were less than were used in the original design.

DR. ZUDANS: This was the stick model reactor?

DR. KENNEDY: Typically stick models, yes, but that included the effects of torsion.

DR. ZUDANS: All right.

DR. KENNEDY: But in certain others, and in the east-west component -- and this was primarily due to torsion in certain areas we got higher forces and moments than were used in the original design.

The only area of the reactor building that we had -- reactor building, turbine building -- that we had what we considered potentially significant problems, was at this intertie. And so we did have to go back and do a careful re-evaluation of the intertie between the two buildings. And we found in the careful re-evaluation that we basically -- the intertie is adequate. It is highly stressed. It is stressed close to ultimate, but that it ignores a potential inelastic capability of this intertie. And that high stress is due to the fact that the torsional effects were not considered in the original design.

But we judged that intertie to be adequate. It is within structural capacity.

DR. ZUDANS: Wouldn't this also be subjected to shears not necessarily just from proportion, but from the fact they have excitation that could be lagging?

DR. KENNEDY: Right.

The shear effects were considered in the original design. The relative shear effects were considered. It was only the additional effects due to torsion that were not considered in the original design.

DR. ZUDANS: What would happen if that wall broke?

DR. KENNEDY: If that intertie broke?

DR. ZUDANS: Right.

DR. KENNEDY: I don't think that would be a major critical problem.

DR. ZUDANS: (Inaudible) for the whole thing, or no?

DR. KENNEDY: The structure is intertied at the base at this location as well, yes. Yes.

I don't think it would be -- I don't judge -- in our opinion, that intertie will not break. We judge it to be adequate, that --

DR. ZUDANS: It would be limited?

DR. KENNEDY: It would be very limited. It wouldn't have any impact, in my opinion, on the reactor building It could have some detrimental impact on the turbine building and on any pipes, et cetera, that join between the two buildings. Sure.

Now, I want to show briefly -- and John Stevenson will go into a lot more detail on this -- I want to show briefly the floor spectra that we obtained out of this re-evaluation. This is the east-west spectra. This is high up within the reactor building where we would expect to have some of the greater differences.

This upper limit spectra here is an envelope spectra that includes the effect of torsion. It should be compared with this spectra that would have been typically used for equipment and design and amplified Housner ground spectra, and throughout most of the frequency range, the floor spectra that was provided to Dr. Stevenson for the review of equipment was significantly higher than the original design spectra.

This was one of the reasons why we say there were possible places where equipment does have to be re-evaluated and certain things done on equipment, is that the motions transferred to the equipment generally are quite a bit higher than was considered in the original design.

Our conclusions are that the reactor building and the turbine buildings are -- the structures are adequate. There are no places that we found where the inelasticity exceeds what we judge to be allowable, your ductility factor of 1.3.

In general, the structure behaves elastic. There may be certain regions where it does n t. The dry well, the structure is adequate. The new seismic loads are quite a bit higher than the origi al loads, but the seismic stresses -- we did go back and check seismic stresses through the drywell. The seismic stresses are low, below about 1200 PSI in the concrete elements.

The suppression chamber, we judged, the seismic review team judged, that it could not fully check the seismic adequacy of the compression chamber. The reason is that we are aware there are a lot of other new hydrodynamic loads that the suppression chamber as to take concurrent with seismic.

Those loads are being carefully re-evaluated right now by the licensee. We did not have those loads. We could not calculate the combined effect of the new hydrodynamic loads with the seismic loads.

Seismic by itself -- our judgment is that the suppression chamber is certainly adequate, but we found the seismic loads on the suppression chamber to be small compared with the new hydrodyamic loads.

Our recommendations are that in the re-evalution of the suppression chamber, higher seismic loads should be used than the original design loads, and we have made recommendations as to what level should be used and that those loads have to be combined with the hydrodynamic loads.

But seismic by itself, the suppression chamber is definitely adequate. We also checked the vent stack. We again found that the new seismic loads were significantly higher than the original design loads, but again that the seismic stresses were very low, on the order of 850 PSI and lower.

DR. ZUDANS: And the suppression, do you not contradict yourself? You say, details of torus bracing system were not available?

DR. KENNEDY: Okay. What that says is that we looked at the torso bracing system. We did not have available to us whether they are -- they are basically tie rods, the bracing system. Whether tie rods had upset threads or not.

Now, if they have upset threads -- and we made this recommendation in the report -- if they have upset threads, we've judged the tie rods to be totally adequate -- i.e., the ductility is less than the 1.3.

If they do not have upset threads, then we consider that the bracing is an area that needs a careful look at impossible replacement.

DR. ZUDANS: You knew where the bracings were located prior to the --

DR. KENNEDY: Oh, yes. The only question we had was whether they were upset threads or not, and it is my understanding that they are upset threads.

MR. LEVIN: Well, Commonwealth Edison has, subsequent to the time that Dr. Kennedy did his evaluation, provided a detailed report on that which is being reviewed. So today I conclude that, overall, it is a ductile system.

DR. ZUDANS: You have the information on bracing?

DR. KENNEDY: We have the bracing sizes, et cetera.

DR. ZUDANS: You have the details of the details?

DR. KENNEDY: Yes.

DR. ZUDANS: Thank you.

DR. KENNEDY: Then we were not able to get in there and look.

DR. ZUDANS: Okay. Thank you.

NRC/ACRS 6-30

Babineau/ Burrell

Tape 5 Page 1

DR. TRIFUNAC: I have a question. The original design for the concrete ducts damping is, as I understand, 5 percent?

MR. KENNEDY: Yes.

DR. TRIFUNAC: And you used what?

MR. KENNEDY: Ten percent in the reevaluation.

DR. TRIFUNAC: Now there was no substructure -- all of this is damping in the building?

MR. KENNEDY: Yes, this is basically a rock site at Dresden 2 and 3.

DR. TRIFUNAC: Now if you were -- -- to proceed with your original 5 percent, how would some of your present -- -- and statements just now a few minutes ago go through? Would they be changed, would they be similar, or --

MR. KENNEDY: Well, we also looked at the buildings with 7 percent damping. We did not look at them with 5 percent damping. We did also look at the structures with 7 percent damping. There would have been quite a few more elements that would not have passed our scoping analysis; i.e., the forces, the recalculated forces would have exceeded the original design forces by more than 1.25 in quite a few elements, and we would have had to go back and do a detailed evaluation of a lot more elements.

DR. TRIFUNAC: For 7 percent?

MR. KENNEDY: For 7 percent.

DR. TRIFUNAC: Okay. So if you were to work with 5 percent, I suppose that this would apply even more?

MR. KENNEDY: Yes, but that is for a Reg Guide 1.60 spectra, and if you change the spectra then I think you would need to be careful about that statement, in other words.

Yes?

MR. LEVIN: Dr. Trifunac, I think, just to make it more quantified, I think if you go from 7 percent to 10 percent in the frequency range of this building it is approximately a 20 percent change in load, 18 to 20 percent, so that is to give you a feeling for how the loads might change just due to the damping.

DR. TRIFUNAC: Now is somebody somewhere along the line going to get to the physics of how good is that 10 percent? So far, as we always hear, Reg Guide says so much, 5, 7, whatever.

Is somebody going to present a physical basis for that job?

Can it be defended? Can you comment on it right now?

MR. LEVIN: Dr. Stephenson will comment on it during his presentation.

DR. TRIFUNAC: Okay.

MR. KENNEDY: I could comment also, but I will bow and let John comment because he has some detailed data.

But the same seismic review team spent a lot of time discussing damping levels, and I think we are all of the opinion that the difference of damping levels can be justified by test data.

I think that the discussion --

DR. TRIFUNAC: By which?

MR. KENNEDY: By test data that we are aware of.

I think the discussion would take a fair amount of time here. But we could get together and discuss that.

MR. THOMPSON: If you had to substantially revise the probable seismic intensity of any of these sites and follow -- are we keeping alert to advances in knowledge about this?

MR. KENNEDY: I should probably --

MR. LEVIN: Could you repeat the question?

MR. KENNEDY: The basic question is: for any of these sites has the -- I guess my interpretation of the question: for any of these sites have we judged that the peak ground acceleration would have to be higher than the original design basis? Is that what --

MR. THOMPSON: All right.

MR. KENNEDY: Does that restate one of your questions?

MR. THOMPSON: Yes.

There hasn't been much emphasis on how one is keeping track of advancing knowledge about the techtronics of these areas. Is that going to be covered somewhere?

MR. LEVIN: It will be covered this afternoon in the A-40 presentation. To answer your question briefly, there are sites where it has increased over the FSAR values.

DR. OKRENT: If I could put these last two questions in a different context. If any of the reactors excluding San Onofre were in the state that San Onofre is located and you had Governor Brown as the governor and so forth, would you use the same assumptions in the reevaluation that you are using?

MR. KENNEDY: That would be my recommendation, yes.

DR. OKRENT: Now are these the same that are used at Diablo Canyon?

SPEAKER: No.

DR. OKRENT: Then I would like to know at some point how you decide when to use this set of assumptions and how you decide to use those that you used at Diablo Canyon. You don't have to tell me now, but again that is part of my interest in understanding the basis by which the staff will decide if something is okay or not.

I am only trying to understand the rationale, as I say, before your role is up.

If you want to elaborate --

MR. ADAMS: My name is Dennis Adams. I was just going to answer your question. No, they are not the same assumptions.

DR. OKRENT: I knew what you were going to say.

(Laughter.)

MR. STEPHENSON: My name is John Stephenson. I am a senior consultant and vice president, Woodward-Clyde Consultants, Cleveland, Ohio.

My presentation will cover basically the evaluation by the senior seismic review team and the analyses done in the area of mechanical and electrical equipment qualification and quantification specifically for Dresden.

We attempted first of all to identify that equipment which would be considered the thought, or maybe a little different than what you were handed, but they are all in there.

The attempt was to identify the equipment, first of all, as to whether it was active or passive because we would use different criteria for evaluating potentially active components and also to identify those items which would be considered flexible versus rigid.

Flexible for this plant would be components which have fundamental frequencies typically below about 20 hertz, meaning that they would have some response. And the fundamental frequencies of the buildings of the Dresden complex are between 5 and 6 hertz, so this is about 3, a ratio of 3 was considered that had flexibility.

The first item then was to identify those components which we considered based on our experience, and this is the experience of the seismic review team, senior seismic review team, might be of concern. They would tend to be the lower bound problem areas. There are literally hundreds of components in any nuclear station -- electrical equipment, mechanical equipment -- that could and in a normal design procedure would be evaluated, but to make this problem trakable, we tried to select those components which in our experience would tend to be a lower bound capability, assuming that if we evaluate these components and they turn out to be correct then, or if not, then that gives us some information about those components which we would assume are of a higher capability.

The five that we selected as a result of our walkthrough are shown here in this table -- the control rod drive mechanisms, the shutdown heat exchanger, isolation condenser, overoperated valves, and the battery rack. And we have on the righthand column the reasons why we as the senior seismic review team during our walkthrough selected these five items for more detailed review.

In addition to this column, which was done by the senior seismic review team, we asked the applicant -- the licensee in this case, to supply us the analysis and design documentation on a number of generic items.

We specifically asked for a horizontal pump, and the one that we got was a high pressure coolant injection. We asked for a vertical pump. These are both examples of active components which have an importance to safety. Liquid control tank, this is an example of a column-supported tank which are usually of concern if there is going to be a sizing problem.

We also looked, obviously, at the more critical items, reactor vessel and core supports, recirc pump. We then looked at piping systems. These were done for the recirc system, the low pressure core injection system, and there was a typical small diameter pipe run made which is called the Quad Cities Typical Pipe Run Test Problem Number 1. And we did a detailed reevaluation of that item.

In addition then, we looked at these other items in the electrical area -- specifically, the motor control centers 250, the dc and the ac. And the reason for the change is one was made by one manufacturer and the other by a second, so that the seismic qualifications may or may not have been the same. Switch gear, transformers, control room panels, and electrical cable raceways.

We tried to identify those items which are in general generically are of importance, of some critical concern with regard to seismic design.

DR. OKRENT: Excuse me. Before you go on, two questions. I have heard Jesse Ebersole raise the question with regard to the valves which is different, a little bit, if I understand it correctly, than what you have put. If a valve has to act, to do something after the earthquake or during the earthquake and if it could be subjected to distortion, sometimes there are close clearances involved and I am not sure whether his concern is picked up in your item 4.

MR. STEPHENSON: It is to the extent that we consider a motor operated valve as an active component. As an active component we require it to meet a criteria of more severity.

DR. OKRENT: Now you talk about the piping, and he is concerned about whether the valve itself will be able to perform its function. And I am not sure those are the same questions.

MR. STEPHENSON: Okay. On Dresden there was no tests of motor operated valves as such. There have been tests of similar valves used on Dresden 2, and specifically in Ginna they were tested. Similar valves. Not the same obviously. Similar manufactured type.

It was found that they did operate under the seismic input equivalent to Dresden, but we did not address it specifically in this case, no.

DR. OKRENT: Well, then a more general framing of the same question: obviously certain valves have to function if you want decay heat removal to work. Would you say that your selection of components is sufficiently extensive that one could be assured that decay heat removal would work in a sense that everything that needs to move or in order for these systems to function will be able to -- whether it is electrical or mechanical or so forth --

MR. STEPHENSON: The answer is no.

DR. OKRENT: Okay.

MR. STEPHENSON: We selected what we thought were those components which we identified as having perhaps the highest seismic fragility. We did not attempt to identify them on a system by system basis.

DR. TRIFUNAC: Not by some main functions but to --

MR. STEPHENSON: Not by function, but mainly by concern that the type of support that -- our experience has been that, for example, that our column-supported tanks give us problems in seismic design. In many cases vertical pumps give us problems because of the long unsupported intake structure.

So we tended to select -- also tanks in general give us problems today.

Yes?

MR. LEVIN: Doctor, I would like to ask one other thing, is that an important element of this evaluation that you haven't heard about today, and that is the report that the senior seismic review team and some of our other consultants write will be used later for an integrated assessment of these plants, at which time systems types will be involved.

As an example, in the example you gave, decay heat removal, considerations such as manually operated valves and things like that are alternatives in addition to using nonsafety systems.

So those kind of things will be integrated in with these conclusions to reach a more integrated conclusion. That is sometime off in the program, but nevertheless it is planned.

MR. STEPHENSON: The next item then was to identify the actual acceptance criteria or behavior criteria that would be used to evaluate the components that we had selected. And this is the criteria that is used.

This is a comparison then between the original criteria that was used on Dresden 2 and the new criteria. The important thing to recognize, the original analysis was generally based on a .1g or what we would call now an ODE input. And the current criteria is related to a .2g SSE input.

We identify then what we would consider as acceptance criteria. These are the same acceptance criteria that would be used on a plant being licensed today. They are basically the criteria of the ASME code as applicable.

Finally, then I want to give a rundown of what we found in doing the evaluation. The first thing we did is we looked at the calculations that had been performed originally by the vendor or by the architect engineer. We supplemented those calculations where we thought it was needed. In many cases dynamic analysis was not performed. The frequency characteristics of the systems were not performed. G levels for design were picked on a -- well, the only way to describe it is somewhat obscure basis. It wasn't obvious then to how the G levels were selected. And the only way we could determine what they should be was to do some dynamic analysis.

So we did do limited dynamic analysis of the components we evaluated. We looked at the original calculations and we performed supplemental analysis in those areas where we did not feel that the original calculations were sufficient. And the result of these -- these were simple. We did not spend in any case more than about three or four engineering mandays of effort. Where it was beyond that we simply put in a report that a reevaluation was required. We only did that level of effort that would take in the neighborhood of about one man -- four to five mandays, one manweek.

And these are the conclusions we reached on the evaluation of the equipment. Control rod drive units and associated hydraulic tubing, design adequacy of the tubing and its support system to .2g SSE seismic event should be demonstrated by analysis. There is no analysis in existence of the tubing itself. There is a seismic test of the control units where they were tested on a chafe table. But the attached tubing and the support of the tubing at the present time there is not an analysis, and in our review of the system it looked relatively low frequency. And we simply don't know at this time whether or not the tubing as it is presently designed would take the .2g earthquake. We are therefore recommending that in this case it be reevaluated by the licensee.

We evaluated the shutdown heat exchanger and found it to be adequate.

We evaluated the isolation condenser. Primarily again the support systems and the areas of high stress and found it to be adequate.

Motor-operated valves, we get back: generic analysis shows motor-operated valves on lines 4 inches should be performed -- less than 4 inches should be performed to show resulting stresses are less than 10 percent of Condition B. This is the ASME code limit for the Condition B or what we used as the active limit for analysis.

Otherwise, stresses induced by valve eccentricity should be introduced into piping analysis to verify design adequacy or provide a procedure whereby all motor-operated valves less than 4 inches be externally supported.

So we make some recommendations. This does not, however, address the specific problem that you have had. We do address that, however, on some of the later plants where we do have test results. And we have used the test results we have gotten from the later plants to satisfy ourselves on Dresden that it is not a generic problem as such.

DR. OKRENT: Did you look at the dc power system sort of in its entirety as part of your --

MR. STEPHENSON: We looked at the dc motor control centers. We looked at the battery racks. And that is it. They are two of the components that we thought to be the limiting cases. We again did not attempt to evaluate on a systems-by-systems basis.

But we tried to identify those components which we considered to be the weak ones.

MR. ZUDANS: Jim, on item 4, what is the implication? You looked only at motor-operated --

MR. STEPHENSON: No, what we are saying is that there were some calculations that had been done by the architect engineer on Dresden which indicated the stresses were relatively low in pipe sizes 6 inches and above, and yet there were no calculations that showed they were -- there were no calculations on 4 inches and below. My personal experience has been that four piping systems 4 inches in diameter or less that you had better externally support motor-operated valves, or you are going to run into stress problems.

It is just a personal experience, and we said either use them in the calculations or externally support them. We are not satisfied with the way they are now placed.

MR. ZUDANS: And for 4 or larger the analysis either was done or your experience said that you don't need supports?

MR. STEPHENSON: That is correct. And they did do some analysis for the larger pipes. Interestingly enough, they did not select any of the smaller piping when they made the evaluation.

We looked at the reactor vessel. We did not look at the recirc pump. There simply were not available any detailed calculations. And we decided that that was a job beyond the one-week type of scoping.

Also, the material that we had in the reactor vessel and internal shroud support indicated it was probably good up to about .6g, which in our opinion is near marginal, and that there is some detailed degree evaluation should be done there as well.

We go on then with the other items we looked at, piping. We looked at the research system, low pressure core injection system in a small diameter typical pipe run, example 1.

And the only problem we had really is there were some assumptions made as to what the fundamental frequency of the piping systems were and those would have to be demonstrated, or alternatively, there may be some overstress in the pipe supports.

The piping itself seems to be okay, but there may be some problems with pipe support ove ress.

These are detailed in the report.

This is again a repeat of what was said earlier. The support or anchorage of electrical equipment, including control panels, instrument racks, switch gear, transformers, motor control centers, et cetera, do not appear in general to have been engineered. Positive anchorage of such components appears to have been decided in the field without any specified material, design, fabrication, or inspection requirements. Supports or anchorage for electrical components should undergo a general engineering review to assure design adequacy.

But basically what has happened, I think, in this vintage plant anyway, that the decision as to how these electrical components will be supported was left to whoever installed them. And you see combinations of bolts, plug welds, tac welds, and every other combination of that type. It is to me fairly obvious that, unlike mechanical components, at least the high safety class components, the decision as to how they were to be supported was not made in an engineering office. They were made by field installers.

Now that doesn't necessarily mean that they are not adequate. It just means that there is no real significant proof that they are adequate.

We did have, there were some tests on instrumentation and control panels not used on Dresden but of the similar components used in Dresden, based on, it happened to be Quad Cities, a sister plant. There were some changes made to the instrumentation recommended. We have not as yet determined that those changes were in fact implemented.

So we are saying that a check should be made that these changes of instrumentation based on the shake table test of comparable equipment should be implemented.

The same is true for motor control centers. There were some tests made. We have some concern about the frequency range of the tests and it would then be a function of what frequency range of the actual equipment installed where there are some limitations on frequency.

Battery racks, we determined that we were concerned about their design, but determined that the only area of real concern was the failure of the wooden battens which held in the batteries themselves. Were they replaced or strengthened we felt the battery racks would be adequate.

Finally then we get into the other electrical equipment. There is perhaps more testing of this equipment than we anticipated for this vintage plant.

Generally speaking, the equipment on the plant itself was not tested but the licensee was able to find comparable tests of equipment at later times and supply that information.

So we feel we have a reasonably high feeling that functional adequacy of that electrical equipment is perhaps better documented in this plant certainly than we had anticipated. That does not mean that additional testing may not be required if you want a complete concern of functional adequacy. But there was a significant amount of it.

The other area of concern is the cable trays. This particular plant does not seem to have any lateral restraint of cable trays. They are the old rod type threaded hangers, and some additional work in our opinion has to be done, either to demonstrate that what is in place is okay by analysis, there is no analysis existing on the cable tray to seismic design, or alternatively, they will have to make some modifications to their supports.

Finally, then I wanted to go on to the question that was raised about damping.

DR. ZUDANS: Can I ask you a question?

MR. STEPHENSON: Sure.

DR. ZUDANS: When the licensee brought forward some test reports on equipment that has been tested after the installation in some other facility, how did you construe that that report applies to the piece of equipment installed?

MR. STEPHENSON: Well, our request of the applicant was, or the licensee was, we need some kind of statement from you, from the vendor, that what you have tested is equivalent or similar to what was installed in Dresden.

In some cases we got that statement; in other cases we did not.

DR. ZUDANS: In other words, you assumed that if the licensee stated that this is an authentic report, you accepted that?

MR. STEPHENSON: Yes. We did not attempt to go in and determine independently that what was in place was in fact similar to what was tested. In fact, I doubt seriously if anybody could do that anymore unless you would identify repeater or switch by installation number.

Now the question was asked, do you have any strong feeling for the damping values we used. All I can say is that there was a paper prepared for the -- -- Conferences last summer in Berlin which compared the existing data that has been experimentally gathered for nuclear systems, nuclear plants, for concrete structures which are similar to the type they are installing in nuclear plants. It will be published soon in Nuclear Engineering.

What I want to call your attention to are these last, columns 6, 7 and 8. Under Column 6 is the Reg Guide 161 values, which would be those that would be used for the SSE for new plant construction today in the United States.

Item 7 is the suggested Newmark and Hall values. This is the NUREG report that we adopted as the SSRT, the damping values we used -- 3 percent for reactor piping systems, 7 percent for mechanical components, 10 percent for concrete structures -- as what we consider best estimate values.

Finally, number 8, are the measured daming values normalized to .9g yield stress, faulted condition, buildings, emergency for component supports, and the damping values that

19

would be associated.

This is based on the actual evidence that exists in the reports that have been published and the tests that have been performed.

I can't explain it other than the fact that these are the numbers and the way they came out. These are the mean values. We also publish the standard deviations and the other statistical parameters. 12.7 for piping systems, 7.7 for mechanical components, 18.7 for concre'e structures.

This one is based on 22 pi es of data. This one is based on about 40 pieces of data. This is based, unfortunately, on about 5 pieces of data.

DR. ZUDANS: Jim, when you say normalized to .9 yield stress, what do you mean?

MR. STEPHENSON: Okay, what we did is we, and as you know, in most cases the actual tests that have been performed have been performed at relatively low stress levels. There are, however, tests in each category that have been performed in laboratories at high stress levels.

And what we did is take the damping trends, if you will. As the stress levels increase in all cases the level of damping measured increased as well. And these are, they have a slope if you will.

What we did is we took the slopes that were determined from the few tests we had where we had results both in the low input range and the high input range. We have tests, for example, where concrete beams were tested above yield. Most of our data, for example, on concrete is tested at probably less than 10 percent of yield.

But we determined then the mean and standard deviation values for the actual low stress results and then applied the damping trend curves developed from the high test -- the high stress laboratory tests to the low stress data to get this number.

DR. ZUDANS: Okay, now (inaudible) your yield was at .1 and you extrapolated the result at .9. What was the stress value at .1?

MR. STEPHENSON: .1 would be, well this would be here. Average of measured data for stress levels at or less than .1 yield for components and piping and 25 percent for concrete. These are the numbers that were actually got in most cases. These are what the projected numbers would be at higher stresses.

DR. ZUDANS: But that is the significant extrapolation?

MR. STEPHENSON: It is, but it is based on the data that exists.

DR. ZUDANS: One thing, one thing. If you take a real structure, it will not be uniformly .9 yield or .1 yield. The .9 yield might be reached at very few points, and at those

21

points I could understand an extrapolation, but in the overall --

MR. STEPHENSON: Well, the same is true even when you test a beam. You only have yield at the point of maximum moment. So even in a laboratory test you have points relatively local -- -- stress.

DR. ZUDANS: Well, but it could be flat (inaudible).

MR. STEPHENSON: Well, for uniform load --

DR. ZUDANS: (interrupting) But you take your reactor building and you model --

MR. STEPHENSON: I agree.

DR. ZUDANS: -- different locations. I think this extrapolation requires to be looked at.

MR. STEPHENSON: I am not suggesting that it is the only extrapolation one could make, but right now it is I think the best we have available.

I agree that, one, you could argue that it is certainly a function of system, overall system stress level as opposed to compo stress level, but at this point I don't know how to make that adjustment.

DR. ZUDANS: For a single component I would buy your extrapolation but not for the --

MR. STEPHENSON: Okay. Well, at any rate, if you use that approach these are the numbers you get.

DR. ZUDANS: Okay.

22

DR. TRIFUNAC: You said example that is extrapolated. How? A straight line?

MR. STEPHENSON: Straight line.

DR. TRIFUNAC: You mean you have a level at a low strain damping and you have a level of high strain damping, gives with a straight line --

MR. STEPHENSON: That is what we did. I am not suggesting, you know, I am just saying this is the first step in doing what you have asked to do. Most of these types of damping values up to now have been judgment decisions. This is perhaps the first, I think, a new step towards quantifying that judgment. It may not be the -- you know, certainly it is not the last work in the field. There is much work being done, particularly in Europe, in this area of testing. The French have a major program, both in concrete and piping, which we hope to get results for in the next six months or so, which will add significantly to the state of things.

But anyway, that was what we, based on this, we feel that the numbers we have used, which is column 7, are not too bad.

DR. OKRENT: If I can carry Dr. Zudans' and Dr. Trifunac's points along to a next step, if the building is only exercising 10 percent damping in part of the building, and if much of the building is only damping at 5 percent, then the building is okay, but the equipment sees something different.

23

And you are analyzing the equipment as if the building in fact everywhere is damping at 10 percent, if I understand the situation.

MR. STEPHENSON: That is correct. The generation of the floor response spectrum assumes a 10 percent building damping.

DR. OKRENT: Now why is that okay?

MR. STEPHENSON: Mainly because we have assumed linear elastic analysis, and if we get up anywhere near the stress levels that we are concerned about in the equipment or not -- and assuming we don't get in the building, we will get significantly higher damping values. So our feeling is that these are reasonable estimates.

DR. OKRENT: Well, I guess I can't really tell. As you may have gathered from what I asked before -- I will state it again -- my impression is everytime I come to these meetings is the buildings are likely to be okay unless somebody missed something like maybe the torsion effect or so forth. It is the equipment that you need to remove decay heat, which most of the analysts relegate to a later study by somebody else. But this is really what has to run.

end T. 5 Bu ell

Tape 6
NRC ACRS
Exh. Chen.
6/4/80
Babineau/
Datfield

DR. STEVENSON: Also recognize that these analyses are done based on floor response spectrum. These floor response spectrum neglect any dynamic coupling between the equipment and the building. And we know from when we do the analysis where we use an integrated equipment primary and secondary system combination that the response of the equipment is very drastically reduced.

For example, a 1 percent mass ratio between the equipment and the building -- and this doesn't have to be the whole weight of the building, this is the mass of the building effectively excited by the equipment -- is equivalent to 3 percent damping in the resident region.

DR. OKRENT: But some of the things that need to work are not 1 percent, they're not a 10th of a percent, they're not a 100th of a percent mass ratio, but they still have to work.

DR. STEVENSON: Well, they're connected, usually connected, with something that is.

And they're all integrated. You can't -- I'm just saying that our generation of floor response spectrum, in general, is a very conservative step in its own right. And my own feeling is that it's certainly consistent with what we have here.

Again, if we look at what the damping for concrete would be at very, very low stress levels, this is 22 buildings, these are not -- this is -- most of this data is based on actual tests of buildings in ambient conditions. These are not tests in beams. The only time they use the beam test results would be to get the extrapolation or the results. That's still fairly high.

DR. ZUDANS: Yeah, but to go from 5 to 10, your response will reduce by about 25 percent.

DR. STEVENSON: Yeah .

DR. ZUDANS: So it's not insignificant, if you're talking about something marginal.

DR. STEVENSON: Well, but still, 5 percent damping, starting off at extremely low stress levels, is a significant damping at the beginning.

DR. TRIFUNAC: Well, the question arises even whether that low-level excavation mapping counts from the concrete itself. Many of these steps just shake the whole system and look at the weight on the curve. And they don't try to identify where that comes from. And some of us have serious questions about whether there could be 5 percent, or half a percent, in the concrete at that level. Because nobody just seems to look at the data and say, well, this appears to count. Probability. Maybe it counts on the s. 'l.

DR. STEVENSON: In all cases -- that -- in that information we put it in. We did try to use the original data and to segregate out the soft structure interaction phenomena. There were some cases, for example, where at very low stress levels you got 14 percent.

You look at San Onofre, for example. The first -- their test result of their concrete was 14 percent. We threw that out, because we figured it was soft structure interaction.

But we -- this information has -- to some degree, it -- obviously, we don't have complete control. These tests were done at different times, by different people, with different competences, different procedures. But we did review the original test results records and try to filter out those items which we felt were not indicative, as you've mentioned, of the concrete response.

DR. ZUDANS: What tests are being planned by the French?

DR. STEVENSON: Well, the French have run a series on concrete, where they've gotten not only large damping but obviously great shifts in frequency, which is an interesting -- you know, the systems become much softer before they reach yield, even before they get -- because of cracking in the concrete; it changes the stiffness.

And they have finished that series. They are now in the process of looking at a series of piping systems, supported as a hot line would, pressurized.

DR. ZUDANS: The frequencies are, in general, lower?

DR. STEVENSON: Frequencies in the concrete. As stress goes up, the frequencies drop, yeah, as you would expect. The more concrete cracks, the less stiffness, the lower the frequency.

MR. KNIGHT: My I just -- when we look at this, particularly with regard to equipment, it seems to me there's a general trend to concentrate on the structural damping and forget about the other damping in the chain. In order to get the response in the equipment, we've got to now look at the damping in the piping system and we've got to look at the damping of the mounting of that equipment.

And our -- and that has a very significant effect, particularly in the peaks. When the damping goes up at all, when the equipment damping goes up at all, it knocks that peak right down.

And our trend, if you will, to date -- and it's a good one -- is to be very conservative in our choice of equipment to have. I think much of the data you see there portrays that fact, and any other data that I'm aware of is consistent with that.

I don't know, do you feel you've got all the pertinent data there?

DR. STEVENSON: The only thing that's not included that I know of is some information I've recently gotten on Humboldt Bay.

But it -- to my knowledge, this is all -- it contains all of the in situ test data that is in the public domain, without exception.

JO-5

MR. KNIGHT: It's well to keep in mind with this, the rationale, if you will, has to include the whole picture.

DR. OKRENT: I agree. Well, thank you.

Last question.

DR. TRIFUNAC: This might be a long one.

DR. OKRENT: Go ahead. But if possible --

(Laughter)

DR. TRIFUNAC: And I don't know whether it's the right time to ask the question, but Mr. Stevenson might comment, maybe somebody else might comment.

Is it correct that many of the equipment components are essentially designed for the peak acceleration at a particular point, because the frequency of the equipment is a high frequency, the fundamental frequency is a high -- I don't mean all the equipment, but a lot of equipment.

DR. STEVENSON: Well, first you identify it as being rigid or flexibile.

DR. TRIFUNAC: Sure. Sure. But can we operate on that basis, that this applies to a lot of equipment. Now, the question I have is this. It was surprising to me, and, I don't know, perhaps to some other people, to see the records that came out of (WORDS UNINTELLIGIBLE) during the last October earthquake, where the accelerations of the base were not unreasonably large and the building did go into non-linear range, and so forth and so on. But what happened was that certain

JO-6

portions of the building experienced large non-linearities; there was a failure of the columns.

Now, that failure of the columns resulted in unusually high -- of, at least, accelerations that I haven't seen before -- very short duration but very high accelerations.

And I think that when we work on this record a bit more, we will find that actually accelerations at various points in the building -- okay, first floor, second floor, and so forth -- were even larger than what we see in the records in here, because the frequencies are extremely high, hundred, two hundred cycles per second, and we even at the moment don't know how to correct for it instrumentally.

But say we do. It's above required -- very high accelerations. Do we have some something in our procedures, the way we look at equipment, that says, well, it is possible that some high-frequency equipment, if we look at it as being designed statically, is going to -- very fragile equipment -- is going to experience accelerations that are very short in duration but possibly quite a factor above the peak acceleration at the particular level of anchoring?

DR. STEVENSON: We have quite a bit of data -- again, it's in the defense area, where we have hardened structures, where we typically, for example, don't worry about response of the equipment till you get above 25 g's, because of the structure, even though that we can calculate that acceleration, the equipment doesn't respond, or doesn't seem to respond, to it.

That's -- we -- I call it the "football helmet syndrome" -- if you put an accelerometer on a football helmet you'll measure 50 g's. And if you apply 50 g's to your brain for any length of time you'd be scrambled all over the lot. But the truth of the matter is, most of this equipment, even so-called high-frequency, is in the range certainly not higher than about 50 hertz. And the durations, the times you're talking about, are very short. And I think that the answer is that the equipment simply does not have time, in general, to respond to it. But it takes -- it would take a major effort to prove that analytically. I think this is, in fact, the situation.

DR. TRIFUNAC: You see, I'm thinking of a small element, maybe some piece of the board, you know, a (WORD UNINTELLIGIBLE) piece of the (WORD UNINTELLIGIBLE) system somewhere, some equipment that is so small or so fragile that we don't ever get the chance to look at it. It may be a critical piece of equipment.

DR. STEVENSON: If you had an accelerometer on that board when they installed it, I bet you would have measured things like 20 g's, 30 g's, instantaneous, half a millisecond type acceleration. If you instrument equipment in shipment, you get some fantastically high g levels, due to just what you're mentioning. But the equipment itself seems not to sense it, because of the mitigating things that were discussed earlier -- that you have tremendous -- you have gaps, you have supports, all of these tend for low, very low, clearances, very low deformations, to isolate the equipment.

DR. TRIFUNAC: Well, getting back to my question, are we trying to look at this at all, in any way, or we are just --

DR. STEVENSON: In this study, the answer, I guess, is no.

DR. TRIFUNAC: Are we looking at this in any other way through NRC or in other --

MR. KNIGHT: Not specifically. We have not zeroed in on that particular point.

DR. TRIFUNAC: Well, I think that we --

MR. KNIGHT: I ought to add that the -- (WORDS UNINTELLIGIBLE) Dr. Stevenson said because -- well, it's a (WORDS UNINTELLIGIBLE) difficult. Could there conceivably be somewhere in the plant a piece of equipment that might be vulnerable to this type of loading, that's something that I can't (WORDS UNINTELLIGIBLE) we're talking about. You know, that it would rapidly get -- you know, I guess I have a little difficulty with you rapidly get to the point where -- if the electrician took the equipment out of the box and threw it down on his table and put it on, then, you know, you might see these type of things.

I think it's beginning to get well out on the fringes of concern. But the direct answer to your qustion is no, we haven't.

DR. STEVENSON: There is some work being done by the

West Germans at this level, because of the airplane crash.

SPEAKER: Well, (WORDS UNINTELLIGIBLE) on the project manager for (WORDS UNINTELLIGIBLE). One of the recommendations made in there does say there may be increase in structural acceleration in high-frequency (WORD UNINTELLIGIBLE) due to inelastic effects.

These potential increase is uncertain and is difficult to predict. But in small (WORDS UNINTELLIGIBLE) system, that could be a factor. We tried to bring attention to this kind of --

DR. OKRENT: I think we're going to have to go on.

DR. ZUDANS: Just a statement, not a question.

Because of the manner, the way the response vector produced for force now, you will never discover this exciting point, because they are just modeled inadequately to have these high frequencies that result.

DR. TRIFUNAC: Well, these are well outside the range of what is considered, yeah.

DR. ZUDANS: Right, because they just model (WORDS UN-INTELLIGIBLE) model. This cannot produce such high-frequency excitation even if the (WORD UNINTELLIGIBLE) has it. You would have to go to a very fine model, and you would probably find exactly these situations that you describe.

DR. OKRENT: Well, I'm going to have to move us to the next area, since, because of the very interesting presentations and the content, we are now at least 40 minutes behind.

JO-10

Let's see. I think we have the choice of either taking a break, which I think I'll do and have us back at eleven o'clock.

Okay? And that will put us a half an hour behind.

(A brief recess was taken.)

DR. OKRENT: Let me ask a question. Can the people sitting at the left end of the room, using my set of cords, can they hear what's going on? Or --

SPEAKER: Not very well.

DR. OKRENT: All right. Would the people at the table try to speak into microphones and also the staff and so forth.

Can you hear me when I speak? I'm wearing one, but I don't know if it's working.

SPEAKER: Not very well.

DR. OKRENT: All right. I'll ask Dr. Savio to go back to the controls.

All right, Jim. You're up.

MR. KNIGHT: All right. We have Everett Rodabaugh here, from Battelle, and for a very short presentation on the work he has done looking at piping designed to B.31.1. Now, that may seem rather abstract at first, but keep in mind that that's the piping code that was used for all of those -- the football -- well, matter of fact, all of the piping in the older plants, and the so-called non-seismic -- much of the so-called non-seismic or non-Class-1 piping in newer plants. I think Everett's work is particular interesting to this group and to us. If you for a moment look at some of the results also from Imperial Valley, where the steam plant there, the basic seismic design went through a rather severe earthquake with little or not difficulty.

And there are several ways to look at that. One is, you might have lucked out, or it might have been different. I really don't believe that's the case. And I think it's significant that we've looked a little deeper and said, well, why is that true? Everett's work, I think, illuminates that question in respect to the piping codes.

DR. OKRENT: Now, the general topic on the agenda is seismic competence of heat sink, and there's a subtopic there, alternate heat decay removal systems, and interaction of non-Seismic Category 1 structures and systems with Seismic Category 1 structures and systems.

Now, are you going to address the general topic?

MR. KNIGHT: There'll be -- there's -- actually, we're divided here. We wanted to address -- at least, as we understood the question -- the general question of the seismic competence of these systems, whether they're designed for seismic loading or not. I think that's where Everett's work fits in. Cecil is -- (WORDS UNINTELLIGIBLE) is here to talk about the interaction question, from the standpoint of the work being done on Diablo Canyon. Then there will be people here who will talk on the general subject of what should the categorization of these systems be.

I don't see them here. But they will be coming.

DR. OKRENT: Let's see. We had a total of 30 minutes in this area. I'm not quite sure what it is we'll hear. But I'd like you to reserve five of them to answer the question or try to address the general issues in addition to whatever specific information you think you're going to give us.

MR. KNIGHT: And the general question being? If you will help me.

DR. OKRENT: Well, let's see. Under seismic competence of the heat sink, I guess, we're interested in knowing, for example, for SCP plants, but it's also really for operating plants, how you're going to judge this. And then there is the related question of is there merit in looking at alternate heat decay removal systems, if you think you're going to have difficulty judging the seismic competence of the existing one, because it's all -- some equipment is not qualified, for whatever reason. And then again, as part of the review -- and I don't mean only Diablo Canyon, the question is raised specifically there, but you have a lot of other plants -- is there some way in which you expect to look at whether it's -- no, is it important to consider non-seismic structures and systems for these others.

MR. KNIGHT: Direct answer: it is important. I believe we have to do it. And the question now is learning how to do it.

And what I had hoped in having Cecil give you a very quick overview of the work that's going on at Diablo Canyon, because that is at the forefront of our learning curve, at the beginning of our learning curve, to receive your questions and reactions to what was developed there so far.

DR. OKRENT: Well, all right, why don't we assume we'll get a short presentation, or two short presentations, but sometime before the end of the period, could we maybe hear how and when you expect to address these subjects, if you do plan to address these.

MR. KNIGHT: I think I can show you where we are.

DR. OKRENT: And we're going to have to stay on a rather tight schedule here, since, as you know, we're running late.

MR. RODABAUGH: Okay, I'll try to restrict this to about ten minutes.

ANSI B31.1 is used, has been used from since 1935. It represents industrial piping as well as the piping that exists in some of these older pipes -- older plants.

And I want to make a few comments on industrial piping as representative of these plants in general. I want to kind of restrict what I'm talking about, though, to piping made of ductile material, not cast iron, and to welds that are of acceptable quality under this industrial piping code, ANSI B31.1.

Okay. We're trying to kind of put in perspective what's the probability we're going to get failure of some of these existing piping systems from an earthquake. Now, over the past, roughly, eight years I have put on lectures on -- in piping short courses, and part of that is to kind of draw up a list of what's the most probable cause of failure in a piping system.

Well, most piping systems don't fail. They last their ten, twenty, thirty, forty years and are then taken out of service and scrapped. But some of them do fail. And this is kind of a list, starting out, at least, in the order of most probable cause of failure.

Well, in industrial piping -- and it's also borne out in nuclear power plant piping -- the most probable cause is corrosion and/or erosion. Coming down the list, we get a little less certain as to what's the next most likely. But certainly vibration, mechanically-induced vibration, occurs often. Water hammer, steam flow, those are encountered in industrial piping and have been encountered in nuclear power plant piping. External damage, it's in the industrial field, for example, buried gas pipe lines, which will run into thousand psi gas pressure.

The most common cause of damage or failure is, an excavator hits the buried pipe.

Just a list here of what all can happen to piping systems.

Thermal fatigue or they can hear of changes in fluid temperature -- and again we have had this kind of failure in nuclear power plant piping systems. Fatigue due to restraint of thermal expansion occurs once in a while.

Now, these failures I'm talking about are through-the-wall cracks; they leak -- they're detected by leakage. There's been, only in very small pipe in nuclear power plants has there been any failure such as a guillotine rupture, that is, too much pipe can vibrate so much that before it's -- a leakage is found, it'll be essentially in two.

Okay, as we go through these, we finally get down to earthquake. And remember, I'm talking to -- over these -- in these classes to, roughly, 50 people who have working utilities, and in oil refineries. Many of them have lots of years of experience, and I always ask them, "Are you aware of any damage to a ductile piping system due to an earthquake?" The answer has been, invariably, "I have never heard of any." So we're talking about types of piping systems that have been in power plants in some of the large earthquakes in California, some of the large errthquakes in Managua. And Bob Cloud, in a report to the ACRS about a year ago, studied what did happen to piping systems during severe earthquakes.

Now, these piping systems are simply ordinary industrial piping systems, supported in the ordinary way. And the answer -- and Bob Cloud is sitting back there -- but the answer is, essentially, nothing happened to them. In severe earthquakes, with no particular design considerations -- maybe some horizontal sway braces here and there -- they survived the earthquakes. And in at least one case, within two hours after a power plant was taken out of service -- came off of service because of loss of load, it was back working again.

Now, this background experience is not just piping. It is what happens to motor-operated valves, what happens to your electric (WORD UNINTELLIGIBLE), what happens to your battery racks. There's lots of experience, I believe, that should be examined but hasn't been yet, as to what did happen in severe earthquakes.

Well, why does piping seem to be so immune to earthquake damage? If we look at the code allowable stresses, S sub h is the basic allowable stress in B31.1, and has been since 1955. These orders of magnitude, half the yield strength, for Austenitic stainless steels, the basic allowable stress is a fraction of yield strength, a bit higher.

Now, this is 1967 and earlier. In later years there's no change in this, but this set of allowable stresses has gone up a bit at this temperature. So, if anything, the codes today are a trifle less conservative.

Now, with those allowable stresses in mind, a rather common practice in piping system is to support the pipe so that if the stress due to weight is about 1,500 psi. This is just to prevent the pipe from sagging too much, you'll fail to have proper drainage. It's just a common industrial practice of putting in those supports so your weight is in the ball park of

1,500 psi.

Well, with that support spacing, you can then put on some static loads, seismic load, and figure out what the stresses would be due to the various g levels.

Okay, here, then, are the margins on the limit moment. Now, the limit moment is something where you put on enough moment so that the plasticity extends to the cross-section and it begins to deform appreciably in a plastic sense but still not failure. This is not failure. But this is the margin against that beginning of gross plastic deformation.

The frequency yield supported like this is in the ball park of 10 hertz.

There are some floor response spectra -- here is from, an example from, Waterford 3, whereas if you have 10 cycles, you are down here in a low g level. Another one from Waterford is this same sort of thing, at a maximum frequency of the pipe of 10 cycles, and you're way down here. But that's not to say that there are not other floor response spectras where your piping response might be up at this two-and-a-half to four g level.

But even at those levels, with this ordinary, typical industrial spacing, the margin against failure, limit moment, is still in the ball park of two and three.

Okay, let's look at a piece of test data. This is Japanese tests. They've put on, simulated what happens in an earthquake by means of some large weights, putting this on a shake table. You're testing the elbows. And sure enough, they did get failures. They got -- after they shook this little piping subassembly with these weights on them -- they did get cracks. Not ruptures, now, even though there's a substantial pressure in there, they still just got cracks.

But here is the ratio of the amplitude they put on to the allowable stress. Typical prose. See, they put on, oh, six times as much as allowed by the code, in order to get these cracks in these many cycles.

So this is -- this is tough pipe we're talking about. And that's why I ruled out cast iron right at the start.

DR. ZUDANS: But the way they applied this moment was, apply it to an elbow?

MR. RODABAUGH: Yes. Well, it's this little setup here.

DR. ZUDANS: Oh, I see.

MR. RODABAUGH: And when that wiggles, the high stresses occur, at those points. And, of course, that's where they got elbow failures.

END TAPE 6

But even with these very high ratios of stress amplitude, which includes an I factor for the album, due to the high allowable, this one for example 231 cycles.

DR. ZUDANS: That means the moment was in excess of that plastic moment?

MR. RODABAUGH: Yes. And that was because this was a very high frequency, and it doesn't have time to go anyplace before the quake, it starts going the other way.

A very severe set of tests.

DR. TRIFUNAC: At what frequencies are we talking about?

MR. RODABAUGH: Frequencies?

DR. TRIFUNAC: Yes.

MR. RODABAUGH: They are shown on this previous thing here. 3.8. See, they just tuned it to those for the test purposes.

Well, I am pointing out that pipe is pretty tough stuff and has a lot of reserve margin in it. Further data along this line is just to take a piece of pipe with a girth butt weld in it, put on a control displacement this time.

This is the kind of an underriding basis for the design procedure given in industrial piping codes and used for these early nuclear power plants.

Okay, we control the displacement and see how many cycles it would take to get a through the wall crack in this

2

girth butt weld and relate it back to our basic allowable stress. These are just samples of fatigue tests under control displacement. Here is the stress range divided by that basic allowable stress.

Okay, you put on 2.9 times the code allowable. You got 40,000 cycles before you get a through the wall crack. Went up to, say 8.8, 7500 cycles before -- well, and with this static pressure.

So again I am illustrating that there is a substantial range for this ductile steel piping material against even a through the wall crack.

DR. OKRENT: That is not material that has been sitting in the reactor for 10 or 20 or 30 years and may have degraded?

MR. RODABAUGH: That is absolutely true. The defects in these tests, like that girth butt weld I just put on, is the typical girth butt weld but no delivered defects.

No, this, we can always imagine that existing defect will get to you.

DR. OKRENT: We don't have to imagine them. We find them, you know.

MR. RODABAUGH: That is true too.

Okay, we get the same sort of things for elbows or more complex shapes, but again we put on control displacement tests and again to get greater and say 300 cycles we have to put on 17 times the basic allowable stress rates.

DR. ZUDANS: It is interesting that the internal pressure is significant to the use of the number of cycles. We even have a direct comparison with and without?

MR. RODABAUGH: No, this, we are not intending to do that here. It is just a few samples mainly to show you what kind of numbers you get up to here in order to get these cycles clear.

DR. ZUDANS: Each of the responses a single test of the --

MR. RODABAUGH: Yes. And there are hundreds more such tests. I am just --

DR. TRIFUNAC: During each test the S_r of S_h is constant?

MR. RODABAUGH: Yes, that is correct.

DR. TRIFUNAC: We are interested in that not really, no. They control displacement, so that is --

MR. RODABAUGH: They control displacement.

DR. TRIFUNAC: So that is not (inaudible).

MR. RODABAUGH: Oh, you are right, yes. It is.

DR. ZUDANS: It is the displacement that is constant?

DR. TRIFUNAC: That is correct.

MR. RODABAUGH: Yes.

DR. TRIFUNAC: (inaudible) cycle between some other value that is indicated here?

MR. RODABAUGH: Well, in summary certainly it doesn't seem like in the severe earthquakes that have occurred that there is any evidence that any piping has been damaged.

Now supports have been known to have been bent or even broken, but as far as the piping pressure boundary I haven't, except for one small branch line that Bob Cloud mentioned in his report, there has been no indication of any loss of pressure intensity.

So what we really said, and this is coming back to how much worrying do we have to do about these existing plants is that you are getting some help simply from past industrial practice in the ordinary support spacing, and then experimental data shows just basically that barring major defects we do have a large margin of safety again for code allowable stresses.

DR. ZUDANS: This industrial experience, really what is the limit of pipe size in industrial experience -- -- on the high side?

MR. RODABAUGH: Well, in refineries there's lots of 30-inch pipe. In power plants there's lots of 14-inch -- well, if you go on to condenser piping it goes up to 40, 50-inch.

DR. ZUDANS: Okay, then all sizes are common?

MR. RODABAUGH: Yes.

Well, that is the main point of my little talk, that there are margins of safety among ductile piping material. The thing I think that has really mentioned before is that what you want to do is look a little bit at those supports that may be in the existing plants. And maybe this work has already been partially done or completely done.

Look at the floor response spectra of these existing plants and maybe check to see if there is enough anchor bolts in them or if the anchor bolts are strong enough. And then maybe you could -- you see, a weight support can be just a long hanger rod and could swing under that. So there may be places where even though you have got this weight support at an appropriate distance you would have to come in with a lateral support.

So that is the sort of thing that strikes me you would be looking at with respect to piping on these early plants.

That is it, Dr. Okrent.

DR. OKRENT: Thank you.

MR. KNIGHT: We would like to perhaps change order a bit. Bob Baer was here and I didn't see him until I looked back over my shoulder. Bob is going to address the broader question.

(Pause.)

MR. BAER: I believe Roger Mattson was down here earlier and hopefully introduced the overall area -- oh, he didn't? All right, well, let me try and give the introduction that I believe he was going to give.

We are prepared this morning to talk about three items and you may want to eliminate some of it because of the time constraints.

What we had planned was for Chuck Graves to discuss Reg Guide 1.139, and that would sort of provide the committee with the status of our current requirements on decay heat removal.

And then I planned to discuss our plans for the longer approach in accordance with Action Plan IIE3.2 and 3.3, which will be looking at a number of alternate decay heat removal systems, and then try and fill in for Gary Hollahan, whose father is quite ill, who has been working for just the last week and a half or so trying to come up with a methodology for judging those plants that are currently operating that do not have seismically qualified aux feed systems.

DR. OKRENT: This would move this into the next agenda item.

MR. BAER: Yes, right.

DR. OKRENT: I guess we should see whether --

DR. GRAVES: It has probably been compressed a bit --

DR. OKRENT: I am openminded if you want to proceed that way.

DR. GRAVES: What we have done at this point is cut Cecil out of the discussion on what -- a group discussion on what they have got at Diablo Canyon and looking at interactions.

DR. OKRENT: Well, with regard to Diablo Canyon I have seen something in writing that the applicant has submitted. Was it something beyond this that you had in mind discussing?

DR. GRAVES: Basically what we wanted to do was to spend five minutes to tell you what Diablo's program consisted of, how we are reviewing it in the Systems Interaction Branch, and give you a schedule for the forthcoming events. I think it would only take about five minutes to do that.

DR. OKRENT: Well, let's see, the document that they have provided is several pages and it is really detailed. Could you not repeat that?

DR. GRAVES: Oh, yes. I didn't plan to go into that.

DR. OKRENT: And just give us then the schedule five minutes or less, okay?

DR. GRAVES: That is where we are on the learning curve.

DR. OKRENT: All right. Why don't we take that right now then before we get into the more general topic?

DR. GRAVES: Okay.

DR. OKRENT: But keep it as short as you can.

(Pause.)

Why don't you assume we can read whatever it is you are flashing on the screen and so don't read it to us?

MR. GRAHAM: My name is Cecil Graham. I am with the Systems Interaction Branch of the new Division of Systems Integration.

Briefly, what I want to do is go over what Diablo Canyon is doing in their seismic systems interaction program, give you just briefly a background of the program and in just a few seconds what the program consists of, the major elements of it, what we are doing to review the program, and our schedule for completing the program.

As a matter of background, the program was developed as a result of discussions held at the November 5th, 1979 ACRS subcommittee on the TMI-2 accident implications. The program consists of looking at seismically induced interactions between nonseismically qualified equipment and safety-related equipment. The requirement has been subsequently documented in the TMI-2 Action Plan as Item II.c.3.

The program consists of a number of major elements; briefly, initial office activities involve identifying the safety-related equipment to be considered; identifying for the purposes of locating the equipment in convenient spatial distributions according to the fire zones; preparing the criteria.

There are a number of so-called walkdowns associated with the program that will involve going out and confirming the equipment that was identified in the initial office activities; walkdowns to determine interactions; walkdowns to determine interactions among the various compartments; and finally, walkdowns to verify that any modifications have been properly implemented.

The next phase involves the technical evaluation of any identified interactions. Finally, modifications, any modifications that are indicated as a result of the technical evaluations.

The program involves an independent audit by PG&E's QA department, and there is also involved an independent review board that will monitor the programs and report its findings to upper management of PG&E.

The Systems Interaction Branch review will consist, first of all, the review will be conducted by an interdisciplinary review team consisting of about three members of the Systems Interaction Branch, also members of the Mechanical Engineering Branch and Structural Branch as necessary, and we have a consultant from Lawrence Livermore Laboratory.

The review itself will consist of two parts, an in-house review in which we will review the documentation provided by PG&E and an on-site audit in which we will look at -- discuss the information provided by PG&E, review the results to date, observe some of their walkon activities of progress, and conduct our own walkdown of certain selected systems.

I would note, Dr. Okrent, that we have invited the ACRS staff to participate with us in this audit as you wished to keep informed of what we are doing.

The schedule for completing this effort is as follows. There have been two submittals by PG&E so far -- May 7th and May 27th -- and there will be further ones as these documents are modified to reflect the results of the staff's evaluation and as new information comes in.

There will be the on-site audit I previously mentioned, will be held on June 17th through 19th. We expect to get an SER Supplement out addressing the results of this evaluation in early August, and we would like to be able to meet with an ACRS subcommittee in mid to late August, and so that we can meet with the full committee in September.

That is just a quick overview of what PG&E is doing and what we are doing. I will be happy to answer any questions you have.

DR. OKRENT: What is the status of Diablo Canyon with regard to the ASLB and so forth, low power operating -- or anything?

MR. GRAHAM: It is my understanding that it will be, around the end of the year they would be ready for fuel loading licenses unless someone can correct that.

Gary, are you --

MR. ZECH: I think that is assuming of course that they have the -- in the sequence of things in the near term all our licensees that we do get to that point where we do recommend to the Commission. It may well be that by the end of

11

the year we are able to do that. Of course after they get their, if they do get a 5 percent license like the other near term -- plants, their fuel load would be around that time, perhaps after the first of the year.

DR. OKRENT: What are the issues that the staff or the ASLB have to open?

MR. ZECH: I don't have firsthand knowledge of the specific issues. I do believe that they are few in number, however. I can get that back to you fairly easily, I am sure.

DR. OKRENT: I mean, is this issue keeping them from getting a final decision from the staff, or is it something else?

MR. ZECH: I don't think this is on the critical path, so to speak, for the staff to consider. They are being proposed to the commissioners as a poor consideration of a licensee, no.

MR. GRAHAM: Dr. Okrent, I would note that we do have representatives of PG&E in the audience, even though this is not a specific PG&E discussion.

MR. ZECH: I think, to answer your question, we don't have specific information that you are asking for, Dr. Okrent, but we can provide that to you. We don't have a schedule per se that we developed for --

DR. OKRENT: Are they seismic issues or nonseismic issues that are open?

(Pause.)

MR. ZECH: No seismic issues.

DR. OKRENT: No seismic issues. They are issues other than seismic issues that are now the ones that you say are on the critical path? Okay, I just wanted to understand.

MR. JACKSON: However, there is, the Appeals Board is, a motion to reopen the hearing is before the Appeals Board on Seismic Issues on new information, primarily the Imperial Valley earthquake last year, new information gathered from that. And that has been responded to by the staff, and we are waiting a ruling on that.

MR. KNIGHT: But in the licensing process, the matter before the Appeals Board, the license was not only -- -- in the licensing process -- --

DR. ZUDANS: Is this nonseismically qualified systems interaction with Category 1 or safety-related systems limited to Diablo Canyon alone, or will it affect other plants?

MR. KNIGHT: Well, I think inevitably it will affect other plants. I don't think there is any question about that. The impetus for the program stems from Three Mile Island lessons learned and guidance from the committee, and this is where we are starting.

DR. ZUDANS: Will it take six or eight months for each plant to do this type of program or what, how do you envision then completing it for all the other plants?

MR. KNIGHT: I would characterize it as a long term and I think (inaudible) possibly a generic subject.

MR. GRAHAM: I would like to just emphasize, as Jim pointed out the committee recommended that we look particularly at Diablo because of its uniqueness in its location, and we are emphasizing that right now in the Systems Interaction Branch. Of course seismically induced systems interactions are certainly only one aspect of the overall question of systems interaction.

One of the things we have learned so far from this program is it is very difficult and yet important to get the systems interactions categorized to a manageable magnitude. You could bite off too much and won't be able to come out with anything practical.

This is one thing we are facing. I think after we finish the Diablo program we are going to look at that, we are going to look at other types of systems interactions, and then we will be making a decision as to how we are going to apply this to other plants.

Right now it is really too premature to say anything about the other plants. We are really being responsive to lessons learned from TMI and also from the committee's recommendations on Diablo right now. But after we finish we will be looking at how we plan to go forward.

14

DR. OKRENT: At the moment the only question that I have is whether there are interactions of the type that I guess you wouldn't see by a walkdown or by questions of physical location that are related to perhaps electrical faults or malfunctions or whatever.

It wasn't clear to me whether that was also part of luck or whether one knows that these, no matter what they are, are not important to the accomplishment of phase shutdown. I don't want to get into that point now.

MR. GRAHAM: The answer is that it will be considered to a limited extent, to the extent that you could identify by actually walking through a plant. Any seismic failure that might initiate an electrical failure, and the electrical failure could even be a subsequent failure or a consequential failure, it is. But to actually take the systems per se, as you would review a complex diagram of the systems and look at almost a reliability aspect of the systems, no, that part is not.

Only those that are at least primarily and secondarily initiated by, assisted by a seismic event.

DR. OKRENT: I am talking about electrical failures that might be initiated by a seismic event. That is what I was referring to.

Well, let me just mention that for now. Thank you. I think we had better go on.

15

Let's see, how do you propose next to proceed then, Mr. Baer?

MR. BAER: Well, I think we proposed to have Chuck Graves give a brief summary of the status of Reg Guide 1.139, which is a current proposed position on decay heat removal, first, and then as I said before, I will talk about our plans over the next few years of coming up with what we think are much broader requirements.

I thought it would be helpful to start with sort of the status, current status.

The next speaker is Dr. Graves from the NRR staff.

DR. GRAVES: The first slide deals with a summ ry of the Reg Guide 1.139 and its predecessor, which was Branch Technical Position 5-1.

Back in 1974 the staff reached the conclusion that there should be a safety grade way to bring a reactor to cold shutdown. That was imposed on industry. As a result there were appeals by industry, reevaluations by the staff, for the first time approval of the position by the Regulatory Requirements Review Committee in 1976, another appeal and another evaluation, and finally an approval by the Regulatory or -- -- Committee in January of 1978.

That approval involved implementation and essentially the implementation was that plants coming in for the first time as o January 1978 full compliance with the position was required. For other plants partial compliance would be required.

There was another requirement that was imposed on us at the time that position was approved, and that was that a regulatory guide would be issued, go out for public comment, and when that guide came out finally in approved form the branch technical position would be replaced by the guide.

The guide was partially changed as a result of public comment, and then Three Mile Island occurred in March of 1979.

As a result of Three Mile Island the Regulatory Guide 1.139 was revised drastically, primarily because a new version in the regulatory guide considers the impact of Three Mile Island-2 event and considers degraded core cooling and accidents.

The original branch technical position did not address LOCA's, did not address degraded core cooling. That branch technical position is the position which has been implemented in recent times.

The new version of the regulatory guide which includes consideration of degraded core cooling and a variety of accidents has been presented to the ACRS early this year and it is planned to have that guide go out for public comment by July of this year.

I would anticipate there would be a significant reaction to the revised version, as there was to the initial version prior to Three Mile Island, and I cannot make a guess as to when an approved guide would appear.

However, in the present version, as far as the branch technical position is concerned, that is a position that is in the standard review plan and was implemented in recent years.

Now I did have some other slides. I will put them on and jump fairly rapidly through them because of the short time.

MR. MATTSON: Chuck, before you go on could you say what you mean by implemented in the last couple of years?

DR. GRAVES: Yes. The implementation in the past few years did not involve in any case full implementation because no new reactors came in. We then had the partial implementation which was specified by the -- Committee.

MR. MATTSON: In other words, were plants that had already filed for an OL at the time the branch technical position was approved by the Regulatory Requirements Review Committee, they are exempt from the full requirements of the branch technical position, but on a case-by-case review they are compared against a table which was approved in the branch technical position --

DR. GRAVES: Yes.

MR. MATTSON: -- which talked about the most important features to consider for backfitting.

18

End Tape 7
Burrell

DR. GRAVES: That is correct.

DR. GRAVES: That's correct.

MR. MATTSON: And it would only be completely applied to a plant filing for a CP after January '78, of which they are none.

DR. GRAVES: All right.

The next slide gives the highlights of the branch technical position and the previous version of the Reg. Guide. I'm pointing out three areas here. First, the main effect of this position was a bunch of requirements that said that the new staff position was that a plant should be capable of being taken to cold shutdown using safety grade equipment.

Previous history was that the hot standby position was a safe end point. In bringing the plant to cold shutdown, one will have to consider the loss of off-site power, signal failures, and also that the plant be brought to cold shutdown in a reasonable time.

The change in the position in saying that hot standby was a safe end point to saying the plant should be capable of being brought to cold shutdown impacted on other systems.

As you recall, this was a branch position which really initially dealt with a residual heat removal system. However, because of the functional requirements in going to cold shutdown, it impacted on other systems such as instrument error, the secondary side in the steam dump, and the chemical and volume control system. And some of these systems -- they were not seismically qualified and therefore were impacted.

There was one other effect -- I'll jump down to the bottom of the slide -- and that is in this branch technical position, there was also a concern for adequacy of supply of good, emergency feed water. This position on the bottom dealing with the auxiliary feed water supply is not addressing seismic capability of the system itself but does require a seismic category supply which was intended to be good water.

And because of lack of information on the amount of good water that would be required to bring a plant to cold shutdown from hot standby, tests were required and up here under V you'll see test requirements.

The object of the test requirements was to determine how long of a time, in fact, it would take to bring the plant to cold shutdown with loss of off-site power considering the worst signal failures, and concerning principally two effects.

One was how much time would it take to mix borated water under natural circulation conditions and the second was that for some plants there are terminal stress problems in the vessel when cooled down under natural circulation conditions such that an extended period of time might be required to cool down and still meet the terminal stress requirements.

With the information obtained from this test, then, information could be obtained to determine how much supply of good water would, in fact, be needed for a plant.

Now, I said it impacted on other systems and to talk about that, and very briefly cover four processes which were involved in this position of saying you have to go to cold shutdown, should be capable of goind to cold shutdown, using safety grade equipment.

We have a problem of circulation with the reactor cooling system needed. We have to worry about depressurization of the primary cooling system. We have to borate the primary cooling system, and we have to remove heat.

Now, these processes now start bringing in other systems.

First of all, with respect to circulation of the reactor coolant during shutdown, the reasons for it, as I mentioned before, mixing of borated water, uniform cooling of the loops and reduced vessel stress problems.

If you have off-site power, then you can use reactor coolant pumps. There is plenty of mixing; there is no problem.

However, with loss of off-site power, you lose the main cooling pumps and go into natural circulation.

I won't go through all the factors under cooldown and natural circulation in the next item. I will jump, instead, to the effects of seismic events.

With respect to cooldown in the system -- and assuming in this slide that the auxiliary feed water system were safety grade, then one of the principal effects of an extra system is the atmospheric dump valves.

These would be required to cool down the plant and the dump valves themselves are seismically qualified. However, the air supply to the dump valve is, in many cases, not.

MR. MATTSON: I remember these slides from the last time we gave this briefing down here and something that occurs to me since Three Mile Island that didn't occur to me then was that if you are in natural circulation, pressure and volume control is something you have to maintain, but you've lost -- at least on BMW reactors, level indication for the pressurizer because it is not seismic category one. So you would by design be solid, and at least a BMW machine during this natural circulation cooldown and then dumping water on the floor, I guess, in order to say you were doing it with all seismic equipment.

Was that considered in what you did originally?

DR. GRAVES: No. In fact, this was prior to Three Mile Island, Roger and, in fact, what we were considering there was extended loss of off-site power, seismic event, for example, loss of reactor coolant pump seals.

We also considered a steam generator tube rupture, or water hammer -- any of those events where a prudent person might want to take the reactor to cold shutdown and we said if those events did occur, then we wanted the system to have the capability of going to cold shutdown using safety grade.

You notice, I did not refer to a Three Mile Island or LOCA.

MR. MATTSON: The only reason I bring it up is because the slide implies that the only effects one has to worry about for a seismic event, and getting into this situation, are the dump valve controls.

DR. GRAVES: Right.

At this stage now with the lessons learned we have the questions of the instrumentation for the level of the pressurizer, a number of other Lessons Learned effects which were not considered in the slide.

But, again, this goes back to the original branch technical position.

Now, signal failures also affect that atmospheric dump valve, and this requirement of the dump valve has been considered in recent implementation of the branch technical position.

We have to depressurize the primary coolant system duri g cooldown, primarily to get at the RHR system. We're starting something typically something like 2050 PSIA but you can't operate the RHR system until you get down to 400, so you need to depressurize.

With off-site power, there is no problem because you have the normal pressurizer spray driven by the coolant pump dynamic pressure on the RCS pumps.

With loss of off-site power, you lose the reactor coolant pumps, you don't have your normal pressurizer spray, and typically one might want to try to use the auxilliary pressurizer spray, which comes from the chemical and volume control system, which now starts to bring in the effects of the chemical and volume control system with respect to seismic requirements and safety grade considerations.

The effect of the seismic event, for example, with respect to depressurization in this sense, again prior to Three Mile Island, was that the instrument air supplies are typically non-seismic and the auxiliary spray valves are controlled by instrument air.

In the case that you lose some of this ability to depressurize, say with the auxiliary spray, there are some alternate methods. One might try to change the pressurizer level. However, some recent events indicate that it is difficult to depressurize by changing pressurizer level because of what we call thermoconduction limiting the water into the pressurizer. I won't explain about that too much unless you ask questions.

You can go to use of the power-operated pressurizer relief valves which we considered in Lessons Learned Task Force. Normal heat loss is so small that you cannot let the plant consider that the heat loss from the pressurizer would cool the system down, because that would take days.

DR. ZUDANS: Would you explain this auxiliary pressurizer spray?

DR. GRAVES: Yes, sir.

Normally, when the reactor coolant pumps are on, there is a spray capability in the pressurizer. The water from that spray comes from from the scoops essentially in the piping, down the drain of the pumps. As long as you have the pumps, you can spray that way.

If you lose that capability, then there is another line leading to that spray which comes from the chemical and volume control system which would then be supplied by water from the charging pumps. Now, that particular path involves a number of valves -- several valves, rather, which are air operated. If you lose the air, you lose control of the valves, and there are two ways to lose that pressurizer spray.

One is that the valve, in the auxiliary pressurizer spray line itself will fail to close if you lose the air supply. You can also lose it by having another valve in a line parallel to it fail to open because of loss of air supply. There are several ways of losing it.

DR. ZUDANS: But these pumps can be operated with off-site power?

DR. GRAVES: I'm sorry?

DR. ZUDANS: These pumps can be operated with off-site power?

DR. GRAVES: Yes.

Now, the question of the air supply is a question of the seismic event, which is --

DR. ZUDANS: We have this type of capability in OPWRs?

DR. GRAVES: The pressurizer spray?

DR. ZUDANS: Auxiliary pressurizer.

DR. GRAVES: Yes, I believe so. I'm not aware of any plant that doesn't have one.

DR. ZUDANS: Thank you.

DR. OKRENT: How much of the chemical and volume control system is seismic class one?

DR. GRAVES: I can only speak really of

Westinghouse. Well, I can make some statements, but from Westinghouse plants that were reviewed recently.

Those portions of the chemical and volume control system which could be used for boration were seismically qualified. The main problem was not seismic qualification of the piping, the pumps and the tanks, the boric acid tanks, the boric acid transfer pumps, the charging pumps -- the charging pumps were part of the ECCS anyway.

The problem was only of control. In other words that the valves -- in some cases there was no alternate path such that the loss of system air could cause loss of function.

Now, in particular, with the chemical and volume control system, the normal mode of operation is to have a letdown line coming from the primary cooling system and going to that system. That letdown line has a number of valves in it, all air operated, and it is subject to when loss of instrument air, it will fail. It will also fail due to signal failures.

Because of that -- and that's a common weakness -- we did require an analysis by Westinghouse to determine that they could, in fact, still borate if they lost that line due to loss of the air supply and they could demonstrate that, with the air system, they could borate without that letdown function and this was primarily because they had fairly high weight per borated water, either 4 percent or 12 percent weight of borated water, which would be supplied by ECCS equipment through fully-qualified lines and where there was no question that signal failures could not knock out the process.

So for their systems, they would be able to meet the seismic requirements in all cases except for the air. In some other plants, I believe in some BMW plants, they could not meet that requirement. There were some weaknesses, I believe, with respect to boric acid transfer tanks and pumps and, as I understand it, a new system has been proposed that is going to come in in July for Midland which involves a new boration system for that plant, independent of the chemical and volume control system which will meet the requirements of that position.

So there you've got it. In their case, they did put a new system -- the plant put a new system in.

DR. ZUDANS: Is there a dedicated pressurizer spray system under consideration, at least at thinking level, other than the --

DR. GRAVES: Well, in a way, there would be if the position were met, you would say that there is a dedicated auxiliary pressurizer spray system which would be safety grade, the full position.

DR. ZUDANS: If you qualified this.

DR. GRAVES: That's right.

DR. ZUDANS: Then the plants could already have it -- and they could be used if the pumps are used for something else?

DR. GRAVES: Well, if you lost the main coolant pumps, then you would have that capability. There is another possibility of course, that it would be used to open the power-operated relief valves and the pressurizer and blowdown to the quench tank. However, for extended blowdown, this would rupture that tank.

DR. ZUDANS: Thank you.

DR. GRAVES: Boration was one of the principal unknowns. Because of this, we required a boration test. That first test was scheduled to be for Diablo Canyon. Diablo Canyon has supplied us with a pre-test analysis. They will be supplying us with operating procedures after they run that test. They will be provided with a post-test report describing the results of a test and how those results could be used to size auxiliary feed water supplies and determine the amount of time taken to cool down a plant under natural circulation.

The main problem for boration is that if you remain -- for example, if you remain at hot standby to perform a previous position, you would still have to borate. Even though you are operating on the safety valves, you are at high temperatures. After Xenon burned out. decayed from the system in about 36 hours or 24 hours, you would have had to borate some, even at hot standby.

In going from hot standby to cold shutdown there is an additional boration requirement. An example of this is underlied the parts per million that I have shown.

With off-site power available, the reactor coolant pumps easily handle mixing. However, again the problem is natural circulation.

The effects of a seismic event, for example, which would be of interest here, again it comes in in the chemical and volume control system, the fact that the instrument air is nonseismic for many plants. There are some plants, as I mentioned, where there is, in fact, nonseismic equipment. which should be considered, and finally atmospheric dump valves which are used to cool down th· secondary in advance of primary, have a nonseismic air supply to them in many cases -- not all cases. Sequoyah does not, to my knowledge.

Alternate methods, again, would be no let-down and that will be demonstrated by test, that they can borate without letdown because of the problem I told you about loss of air supply of letdown mines.

There is another possibility of borate by changing pressure level and contraction. This has been demonstrated by analysis only.

Coming just about to the end.

Heat removal primarily concerns the steamdump valves and the auxiliary feed water system which will be treated separately. Again, the problem with the steam dump valves is not the valve itself, but the air supply.

Now, this was the implementation I discussed before, full implementation for all plants doctored after January '78 and partial implementation for plants in-house before.

The partial implementation involves primarily the auxiliary feed water supply and acceptance of manual actions to correct for nonseismic instrument air, which was allowed in the implementation.

I believe that's all that I will cover, since this meeting is limited to seismic events.

I tried to illustrate, for example, the status of the staff work in this area. One, with respect to the branch technical position, which is part of the standard view plant and has been used in implementation. The second is a Reg. Guide which has been revised and has gone out again for public comment and which includes effects of Three Mile Island.

Are there any questions?

DR. OKRENT: I think maybe the following discussions are related to this?

DR. GRAVES: In one case, Bob, you're handling auxiliary feed water which, in a sense, is part of this. This position impacted on auxiliary feed water primarily through the supply, not the system itself, but the supply of feed water.

DR. MATTSON: We understood your agenda item two to go generally to the goodness of decay removal and we have a couple of things going on. First, we wanted to talk about the 1.139 and emphasize the seismic things that were treated in there and now Bob Baer is going to summarize, from the TMI action plan, what we've agreed to in response to the committee's earlier comments on decay heat removal generally, which will, of course, include seismic considerations.

And then, specifically, the question of seismic qualifications of auxiliary feedwater systems.

DR. BAER: Thank you.

First, let me briefly discuss our longterm plans in regard to the overall problem of decay heat removal. Task action plans IIE 3.2 and IIE 3.3 will involve the study of alternate decay heat removal systems.

The ACRS in their comments on the draft action plan commented that there is a need to evaluate these heat removal requirements in a comprehensive manner and the staff agrees with those comments, and we intend to interpret those two action plans I just mentioned in a very broad sense to study all the alternatives that seem to have some merit.

We want to meet with the appropriate ACRS subcommittee and we have to identify which one that is and consult with them on all aspects of this problem and I think there are at least three key items: One, determine the objectives of alternate decay heat removal systems. There are three or four that people have mentioned that I am aware of. There may be more than this, and their objectives seem to be a mixed bag in some cases.

And then, decide upon the functional requirements for these decay heat removal systems, and then the candidate systems and some likely candidates, at least that have been mentioned, have been the bunkered system, as independent as one can make it from other decay heat removal systems.

Bleed and feed has been suggested as a viable alternative.

The staff, in responding to the results of Three Mile Island has taken the route as a first step of getting highly reliable aux feed systems and that perhaps is a good approach.

People have mentioned something called a high pressure RHR and I'm not sure whether that's the same as a bunkered system or not, but there are a bunch of candidate systems that we think ought to be studied and plan to study.

We think this will take about a two or three year period and, at the end of that time, hopefully we will have firm recommendations on requirements both for new plants and for backfitting of existing plants. And that leads to the question of what to do in this interim time period of two to three years, especially for those plants that do not have fully seismically qualified aux feed water systems.

On May 20, Roger Mattson wrote a memo to Mal Ernst, my boss -- I don't know if you all know Mal. He is sitting there between Roger and Gary -- and, in his memo, Roger directed Mal to develop a specific -- I'll read it. The words are very good -- "to develop a specific rationale and approach for dealing with the question of aux feed water systems seismic qualifications.

By the way, Dick has copies of this memo. I don't know whether they've been distributed or not, and there is certain back-up attached memorandum to it, also.

For those plants that do not have an aux feed water system designed to seismic category I requirements, developed guidance to the division of licensing so that they can make a decision whether or not there is a basis for continued operation for the two to three-year period that will be required to study all the decay heat removal alternatives in accord with the Task Action plan IIE 3.3.

That assignment was assigned to me as head of the

Safety Program Evaluation Branch and Gary Hollahan has been working on this problem for about a week and a half -- well, actually about a two-week span but has had some other duties and probably has devoted a week and a half to it.

Gary was going to make the next part of the presentation but unfortunately his father is very ill and he left on an emergency situation yesterday. So I have his presentation slides. I have discussed this enough with Gary so I'm certainly generally familiar with the approach that he is trying. Hopefully I can field the detailed questions, but I may not in all cases.

He has been working with people in the systematic evaluation program branch, trying to formulate an approach that we can use to determine the adequacy of the basis -- well, really, whether or not there is a basis for continued operation of those plants that do not have fully seismically qualified aux feed systems.

And the first cut of this, by the way, was performed by the Division of Operating and Licensing. I think the SEP branch, specifically. I think they came up with ten plants as a first cut that are, let me call them suspect at this point.

What I hope to do in the next few minutes is run through a series of slides, define the statement of concern, the scope of review, the review approach, the review steps, and then say a few words -- I don't have individual slides -- on plant application and expected results, but to say just a few words about that.

The concern, simply stated, is that in many plants, the auxiliary feed water system was not designed for seismic category one. . Reg. Guide 1.29.

And the safety implication of this needs to be reviewed and appropriate licensing action needs to be formulated.

And, as I said, we are trying, in my branch, to come up with a methodology that would provide guidance to the Division of Licensing, Division of Systems Integration and the Division of Systems Engineering, performing detailed reviews on the plants. And the steps that we see ultimately have to be performed is a clear identification of those plants where the auxiliary feed water system is not completely designed outside of the category I criteria.

And as I say, the first cut made of this, there appear to be about ten plants. Since Gary's been working on it, there are a couple more that he has come across that may be added to the list. On the other hand, as part of, you know, Lessons Learned, which required certain plants to have procedures for using seismically qualified cooling water systems as a source of water for the aux feed.

Some of the plants involve carriers seeing in addition to the original ten. The problem seems to be the that the condensation storage tank was not seismically qualified. I left out that key point.

But some of those may have physical means and appropriate procedures for using seismically qualified cooling water systems.

We have to certainly identify all the plants that are involved, evaluate the importance of safety, and I will get into that in a little more detail. We are thinking of a risk assessment approach and then, depending on the results, recommend appropriate licensing action.

The basic review approach that we're trying to come up with the methodology is really to compare the risk of a core meltdown to a seismic event, with the risk of core meltdown due to other events. We think this is probably the most rational approach to take on most plants where you do not have fully qualified aux feed systems -- seismically qualfied aux feed systems.

It's really a two-step process. One is the probability of a given value of an acceleration and the SEP people have reviewed -- many of these ten plants are SEP plants. Not all, but many of them, and they have done fairly extensive reviews on those plants and I think, as I understand it, have a pretty good feel for what sort of SSE we would require for those plants today -- not necessarily what they were designed to do originally, but what we would require if we were licensing those plants today.

And then combining that sort of SSE and a prob.bility of having something lesser or greater than that, and with the probability that the system would actually fit as the function achieved a g value. One of the previous speakers discussed the fact that there is a fair amount of data that indicates that the equipment can survive fairly significant seismic events, whether or not they were specificaly designed for it.

And there are some techniques that Gary has been working with, I think with Howard Levin -- I don't know whether Howard has left or not; I saw him here earlier --

DR. ZUDANS: When you are talking about failure here, you are talking about an auxiliary feed with the system.

DR. BAER: Yes. We are concentrating on aux feedwater systems in this particular case. Now, for those plants that don't have fully qualified aux feed systems, we'll look to see if there are alternates like the feed and bleed, and whether those will work with some probability and those have some other problems besides the operability. There may be some questions of fracture toughness that are really exceeding the Appendix G values of having the plants at high pressure and cold vessel temperatures for feed and bleed.

DR. ZUDANS: You know that this system could fail even without being Category one. You would have to apply all the other loads to that system, not just the seismic. So it has to be just one component, not necessarily the largest.

DR. BAER: I'm not the stress analyst. As I said, we're going to be trying to define sort of a general methodology.

As I understand it, there are techniques -- they are probably largely judgment, where people who walk through a plant can make some judgments as to the G value that various equipments could withstand. I assume that they are considering other loads in making those judgments.

DR. ZUDANS: So this review that you described did not say that?

DR. BAER: I have a slide here on the review steps, which is in a fair amount more detail.

This is not a key part of this. Hopefully, Gary put it on this slide and I'm about to take it off, we can do this by groupings of plants, both in terms of the equipment that isn't seismically qualified, group plants in that manner, and also by what we think is a reasonable SSE by today's standards.

If we can do this, it would mean that perhaps we don't have to review each plant individually but could use one review for several plants. But that is not a key part of the approach because each plant could be reviewed individually if necessary.

One has certainly to define the aux speed system, and define the detail needed, whether it is system or component. Although I think we probably feel that it is component level at least right at the moment. Then working with the SEP people and Jim Knight's people to determine the probability of a given ground acceleration as a function (g), and determine the appropriate methods treating system redundancy. This is really a just a fault tree approach where there is certainly credit for --

I will only add one step. The basic approach assumes that you can make a determination that the equipment has a fairly reasonable probability of withstanding the (g) values that we think will exists. You are not in the part of a curve. If one plots probability of failure versus (g) value for some component based on a knowledgeable person's judgment, there is some (g) value where it is almost certain to fail, which is failure probability of 1. There is some value of .5 where the curve is very sharp down there.

Hopefully, we are working on the tailend of this curve where the probabilities are relatively low. If the judgment is that the kinds of G values that one might expect at the site are such that you are in the steep part of the curve, then you don't have to go through the detailed analysis. You come out with a probability of .5 or 1, or something where that component is going to fail, and that is certainly not an acceptable probability. You don't have to go through a very detail analysis.

But for those situations where you are in this range of a curve, it does look like the combined probabilistic technique has some merit in making a rational decision, and that is what Gary has been trying to work out such a technique. There is literature along this line. There is some literature favoring the technique, and then there is some literature which Dr. Okrent has co-authored which I think has some concern with the technique.

DR. ZUDANS: Looking at this slide and the previous slide, I assume that implicitly when you talk about safety factors, you talk about failures, you are including in the model all other loads that that particular system sees, not just just certain.

DR. BAER: Yes, I think you are absolutely correct. I think we are going to be looking to the people in the division of engineering to really generate these sorts of curves. As I understand it, if one can define this .5 probability of failure, I think there is the technique for to just put the log normal for lower energy value. But the judgment, and I think that it is largely judgment as to the G value that would cause failure, would have to include all the other loads.

DR. ZUDANS: Then you will decide on some rational basis as to what G value numerically will be acceptable for you not to be concerned about that particular plant.

DR. BAER: Yes, and that would be Jim Knight's people, or really Jackson's people -- He is shaking his head.

DR. ZUDANS: I have to go back to Dr. Okrent's original question. What number do you have now in mind, .001?

DR. BAER: I think that it is plant specific.

DR. ZUDANS: No, for the acceptance, can you fall on this curve someplace in your probability of failure is, say, .001. Is that acceptable, or do you go to .0001, or just .1?

DR. BAER: I think, jumping ahead, I think what we will find -- First of all, we have to determine that this technque is usable. It is combined with a redundancy in the plant. So it is a failure probability of individual components working through a system failure probability.

I think what we will find at the end is there will be one group of plants, a better group where the probability of a core melt due to seismic event is no worse than the probabilities determined in WASH 1400. I think we would say that is adequate for the next several years.

At the other end, I think there is going to be at least a couple of plants, from very limited knowledge of the plant situation, but there are couple of plants that were designed by the uniform building code where this technique may not be usable at all. I don't know what the decision would be. That is a licensing decision, but certainly it is going to be hard to say for some of those plants, "Hey, we are satisfied."

I think there will be a group in the middle where maybe we can identify specific fixes that have to be made on a very short time basis to allow them to run for the next several years.

This is my guess as to how the results will turn out.

DR. MATTSON: Let me try to add to that answer.

We are growing accustomed to coming down here in a variety of ways and being asked the question, "What is the number you are using as a general rule of thumb to decide whether something is a problem or is not a problem." There are some of us who are trying to develop that rule of thumb and are more comfortable with that approach than others.

We will be trying to develop a rule of thumb in the division of safety technology, and so questions like this, we will try to use such a rule of thumb. But Bob is saying that today he does not know what it is. The man has not worked on a project long enough to know what he is going to recommend for this particular example, but we are in sympathy with the need to answer that question. We just cannot answer it today.

When we report on the results of this study, and make recommendations to Mr. Denton, I expect us to have made a legitimate attempt to derive such a number for at least applicability in this situation.

DR. ZUDANS: That is acceptable. I should not have asked the question.

What is getting increasingly unclear to me from this presentation, and I don't want to make any negative statements, what is the stated objective of this study? What are you trying to prove at the end? When you have finished all this here, what are you going to be able to say, yes or no, or some qualifiers?

Is this the objective? You want to go through a plant and say, "Your auxiliary feedwater system was not designed category 1, but because the review results are such-and-such, it is all right." Is that what you expect to be able to say?

DR. BAER: We hope to be able to say that on some plants, that it is okay for several years while we are looking at it --

DR. ZUDANS: -- for three years specifically?

DR. BAER: Yes.

I am guessing, and I think on some plants --

DR. ZUDANS: That is the objective?

DR. BAER: I will not say that that is the objective. We are trying to come up with an honest decision, and I think some plants will fall in the category where we can say that. I think others will clearly not fall into that category, and the question would be, can we identify then some fixes that would allow them to fall into that category. If not, then I think the decision would have to be made that they probably could not operate until they made some fixes.

DR. MATTSON: That says it, I think.

The purpose of the study is to find out for the one place where we don't think we have it, that is, qualification of aux feedwater, whether we can wait another two or three years to finish the long-term study that we have agreed is necessary to do. Once we have pinned down seismic qualification of aux feedwater, then we are comfortable to wait the two or three years to finish the longer-term study. It may turn out that there are some plants out of this group of 10 or more that have to take some kind of action between now and three years from now in order to justify their continued operation.

DR. ZUDANS: Okay, that means really a very specific objective, and if you could by some study like this show that although officially the system was not qualified category 1, it would pass the requirement, that would be the answer that you would be looking for.

DR. MATTSON: Yes, if they were safe enough for some other reasons that we would show through risk assessment, through the relative contribution to total risk that this particular aspect of the design contributed. That kind of argument is what he has described here.

DR. ZUDANS: Thank you.

DR. BAER: The next step would be, construct a fault tree for each plant or group of plants. Let me go back to the point that I tried to make earlier that we are hoping that we can group the plants both by amount of redundancy and G value for the site, but that may not be possible. My branch is going to be trying to provide guidance on the methodology. Other divisions, I think, will have to try to implement this. The hope would be that you would not have to do it for each suspect plant individually.

It turns out that the 10 suspect plants that have been identified that is eight sites, so it would not be, I don't think, too burdensome to do eight individual fault trees, and the methodology eight times. If the list of suspect plants increases, then it would be desirable certainly to try and group them in some manner.

The next step is to add appropriate decay heat removal paths to the fault tree, and this would be things like bleed and feed, and see what the probability of those working successfully is.

Then perform the risk analysis. Perhaps this is the answer to your question. One of the things we have to do is define a criteria for short-term actions, long-term (two to three years) study. I think Gary is optmistic. He thought maybe there are some plant you could say were good forever more. But I think that is probably overly optimistic.

Then we will have to, obviously, be getting information from the utilities in doing this work, either individually or again as groupings. There is some available information on the probability of failure for various (g) values, and these are listed in the handouts.

DR. OKRENT: Excuse me, the reference that you give there represent different estimates of (g) value. If that is the best available information you have, you had better start again, I think. The last reference you show there partly was intended to show how poor the situation was.

DR. BAER: Yes, Gary pointed this out to me.

DR. OKRENT: There has been stuff that is better since that.

DR. BAER: I will tell Gary when he gets back.

There is information on the probability of component failure, you are right, on the first slide, and I have forgotten that it was on (g) values. There is some work done by Newmark, and by Dr. Okrent, which I understand has some qualms with this approach, and I have some also.

DR. OKRENT: No. I think the methodology you have outlined is just what I would recommend that you do to study the problem. I don't have any qualms you are taking. We had qualms with the way it was done in WASH 1400, which I think was wrong, but also partly because of the input that was put in as well as the methodology. The approach that you are outlining, I don't have any problems with.

DR. BAER: As I understand, again from Gary, some of the concerns expressed, I thought he said by you, are certainly valid concerns. Even though major components might be judged to withstand a certain (g) value with a fairly high probability, some of the instrumentation and controls might not. He and I talked about how to try to factor into the event trees in an appropriate manner.

DR. OKRENT: I assume that would be somehow included if it is vital to the function.

DR. BAER: Then there is some work that Don and I have done, I think mostly as part of the SEP program, where we have looked at a number of these plants that are suspect, which as I said were SEP plants.

I am frankly not familiar with what the other two references were. As I said, Gary prepared the slides, and he was prepared to give the presentation until yesterday morning.

But the only other items that I would mention, if I can find my first slide here -- We don't have individual slides on either plant application or expected results. I think I have already talked all I can about expected results. We expect to see some plants that we are comfortable with, and some that we are not to varying degrees.

The plant application aspect was merely the point as to whether we would have to do each plant individually or not. There is no doubt that if it is a limited number of plants, I think we could have the resources to do each plant individually. If we can somehow combine plants by redundandy, and reasonable (g) value at the site, then we will try to do that and reduce the number of specific studies that we have to do.

That concludes my formal presentation.

DR. ZUDANS: In principle, you have two groups of plants. The ones that already have category 1 auxiliary feedwater systems, and the others whose auxiliary feedwater systems do not satisfy category 1 requirements. Is that a correct statement?

DR. BAER: Yes, sir.

DR. ZUDANS: Is it possible that some of those which do not satisfy category 1 requirements, in fact, would pass the requirements without any major changes?

DR. BAER: I don't know.

DR. ZUDANS: This study would not be to carry to such a great detail as to conclude any risks, and what-not?

DR. BAER: I think if someone could make that judgment.

DR. ZUDANS: Not the judgment, the analysis.

DR. MATTSON: There are clearly some that could not pass seismic class 1 without major modifications. That would be a plant that had most of its aux feedwater system housed in a building that was not seismic category 1.

DR. ZUDANS: I understand that. But there might be some which would, or isn't there?

DR. MATTSON: There might be, in principle.

DR. BAER: I don't know if Jim could speak to that.

DR. OKRENT: I would rather not get into the detail of specific plants if I can help it, because I think what we are trying to see here was whether the staff was addressing the subject, and how. I think they don't have answers at this stage.

DR. ZUDANS: The reason I asked the question is that it would simplify the amount of effort it needed if you could make an assumption that when we look at the plant, it seems like it might satisfy. Like Roger says, many of them, there is nothing to look at and they won't.

DR. THOMPSON: My question is a little bit similar. If the level of suspicion, or the level of concern is high enough at some plants, on a judgmental basis can't some corrections be started now without some long study?

I understand the study, and the study looks good to me, but you also have expressed a high level of concern. I am a little bit confused as to why some of these things are not clear enough that they can be done.

DR. BAER: If Gary's father had not gotten so ill, the hope was that he would complete this methodology by the end of next week, so that we could actually start working with the division of licensing, and get going on some of the plants.

There has been a first cut made by people on the SEP program looking at alternate system. We think we have a feel for those plants that we're the least comfortable with. Certainly those would be the ones, I think, we would try and look at first.

I don't know, but I am hoping that it is a matter of a couple of months, and not more than that. If it is more than that, we are going to get such a large fraction of this several years that it would not make sense. But we are trying to do it fairly rapidly.

DR. EBERSOLE: The matter of upgrading the aux feedwater system can get to be a sticky business. As you said, they may be another in other buildings and it might be tough to upgrade them in their present configuration.

Are you looking at alternatives to doing that? I will suggest one that I recall we looked at about 12 years ago which was to provide qualified blow-down systems for the secondary side, using adequately qualified valves of suitable size discharging through atmosphere, if necessary, on the thesis that if we could get the pressure in the boiler down low enough, we could put water in with the firehose, or any other low pressure system that happened to be qualified, without bothering, you know, to require the presence of the standard aux feedwater system.

The problem is that in the normal configuration you have got to get it in at high pressure, 1100 tons, and you don't have qualified blow- own systems.

We were not successful because we did not get any support from any direction to do that. But I think that it might be a viable option.

DR. BAER: Certainly, I think, for those plants where we would have a great deal of difficulty showing this risk technique giving reasonable results, that is an attractive option if you can dump valves big enough.

DR. EBERSOLE: If you put enough of them on.

DR. BAER: There are holes in the steam generator, but if the alternative is shut down for three years until we come up with our long-term solution, I am sure plants would think the alternative you are suggesting is pretty good.

DR. EBERSOLE: We were doing it with the natural background of the SAR system and the boiler, which does it as a routine part of the safety rationale, semi-a tomatic release.

DR. OKRENT: Does the staff have other material to cover, or have they covered this part of it?

DR. MATTSON: That completes what we wanted to do.

I gather from your remarks you would like to hear back on this subject once it is complete. If you would, what is the right subcommittee to come back to talk to?

DR. OKRENT: There is an ad hoc working group, or subcommittee, or something, on decay heat removal systems. You can contact the ACRS office, and they will presumably know. The Extreme External Phenomena Subcommittee, we are looking in particular at the seismic aspects. They would try to look at a more comprehensive portion of it. We might give in some input in the seismic area.

Could I ask, you indicated this two to three year review. Is this to be done with the aid of the Office of

Research? Is this going to be done by the staff of NRR? Are you going to have technical assistance programs? What do you envisage?

DR. MATTSON: Those are all alternatives. I don't think we have that planned yet. Mr. Baer is stepping away from the microphone quickly. He is the one charged with coming up with the plan.

What it says in the TMI action plan is that we will develop one, and we will consult with you in doing it. This memorandum that Bob referred to goes one step further and says that it looks like it is going to take a couple of years to complete that. It certainly should not take more than a few months to get that plan agreed to, I think.

There is the outside possibility, you know, that this thing might get declared an unresolved safety issue. We are going to report to the full committee later this week on how we have done in defining new unresolved safety issues. I guarantee that that unprejudiced, from a management point of view, selection process because I don't know what it says about this one or any other.

You may see Friday that this has passed the preliminary screen, and it has been declared an unresolved safety issue. That is one possibility. I don't know.

DR. OKRENT: That would depend on what priority you gave it. You could declare it an issue and give it a priority C or D, and that would be that. The issue would be solved.

(Laughter.)

DR. OKRENT: The reason I raised the question, you may know we had a meeting of the Reactor Safety Research subcommittee, and the full committee is going to spend a lot of time on this Friday. In fact, an invitation has gone out or will go out to Mr. Denton and to Mr. Minogue to give us their thoughts this week, if possible, on the proposals that Research is making for its program.

I did not see in what Research presented Tuesday any focus that would be addressed to the particular task you have identified. That could mean that they are not the rights ones to do it, that you have in mind doing it another way and you don't need them. But if you do have in mind that they might be important, not necessarily the only resource, maybe you don't want to wait two months to identify this.

DR. MATTSON: When you agreed to stick with the agenda this morning, at the outset I agreed to a meeting at one o'clock 15 miles from here to take up that subject.

DR. BAER: I would comment that there is a II new 3.4, which I think is the research activities, and it is to be scheduled. I am sure they will be looking to us for some guidance.

DR. OKRENT: Looking to the agenda, in fact, which looks like the original one was more real, maybe we will be able to make up time later, we still have not covered one item which was in the session either that was to end by 12:00 or 12:30, and that was, namely, what should be minimum (g) value figures for sites that one ordinarily puts in areas of low to moderate seismic hazard. This is not strictly related to auxiliary feedwater.

I did not know whether Dr. Mattson wanted to be here for that discussion particularly -- he is shaking his head, no. What I think I will propose is that we break for lunch now, and when we get back we pick up that part of that topic, unless you have another suggestion.

MR. KNIGHT: We were discussing it, and later on we are going to get into perhaps the action plans with the staff. I think that it is most appropriate to discuss that.

DR. OKRENT: Would you like to defer that?

MR. KNIGHT: We could defer it until that time.

DR. OKRENT: When would you have in mind, then?

MR. KNIGHT: To describe the past action plan for the correct seismic design criteria.

DR. OKRENT: Fine.

DR. JACKSON: We do not have a presentation prepared. My understanding was that there would be a discussion of this item, and we would be here to comment.

We have no position on minimum (g) values.

DR. OKRENT: Okay, we will shift that, then. Please remind that it will be item 5.a. coming up between five and six.

Does anybody want to raise any special point on what we have dealt with in the last hour?

DR. ZUDANS: Could I restate what I said to you?

In view of what Mike mentioned, these high (g) levels in the structure, and in view of the fact that the new criteria will allow some non-elastic deformation, it occurs to me, and I guess this was discussed before, there are energy sources within the structure itself in addition to the energy being entered through the foundation from the source. Maybe energy such energy sources in large cracks can develop and release this energy, and may create local loads that are way in excess of what you would expect being transferred from the foundation. If this an important category of load, maybe that should be somehow evaluated.

DR. OKREND: Maybe we can Mr. Knight to reflect on this during lunch, and tell us whether he thinks it is already being addressed, or whether it is important, or unimportant, or whatever. This will give him a chance to talk with his people.

Is that fair enough?

MR. KNIGHT: Fair enough.

DR. OKREND: Okay, let's be expeditious in eating our lunch, and be back in 50 minutes.

(Whereupon, at 12:50 p.m., the subcommittee adjourned, to reconvene at 2:00 p.m., the same day.)

AFTERNOON SESSION

DR. OKRENT: The meeting will reconvene.

Mr. Zech?

MR. ZECH: I would propose, since we are still lacking a few members, to proceed to the minimum g value discussion. Bob Jackson is here to provide comments or answers to questions as best he can.

DR. OKRENT: Okay. Well, all right. Let me make one or two coments in this regard. Actually, there has been a fairly long history of discussion as to what should be the minimum g value or the floor for design of nuclear reactors. It began as early as the first draft of the seismic and geologic criteria, and there have been continuing discussions now and then.

As you know, the value that is used in the criteria as the Commission adopted them is 0.1 g. The Committee at times in the past has written memoranda suggesting studies to see what were the pros and cons of the larger floor, and so forth. In recent years, the Committee has recommended on some specific reactors that were in for OL's that the staff and the applicant review these specific reactors to see that there was additional margin over and above the design basis that had been used, especially with regard to systems that you need for decay, shutdown, heat removal, and North Ana and Davis Besse and Sequouyah all fall into that category.

The staff on more than one occasion has re-examined the seismic design basis for a plant that it had approved at the CP level and found for one reason or another it would like to have a higher value, and sometimes they have satisfied themselves by re-analysis. Sometimes it has led to design changes and so forth.

So, there is a little bit of history of this sort. At a recent full Committee meeting there was some discussion on this question. It was partly in connection with a specific site where there is a reactor under construction and where there had been questions raised because of new information that has been developed, and again, I am talking about sites for which the design basis is less than 0.2 g in general. Of course, there are other sites above 0.2 g where questions have been raised and are being raised, but at the moment I am restricting myself to the sites that fall into the more quiet kind of seismic region.

So, the Subcommittee was asked to talk to the staff about a couple of things. I suppose one is the old question: for future plants, does it make sense to raise the floor?? If yes, why? If not, why not? Or, if yes for certain plants, or whatever is the reason.

And a second question was, for plants that have CP's, or even plants that are near-term CP's, let's ay, where there may be a greater facility for increasing your capability to provide shutdown heat removal for earthquakes now than there would be for changes you have to make after the plant was finished and operating, should there be some kind of look, and so forth.

So, that is, I guess, a little bit of the background on what is the staff's thinking. I will give a personal opinion on a sort of a philosophic part of this. I question that the staff does the utility a favor by using a low g value at the construction permit stage, you know, and by low I mean .1 to .15, based on what little I know about the costs of going to somewhat higher ones, based on my empirical observation of the frequency with which this has already had to change, my expectation of the great perturbation that would occur were you to have only one of our four earthquakes that occurred in the Mammouth, Yosemite region, almost anywhere east of the Rockies.

I mean, I could foresee a petition from the Union of Concerned Scientists asking that all reactors east of the Rockies be shut down because here is an earthquake not in your previous history, you know, and the trouble is, the way you have chosen to Appendix A is to say, we will use the history in the region, but you haven't in general said we will go somewhat beyond this probabilistically, and what is happening is, when you get estimates of the probability of your SSE's, in fact, the Tera numbers turned out to be, you know, one in 1,000, one in 2,000 -- smaller than -- the frequency is larger than one in 4,000, I think, for the SSE, for many plants, and so forth.

So, again, I say, it is not clear to me you are doing a favor to the applicant, and I doubt that he is doing himself a favor, either, by proposing these low values. It is a hell of a lot harder to have to go back and fix things up selectively.

So, again, looking to the future is one kind of question, and we have somewhat more time, but at some point people may in fact be applying for more construction permits, and I think it is always better not to wait until they have done their design to tell them, gee, I think it would be nice to change this, or you should change it, or whatever.

But there is a more -- oh, I don't want to use the word "pressing," but a problem that is more directly here, namely, for those plants that have a rather low design basis, and let's for the moment talk about, let's say, near-term CP's or CP's, is there something that it pays to think about doing that might, from a cost benefit point of view, from a public safety point of view, for whatever that might be worth doing?

Now, I guess maybe on the ACRS we don't stay within legal confines. We can conceive of part of a reactor being designed to one basis, and another part to another basis, and it doesn't automatically afford us fits, for example. We have already, as you know, as I indicated earlier, recommended that the systems you need for a safe shutdown have addtional margin. I think as you can tell it reflects a judgment on our part that big buildings are likely to be okay, the scram is likely to work unless some of those lines we saw on some of the scram systems give a problem, but you are going to have to remove heat for a long period of time, and you may or may not have off-site power for that period of time, and so forth.

Well, anyway, that is a kind of introduction, so we would be interested in your comments.

MR. KNIGHT: I am Jim Knight.

I guess maybe another category of activity -- it may be inclusive in what you have already said, but aside from near-term CP's, we certainly have OL's, near-term OL's, and we have some that you have asked us to go back and look at in the process of reviewing.

Bob Jackson in a moment will talk more directly, perhaps, to the future, and the desirability of such a floor, and I certainly agree with you that were you to -- were we as an agency to say, this is a prudent thing to do, that we should have a floor that is higher than we have had in the past, the time to do it is now, for a number of reasons, certainly with regard to any plant that is in the -- still in the planning stage. Regardless of the objections which one might hear about all the engineering that has already been done, that would prove to be miniscule compared to the degree of retrofit that might be necessary.

If I may, just for a moment, look at those plants that are much further along, let's look at the plants that are already built, and ask ourselves, well, what do we gain by saying, hey, you really ought to go back and re-evaluate this plant -- let's say it's a .15 g plant, for instance. You ought to go back and re-evaluate at .2. I am personally firmly of the opinion that it is far more meaningful to say, well, rather than do that, we ought to go back and do the type of thing that we are just now starting to do with Diablo Canyon and others. By that I mean, go back and look at the implementation of the criteria rather than changing the criteria. Go back and walk through the plant, look for interactions, look for places where the criteria wasn't properly implemented in the first place.

I think the gain in, if you will allow me to use the term, seismic safety, is far greater there than by upping the g value by some increment, unless it were an extremely large increment. The type of thing you get when you up the g value is a little thicker wall on a pipe, if you were going to do that, to get some more hangers on the pipe.

Conceivably, if it was early enough in the game, you might -- the wall might be a little thicker, or have a little more reinforcing bar. You might make your concrete mixture specifications a little higher, but I don't think that is the type of thing that all of our experience tells us are really the soft spots in assuring that you have adequate seismic resistance. Those soft spots seem to be more in the line of, was equipment tied down in the first place? Was adequate attention paid to adjacent equipment that may not have been required to be reviewed?

So, I guess my plea, and I put it in those terms, would be that we not get so engrossed in the value of this -- well, perhaps I would phrase it differently: that we not become myoptic with regard to the place of the g value per se in the overall picture, and that we keep in mind not only the criteria, but the implementation of that criteria, and how significant that is in the end product.

That is not to say that starting from now, starting with a clean piece of paper, that if one were going to decide for a number of reasons that a floor, a higher floor, .2 g, if that was it, would be a good thing to do, that any of this previous argument is a reason not to do that.

There are a number of other -- I guess one might even call them pragmatic reasons for doing that, and principle, I think, in your mind, Dr. Okrent, is the fact that regardless of arguments about margin you may have beyond what you intended in your design, you open yourself to litigation and endless dispute about the proper design of the plant if in fact you discern at some point that the plant next door or the plant in the same region should be designed to a higher number.

So, I don't dispute the fact that it is -- if one discerns it to be a good thing to do, then one ought to do it across the board. I think that is a very viable regulatory posture, but I do have misgivings about -- for some relatively small increments of g value going back and causing a lot of analysis to be done when that same time and energy could be used, I think, in better ways.

DR. OKRENT: Well, if I can comment briefly, as you know, the committee on existing plants has tried to persuade the staff that they should do the kinds of things that you are now doing, namely, look in detail at the systems you need for decay heat removal, and not just look at some typical buildings and some typical pipes, and so we agree, I think, with the thrust of what you are saying, and I think if you assure yourself that you have adequate margin for everything you need for the existing plants with regard to decay heat removal, and that means presumably you are not going to have to start using borderline damping factors in order to fit your stresses or use some procedures that you wouldn't ordinarily do, or whatever, but that in fact you look at everything that does have to work, and everything whose failure you can't tolerate, that this would be a useful approach.

But that is a different question, I think, than what you might do for plants where they are not completed, or where in fact they are not even designed, or certainly, let's say, if designed, they have not had a CP application, or whatever.

So, I think the two topics are related, but they are two separate topics.

MR. KNIGHT: Yes, indeed. I think it is perhaps appropriate that Dr. Jackson address what was in essence your second point, that perhaps we should look at the reason why or why not some floor, some floor other than that which we now have is appropriate.

DR. JACKSON: I don't have a formal presentation made at all, and as you say, this topic has been around for a long time, although I have never seen it addressed in any specific way. I have seen bits and pieces of it in recommendations on given sites, specific sites, since I have been involved in the branch.

DR. OKRENT: I can assure you it was a subject that was discussed in connection with the first draft of the seismic and geologic criteria. It was a general recommendation in a letter by the Committee. It was -- There was a memorandum --

DR. JACKSON: I don't doubt your --

DR. OKRENT: -- requesting a study be done. There are a variety of --

DR. JACKSON: I would comment that the current staff, as we are, are really not familiar with that history, and I guess we would like to start on a new footing.

I think I have as many questions about this topic as you have addressed to us, and I think I would like to make several comments.

We do for all new sites have to work within a regulation, and that is Appendix A to Part 100, and the concept of the problem or the necessity to come up with a g value at all is directly related to the tectonic province concept and how you deal with earthquakes in the eastern U.S., and therefore the necessity of coming up with an acceleration anchor point for a response spectrum is a result of that appendix.

There are other ways of doing it, as will be discussed in the Task Action Plan A40 SSSP work, and that is why I thought that this discussion might precede that by a little bit and follow on.

We are working with a minimum g value of. I have heard .2 mentioned. I don't see why .25 wouldn't be just as good, or .3, or take whatever a standard plant is and mention that. We would encourage it, from the Geosciences Branch point of view. It would ease our review role and review burden tremendously.

I think that this is actually being done, as I understand it, in the independent spent fuel regulation, in which a minimum acceleration g value is used, in sort of a reward system. Say, stay out of high seismic areas such as New Madrid, Charleston, a few others, sort of undefined, and use .25 g, and you can do a minimum of investigations, especially the regional investigations. Only do foundation investigations.

So, it is a reward system to the applicant. So, I don't see any problem with developing or specifying some minimum g value. The problem then becomes, what is it, and then, what is it used with? I don't know when the early recommendations were made whether they were intended to be used with the Reg. Guide 1.60, or were they intended to be used with the 1940 El Centro spectrum, or were they intended to be used with a Newmark spectrum? I don't know the history of that, but I think it is a matter of how it is used, and we have adopted as a standard review practice to use it with Reg. Guide 1.60.

A week ago, just about a week ago, we were one floor up discussing a site in New England that has been through six years of hearing, the Atomic Safety Licensing Board, the Atomic Licensing Appeals Board, and finally it is before the Commission for decision as to whether or not a number of seismic issues should be reconsidered by the Commission themselves, the Commissioners themselves.

It basically relates to Appendix A application and g values.

It would be our recommendation that -- we have already recommended that Appendix A to Part 100 be modified, that that be implemented and begun as soon as possible, and that this element could be considered under the rulemaking of modifications to Part 100. It would certainly be a viable topic to consider in that rulemaking.

Of course, you know, we have a limited number of CP applications coming in, so there is no -- appears to be no great emphasis on moving forward with this at this point in time, either due to manpower or budgetary considerations, as I understand it.

The other item which relates to the paper that -- the presentation that will follow by Tera Corporation, Larry Wight and Leon Reiter, relates to the site specific spetra program, and the work that has been done in the SEP program, which is believed to be more realistic than a tectonic province approach in determining a vibratory ground motion to be considered for design, would indicate that, for instance, a .2 g for a good deal of the eastern U. S. may be very conservative, if you are dealing with an earthquake with a one in 1,000 to one in 10,000 return period.

Maybe that could be discussed during that presenteation.

So, I think that -- just another comment. You were commenting on the Mammouth Lakes earthquake. What has bothered us is, supposing that in years to come, we come to understand that the Charleston earthquake, which has been stuck at Charleston for -- since 1972, officially, I guess, begins to move around within the coastal -- Atlantic coastal plain province. That would affect about 20 to 30 plants, and would raise their acceleration g values about double or triple in some cases.

So, the way to deal with it, we think, is probably by a more realistic approach, probabilistic approach, rather than a tectonic province approach as currently used.

As you stated, the current problem we have is with OL reviews, operating license reviews for plants that were reviewed at the CP stage by -- with the understanding of Appendix A, but not necessarily under legal implications of Appendix A to Part 100. So, we are now reviewing those, and we know more about the regional geology and seismology, because of programs that were implemented around 1973 by the NRC, seismic monitoring stations, intensive work and, I think, about $7 million in geology funding by NRC research.

All that is just beginning to come out, in fact, in reams of reports which I get every day, and it is hard to implement that into an OL review. In fact, we are doing a CP review at the OL stage for most of the OL plants. If you review the construction permit SCR's for those plants, you will find that they were two paragraphs that were done by the U. S. Geological Survey, and Dr. Newmark, or John Bloom associates, many of them, and very simplistically.

Our burden of -- The current burden on branch members in the Geosciences is extreme, and that burden is placed by the Atomic Safety Licensing Board and the Commissioners themselves. The burden of proof is very high. We do not currently do an audit review in any way. We do as complete and comprehensive a geology and seismology review as is possible. It is very different than many other branches within the agency, and this has been forced by hearings and the rulings from those hearings.

So, more specifically to your question, I say, for future plants, if yes, why. In a very parochial sense I say, yes, we should have a minimum g value. It would clearly ease the review burden. It would decrease the amount of time it takes to review, and it would cause concentration by the engineers at an earlier stage on the need for designing to a higher level.

Now, for plants that have CP and OL within the current regulation which we have to work, we are not explicitly allowed to use probabilistic methods. In fact, there are board members and Commissioners who believe that indeed it does not allow the use of probabilistic methods.

So, I think we would like to move toward a modification of Appendix A to Part 100, possibly develop a different approach for siting, and use as a foothold on that the SSSP work that has been done up to date. Also, refer to the Sequouyah work that was done during the Sequouyah operating license review.

That is about all I had to comment on, and I think I have touched all the points that I had listed.

DR. OKRENT: Mr. Page?

MR. PAGE: Mr. Chairman, I concur with everything Bob Jackson said, but I would be a stonger advocate of raising the minimum g value probably, and the revision of Appendix A, 1.60, and the reason would be partly geological, largely geological and seismological, because I think that literally a number of the g values that were used in their early days and which apply to plants that are now operating are a bit low, and that isn't to say that these plants are hazardous, but it is to say that the margin of safety would be more comfortable if those g values were higher.

There was a time when applicants -- or two was about the lowest value they thought could gain approval. Well, I would make that lowest value substantial, and the reason is that I believe that most parts of the United States will some day be subject to an earthquake, and possibly of a modified Mercali intensity of VI or VII

I see on older maps that a number of earthquakes have imposed an intensity of VII on an area as big as four states. Well, in the future, when we look at the past performance of the earth's crust, are those same areas going to be the only ones that will appear on future maps? I don't think so, and I think -- I agree with the statement that was in one of the Livermore reports that the margin of safety should be just about proportional to the depth of our ignorance. We have seen over and over as time goes on the seismic hazard doesn't get less. It always increases. That is simply because of incremental new knowledge. So, it is because of this that we now see that where some plants were built with a g value of .1, it should have been higher.

I am not saying that it should have been .15 or .2, but for comfort, I would propose at least .2 g's for a minimum value, and possibly even greater, but this in the long run might save money for the utilities, because so many times they have had to go back and review their plans and try to support their position, and maybe even reinforce the structural features of the plants.

It would save quite a lot of mental anguish on all sides, and more literally, I think it would be safer. But I am not -- I wouldn't want to press this if the disadvantages would outweigh the advantages. I would not want the individual study of plants to be reduced if such a floor value were raised, say, to .2. I would want each individual case to stand on its own feet, and I think that each individual site certainly ought to have its own -- its own seismic response calculated, but there could be a substantial minimum value.

I think that as far as retrospective examination of non-conforming plants goes, that is always -- that is a problem that will probably always be with us, as long as new plants are built, and as long as knowledge keeps increasing, whether it is for seismic considerations or otherwise, I think the older plants are always going to have to be reviewed, and I don't think they should be declared hazardous solely because they don't conform to a minimum g value. They just have to be judged individually.

That is all I have to say.

Oh, one other thing. I thin Bob Jackson implied that this is intertwined with the seismic or tectonic provinces concept, and to some extent I guess that is how it originated, but I would divorce it from the provinces concept, and I would just make a sweeping minimum value from coast to coast in the 48 coterminus states, and likewise in Alaska and Hawaii.

MR. MAXWELL: Well, I was very much impressed this morning by the presentation of Mr. Rodabaugh, which seemed to indicate that we had some fullscale models of what happens to piping and valves and cooling systems and so on, in areas where large earthquakes have occurred, and you seem to imply that nothing happens.

If this is the case, then perhaps we are going in the wrong direction. I am not implying that we should necessarily decrease the value of g, but I am wondering if it is really realistically necessary to raise it, because I think what happens is that when you fix it, it is not a ceiling, it becomes a floor, and it will be raised again inevitably, and you have the same problem facing you.

I am not sure how high you would have to go before it would become a true ceiling. I just don't -- I don't feel comfortable about handling the problem this way.

DR. OKRENT: I am not sure why you suggest the word "ceiling." I think whether there should be a higher floor was a question.

MR. MAXWELL: Well, I think, in addition to the fact that indeed we are getting more scientific information, I think the effect of people constantly considering these problems has been that no one ever has the guts to lower it, but it is awfully easy to raise it. It is a kind of a psychological effect more than a scientific one, I believe.

DR. JACKSON: You may see some lowered a little later.

(General laughter.)

DR. OKRENT: Well, let me make one or two other comments. There has been reference, I think it was by Mr. Jackson, to one in 10^3, to one in 10^4 --

DR. JACKSON: I didn't hear the beginning of your question. I am sorry.

DR. OKRENT: It wasn't to be a question. It was to be a comment.

DR. JACKSON: Oh.

DR. OKRENT: But there has been the suggestion that maybe 0.2 g was conservative, if you were talking about an SSC in the range of one in 1,000 to one in 10,000 a year return frequency. Let me say first, when the staff was first trying to set safe shutdown earthquakes, they were looking for much larger return frequencies, and if I can go back to history, I can remember on one occasion -- it was in connection with the Greenwood review, when Carl Step, under pressure, volunteered his estimate of the return frequency at that site, and he said one in 10^5, and this sounded like a big number at that time. This is per year. Big enough that the committee wrote a note to Mr. Munsing asking how this matched the one in 10^7 number, and that was for a serious accident from any single source.

Now, since then, the staff's estimates and everybody else's have come down to one in 10^4, one in 10^3, and I am excluding specific sites near the San Andreas, where in fact it could be larger, but there indeed you don't have the typical logarithmic function, so it doesn't matter.

It is not clear to me that because what was chosen in the sixties and early seventies, and which really set a pattern that was hard to break out of, since that choice has led to now an estimate of return frequency of one in 10^3 or one in 10^4, that that is right, that this is the proper level of safety that the Commission should be seeking.

It may be, but I for one have not seen that demonstrated, and as you know, I question WASH 1400 as an authority for adequacy of seismic design.

So, I would suggest the staff not look to the future and use the one in 10^3 and one in 10^4 number as being adequate unless they have a lot more on which to go than has been -- I have seen them publish so far.

Now, again, it is hard to modify existing plants, and if and when, in fact, one thinks that some of these things may be marginal, at least the ACRS has chosen to be selective in saying where it thinks the emphasis should be placed in looking, but again, let me just say it is not at all clear to me that the Commission would be satisfied in a conclusion that the probability of a damaging accident to the core was one in 10^4, for example, from earthquakes.

Now, I don't go from the probability of the earthquake to the probability of a damaging accident, but the factor of one, that is a factor that remains to be determined, obviously. I don't think it is 10^3 or 10^4, as some people have suggested. I think not by a long shot.

DR. JACKSON: In somewhat of a response, I think that as geologists and seismologists, we always feel picked on for accommodating all the mistakes that everyone else is going to make along the line, but by achieving a higher level earthquake is not necessarily a way of making the plant built better, which seems to be what the need for the end product is. Have a higher g value, and therefore you will have more done, and you will have it looked after a little bit better, and in effect you come up with a better plant. But I know Jim and I have discussed this at length, that we deal with generally a poor data base, and the earthquakes exist, the historic data base is relatively short, and we are dealing with the site, and that input goes in as a specified input within a given standard review plan.

Now, the engineers can then use that and subdivide it up through the plant, and then within separate component within the plant, in any way that they see fit, and I think that we have a standard approach, or currently have a standard review plan approach which attempts to deal with this.

In terms of the probability of occurrence of earthquakes, SSE's at given plants, I know Carl never liked to work with or give numbers, because they are always used too freely and without caveats, which seismologists and geologists always add, and it depends on the data base that was used, how that number was achieved, what specifically was being talked about in terms of the response, what kind of data set went into it in terms of, say, upper magnitude cutoff, and whether you include small earthquakes in that data base distribution, and I don't know how much that factored into his number on Greenwood.

However, in looking back at the past plants and the way they were done, and they have all been through hearings, it appears that this is about the level that it falls out.

Now, earlier there was a discussion on LaCrosse and GE test reactor, GETER. At LaCrosse, for instance, we feel that the probability of occurrence of an SSE there is very low. There is a very limited data base of earthquake information in that area. The early estimates were something, a minimum of one in 1,000, and there was no upper limit given that I recall. It has been a long time since I have looked at that report. And we agree that it is a low probability event.

What led to the show cause order, however on that site is that it was -- the decision was made in very simplistic description that the occurrence of the earthquake was, in effect -- the probability of occurrence of the earthquake was, in effect, the probability of occurrence of an accident, that there was little margin in the liquefaction aspect if you got the SSE, and there was little structural margin left, and that I will leave to Howard Levin, if he would like to discuss it.

On a GE test reactor, the probabilities that you mentioned of one in 100,000, one in a million, were one report by one group. On the other side of the same review spectrum of reviewers working on this is that the probability is one, that it can occur tomorrow, and that we as regulators have to -- usually fall somewhere within that spectrum of thought.

DR. OKRENT: Excuse me. You say the probability is one, it can occur tomorrow. It can occur tomorrow, no matter what the probability is, so I -- those are not sequiturs.

DR. ZUDANS: It is certainty, not probability, if he says one.

(General laughter.)

DR. JACKSON: The probability is very high that it is going to occur then, which would be, for instance, the U. S. Geological Survey position on that particular site.

So, I just -- I wanted to point out that the numbers that you were utilizing may not be in complete context.

DR. OKRENT: I hope -- I don't think that is the U. S. G. S. position, that the probability of offset under the GETER plant is one per year or in some unit of time commensurate with a man's lifetime, or that it is in fact anything close to that. I don't remember reading anything by them that says that that is the kind of probability. Did I miss something?

DR. JACKSON: I don't think you will read things like that. They just -- My point here is not to discuss GETER. We can discuss that directly with them at the meeting in a week or two.

DR. SIESS: Not in San Francisco. Not if you are right

(General laughter.)

DR. JACKSON: In fact, in Sunall, which is right down below the dam which straddles the Calivaris fault, if you believe in real hazards -- But the point I was trying to make was that as a regulatory group, which is what we are, within the branch, there is a sweep of input, a spectrum of input, if you like, from a variety of consultants and peer groups that we deal with, and it is very difficult.

There are those who embrace probabilistic methods wholeheartedly, and there are those who reject them totally and we are trying to deal with that in a formalized, legal, licensing process, and that is what our difficulty is in embracing some of the -- many of the recommendations that have been made by this committee.

And that constitutes a good portion of the staff within NRC itself.

MR. KNIGHT: While you are reflecting, if I could add just one other point, I guess I am troubled, as are many of the folks on our staff, by your feeling that -- well, what seems to be a feeling that, all right, we have a phenomenon here with the likelihood of occurrence 10^{-3}, 10^{-4}, and are either unsatisfied, depending on who you talk to -- some are unsatisfied; some are unwilling to consider the other steps necessary to get you to some unacceptable consequence.

You have alluded to the fact that you don't believe the likelihood of a damaging accident to the core is as low as some would hold. I guess I have difficulty myself seeing how increasing the g value, for instance, albeit it may give you an event with a lower probability, really gets to the heart of that problem. I really think it is probably equal if not more, equally if not more important to find out why, if in fact it is true, that the likelihood of a damaging accident to the core for what amounts to a design condition isn't as low as you would like it to be and as some think it is, and upon finding out why it isn't, do something to fix it.

I think the --

DR. OKRENT: We are on a mixture of topics.

DR. SIESS: Jim, when you said increasing the g value, are you talking about minimum g or just the g value in general?

MR. KNIGHT: At this point, I don't differentiate between them. If you have a design level --

DR. SIESS: I don't see why you don't? We have got a minimum g level. It is one-tenth. That is in Appendix A.

MR. KNIGHT: Yes.

DR. SIESS: And we have had it. And Dave is not suggesting that we not have it. He is just suggesting that we raise it, within the same philosophical framework that we put it in in the first place. The idea of a minimum g was, no matter what you think, we want at least a tenth, and the suggestion is that no matter what you think, we want at least two-tenths. That is based on ten years of experience, and the minimum is going up. I don't think Dave has even gotten the escalation in there for the next ten years.

MR. KNIGHT: Well, you see, I don't disagree with that in principle at all. My concern is that we may develop a mindset, if you will, that we will pop this minimum g up and solve all the problems.

DR. OKRENT: Jim, I don't think we need to mix the different things as if they are incompatible, you can only do one and not the other, in the same way some people seem to feel if you have solar energy, you can't have large central station plants, and vice versa.

If we stay on the question of minimum g for the moment, it seems to me one can make a couple of cases for raising the minimum g to some value like .2 or .25, or -- which -- wherever is the need in the change in construction costs, a real need in construction costs, or difficulties, or this sort of thing.

In the first place, from the technical point of view, we just heard more than once that the current SSE's seem to lie in the region of one in 1,000 to one in 10.000 per year. Now, in my mind, we have no basis for assuming there is a cutoff, in other words, that we cannot have a larger earthquake at the site, and I have to sort of assume it goes up by about a factor of two for a decade in frequency, crudely.

So, in other words, if it is really a one in 1,000 a year earthquake, then the one in 10,000 may have twice the g value, whatever it is you design for. And it may go up still further with some decrease in the probability, in some crude way. If you are designed for .1 g, you can probably take the .2, but I don't know whether you can take the .4. If you are designed for .2, you can probably take the .4, and so, there is a difference in your ability to absorb into bigger structures or whatever some additional amount.

Otherwise, I think your one in 1,000 to one in 10,000 would be intolerable.

MR. KNIGHT: Oh, agreed.

DR. OKRENT: You would be -- really, because if you thought the structures really couldn't take -- unless there was really a design error, if they couldn't take it, it would really be intolerable. Okay.

So, there is this kind of gain that you get in a -- what I will call a technical safety point of view by going to the higher floor, where you would have used a considerably lesser value. I don't mean .18, but .12 -- we have a certain number of plants at .12, for example.

There is also the legal question to which I alluded, legal in the sense that either because of changes in procedures or because of the fact that there is a change in information, you can get into a situation where not just one but a large family of reactors, and somebody cited one case, which has been cited around this table -- Around this table, it has been suggested that maybe you have to move the Charleston earthquake all along the coast. I have heard it here, in discussions, and what would that do to a lot of plants?

Now, the ACRS chose not to press that as a likelihood, but there is the possibility that if you have too low a value for many plants, that a large family will get caught by an earthquake different than what has occurred, and I think this is -- or some new information that, gee, what has happened in the last ten years or so, or 15 years, says we should expect this to occur. It seem to me that is a high probability event, that you are going to have a big surprise in the eastern U. S.

There have been so many developments since we first started looking hard. Let's say, if you just look at the changes that have occurred since people proposed design bases for Bodega Bay and San Onofre 1, just think about it a little. That is only 1963 to 1980, 17 years. These plants have to survive a lot more than 17 years, presumably, economically, I mean.

DR. OKRENT: Chet?

DR. SIESS: Dave, you presented one argument that I think I will argue against, because -- just to save the staff time. There has been such a turnover in the staff, they may have forgotten the argument they gave in answer to it the last time.

(General laughter.)

DR. SIESS: But you pointed out that the two-tenths g design as compared to, say, a one-tenth g design would have an additional margin, and as I recall, the last time that was suggested, somebody pointed out, yes, but then the plant where the initial design was four-tenths g would not have that additional margin, and we would now have plants with varying margins.

At least part of the answer to that is, I don't think we have constant margins now. I don't know how much they differ. I am eagerly awaiting the SSMRP results to find out. I am not holding my breath.

DR. OKRENT: Again, I tried to allude to that a little bit earlier. I think where you start at a rather high design value, .7 g, for example, I don't think you expect to go up by a factor of two, and then another factor of two, going down the probability curve, you have a changed shape of the curve, so when you get to the high g values, there is a change in the probability curve.

DR. SIESS: I think without being very quantitative you have to agree that if you look at the eastern U. S., just like Ben Page said, we would all be a lot more comfortable if everything had been designed to a minimum of .2 g's. I believe the staff would also have found the SEP review a lot easier if all those earlier plants had been deigned to .2 g.

It is hard to quantify that, but once I have accepted a minimum, I have to admit, I am more comfortable with .2 than .1, and I believe my opinion has changed on it over a period of 12 years, maybe because I am less ignorant, maybe because I am more. I am not quite sure, Ben, the depth of whose ignorance you were talking about, yours or mine.

MR. PAGE: I know mine is deep.

DR. ZUDANS: With respect to the margin, as it changes with increasing g level, it is quite clear that as long as the g level is not the controlling load in the system, you have a substantial margin, likely. As you increase the g level, that margin will be used up more and more, and you may not have anything but what is specified.

So, in the final analysis, some plants, some components will be the same whether they are designed for .1 or .2.

DR. SIESS: That is very component sensitive.

DR. ZUDANS: It is sensitive. There is no generalization on that.

DR. OKRENT: I agree.

DR. SIESS: It is different for structures than it is for --

DR. ZUDANS: But -- well, I was going to add the same thing.

DR. SIESS: Well, that is just another way of saying we don't have uniform margins now, and I don't know of any way of getting them.

DR. ZUDANS: No. Exactly. There is no way for us to tell exactly what margins we have, regardless of what --

DR. SIESS: Oh, yes.

DR. ZUDANS: Not precisely.

DR. SIESS: We are spending a few million dollars to find out what kind of margins we have.

DR. ZUDANS: That is all right. It is just money.

DR. SIESS: Don't tell me there is no way.

DR. OKRENT: Well, let's see. Are there other commens in this area? I am not sure --

DR. JACKSON: I would just like to make a final comment, that we are not opposed to a minimum g value, and we discussed it many times. It is a matter of developing a rationale for what it should be and what the numbers should be, and having a basis for it. We have a responsibility also to the utilities and the applicants who come in and fight with us. The staff has never been known to encourage low g values.

DR. SIESS: I would propose we use the same rationale we did for the .1.

(General laughter.)

DR. JACKSON: You can't quite do that these days.

DR. OKRENT: Well, let's see.

Are we ready to deal with the item on -- I guess it was Agenda Item 3, Specific Topics Dealing with Current Seismic Design Practices? Is Mr. Levin first, or whom?

MR. ZECH: Dr. Okrent, Item 3A, I was just reminded by Howard Levin, was discussed this morning as part of his initial discussion.

So, we can go on to Item 3B.

DR. OKRENT: Do you feel, then, that 3A is in hand? Is this the essence of what you are saying, that you are picking it up?

MR. LEVIN: That is the essence of the response. The only thing that may not have become totally clear is that there was -- we didn't dwell on this this morning, but there was an I&E information notice issued to operating reactors, Notice, I believe it was 80-21, which dealt with this issue.

At this time, the staff, pending a more detailed review on the SEP, is evaluating whether to take further action as far as other operating reactors, but nevertheless, those licensee have been notified of the issue.

DR. OKRENT: What is the date of the I&E notice? Do you remember?

MR. LEVIN: I believe it was May the 16th.

DR. OKRENT: Oh, it was a recent one?

MR. LEVIN: Yes.

DR. OKRENT: All right.

So, then this is currently something which has been called to the attention of all the operating reactors, not just the SEP reactors?

MR. LEVIN: That is correct.

DR. OKRENT: All right. I think we can treat that then -- Okay. I am sorry. Then we can treat that as covered. Fine. Let's go on.

MR. KNIGHT: To Item 38?

DR. OKRENT: Yes.

MR. KNIGHT: I guess everyone is familiar with the item, a letter from Mr. Perez that raises a note of concern over the fact that some of the tests that are being applied for equipment are extremely high, and he mentions a number up to 18 g's, and this develops largely from companies developing enveloping spectra to test equipment either for various locations in a plant or to cover a broad spectrum of plants.

And a particular note of concern here is the fact that the equipment is sometimes -- I hesitate to use the word "often," but the only piece of equipment of that type that is going to be tested, and it was purchased, so it is put on the table, tested up to relatively high levels, and if it succeeds in passing the test, it is then shipped to the plant, and installed, to function throughout the lifetime of the plant.

His particular concern is that it has now already seen the SSE, and probably something far greater than even one that Dr. Okrent could come up with.

(General laughter.

MR. KNIGHT: And it is now installed in the plant.

It is a tough question. It is a damned if you do and damned if you don't in some respects, because another argument that often comes up is, well, if you don't test the equipment, how do you know that the prototype was truly representative of what you've got in the plant, or on the other side, how do you know what is in the plant is truly representative of the prototype that was used.

As a matter of practice, we would -- the staff would prefer that prototype equipment be tested, and all of the programs that we have developed and asked industry to emulate are based on prototype testing, and we fully intend that to be a prototype piece of equipment with the actual production line equipment being installed in the plant.

The fact, however, that the g levels seem rather high, the 18 g's seem rather high, I don't think is indicative of the fact that the equipment is necessarily being damaged, or for that matter even severely tested. I think much of the equipment could take something even much higher. You see similar equipment undergoing tests for military applications, and the g loadings are much, much higher.

The only protection against such a concern is a QA program where the equipment is inspected after it is tested. You could certainly, if there are structural problems, either brackets or doors or drawers that have actually been stressed to the point where they are no longer functional, no longer usable, that, of course, has to be corrected. The equipment has to be further shipped, installed, and still pass all of its installation tests, which is a good if not perfect test to assure that there certainly isn't imminent failure likely.

There isn't an easy answer to it.

DR. ZUDANS: Mr. Knight, I was led to believe -- I discussed this question with some of you people, and I was led to believe that NRC has made it very clear in IEEE meetings that they would not accept tested -- would not allow the tested equipment to be installed in the power plant, that these things still have -- you know. Errors occur of that nature, and some pieces, very expensive pieces have been installed.

Don't you have a position, a policy that says you shall test it but that piece is not going to be subjected to two design basis accidents, just one, where one already has happened with certainty, and there is no way for you to decide by inspection whether or not it has been damaged? It is just not likely that you will be able to penetrate every relay, every little piece inside, and be able to say that it is not damaged. It functions all right.

MR. KNIGHT: Well, that is true. No, I would have to -- I cannot say that there is a prohibition, if I could perhaps put words in your mouth. The staff does not have a prohibition against testing equipent and installing it in the plant. There are a number of instances where we have caused people, for instance, to take equipment that is already installed in the plant, take it out and test it and put it back as the lesser of a number of evils.

I think, too, your level of concern will vary If you were concerned -- Let's say it is an electronic chassis, a fairly large electronic chassis. If you were concerned at the next SSE it might undergo structural failure and collapse, entirely wiping out that function, that, of course, is an absolutely intolerable situation, but I do believe you have adequate protection against that. You know if you have seen wrinkles or if you have stressed the -- the chassis of the cabinet.

Now, as far as an individual component, some small electrical component, failing, it might indeed temporarily, at least, knock out the function of that piece of equipment, but I don't believe that it is -- the failure rate is that much more likely than could occur at random.

DR. ZUDANS: But the real issue is not whether or not you can convince yourself or a guy who installs that the equipment still functions. The real issue is that you are now requiring to design equipment for two design basis accidents. That is what it is.

MR. KNIGHT: Yes. That is certainly not a chosen path.

DR. ZUDANS: No, but that is the practice.

MR. KNIGHT: You may well have equipment that is capable of taking -- and this is the fact -- that is capable of taking a number of design basis accidents.

DR. ZUDANS: But the equipment is not designed -- the designer does not have that prescription. He only designs it to support or sustain one design basis accident. Now, if by happenstance after it is tested and has not failed, all of a sudden he thinks he can do two, I would want to test it for the second time before it goes to the plant, because three out of -- two out of three is better than one out of two.

MR. BOSNICK: Bob Bosnick.

I think Dr. Zudans may be getting at the fact that in some cases there are fragility tests done, and that kind of equipment we certainly wouldn't want to have installed in a plant. By and large, most of the testing that is done, the equipment is not installed in a plant, but as Jim has said, there have been cases where this is the only piece of equipment.

DR. ZUDANS: Yes. Well, I am talking about the -- environmental qualifications for optimum LOCA situations, where you not only have the environment as you anticipate in LOCA, but also it is made conservative, so the temperatures are higher, durations are longer, concentrations of your spray are stronger, so you really expose it to something that is conservative, and that is the equipment I am concerned about.

MR. KNIGHT: All of my remarks were aimed at only seismic testing.

DR. ZUDANS: Only seismic? Okay.

MR. KNIGHT: I really can't address the environmental qualifications.

DR. ZUDANS: Just the seismic.

MR. KNIGHT: That is all I can address.

DR. ZUDANS: So you must -- and you design then for two SSE's. Okay.

DR. OKRENT: It seems to me the thrust of the question posed is not so much to large mechanical structures where in fact you have a chance of seeing cracks or torn out bolts or whatever it is. The question that is posed more about the things that are supposed to function inside, and how do you know that they will function as they are supposed to given another earthquake.

I guess I can't tell that you have a basis for this from what has been said. You may have a basis, but I didn't think I detected something other than judgment, and here is a judgment that -- it may be that judgment is not always adequate. It might depend on the specific instrument and the specific amount of shaking or whatever.

This is my understanding of the question being raised. It seems to me there may be a need to think on it a little.

MR. KNIGHT: Well, as I said, it is not a procedure of choice. That is -- And I find that completely consistent with what we have said here. It is not a desirable thing to do. When you get down to the point of making a value judgment, and that is what you make, it is, as you say, extremely component and equipment specific.

There certainly are a lot of things -- You say you don't have a basis. Inspection of that piece of equipment provides you with a lot, and I would never try to fool anyone and say it gave you all you need to know, but when you look at the type of failures that typically occur under this type of testing, and that is important background to have, the worst things that happen are that things like drawers that are mounted on slides and are supposed to be locked in place come flying out, and -- or if it is a piece of electronic equipment, a large number of the cables which fit onto spade type terminals are either pulled loose or come off.

That type of thing is not -- if it doesn't happen during the test, if you don't see stress, then you are pretty well back to a, if you will, as built situation. That means you have normal stress. There are a number of things you can see.

DR. OKRENT: Well, that is sort of what I consider the mechanical engineering approach to electronics. You know, when my television doesn't work, I kick it, because I am a mechanical engineer.

(General laughter.)

DR. OKRENT: But if that doesn't help, I sort of can't tell what is wrong inside, or is there something wrong inside, and I am a little bit unconvinced.

Lipinski, and then Ray.

DR. LIPINSKI: Well, the gist of this Perez letter was the fact that it was tested at 19 g's for 40 seconds, was that it was not then verified for integrity. Now, when these panels are delivered to the plant and connected, before that plant goes into operation, I would venture to say that everything in that panel ends up getting verified as part of the connection and plant checkout.

This may not be true to 100 percent, but I would venture to say that most of that equipment does get tested before you go on to power operation.

DR. OKRENT: Well, is it integrity against a second SSE?

DR. LIPINSKI: No, that is not the gist of this letter. That is another aspect that has been brought into the discussion.

DR. ZUDANS: Well, if you read the letter, he also mentions second SSE.

I think he does.

DR. OKRENT: Yes, that's right. I think the gist of the question is the second SSE.

DR. LIPINSKI: Well, then it says, enter a line for subsequent safety related functioning. It doesn't necessarily say, during another earthquake.

DR. ZUDANS: Well, it says here they are designed for two SSE, because one you gave to it and the other one will eventually come. Hopefully not.

DR. LIPINSKI: Yes, that was the second from last paragraph.

MR. EBERSOLE: Wouldn't it be possible for the manufacturer to certify that his equipment was not subject to cumulative fatigue damage under such --

DR. ZUDANS: No way.

MR. EBERSOLE: -- to eliminate the concern?

DR. ZUDANS. No way. He cannot.

MR. EBERSOLE: Well, it is implied that there is accumulated fatigue damage.

DR. ZUDANS: That is exactly the reason, because --

MR. EBERSOLE: But you can design equipment which is rugged enough not to have that problem.

DR. ZUDANS: But then you could qualify that piece of equipment by analysis, and not subject it to testing at all. But you cannot analyze a complicated panel like that, because there is just no practical way to model it. There are so many little parts, and in such intricate configurations. You can't do that. So that is why they are testing.

MR. EBERSOLE: We don't know but what the number of cycles that it has been tested here in fact has accumulated some fatigue level.

DR. ZUDANS: No, and you don't really know to what level the tests were made. You know only what the purchasers specified, but what the device can do that this equipment was placed on could be completely different.

MR. EBERSOLE: Would you know if you tested samples to destruction?

DR. ZUDANS: Yes, that would be a different story.

MR. EBERSOLE: Well, I mean, it would give you a model then to work with.

DR. ZUDANS: That is right. It would give you the margin, some kind of idea.

MR. EBERSOLE: Yes, you would have a working margin.

DR. ZUDANS: That is right.

It may well be all right. If you say, Jim, that it is very equipment sensitive, that is quite true. Maybe in some cases you can really -- maybe you can have a component in that piece of equipment that has been tested many, many times, can survive maybe 50 SSE's, not just one. Then you can check for the rest, whether it is sitting in the structure where it is supposed to be, or pieces are not falling off.

But the principle in general is not acceptable.

DR. OKRENT: Jerry Ray?

DR. RAY: I was just going to make the point that Walter made, plus this fact. In a plant with which I was familiar, the tests of the components on the panels, in addition to functional tests, both as individual components and as parts of systems which are tested in entirety to perform their function, there is a visual inspection.

The relay engineer, for instance, will go over the relays, and he will actuate the systems and look at the baerings and that sort of thing and make sure there is no damage there that is going to, well, let's say, cause malperformance at a later time.

That doesn't mean it would naturally or necessarily pass a second SSE, but it does indicate that there is no damage there that would not be evident as a result of a functional test, if you are with me.

DR. ZUDANS: Yes, I am sure you can check the functionality. It will function all right. But like you said yourself, there is no assurance that it will pass a second SSE.

DR. RAY: That is true, but what I am saying is that there is no evident phsyical damage there without him seeing it, if the inspection is proper.

DR. OKRENT: Okay. I think what we will have to do is leave this as a question, and the staff may feel it is completely okay, or they may -- in which case they can let us know in writing, or if they may want to think on it some more, or whatever. Okay?

Is that a reasonable thing for now?

MR. KNIGHT: Yes, it is. Certainly.

DR. OKRENT: Let's see. There was one other point in this area. Did you have a comment, Jim Knight, on this one, too?

(Pause.)

MR. ZECH: The next item we have, Dr. Okrent, is Item 3C.

DR. OKRENT: Yes.

MR. ZECH: Mr. Bender's letter. Leon Reiter from the staff will address that.

MR. REITER: I am not quite sure -- this is Leon Reiter, Geosciences Branch -- if I am addressing the issue that has been raised, but an issue which is often raised, and which perhaps is being referred to, is the difference in approach in the evalu.tion of seismological hazard being taken at San Onofre Unit 1, and that of San Onofre Unit 2 and 3. Now, is that the correct question?

Maybe it is not the question, and I will acceded to somebody else.

DR. JACKSON: The problem is, does the term "seismic evaluation" mean an engineering evaluation of the plant, or the seismic hazard analysis? We were unable to understand that letter.

DR. OKRENT: I am reluctant to speak definitively on behalf of Mr. Bender, so let me suggest that you define the question as you understand it, and answer it in those terms, and then if you are answering a different question, he will tell us.

MR. RIETER: I will define the question I can answer.

DR. OKRENT: All right.

(General laughter.)

DR. OKRENT: I agree. His wording is a little bit ambiguous in that regard. Go ahead.

MR. REITER: All right. There are three units in various stages at the San Onofre site. One of them is a unit which is already built and in operation at San Onofre Unit 1, and there are two units, San Onofre 2 and 3, which are rapidly approaching completion, and SER's we hope will be written in the near future.

In the evaluation and determining the seismic hazard or what the ground motion, the free field ground motion at the site should be, various approaches are being taken. The problem associated with San Onofre Unit 1 was that it was designed previously, years ago, with the -- "a low g value" and a static -- under static assumptions, and perhaps Harold Levin can address that more. But it was recognized that perhaps this might be insufficient with regard to the kind of ground motion you might expect today, given what we know about ground motion as such.

The Units 2 and 3, on the other hand, received their CP much more recently, and the basis for that was -- the design spectrum was a .67 g Newmark type spectrum.

As a result, two projects have been -- or two kinds of work have been converging. On one hand, the problem was to make an assessment of what kind of ground motion we might make or we could expect that could be used in design or in a re-evaluation and backfitting of San Onofre 1, part of which has already started, and some of it has taken place, and again, the structural aspects will be discussed later.

In this approach, the applicant chose to -- the licensee chose to pursue a technique which involved numerical simulation of the kind of motion you would get from a large or moderately large earthquake which occurred offshore, namely, the seismic threat is viewed as the occurrence of a magnitude, let's say, six and a half or seven, a surface -- magnitude six and a half or seven earthquake occurring on the offshore zone of deformation, which is some eight kilometers from the plant.

And the applicant chose to attack this problem utilizing numerical simulation, an attempt to use principles of physics to arrive at what this ground motion might be. It was a very extensive study carried out, probably the largest study of its kind, largest scale study of its kind.

The staff has reviewed this. We have our own review board. We have received several inputs to this, and presently we are working on the last aspect of this particular study, that is, to qualify the study by seeing how well it could -- it matches up to the predictions or how it could match the results observed in the Imperial Valley earthquake in 1979.

In other words, this is a numerical study based on a theoretical deterministic model.

In the San Onofre 2 and 3 evaluation, there again, the design is already existent, and decided upon, and in the CP stage, and in this case, a more emperical determination or evaluation was used.

In that case, the size of the earthquake was one item which is being evaluated, and through various types of geological and seismological-geological investigations. Then, I guess the important part is, once that size of earthquake is arrived at, then the ground motion is being simulated by an emperical study, namely, by taking the existent data that we have, that is, primarily data in ranges of 30 to 50 to 100 kilometers, and extrapolating that inward, both in terms of getting -- finding out what it would be at a distance of eight kilometers, and taking that set of data and trying to predict what it would be at magnitudes six and a half and seven.

And again, presently, this data is being evaluated. It was really done prior to the 1979 earthquake, and now it is being evaluated in light of the 1979 Imperial Valley earthquake.

The staff used this -- both these approaches as different ways of evaluating the same problem, and we think we have a unique opportunity here to look at the kinds of -- a unique opportunity of attacking a very difficult problem, and that problem was estimating the ground motion in the near field, by the various and best available means we have, namely, both emperical and theoretical, deterministic, and now the third factor, namely, an earthquake of similar size occurring which was well documented.

We view the seismic hazard at that site as being the same, namely, that whether the plant is old or new, the kinds of ground mot n you might expect from an earthquake is the same, and e have directed the utility, Southern California Edison, to evaluate, compare and assess the differences, if there are any, between the different kinds of approaches and the results that they are getting, and we will -- we are reviewing this in terms of two bits of information to arrive at the same best answer -- at the best answer we can get, namely, what is the kind of ground motion we can get from the kind of earthquake which is being considered the safe shutdown earthquake?

DR. OKRENT: Dr. Ray?

MR. RAY: Could I harken back to the question of the two SSE tests?

DR. OKRENT: In a minute. Let's make sure there are no further points on this. We will let Mr. Bender read the transcript and see if he has any further questions in this regard. I think you have addressed his question, though. That is my guess.

All right, Mr. Ray.

MR. RAY: Walter and I just had a little bit of a sidebar conversation after the earlier exchange, and it leads to a theoretical question. What would be the policy if we have a plant that had experienced and had survived -- I mean the plant in its entirety, now -- an SSE?

Would it be permitted to go on and operate, or would it then be at the end of its life? Would you shut it down permanently, or would you let it go on?

DR. OKRENT: I believe that the staff has no policy in this regard, and in fact Mr. Shao and his people have suggested that research might be a relevant subject for plants that exceed the OBE. In other words, it is not necessarily if they get to the SSE. What do you do with the plant that has really gone beyond the OBE?

I don't mean just one pitch on the acceleration, but many, many cycles. It is really something on which there is no regulatory policy, I think.

MR. RAY: Well, from an operator's viewpoint, which I have a little bit of, I would have a hell of a lot more confidence in the survivability of the components on a panel after installation with the proper inspection, physical inspection, and testing as individual components electrically and as components of a system, I would have a hell of a lot more confidence in them surviving another one than I would have of production equipment taken off the -- off the shelves -- I am talking about the components on the panel now -- and put on this panel, because it had gone through the SSE.

And this is true of the whole industry, everything in it. You are confident of the survivability of lightning, for instance, and I realize the consequences of a lightning failure are a hell of a lot less serious than the consequences of an earthquake failure, but every component on a system, the operating people are more confident of and have a higher level of reliability and confidence in equipment that has survived such things.

A transmission line going through a season of lightning, after it goes into service, your biggest apprehension for it is, will it survive the first season of lightning, and so on. And after it has, then you know what you've got, and I would submit that this is true of what is on that panel.

DR. OKRENT: It probably is, but now and then buildings fall down due to the aftershock.

MR. RAY: Sure.

(General laughter.)

MR. RAY: And one of these days a comet is going to hit this plant, too.

DR. OKRENT: But that is not quite the same probability.

I see a hand at the rear. Will you identify yourself, please?

MR. SHULMAN: I was just wondering, isn't it true that --

DR. OKRENT: Will you give your name, please?

MR. SHULMAN: Jeff Shulman.

Isn't it true that these units are also tested by OBE events? And wouldn't that give you an additional margin?

MR. KNIGHT: Jim Knight. It depends on how you -- it depends on what side of this argument you are on, I guess, but yes, they are in fact -- the requirement is that they be tested through five OBE's. Yes.

If it is allowed, Mr. Chairman, I would just like to say amen to Mr. Ray's comments.

DR. OKRENT: I understand what he is saying, and I can sympathize with that point of view for -- certainly for certain classes of equipment, but I think there is a question that is raised here, and in fact there even is a question of what do the regulations permit, and are you allowing your own regulations to be violated.

I don't know. So, I would like to leave this one that we hear from the staff in some way, either that they know what the answer is and why, or they are going to look at it more.

DR. SIESS: I don't even know how you would know it had been the SSE that hit the plant.

DR. OKRENT: The SSE precisely I don't know, but they are supposed to have some instrumentation that should be working that would tell them it is about the SSE.

DR. SIESS: Would you take the celogram and do a response spectrum and see if it exceeded the -- Do we define it?

DR. TRIFUNAC: Humble Bay, for example, had the --

DR. SIESS: No, I mean in the regulations, did we define how you would know -- Let's take the requirement that you -- if you had the OBE exceeded, you must shut down and test. Is the OBE defined in such a way?

MR. KNIGHT: Well, the OBE is defined as the g value and as a response factor What is actually utilized to determine whether or not the plant has exceeded the OBE are the design spectra at various locations in the plant, and the free field, but certainly on the slab. If they have exceeded any one of those, by definition, at le st it would be -- it is not -- by our definition if they have exceeded one of those design spectra, then they have exceeded their OBE.

MR. EBERSOLE: On this matter -- Mr. Chairman, on the matter of seismic -- the susceptibility of these control panels, I think the probability is that even though these packages with this seismic equipment may be marked "Fragile" when they come to the station, it is probably more likely that some of them will have been dropped anyway, and they would have in fact been given a seismic test, unbeknownst to anybody.

And that has to be built into the product.

DR. OKRENT: Not in three dimensions, though.

MR. EBERSOLE: Not in three dimensions, but it may be just the worst dimension.

DR. OKRENT: Well, I think we had better go on to the next topic, if we may, and I am looking for something that Mr. Savio prepared. This is use of a seismic scram. Let's see. Does the staff have something to tell us in this regard?

DR. SHAO: Yes.

(Pause.)

DR. SHAO: My name is Larry Shao, NRC staff.

I will have a very short presentation. I am not going to discuss pros and cons of seismic scram. I will just discuss some information I know related to seismic scram.

Maybe somebody else can cover pros and cons of seismic scram.

As far as I know, seismic scram systems were installed in power reactors in Japan, and also installed in some research reactors in the United States and one power reactor in the USA, and that is Diablo Canyon.

The Japanese practice is, usually the Japanese seismic analysis requirement, they require two analyses, one static analysis, like our uniform building code, and another dynamic analysis, like our requirement in the NRC.

For the seismic scram value it is set at one-third of a design static coefficient and about two-thirds dynamic response acceleration at the location of installation. The greater of the two.

On the dynamic response acceleration, they have two values. One is shock, called S1, and the other one is called S2, and S1 is the maximum designed earthquake, and S2 is called extreme designed earthquakes.

DR. OKRENT: Which would they use?

DR. SHAO: This is S1, the small one.

DR. OKRENT: The small one. So it is two-thirds of the smaller one.

DR. SHAO: Two-thirds of the smaller one. I will show you a number of the small earthquakes. These are different plants designed for S1. They are very far, around .1 ag to .30 g.

DR. SIESS: Wait a minute. Those are S1 values?

DR. SHAO: S1.

DR. SIESS: Move over.

(Pause.)

DR. SIESS: Okay.

DR. SHAO: S2 is about 1.5 of S1. So whatever the number, multiply that by 1.5.

DR. ZUDANS: Larry, what are those units? G-a-l-s.

DR. SHAO: That is gals. That is .25 g. By a thousand.

DR. SIESS: Two-thirds of the -- No, it is two-thirds of the lower. It is about two-thirds of the OBE.

DR. SHAO: So, it is actually the seismic scram is set around -- between .12 to .2.

DR. SIESS: Or roughly two-thirds of the OBE?

DR. OKRENT: Yes.

DR. SIESS: If you consider the S1 the OBE.

DR. SHAO: This number is slightly higher than the OBE, I would say.

DR. SIESS: Yes. Okay.

DR. OKRENT: It is a little less than half the S up there, if you want the setting.

DR. SIESS: Yes. Right.

DR. SHAO: Okay. This order -- a lot of research reactor units, as I say, have seismic scram, and the reason was, somebody said the reason was for the research reactor the design is not as good as the power reactor. That is why they allow seismic scram.

That is all I have, Jim. Maybe -- I don't know whether you want to discuss pros and cons of seismic scram.

DR. SIESS: If you considered there S1 is equivalent to the OBE, that is two-thirds of the OBE, if you consider there S2 as equivalent to the SSE at the one and a half ratio, that is about a half the SSE.

DR. OKRENT: Right.

DR. SEISS: Which, we asked Diablo Canyon to do half SSE.

MR. EBERSOLE: Have they had any nuisance trips to speak of?

DR. SHAO: They have had no trips so far, because their setting is pretty high.

DR. SIESS: Well, what are you thinking about, the actual?

MR. EBERSOLE: Spurious trips.

DR. SHAO: Spurious trips. They haven't. Even during the earthquake at Fugoshima.

MR. EBERSOLE: They employed coincidence to prevent that, right?

DR. SHAO Yes.

DR. OKRENT: Jim?

MR. KNIGHT: If I may, Jim Knight again.

I know Larry has been involved more recently than I, but a couple of other points that we have talked about from time to time, one was the fact that I guess some, two out of three, I guess one was even three out of four required to get a scram at instruments in several locations in the plant.

The other was that in discussing it with the Japanese officials and asking them, well, what is your reason, what is the rationale or the purpose, and after some kind of glances went all the way around the table, the spokesman finally said, public opinion.

DR. SHAO: By the way, I did ask him what was the reason for installing seismic scram, and they really don't have good reasons, and they can't answer the question, and supposedly there is a report on seismic scrams that is going to be received in one month or two from Japan on seismic scram.

MR. EBERSOLE: Did the Japanese outlaw mercuroid switches in all their control systems, including nonsafety systems? These are puddles of mercury that slosh around. Did they outlaw those?

DR. SHAO: They outlawed those systems?

MR. EBERSOLE: Yes, did they --

DR. SHAO: They classify the systems a little bit differently from the way we classify them. They are Class A, Class B, Class C.

MR. EBERSOLE: Well, the most notoriously sensitive control element is a mercuroid switch. It is just a puddle of mercury. It sloshes around and makes contacts and breaks contacts upon the slightest disturbance.

DR. SHAO: I see.

MR. EBERSOLE: I would have thought the Japanese would have outlawed it.

DR. SIESS: Why?

MR. EBERSOLE: Because they produce all sorts of spurious actions on just the slightest tremor.

DR. OKRENT: Well, Jesse, if I can, I would like to try to stay on the subject of seismic scram or pros and cons. Let's see. Lipinski has asked for -- and then there is a hand in the back, and then Siess.

DR. LIPINSKI: On your vu-graph, you referred to one power reactor in the USA. Which one is that?

DR. SHAO: Diablo Canyon.

DR. LIPINSKI: And how is that seismic scram set, at what levels?

DR. SHAO: I think it was set at --

DR. SIESS: It was suggested by a couple of members of the ACRS that it would make us happy if they had a seismic scram, and they agreed to it. I would say, with alacrity, except that they were under some pressure at the time. And it was set at, what, OBE. Am I right, Jim?

DR. OKRENT: I can't recall.

DR. SIESS: Do you remember.?

DR. OKRENT: The committee recommended they consider it in the construction permit letter, so it goes back -- they also suggested it on San Onofre 2 --

DR. SIESS: What level is it, Jim? Do you know?

MR. KNIGHT: I have the Diablo Canyon emergency earthquake procedures here in my hand, and I am searching, but it was set at --

DR. OKRENT: Probably half SSE, I would think.

MR ALLISON: Dennis Allison. It is set at .4 g's.

DR. SIESS: All right. A little more than half SSE.

MR. KNIGHT: And it is a -- is it two out of three, Dennie?

MR. ALLISON: Yes, if I recall. It is either two out of three -- I think it is two out of three.

DR. OKRENT: Let's see. Yes, would you identify yourself, please?

MR. HAN: Frank Han, TVA.

So I can keep up, would somebody comment upon what seismic scram is, and how it is different from our safe shutdown on -- and OBE?

DR. SHAO: Seismic scram is a system so that when an earthquake -- the system automatically shuts down, without manual --

MR. HAN: Automatic?

DR. SHAO: Yes, automatic.

MR. EBERSOLE: Mr. Chairman?

DR. OKRENT: Yes?

MR. EBERSOLE: How do the Japanese -- what is their rationale in requiring the seismic scram? Is it to get a a head start on a shutdown, and so reduce the heat problem? Or is it to ensure a scram in the mechanical context?

DR. SHAO: Even though I don't want to discuss it, let me throw in a vu-graph on pros and cons.

DR. SIESS: He just happened to have one.

(General laughter.)

DR. SHAO: I just throw it out for discussion purposes. Actually, I went to Japan, and I tried to discuss this with the Japanese officials, and they really cannot give me any justification why they install the seismic scrams, so we discussed it and then I wrote it down.

DR. SIESS: Did you discuss it with the regulatory people?

DR. SHAO: Yes, I discussed it with Midi, the regulatory authorities. Supposedly experts.

DR. SIESS: Is Midi regulatory?

DR. SHAO: Yes, Midi is the regulatory --

DR. OKRENT: Why don't you leave it on, Larry?

DR. SHAO: Okay.

DR. OKRENT: I would like to look at those points.

If we look at the first disadvantage, it superimposes thermal transient loads on earthquake loading. The fourth one says, reactor trip likely without a seismic scram. So, I would say you are saying this is likely to occur whether you have the seismic scram or not, so if it is a disadvantage, it is only half the time, or whatever it is, because it is -- or more, because if you have something set at that level, you may be scrammed for other reasons.

DR. SIESS: But Dave, we do the first one anyway.

DR. SHAO: This one, I don't know whether I agree or not. Two degrees on the Livermore report.

DR. OKRENT: Well, yes. I would suggest you take that one with a grain of salt.

Now, the reactor trip without an effective RHR is not enough. I absolutely agree. On the other hand, we don't want to scram the RHR. We want it to work. All right, now, there is one thing you don't have on your advantages which may or may not be a real advantage, but I have not seen a discussion or a look at whether if you scram early in the earthquake and initiate things that happen automatically --

DR. SHAO: Create another accident.

DR. OKRENT: No, no. Let me -- I will finish my scenario, and --

(General laughter.)

DR. OKRENT: -- and then you finish yours.

When you have a scram you initiate certain things. You start lining up certain valves that you want lined up for the RHR. You start turning on certain pumps and so forth, and this is done, if you have the seismic scram, earlier in the earthquake than if you wait for a scram because the turbine is shattering, or whatever it is.

Now, earlier today we talked about how there is quite a bit of equipment that isn't even designed for the earthquake, and I would say the situation is a little bit unclear as to whether you can tolerate all the failures that might occur. I think it is a little unclear as to just what can happen in the control room in an earthquake besides the operator getting hurt, as to whether you will get things tending to move the wrong way, or whatever, or maybe open circuits where something should work, but, you know, you have failures.

So, it seems to me the one thing that I haven't seen in the Livermore report or in your list of advantages is the potential for intiating the shutdown heat removal process earlier, maybe before it is harder to do, because a valve won't move any more because of distortion, or a motor, you know, doesn't want to go, and so forth.

DR. TRIFUNAC: Would you put in some seconds in your discussion, or some time factors? When you say before, or earlier --

DR. OKRENT: Before -- well --

DR. TRIFUNAC: -- it would help to understand what you are saying.

DR. OKRENT: I think if you are scramming on half the SSE, or 60 percent of the SSE, which is probably the number I would suggest, about 60 percent of the SSE, if you have a strong earthquake, you should be getting the signal out, say within a couple of seconds after that first --

DR. TRIFUNAC: I am not asking about the earthquake. I am asking, you initiate the scram --

DR. OKRENT: yes.

DR. TRIFUNAC: -- and all sorts of things are happening in the plant mechanically.

DR. OKRENT: Oh.

DR. TRIFUNAC: How much time does it take to accomplish something that is worthwhile?

DR. SIESS: About the length of the earthquake.

DR. TRIFUNAC: What?

DR. OKRENT: About the length of an earthquake.

DR. TRIFUNAC: So we are talking about five, ten, fifteen, twenty seconds, depending on --

DR. OKRENT: It depends on what you want to do.

DR. SIESS: For some valves to close, we are talking about tens of seconds, aren't we?

DR. ZUDANS: Yes, but the scram occurs in a fraction of a second?

DR. OKRENT: The scram occurs -- should occur quickly, but there are other things that you want to occur.

DR. TRIFUNAC: But you have to accomplish certain things, do you not?

MR. EBERSOLE: Well, are there any precursors to the main shock that would be useful?

DR. OKRENT: Well, that is what the Livermore approach was on trying to anticipate a large earthquake.

DR. TRIFUNAC: But in many cases you could make a decision with -- logic well before any strong shaking comes in. Not in all cases.

DR. OKRENT: But I think you only gain a second, or -- well, maybe more. All right.

DR. TRIFUNAC: More than that.

DR. OKRENT: But I would rather for the moment see whether there is interest in something that is not too big an advance in existing technology, and whether there are -- Some time, it seems to me, earlier today we had a vu-graph, didn't we -- Oh, Vic Savio remembered where it was. Oh, during the SEP presentation, they mentioned that there was interest in design adequacy of the tubing and the support system for the control rod drive units and associated hydraulic tubing supports for Dresden 2.

Now, there, I don't know whether you have a question that relates to the efficacy of scram itself. Maybe. But I think if there is a question there, it still is not likely to go the first oscillation, and maybe you would get scram before the thing failed, whereas if you wait 30 seconds into the thing, you don't.

I mean, it could on a, let's say a weakly designed system provide margin to unscram itself. Chet?

DR. SIESS: If I had a circumvential pipe break just outside the vessel where we postulated for the asymmetric load situation, would the scram have to occur during that blowdown, all those forces going through the water?

DR. OKRENT: Well, having the big break then?

DR. SIESS: Yes.

DR. OKRENT: The scram isn't vital.

DR. SIESS: Well, I am thinking of some of these other things you are talking about, maybe. I am just trying to see about the combined forces, whether that is --

MR. EBERSOLE: On a PWR, the scram isn't vital in any case, because reflood would kill it, but in the BWR, at some point later in time, the rods have to go in.

DR. OKRENT: On the BWR, that is true.

MR. EBERSOLE: But not in that interval.

DR. OKRENT: Well, they have a boron injection system.

MR. EBERSOLE: Well, I mean, while he is voiding on a BWR he doesn't need it, but when he starts to refill, he will.

DR. SHAO: Another major advantage is that the operator may be so nervous when a big earthquake comes that he doesn't know what to do.

DR. SIESS: But unless something happens to the plant, he doesn't have to scram. I mean, we are assuming somewhere along the line that there is going to be a failure in the plant that we want scrammed, and we want some other tings done.

DR. SHAO: Yes, but because of the other, he may come down to the --

DR. SIESS: We either have a pipe break, or loss of all the power, where a lot of scramming is needed.

DR. ZUDANS: If you have a scram, there are a number of operator actions associated with it which he has to scurry around and do very quickly, and many things have to start opening and closing, and somehow I have an uneasy feeling thinking that the whole thing shakes and I am trying to move things in a given order. I would rather sit down and wait until it is over.

MR. EBERSOLE: You mean you don't want to start all these processs?

DR. ZUDANS: No. Not until it is through. It is a question of 15 seconds. I would do it by hand after it is done, if I can.

DR. TRIFUNAC: If you have a big earthquake, it doesn't stop for some time.

Tape 13
/ACRS
6/ /80
Babineau/
Oatfield

DR. SIESS: Larry, have the Japanese had any experience with scrams from earthquakes resulting from the turbine vibration?

DR. SHAO: No.

DR. SIESS: They haven't. Haven't they had earthquakes big enough to shake those turbines?

DR. SHAO: Yeah, but so far their scram has not been working, so far.

DR. SIESS: I'm not talking a seismic scram.

DR. SHAO: I know that. The vibration. They had no (WORD UNINTELLIGIBLE) trip so far, I understand.

DR. SIESS: Are they set unusually high?

DR. SHAO: They're set at around two-third of the S1. That's pretty high.

DR. SIESS: No, what I'm talking about is turbine vibration. Most plants have gauges on the turbine bearings that will automatically shut the turbine down if the vibration is excessive. Some don't. Some just have annunciation of it, they have told us. And others have automatic turbine trip on vibration. And, of course, most plants, a turbine trip will cause a scram.

And I was wondering if the Japanese have experienced scrams due to turbine trip due to earthquakes.

DR. SHAO: You mean seismic scram or just regular scram?

DR. SIESS: A red-hot seismic scram.

DR. SHAO: Turbine trip scram. Okay. I can't answer your question now.

MR. KNIGHT: If I may, Dr. Siess, somewhat -- talking now about the 95 percent confidence level, I'm pretty sure that, while it's not a nuclear plant, when the Imperial Valley earthquake struck, the fossil plant that was in the immediate area had a loss of load because of the turbine vibration, which would be analogous here. The turbine system shut down on a vibration signal. And that would give you a trip in the plant. So the forces were there and the sensitivity was there.

MR. LIPINSKI: I have a miscellaneous question. Under what g forces do the cooling towers come down?

SPEAKER: Some of them come down without any.

MR. KNIGHT: The cooling towers are not usually -- right now I'm searching my mind to see if there's an exception -- Category 1 items. They are not designed for seismic loading at all.

MR. LIPINSKI: I understand that. So the question how low would they come down relative to the reactor design?

MR. KNIGHT: I couldn't tell.

DR. SIESS: Are you talking about the big --

MR. LIPINSKI: The big cooling towers.

DR. SIESS: -- concrete cooling towers?

MR. LIPINSKI: Your heat sink when you're on your turbine.

DR. SIESS: (WORDS UNINTELLIGIBLE) earthquake, but they probably, in some cases they might not get very much. What's the period on those in earthquakes?

MR. EBERSOLE: Is that topic going to be covered in five?

SPEAKER: (WORD UNINTELLIGIBLE) cooling towers?

MR. LIPINSKI: Yes.

SPEAKER: (WORDS UNINTELLIGIBLE) seconds, a substantial period.

DR. SIESS: Anybody ever look at their seismic resistance?

SPEAKER: Not that I know of.

DR. SIESS: I've never heard of one failing in an earthquake. But then --

SPEAKER: (WORDS UNINTELLIGIBLE) in wind shear, for example.

DR. ZUDANS: This Lawrence Livermore report recommended there might be increased risk to society if you do the scram.

DR. SHAO: Yes, slightly increased.

DR. ZUDANS: Yes, every time you scram the reactor you do add risk.

DR. SHAO: (WORDS UNINTELLIGIBLE) training load.

DR. ZUDANS: Right. You would superimpose the transient thermal loads to risk as well as --

DR. SIESS: Yes, but we don't expect to scram it very often in the path of the SSE. Once in a lifetime.

DR. SHAO: If you set at half.

DR. ŽUDANS: If it's set at half the SSE, the plant is designed to take that kind of a situation.

DR. OKRENT: Well, as I said earlier, I would, myself, suggest somewhat above half of the SSE, like 60 percent or something like that.

DR. SHAO: Here we're talking about he has two-third.

MR. EBERSOLE: Except from the liability standpoint, if we've got a turbine vibrometer scram, we've got a seismic scram. And so the, whatever the ill effects of it are we have inherited automatically.

DR. SIESS: But, Jesse, all plants do not scram on --

MR. EBERSOLE: I know they don't. I'm saying, we evidently are -- either have the whole field in front of us, some of them do and some of them don't, and we evidently don't know which does. And certainly we don't know how reliable it is, either.

DR. SIESS: Would it be slower? It's a contact -- it's just as fast as contacts you make (WORDS UNINTELLIGIBLE). It's in milliseconds.

If it doesn't trip, the stop valve's got to close and they've got contacts.

SPEAKER: The contacts are on the stop valves.

DR. SIESS: Yeah, but that --

SPEAKER: (WORDS UNINTELLIGIBLE) compared to the contacts (WORDS UNINTELLIGIBLE).

DR. SIESS: Well, that's about three milliseconds. Just like a gunshot.

DR. OKRENT: Will you get the vibratory motion at the turbine this fast on the first --

MR. EBERSOLE: I believe you would. I expect it's sensitive, too. But it's just a one-channel thing.

DR. ZUDANS: And it's not likely the turbine bearings could survive under load.

MR. EBERSOLE: So we may, in fact, have many seismic scrams and not know it.

Do we know it?

In short, we don't know how many seismic scrams we got right now.

DR. SIESS: I think not. I think most of the descriptions we got, even if they got a trip on the turbine vibration, they've got a warning set before that. And so a lot of the turbine trips that they get due to vibration they know were not due to seismic, because they could see the vibration increasing up to the warning level and then --

MR. EBERSOLE: Oh, yeah, but it would still respond to a seismic event.

DR. SIESS: Well, but we don't know how many of the turbine trips --

MR. EBERSOLE: No.

DR. SIESS: -- are seismic. But we probably know a lot of them that aren't --

DR. SHAO: Yeah, a lot of them.

DR. SIESS: -- because you got the preceding warning of an increased vibration over a period of time.

MR. EBERSOLE: Oh, I understand that. But that would not prevent it from responding to a seismic event (WORDS UNINTELLIGIBLE).

DR. OKRENT: Well, I guess my question is, does the subcommittee want to try to propose something to the full committee for possible transmission to the --

DR. SIESS: I haven't heard a real staff position, let's say, against a seismic scram set at two-thirds the SSE. That's the closest thing to it. There are some pros and some cons. And the first con up there, the superimposition of the thermal transient loads and the earthquake loads is something we don't have to have a seismic scram to do; the NRC does that without a seismic scram. Right? On most of the structure.

And the last one got eliminated, because at two-thirds SSE and two-out-three logic you won't have a problem.

And the third one, it'd be nice to see some studies. The NRO studies were not based on the same thing we are talking about now, I don't think. They were talking an anticipatory scram at a much earlier stage, weren't they?

SPEAKER: Very low level.

JO-7

DR. SIESS: At a very low level.

DR. ZUDANS: There is a report on advisability of seismic scram just recently.

SPEAKER: Not recent.

DR. ZUDANS: June 30, '76.

DR. SIESS: He said he read it recently.

MR. EBERSOLE: Mr. Chairman, it would seem to me that the first thing we have to do is to figure out whether, in fact, we don't have seismic scrams and we don't know it. And then we can talk about whether we like them or we don't like them. And I mean by virtue of the fact that we don't know how many of the turbine vibration trips would, in essence, execute a seismic scram.

As I hear this, we don't know how many seismic scrams we've got. Maybe we don't have any. Or maybe we've had many. And I don't know.

And if we've got many and we don't like them, maybe we ought to cut them loose.

But as I hear this, we don't know what's in the field.

DR. SHAO: No, right now we don't have scrams, obviously. Except --

MR. EBERSOLE: You've got a vibrometer, which, in turn, executes a turbine trip. And a few milliseconds later a stop valve closes. And that has a scram contact sign.

DR. SIESS: But Larry, any disadvantages to the seismic scram are also disadvantages to an automatic turbine trip on vibration.

MR. EBERSOLE: Right.

DR. ZUDANS: Not quite. No, because nothing else is shaking but the turbine.

DR. SHAO: Well, you see it in combination loads with earthquake.

DR. ZUDANS: That's right. (WORDS UNINTELLIGIBLE).

MR. EBERSOLE: Yeah, you're right. Correction.

DR. OKRENT: Jim, what's the staff position?

MR. EBERSOLE: What's the staff's position?

DR. OKRENT: What is staff's position?

MR. KNIGHT: The -- Jim Knight -- the -- as you remember, back some time ago, the staff -- and I hesitate to say "took a position" -- they, at least, offered a strong opinion, and it was in general opposed to seismic scram. I don't really believe that that should be characterized as our present position. I think we ought to be, could be more properly characterized as having an open mind, looking for advice and counsel from this committee and willing to look into the matter.

MR. EBERSOLE: Well, Jim, that implies that they know that they don't have seismic scrams induced by turbine vibrometer trips.

MR. KNIGHT: No, I didn't say, didn't intend to imply that. There are a number of circumstances -- well, actually, there's a large number of reasons why one would expect, given particularly a large earthquake, what we're talking about is a half, two-thirds the SSE, that almost any plant would trip at some time, either because of turbine vibration, either loss of station transformer or insulators falling out, falling down out in the yard.

DR. SIESS: Well, Jim, I think you should have characterized the staff's previous position as not being opposed to seismic scram on the grounds that it was bad but as being opposed to requiring a seismic scram because they had no reason to think it would be good.

MR. KNIGHT: That's an excellent characterization.

DR. SIESS: And I believe that was based, to some extent, if not entirely on the Livermore report. And I don't recall what kind of scenarios they went through to arrive at the fact that it would not be a significant contributor to reducing risk.

DR. ZUDANS: But what escapes me is, why would this improve the situation as compared to what exists now? You have to shut down the power plant after OBE has been experienced.

DR. SIESS: You have to postulate some kind of an accident simultaneously with the earthquake -- a pipe break, a LOCA, a large LOCA, where the seismic scram gives you a head start on shutdown, gives you a few seconds on decay heat removal gain before the water goes out. And I think that that, at least, JC-10

some people feel that, a few seconds might make a nice difference sometime. It starts containment isolation. It does other things.

DR. ZUDANS: Not containment isolation.

DR. SIESS: Well, it depends on the logic and what else goes on. I may be wrong.

But now there you've got a sort of a hazy scenario. It seems to me, one thing the staff could do would be to -- on some basis, under contract, PAC, or research -- to try to look at some likely accident scenarios, and not limit it to large LOCAs, and see if there is an advantage. And do it imaginatively; don't assume everything is going to work perfectly all the way.

Now, Dave says, you know, there are lot of things have to happen for 15 or 20 seconds and maybe it's nice to get them started earlier. On the other hand, it may not be good to try to be closing all of these valves while things are shaking; you might be better off to wait until after it's over, assuming nothing broke. If it did break, maybe it's good. If it didn't break, it's bad. Look at the analysis.

DR. ZUDANS: Chet, I think that is a good recommendation for the staff, rather than saying put in a scram.

DR. SHAO: And, Mr. Chairman, I think Art Cummings of Livermore ought to say something about it. He's the author of the report.

DR. OKRENT: Mr. Cummings.

DR. CUMMINGS: Art Cummings, Lawrence Livermore Lab.

JO-11

I just wanted to make a comment about whether we had seismic trips (WORD UNINTELLIGIBLE) on these plants or not. However, (WORDS UNINTELLIGIBLE) finished with it several years ago, so I'm not completely up to date. But at that time, our understanding was, many of the utilities purposely did not have the vibration monitor on the turbine set to react, because they just didn't want to operate in that mode. And we didn't do a complete canvas, so I don't know how -- how valuable that trip mechanism would be.

However, on recent work on our SSMRP, we had -- have -- are looking carefully at the chance that a plant would trip at the OBE and above level, which I think is the level we're talking about, given an earthquake, because of loss of off-site power. The ceramic insulators in that regard are a key issue. And we don't have our results in yet, but the indications are that there's a very good chance that the plant will trip above the OBE because of this mechanism. And, as a matter of fact, the Diablo Canyon study that was done by SAI, they assumed that the plant was going to trip above the OBE.

So, you know, at the levels we're talking about, there's probably a very high probability that the plant will trip by itself.

MR. EBERSOLE: May I ask you a question? You said the operators -- they deliberately avoid, you said, setting the vibrometer, so that they would not trip the reactor. Did you

JO 12

mean trip the turbine?

DR. CUMMINGS: I was, I guess, I can't remember now, it's been some time, but I think we were talking, we were concerned about that, about reactor turbines.

MR. EBERSOLE: Well, what I'm trying to get at is, if you trip the turbine, a reactor trip is almost instantaneously a consequence.

DR. CUMMINGS: Yes.

MR. EBERSOLE: But you trip the turbine with the vibrometer. And then the second event is the reactor.

DR. CUMMINGS: Well, I'm trying to search back in my memory, but as I remember it, there was a distinction between this and the conventional steam plant. In the nuclear plant, for some reason, they did not want to have this trip on this vibration.

MR. EBERSOLE: Then they didn't trip the turbine.

DR. CUMMINGS: That's right. That must be -- that must have been it.

MR. EBERSOLE: Yeah, that's probably why.

DR. CUMMINGS: And this was not a complete canvas, so I can't talk with the percentage of plants that were in this.

MR. EBERSOLE: Mr. Chairman, in connection with the matter that was brought up a while ago about the LOCA possibly occurring nearly coincident with the earthquake, I think I'd like to call out a distinction for consideration between the

JM-13

boiler and the pressurized water reactor.

In a LOCA, the boiler must insert control rods in the face of whatever dynamic forces exist during the LOCA, not to mention those which are additive from an earthquake, because the subsequent flooding that takes place is done with clean water, no boration, and unless the reactor is effectively shut down by virtue of the fact that it's going to operate at 50 pounds -- and I don't know that -- later on, then it will return to some critical power level and contribute fission heat to the decay heat problem, an arrangement for which the containment is not designed.

Therefore, it would be more advantageous to trip a boiler than a pressurized water reactor, because you must get the rods in. And the sooner you get them in, the better.

Not so with a PWR, because it's reflooded with borated water and you don't really care whether the rods ever go in.

MR. LIPINSKI: Plus, the BWR scram system must function.

MR. EBERSOLE: Must function. The whole complicated arrangement, including the rods themselves have to go in. I don't know of any studies that show how much reactivity would be left with 50 pounds. Or the void fraction that would be associated with that. But I imagine that wouldn't be too much. That being the containment pressure.

DR. OKRENT: I'm not sure whether there is a proposed or possible subcommittee position. Maybe we should come back later today, let people think about it, and see whether there is some kind of a position.

And quickly go on to topic five, because we're running about an hour late.

Did you have something in topic five, Jim?

MR. KNIGHT: As you may judge from my hesitation, but not really. We had thought that some of the discussion this morning with Cecil on (WORDS UNINTELLIGIBLE).

DR. OKRENT: I think that covered part of it. Do you have anything to add to that? Or shall we leave it for questions?

MR. KNIGHT: I would rather leave it for questioning.

DR. OKRENT: All right. Jesse, I think this is a subject you're particularly interested in, so --

MR. EBERSOLE: What is the present plan that the staff has to examine safety significance of non-safety items in aspect to their seismic failure inducing failure of safety systems?

MR. KNIGHT: We had a brief presentation a little earlier today that displayed the state of our development along these lines. As you are aware, I am sure, we have a program ongoing now at Diablo Canyon where it would place this burden upon the applicant, to, at least from the standpoint of seismic -- well, there's, I guess we didn't mention it this morning, but there will be some similar efforts made on Indian Point. And Zion, I guess. I'm not sure; I can't remember.

JO 15

We're at the -- we're very low down on the learning curve. But the approach is one that's characterized by the committee, dividing the plant out into areas, using, in this case using, the fire zones, walking through section-by-section, listening to that equipment which is seismic designed, that which is not, taking the next step then and going through a scenario that says, well, if this equipment failed what would be the consequences, what would be the interaction, either in this compartment or if there's connections to other compartments, with the object when this procedure is finished of either having culled through the list of possibilities to the point where we've now identified things which should either be exposed to further analysis -- for instance, if that pipe failed do we have an unacceptable consequence, now let's go back and look at whether or not that failure should be considered, or -- and in some cases it may be done with -- let's say for expedients, saying this is something that could happen, our analysis might make it go away, but it's easier to fix, fix it, whether that's a barrier, whether it's a screen, whether it's the movement of the pipe under the condition. That's the program right in a nutshell.

MR. EBERSOLE: Jim, are you saying that the (WORD UNINTELLIGIBLE) engineers haven't, in fact, looked at this long before now? That you don't think -- what do you think?

MR. KNIGHT: I don't know. I think that's -- I think that's absolutely correct.

MR. EBERSOLE: The reason I say that, I remember a huge domestic water storage tank on the top of Browns Ferry which became a missile. It was a non-safety tank, but it had to be seismically qualified to keep it from becoming a missile and going through all the floods below it.

MR. KNIGHT: Well, there is a -- I think that was one level of sophistication, and there are, certainly, similar things that have been done in other plants. This stemmed from the requirement that non-Seismic Category 1 equipment should not, or cannot, if our regulatory process is successful, damage or impair the function of Category 1 equipment, seismic equipment.

But the glitch, as I think we could now characterize it, was that that failure -- or, at least, in many cases, that failure -- was assumed to be benign. And this is really the big issue that we're going back and looking at here.

MR. EBERSOLE: Could that examination really be just a refined version, or a pointed version, of the system interaction study that we proposed for Indian Point?

MR. KNIGHT: I think it is. I think it's -- I would, looking ahead and crystal-balling a little bit, I would look to see the two melded together, where the seismic aspect of it is simply one facet under review.

MR. EBERSOLE: If we're going to do this, I'd like to urge that we look at the electrical systems and in particular we look at the DC systems which are used to control non-safety AC circuits.

And I'll give you a reason for this, which is just a model. If we have a seismic event and we have some trouble in the containment -- this is just one of many scenarios -- we will produce conditions in the containment that will produce numerous -- because of the (WORD UNINTELLIGIBLE) environment -- numerous challenges to non-safety systems.

Therefore, we ask en masse that many circuits clear because of the faults that are occurring inside a containment. Many there's 50 of these. The biggest one is the reactor cooling pump, which has a penetration about so big.

And you, therefore, simultaneously ask these non-safety grade circuit breaker systems, which are piloted by non-safety grade DC batteries, which are teetering off in the aux' building someplace, or turbine room someplace, you ask them at that point in time to clear. Every one of them needs to clear, if the battery works. And none of them clear if the battery doesn't work. And the rationale may be, in fact, that even if the battery is there, they have no reliability there in the usual context of reliability. And it may very well be that many of these penetrations are, in fact, the weakest link from the point of time constant to failure, than the motor, or the water, the wiring, anything else.

Right, most of the breakers are keyed in time-constant respect to the motor. And the wire along the way -- do you follow me?

MR. KNIGHT: Sure.

MR. EBERSOLE: So you may have a number of holes in what you thought was a good containment. And unfortunately, they will face the aux' building, in which are located the support equipment for safe shutdown as well as the operators. So this type of scenario gets a little messy, and I think it has to be looked at.

This is a -- you know, it's a non-safety system influencing safety functions in a rather complex way.

DR. ZUDANS: Jim, you mentioned that the other systems interaction study might address this. I don't think it will, because you do not include non-safety systems in that. That is very limited to --

MR. KNIGHT: I'm sorry, I missed the opening part of your question.

DR. ZUDANS: Well, when Jesse said couldn't this be a part of systems interaction effort that you have now under way, for Indian Point --

MR. KNIGHT: Okay. I played on the word "couldn't" it be. It may not necessarily be at the moment, but I don't -- I think it isn't --

DR. ZUDANS: The way it's defined, it isn't. It is far from it.

MR. KNIGHT: Well, we're taking some small steps first.

DR. ZUDANS: Because you can't really include everything in it. You are already not including all safety-related systems in it, let alone non-safety-related.

MR. KNIGHT: Well, we clearly have to build on that.

DR. ZUDANS: Yeah, it's too difficult. Maybe it's not a bad idea to walk through, look at things that could interfere with each other, and then see whether they can sustain or support the particular loads. It's a walk-through idea not bad.

MR. KNIGHT: I was thinking in the context of some time ahead.

DR. OKRENT: I think the example raised by Mr. Ebersole is one that would not necessarily arise from the walk-through procedure.

MR. KNIGHT: I think that's right, yes.

DR. OKRENT: And when I earlier said I thought you should look at electrical things, I didn't have his very good example in mind, but I was sure he had some.

MR. KNIGHT: I don't -- I'm saying, but no means do I mean to counter your statement, I agree entirely. We may have done, may be doing, particularly to the Diablo Canyon review, an injustice if we speak of it just as a walk-through, because there are a good deal of system studies going on at the same time.

We're not deep enough into it for me to comment as to exactly what they're doing, but it's quite an extensive program.

JO-20

DR. OKRENT: Any other questions on this point that subcommittee members want to raise?

Well, if you think of any, we can perhaps squeeze them in later.

Let me suggest we take a break of about five or eight minutes. And we'll begin with the next agenda item, which is number seven.

END TAPE 13

(A brief recess was taken.)

Tape 14

DR. OKRENT: The subcommittee will reconvene.

MR. BAGCHI: I am currently the task action manager for A-40, which is seismic design criteria, short-term. My name is Goutam Bagchi.

I am really going to present a very brief introduction. There are only four slides.

I would like to take this opportunity to introduce the speakers that would follow me, and like to invite, really, ACRS consultant comments and comments from the members of this particular subcommittee on the recommendations that have already been made on phase one, as I go through this, I would like to point out.

The objective of this program was to develop capability to evaluate the adequacy of seismic design of operating plants and plants under construction. So the criteria that have been developed really try to address both areas. To develop methods to quantitatively assess the overall adequacy of seismic design

JR 21

for nuclear in general -- I might say that this is a fairly broad objective within the short-term. One might say that the full objective was not achieved. Something short of a big seismic program like this, since MRP perhaps couldn't do it. And then revise seismic design criteria as that became appropriate.

MR. EBERSOLE: Does that include an investigation, a new investigation, of whatever may be required to execute a safe shutdown in a seismic event? For instance, I take it, it will automatically now include aux' feed water. And I'd include station batteries, which have heretofore been non-seismic. It might not; I don't know. But does the scope of your work include a reevaluation of all those things which in the past have not been required to be seismic but now might need to be after we think about them a bit more?

MR. BAGCHI: The answer is really no. As I was going to the next slide, I will show the approach that has been taken in this action plan.

MR. EBERSOLE: Do you intend to do a reexamination, then, of what should be seismic and what should not be seismic?

MR. BAGCHI: That was not part of the scope of this work.

MR. EBERSOLE: Okay.

MR. BAGCHI: There were a few things that we looked at. Broadly I might classify these as engineering aspects of seismic design criteria, the other was seismologic aspects; and phase one really relates to the engineering aspects, and phase two to seismic input definition.

Phase one tasks have now been accomplished. There are, as you see down there, one, two, three, four, five, really five independent studies, followed by a review of all of those studies and recommendations where appropriate.

Today, due to the lack of time, there are only three topics that are going to be discussed. That is, the soil-structure interaction, and Dr. Shaw, of Dappolonia Associates, and they were the contractors for this project, will make the presentation; and then the major thrust will be on the seismic hazard definition that was started to alleviate the problems of systematic evaluation program in operating plants, and that project is site-specific response spectra; there will be a meteorology discussion by Dr. Wight, of Turr (phonetic) Corporation, subcontractors to Livermore, who were the contractors for this project; and Dr. Reiter, of Geosciences Branch, will summarize the seismological issues; and Howard Levin will go over some of the structural aspects.

And finally, in phase one, the accomplishment goes like this. We have a bunch of criteria that have been summarized in the report. It has -- should have been published before we came to this meeting today. But I have been assured that this week it will come out as a NUREG report.

Besides that, our recommendations for specific sections of the standard review plan and regulatory guides, as indicated here: 1.60, on the standard response spectrum shape; 1.61, the damping values; 1.92, deals with the three-dimensional input and closely spaced modes combination criteria; and 1.122, has to do with development of in-structure spectra.

Then going into the standard review plan sections: 2.5.2 discusses the definition of vibratory motion, design motion at the site; 3.7.1, the structural aspects of seismic input definition; 3.7.2, structural design criteria; and 3.7.3 was system design criteria.

Some of you may be familiar -- 3.7.2 and -3 were very similar in their -- in the topics that they discussed. So we decided that it would be beneficial to combine 3.7.2 and -3 into one section that deals with broad design criteria for both systems and subsystems. And 3.7.3 now devotes entirely to special structures.

With that, I would like to present the next speaker, if there are no more questions.

DR. OKRENT: I have only a procedural question. The time we have allotted for the overall subject is four hours, and I plan to not cut into your time, even that'll mean we run an hour late. We had indicated an hour for soil-structure interaction, two hours for site-specific spectra, and then a half hour for phase one recommendations. Are those good times? Is that the way you want the time subdivided?

MR. BAGCHI: I think that that's a fair representation of the time, except for the last one -- a half hour is a bit short; that's why I'm cutting down my discussion quite short.

DR. OKRENT: Okay. So we should use your --

MR. BAGCHI: Two hours should be, yes.

DR. OKRENT: So we should use your -- the 15 minutes that you saved for the recommendation part?

MR. BAGCHI: Yes, sir.

DR. OKRENT: All right, so -- all right, I'll proceed that way and try to follow within that framework.

MR. SHAW: My name is Don Shaw. And I'm going to talk on the work that Dappolonia did relative to investigating some topics in seismic input and soil-structure interaction.

The nature of the research study was really divided into six tasks, and all of them focused on the sections of the standard review plan which Dr. Bagchi just got through mentioning -- 2.5.2 and various parts of 3.7.

So I think what I would like to do is, I would like to run through quickly what the six tasks were, and then I'll discuss them independently. And when we get to the discussion of the task, perhaps the most -- the best way to do it might be if we can just ask questions as we go along, instead of trying to wait until we come back, because of the fact that the tasks are all somewhat unrelated, although they're related in an overall sense.

No, wrong way. No. Which way goes -- this is four, right? That -- no, that's okay. It was all right the way it was. But this one is forward. Okay.

All right. Task one focused on a definition of seismic input, of the -- well, it actually focused on two things: one, the definition of seismic input from the standpoint of vibratory ground motion, and that was to look at whether or not there were significant effects resulting from surface topography which should be considered in assessing what, say, the peak ground acceleration should be at a given site; the other aspect related more to 3.7 of the standard review plan, and looked at whether or not the assumptions which are used in some specifically named computer programs, namely, SHAKE and the LUSH/FLUSH computer programs, would be appropriate if you had a site which was non-horizontally layered.

Task two looked at reg' guide 160 input and interfaces with some of the concepts involved with site-specific spectra. Specifically, we were looking at the filtering effects of sites.

(Pause)

We were looking at the filtering effects of sites on what the frequency content would be, given that we have a reg' guide 160 spectra which is site-independent. And we also looked in this particular task at what the effects of motion at depth are, some questions relating with what's become known as the 60 percent rule in the standard review plan. And we also looked at where we might specify the control motion associated with a design earthquake.

Task three considered the fact that the standard review plan as presently written basically, if not directly, by implication says that all earthquake energy arriving at a site does so from vertically propagating shear waves. There's been a lot of recent data accumulated that tends to throw many members of the profession into doubt as to whether that assumption is valid. So one of the other tasks was to look at what would be the differences if we took and said it was due to surface wave propagation. And in this particular case, we looked at where it would be 100 percent surface wave propagation as opposed to combinations of surface and body waves, which is probably closer to reality.

Task four is a nitty-gritty numerical analysis task. It relates to the methods by which the non-linear strain-dependent soil properties are accounted for in the programs SHAKE, LUSH and FLUSH, which are specifically mentioned in the SRP; and the objective was to see if we could tell whether or not it was adequate and under what conditions it might be inadequate or if it was adequate under all conditions.

Next slide.

Task five also was sort of a -- well, there's a lot of physical phenomena, namely, for all recorded earthquake events we always have three-dimensional soil excitation and yet the current state of the art of treating the strain-dependent soil properties uses a plain strain assumption for calculating the strain from which the soil properties are determined. So we were trying to look at what is the implications of that assumption.

And the final task related to really looking more at the way embedment effects are treated in soil-structure interaction analyses; given the standard review plan, in 3.7, there is a table which specifies for various degrees of embedment acceptable methods of analysis for soil-structure interaction. So we were trying to find out the validity of that table one way or another.

Okay, now I'll go back to task one. And go on to the next slide. Okay.

First was the influence of subsurface topography and surface topography. And I will say that the work on which this -- our conclusions for this subtask was based was 100 percent based on literature review of available information at the time that we did the study.

If you'll go to the next slide.

We see here, well, I think this is one of Dr. Trifunac's results, for -- no? Well, okay. You're -- you did have some that we looked at, for cylindrical valleys and elliptical-shaped valleys. But this is a ridge, so --

The main reason, we have a lot of different kind of results published like this, the main point to that curve is that the effect of the surface topography varies with frequency, and that is somewhat to be expected because the wave length of the surface wave depends on the frequency and its relationship to the heighth of the surface feature is going to determine what its effect is relative to transmission and reflection.

The next slide.

Based on the results of all this, we reach some conclusions, and the bottom one probably should come before the top one. That has to be looked at very carefully and understood. It says the influence is on the order of plus or minus 50 percent in limited frequency bands. This means that if you think of the earthquake energy arriving at a site as being composed of a summation of a number of waves, all with varying frequencies, that some of them in that summation might be affected by plus or minus 50 percent. That doesn't mean that the peak ground acceleration resulting from back substituting and resumming would be affected by plus or minus 50 percent, because that would certainly be significant.

The first one, when we say "effects are small compared with the effects of other assumptions," that's an opinion stated from what we looked at. I don't think that we generated, nor has there been generated, any detailed quantitative absolute evidence to prove that. But we felt that the general results that we looked at, from all the studies, indicated that there were other problems which probably would lead to more error than what the effect of surface topography would be.

The second task looked at the -- the second part of task one looked at the effect of non-horizontally layered sites. As most of us know, we rarely have the horizontally-layered site. Far more typical might be sites where we have a stratigraphy with an angle of dip that might be a shallow, it might be a steep angle of dip. And then in some events we even run into places where we have kind of an undulating layer concept in the sub-surface.

So we broke this up into two different aspects, one being dipping layers and the other one being undulating layers.

Okay.

So we looked at sloping bedrock at angles of dip of 10, 20, and 30 degrees, then at what depth if you make a horizontal approximation you should put that, and then again the undulating layers, as I mentioned.

To perform the analysis of what was the effect of the dipping layers, we used finite element analyses, and actually the LUSH or FLUSH computer program. The sticks that you see up above there were a stick model of a typical reactor building structure; I think that it was a Mark 3 that we were able to extract from the literature. And that is a -- of course, a horizontal layering, the different shades in there representing different layers.

If we go to the next slide, you will see that here was the finite element model that we obtained from the -- or that we used for the sloping layer, and there were many of these models and we investigated a variety of numerical effects, but if you go to the next slide --

DR. TRIFUNAC: Could you explain how the excitations get into this model?

MR. SHAW: Okay. The excitations in this model were based upon the assumption that we have vertically propagating shear waves, so that the -- all of the points along the base of the model were given the same earthquake excitation, a time history -- well, actually, we, in order to look at the thing, to be independent of the time history, we did it in terms of harmonic response. So we supplied a unit harmonic input equal at the nodes at the base of the structure. And we did run several other models that were deeper, to investigate what the effects of the depth at which we specified that were. I have only shown that as an example of the type of a model that we did.

You can go to the next slide. Well, I knew one would get missed.

The basis of our conclusions was to take the varying frequency content between, roughly, 10 and 25 cycles per second for the harmonic excitation, look at the harmonic amplification function of frequency, and compare it between the horizontally-layered model and the dipping-layered model. And the basis of the -- of our conclusion that there would be no effect for dipping layers resulted from looking at these curves and saying whether practically -- obviously, mathematically they are not identical -- but practically whether they were significantly different or not. And what we found out was that the horizontal approximation is implied in the, especially in the, SHAKE computer program, is good for dipping angles up to 20 degrees, and beyond that you start to get into an area where you would expect that there would be errors generated by assuming that it is a horizontally layered site.

The average depth beneath the structure, sort of, beneath the center line of the structure, appears to be the place to put your horizontal layers if you have a dipping site. And this was a -- probably already known, but the collateral conclusion came out that we had to get the boundaries out about two or three times the depth of the model to get rid of some of the reflection problems that are associated with finite element. And this was done with some of the early versions of the transmitting boundary.

Next.

Yes?

DR. ZUDANS: These analyses were all done strictly with the linear and materials properties? In other words, you did not interact for strain effects and things of that sort?

MR. SHAW: What we did there was, we ran some SHAKE

JO-32

analyses of -- in a one-dimensional sense, to get levels of strain associated with the level of the excitation which we were going to be using. And we made it -- we didn't iterate, but we tried to get strain-compatible properties at the associated level of excitation.

DR. ZUDANS: It's a one-shot deal, in other words.

MR. SHAW: Yeah.

DR. ZUDANS: You run it like a quasi-static with a harmonic input. Yes?

MR. SHAW: Yes, that's exactly right.

DR. ZUDANS: And your strains were rather small, then, right? Or rather --

DR. ZUDANS: I forget the amplitude we ran, what the amplitude would figure out to. I would say in this particular analysis they were all small. Okay. It was in other ones where we got into the peak. Because in the harmonic excitation there was no way to say what was the peak level of ground acceleration. Right? Because you're looking at one component per run. So it was linear; I correct myself.

DR. ZUDANS: Okay. And one more question. When you showed that sloped bottom, you showed the finite element model above the slope and it was a homogeneous --

MR. SHAW: It was a finite element model below. It just turned out that way on the slide. I apologize for that.

DR. ZUDANS: All right. Yes, okay.

MR. SHAW: That was a rock layer, and it was very stiff. And we looked at, even, investigations of what happened if you threw it away and tried to specify it along a boundary.

But, yeah, there were finite elements in that region.

I guess we can go on with -- for the undulating, the layer problem, there obviously are an infinity of possible undulations and it was just impossible to look at everything you might get. So we went, really, to some work that we had done on an actual reactor -- this reactor was located in Italy -- and it had a site stratigraphy looking like that. And one of the questions which we addressed as part of some soil and seismic analysis consultation was: can we use horizontal layering, can we use SHAKE for deconvolution, et cetera, given that type of a site? So we went to those results and we reran them to kind of refine the numerical accuracy, and -- if you'll go on to the next one -- we came to these conclusions, that we had a little difficulty doing it because at the time we did it there were some numerical problems in the LUSH/FLUSH programs relative to the way it does its numerics, so these were not things that are of a conceptual nature, they were just nitty-gritty numerical analysis, but, in general, what we seem to feel was that the fact that you can get closely spaced site modes -- which would not show up if you had a horizontally-layered assumption, they would vary by a more regular interval and certainly wouldn't be closely spaced -- could lead to unconservative results if you try to do a horizontal approximation.

DR. ZUDANS: Why would you have any trouble in a quasi-static solution?

MR. SHAW: Pardon?

DR. ZUDANS: Why would you have any numerical problems in a quasi-static solution?

MR. SHAW: Because the numerical problems that arose relate to the fact that the FLUSH/LUSH package computes a sequence of frequencies and it interpolates other frequencies. Based on a single degree of freedom representation of what might happen between two frequency points.

DR. ZUDANS: Rather than analyze the system for all, everything?

MR. SHAW: Yes. One of the ways that it manages to cut down the cost of analysis is by reducing the number of frequency points that you're going to have to consider by using this interpolation function to go between the points in your views. And it assumes that there won't be any closely spaced modes, number one. And we were getting --

DR. ZUDANS: So it divides by zero?

MR. SHAW: Well, yeah. We were getting tremendous amplifications with the problem that we had, and we weren't able to get some even numerical stability, I guess you would say; the numbers were looking like an amplification factor of 200, which we felt made no sense whatsoever, given even the straight out

-35

material damping of the soil.

DR. ZUDANS: So it was not a physical problem; it was a numerical.

MR. SHAW: Yeah. We -- we did enough work to convince ourselves that our conclusions were valid, although the problem of the interpolation function, at the time we did this, remained. I am of the opinion, anyway, that John Lysmer (phonetic) has done something regarding that problem; I'm not sure exactly what, I haven't talked to him within about the last -- well, I haven't talked to him since Pasadena. So.

Next.

Okay, task two -- we can go to the next one -- reg' guide 160 input. And as I mentioned previously, we were looking at the filtering effects of sites relative to site-independent spectra and the appropriateness of the standard review plan 60 percent rule for motion and depth, and then a recommendation on the location of control motion.

We used three different sites in a one-dimensional vertically propagating -- either shear or compression we were running horizontal, so we were running vertically propagating shear waves. One of the sites was shallow, over rock. The other one was, I guess, of medium depth and had some stiff properties to some layers. And the other one was sort of a deep alluvial site.

We did this predicated upon some, again, experience we

JG 36

had had with the SHAKE computer program, where when we got into certain kinds of sites we started to get some non-converging results and when you try to deconvolute and since one of the things was looking at the way SHAKE does its numerics we tried to combine this with some of the results we're going to have in task four. So.

What we have plotted here was the ratio -- okay, we have an artificial time history matching reg' guide 160. Now, we did these analyses with artificial time histories, white noise time histories, and real time histories, Melendy Ranch and I can't remember the other one, to look for varying frequency content.

This one here was a reg' guide, and it was scaled to a 0.4 g peak acceleration at the surface. Now, in this particular analysis, the non-linear effects are being taken care of out of the way the SHAKE computer program does it, through the equivalent linear integration procedure.

And the main focus of this was that you see a frequency dependence across the bottom and the curves are a plot of the ratio of the response spectra at the surface to the response spectra at depth. So that if there were no filtering or amplification effects of a site, one would expect to see that a constant value of one all the way across, or perhaps .8 or .6, but still a constant value.

And the point from that particular result is that

JO-37

certainly the ratios are not constant with frequency. Therefore, this frequency dependence of that ratio is a function of the site characteristics. And --

DR. TRIFUNAC: (UNINTELLIGIBLE)

MR. SHAW: Pardon?

DR. TRIFUNAC: (WORDS UNINTELLIGIBLE) like that. You'd expect a cosine curve.

MR. SHAW: Okay, we can maybe -- I guess the point to this was that -- and still, it's significant, I don't want to talk to you -- there are filtering effects due to the site stratigraphy such that it's unreasonable to think that one spectra would be applicable to all sites; and at the same time, this information was used relative to the 60 percent rule, that you see here the ratio at a depth of 20 feet and the ratio at a depth of 40 feet to the surface. We felt that that substantiated that trying to force something to be a constant ratio was going to either -- if you had to pull the valleys up to the constant ratio, was going to lead to extra conservatism. And I realize that this is a somewhat controversial topic, and from the -- I guess maybe a sentence that wasn't written in the report, where we feel that the 60 percent rule is physically inconsistent with reality; that would have to be amended to say inconsistent with the assumption of vertically propagating body waves.

If all of the excitation came from surface waves, we looked at that, I don't have any numerical results with me, but

JC 38

we again found a frequency dependence, as one would expect, if everything is 100 percent surface waves.

The criteria of 60 percent, if we have a combination of surface and body waves, is just something that wasn't considered in what we did.

DR. ZUDANS: Could you return back to your argument that these lines should have been horizontal?

DR. TRIFUNAC: But they shouldn't be.

DR. ZUDANS: And what should they be? I mean --

DR. TRIFUNAC: Cosine functions.

DR. ZUDANS: Cosine functions. But this is --

DR. TRIFUNAC: Two times eight is sixteen. Four times four is sixteen. This book is in agreement with what they should be.

MR. PAGE: What was the key word, Mike? What should they have been?

DR. TRIFUNAC: Well, this is a standing law of Baron (phonetic). As the wave comes up, it gives the boundary. It comes back up. You have a cosine dependence with that.

MR. PAGE: Oh, a cosine.

DR. TRIFUNAC: In a hard space. And this is not a hard space. This is a variable thing. So it's like a cosine but not quite. So it should be like a cosine function.

MR. SHAW: Yeah. But I guess I misintrepreted. You -- in physical reality, you expect a non-constant value. That was what we were trying to prove. Okay?

DR. TRIFUNAC: Oh. Okay.

MR. SHAW: The physical reality of the situation is, is that you would not expect a constant ratio of all frequencies, because of the standing reflecting wave effect.

Okay. The --

DR. TRIFUNAC: That is only if the waves that (WORDS UNINTELLIGIBLE) --

MR. SHAW: That's exactly right.

DR. TRIFUNAC: -- (WORDS UNINTELLIGIBLE) that they are not. So --

DR. ZUDANS: And, Don, in this exercise did you change your materials properties along the frequency?

MR. SHAW: These were run with time histories and SHAKE, and yes, we did.

DR. ZUDANS: That every frequency has another set of materials properties, or you determined them once and used them throughout?

MR. SHAW: Well, the -- what you're seeing is response spectra now, not harmonic spectra.

DR. ZUDANS: So the history was done with one set of properties.

MR. SHAW: And then response spectra were determined -- no, no. We determined time histories. These were done with time histories. Okay? And they did account for, in the manner which the SHAKE program does it, by the equivalent linear integration procedure, they did account for the non-linear strain-dependent properties.

Now, once the time histories were generated, we calculated a response spectrum. And what you see is the ratio of that calculated response spectra to the original surface response spectra.

DR. ZUDANS: Okay, I understand this step. This step takes the history record and simply transforms into spectra. Okay? This does not involve a structural analysis in itself.

But the other step, when you used the time history, how does the program do this linear equivalent integration? Does it perform new, compute new set of properties on each step? Or it determines the linear equivalent properties for a largest strain that's anticipated and use them throughout the analysis?

MR. SHAW: We can cover that now or later. You raise it, we'll talk about it now. When in task four we look specifically at that. The --

DR. ZUDANS: Okay. We'll --

MR. SHAW: No, let's -- you've brought it up and I think it's worth talking about now.

The equivalent linear integration procedure is used by the SHAKE program and the LUSH/FLUSH package. And the way it does it is, it does a linear analysis and it determines, a linear analysis using Fourier transform methods, and it determines a response, a strains for as if it were linear, over the entire range in whatever time history you happen to be using. Then it goes in and computes what the peak strain was at any point in that time domain. It now takes a factor, which is typically taken to be .6 or .65, times that peak strain; it goes to the non-linear strain-dependent damping and strain-dependent shear modulus curves, at that effective strain, is what that's called, and it picks off a new value and it substitutes that back in, redoes the analysis.

DR. ZUDANS: But it does with one set of properties.

MR. SHAW: That's right.

DR. ZUDANS: So, in other words, for each integration station, for each finite element, you have just one set of properties, which are determined from the first run, where you got the maximum strain, which was taken as a linear, initial linear.

MR. SHAW: That's right.

DR. ZUDANS: So all of this subsequent analysis is with the different properties at each point but it's still strictly linear.

MR. SHAW: The method, when I get to task four, we did --

DR. ZUDANS: Okay, I think that you gave me the explanation. Thank you.

MR. SHAW: Yeah. Okay. Let's go on, then, with some conclusions that the response spectra at surface is not a constant times the response spectra at depth, as a physical reality; therefore, we read that to believe that it was unrealistic to require it to be a constant.

Varying stiffness and damping properties of soil profile lead to -- well, that's just saying that the site character, site characteristics relative to stiffness and damping are going to change that, that ratio, as a function of frequency, depending on what those properties.

Go to the next.

These are again some numerical results which were run. This one shows a number of different -- I see we have Hollywood East, West, Melendy, and Cono (phonetic) in Italy, and an artificial time history, which is the one which matches, has a spectrum matching reg' guide 160. And this was site number two, which was the medium depth. And we're plotting the peak acceleration versus depth. And the -- one of the things that comes out of this is that when you get to the high acceleration levels, you start to get divergence. In this caes, we had a relatively simple error right in this region, which probably led to the problems. But the overall problem is relative to the deconvolution procedure, that -- we'll get to this later on the equivalent linear integration again, that the more non-linear you become, the worse the procedure gets, to the point that, in this particular case, it diverges; and we had had this phenomenon in some

JO-4?

real sites also.

If you go to the next one, we'll see the similar thing. This was at a deep alluvial site. And again if the acceleration, the ground acceleration, is relatively low, the thing converges and doesn't show a great deal of variation; there as the ground acceleration goes up, the numerical problems with the method of deconvolution increase.

Let's --

DR. ZUDANS: You are showing at top what the input, and the bottom is the result?

MR. SHAW: Yeah. The bottom is the peak.

DR. ZUDANS: This is the deconvolution, I see.

MR. SHAW: Yes. Right. The line going down is the peak that I have on all three in there.

DR. ZUDANS: Yes.

MR. SHAW: Yeah, this line is peak acceleration versus depth, going down.

DR. ZUDANS: In the process of doing the deconvolution, right?

MR. SHAW: Yeah. Well, the final results that -- well, it may be, may be as final as you can get.

Yeah, because the point of the output, the things like required for liquefaction analyses, in particular, is the effective strain in individual layers. So what you get is a peak strain output as a function of depth relative to the way you

JO-44

made your SHAKE model.

So those are the -- where they converge, they are the converged results. Where they didn't converge, they are no results.

DR. ZUDANS: Now I am just curious. Is this to be expected, that you have much larger accelerations at the bottom? Or those you describe as no good results?

MR. SHAW: We have gone round on that question a number of times.

DR. ZUDANS: Tell me what it is. It would look strange (WORDS UNINTELLIGIBLE).

MR. SHAW: Which -- you mean in the areas over here?

DR. ZUDANS: Right, right. Those.

MR. SHAW: Well, we are numerically highly suspicious of this. We are tending to believe this in here as being possible anyway. But when you start seeing -- this occurred for an artificial time history, and this was the first experience which we -- and "we" being not only the group that worked on this project but the nuclear group at Dappolonia -- first encountered problems with deconvolution for liquefaction analyses using artificial time histories. And we couldn't get them to converge very well regardless of what the acceleration levels were.

And one of the reasons believed to be the basis of that is that there's a extreme richness of high frequency in reg' guide spectra, if you make an artificial history matching reg' guide spectra. And as I get more into task four, it's the treatment of high frequency by this equivalent linear integration procedure that can lead to some erroneous results, and erroneous in the sense here that, I guess, you don't even know whether they're believable, because you've essentially got them on numerical instability of some form.

DR. ZUDANS: Okay. Now if you would look at your picture and say would the agreement mean that anything deeper than rock 22 should be forgotten and discarded as useless?

MR. SHAW: For all of them? For all of them?

DR. ZUDANS: Yes.

MR. SHAW: Because of the fact that you show a --

DR. ZUDANS: Because all these show a -- a break -- break something.

MR. SHAW: -- a -- you show a break.

My opinion in a site with the layering, I don't remember the exact layering we had there, having done some very early deconvolution analysis work, would be that they're -- when you get into the stiffness effects you can expect to get some degree of change, which might lead to locally higher but not -- you wouldn't expect it to be generally higher as a trend.

And no, I guess I wouldn't -- to answer your question, I don't think that I would automatically throw away everything below 122 feet.

DR. ZUDANS: And I guess but your confidence is shaken

JO 46

END
TAPE 14

just like mine, yes?

MR. SHAW: Yes.

300 7TH STREET, S.W., REPORTERS BUILDING, WASHINGTON, D.C. 20024 (202) 554-2345

#15

MR. LUCO: What was the frequency cutoff?

DR. SHAW: Pardon?

MR. LUCO: The frequency cutoff in these --

DR. SHAW: The frequency cutoff? Well, initially I think we started it around ten cycles per second, and we started dropping it down to five as we tried to get answers to converge. I am not -- I guess I can't answer that detailed question right this moment. You are speaking of the cutoff frequency in the shake program ?

MR. LUCO: Right. So you have problems even if you cut the frequency to five?

DR. SHAW: We have had problems, yes, where we didn't get a number that led to a converge solution until we got it down around two or three. Now, we don't think that is realistic. That was just a numerical exercise that we have run through.

MR. LUCO: What does it -- excuse me. Go ahead.

DR. ZUDANS: Have you tried to limit the amount of damping that you could have in the soil? I have some experience where if you constrain the damping to be no higher than, say, 5 percent, then all of those problems disappear.

DR. SHAW: No, I don't believe that as part of the work that went into this study, we did anything like that. We did address the problem, and that wasn't one of the things that we looked at, but I think it is a good idea. We did address the problem of, if it doesn't work, what might make it work, and there was obviously -- there is an interaction between the frequency content, et cetera, and that just wasn't one of the things that we looked at, but I would -- I think it is a good suggestion.

DR. ZUDANS: What is the meaning of cutoff frequency in this program?

DR. SHAW: A numerical --

DR. ZUDANS: No, what does it mean? Is it --

DR. SHAW: Do I know what it means? Not being the guy running that. Let's see.

DR. ZUDANS: Well, if you don't have the answer.

DR. SHAW: It should be -- Yes, I guess I can't really give you a nitty-gritty mathematical -- Maybe you can tell me better than I can.

MR. LUCO: The frequencies higher than a certain value of --

DR. SHAW: Yes, conceptually, that is what is supposed to happen, but I don't know the nitty-gritty of how it works. It has a filter built into it, because it is working in the frequency space.

MR. LUCO: You neglected -- In your analysis, you neglect all of the components higher than a certain frequency.

DR. SHAW: Because you are doing it -- instead of doing it by numerical integration, you are doing it using 48 transformer methods, why, you can do that.

DR. ZUDANS: So in your 48 integration you ignore the frequencies beyond --

DR. SHAW: That is right. That is a variable which you can change when you do an analysis.

DR. ZUDANS: For a 48 transfer to be valid, you have to go to infinity.

DR. SHAW: Discrete 48 transfer.

DR. TRIFUNAC: But this way you can get any peak acceleration you want.

DR. ZUDANS: By cutting it off anywhere you want.

DR. SHAW: That's right.

DR. TRIFUNAC: By cutting where you like, you get anything you like.

DR. ZUDANS: Therefore, it really, while it is a very valuable exercise, but, boy, it is totally useless, to say the least.

DR. OKRENT: We have about 20 minutes for this particular topic.

DR. TRIFUNAC: Can I ask a question?

DR. OKRENT: Yes.

DR. TRIFUNAC: I may be asking the wrong question at the wrong time, but do I understand that the La Palonia had the task to analyze the effects and the present state of the art of soil structure interaction? What was your task, actually?

DR. SHAW: On this particular --

DR. TRIFUNAC: The whole package that you are talking about, yes.

DR. SHAW: On the whole task -- no, the whole task was, to the degree within the scope of work which we -- which we could do, look at a general topic, but the specific aspect was looking at these named criteria, the SRP methodology relative to whether or not it needs modification, was the --

DR. TRIFUNAC: So is my understanding correct that it was La Palonia that decided that it was going to look at these questions in the framework of shake, lush, and flush?

DR. SHAW: No. No, no, the scope of work for the study specifically named that the shake, lush, flush programs, because they are specifically named in the SRP, were a subject of the investigation.

DR. TRIFUNAC: So it was NRC's choice that shake, lush, and flush be used, actually?

DR. SHAW: That's correct.

DR. TRIFUNAC: Thank you. That was my question.

DR. SHAW: Okay. Well, let's try to hurry through with the rest of it here. There are some significant things here.

I think we have gone through this fairly well by now, so I will go on, and now we are into the subject of surface wave propagation, and just as a brief review of the significant difference between surface wave excitation and vertically propagating body waves is that on the bottom of any foundation, all points A, B, and C, their excitation is not in phase if it is a surface wave, whereas for a vertically propagated body wave, they would be, at least assumed to be in phase, at least at the base of the model they would be assumed to be in phase.

Now, to represent this, we used a more or less simplified approach of a Winkler model developed by Jack Hall for preserving rocking as well as translational stiffnesses, and a small computer program that looks at the distance which the wave has to travel in order to look at what the excitation is, if you will go to the next slide.

And we went back to our model of the reactor building, and looked at the response spectra for those directions at those locations on the building for varying lengths of time that it would take the wave to travel, and when you say that the time for the wave to travel across the foundation is zero, you have come back to the vertically propagating body wave assumption. So, if we can go to the next slide.

Okay, this is just -- there were probably 50 or so of these kinds of plots generated, and what we see here is, if we were to go back to that other figure, this is a point on the foundation out at the periphery, and PA=0 would correspond to body wave excitation. PA=.16 might be something like a maximum time averaging effect that you would get out of a surface wave for a relatively slow wave propagation speed at the site.

As indicated here, the body wave excitation is generally -- right here -- greater than the surface wave excitation, which did consider both the effects of translation and any induced torsion, so, go to the next one.

On this one, again, you see something that led to our conclusion that this is up at the top of the building, and at the top of the building, we find that the surface wave excitation has a spectra exceeding what we would get from body wave only excitation, and that led to the conclusion that surface wave phenomenon, one, that in general you reduce the pure translational excitations, and then you go back and add something in due to torsion and rocking, and I think the main conclusion is the third one, that at some points in the structure, it may be unconservative to neglect the effects of surface waves.

We can go on to Task 4.

Okay. Now, we are back to our problem of the equivalent linear integration procedure, and we have probably discussed it at fairly great length, but in order to try to get a handle on what was going on, we went back to those analy_es that we did in conjunction with Task 2, where we started with the surface time history, or I think in this case because we were going up and using the charsoil program, we started at the base time history and went up, and made some comparisons, where we used white noise input so that it would be rich in frequency content and wouldn't necessarily mean that these respectors would reflect bias in the frequency content of a given time history.

Now, the charsoil program, unlike the shake program, uses what is more conventionally thought of as the non-linear method, which is one time step at a time. It goes back and gets the correct soil property, and then moves forward in a method of characteristics manner, but I guess we called it piecewise, linear over a small increment of time, but you check it constantly instead of running throughout the entire analysis.

You see -- in this particular one, you see that the charsoil has kept the high frequency up compared with the shake. This psf is that peak strain factor. That is -- effective strain is .6 of the peak strain. You see the effect of the incorrect way in which damping comes in, and the limiting of damping may be a big way towards solving that problem. We can go to the next slide, and I can maybe make that a little more clear.

One of the things that happens is that this is like a low frequency historesis. You would have maybe superimposed on top of it a very small historesis group of high frequencies, but to the equivalent linear integration procedure, if I had a small historesis group here, it would come out here to this peak point and in essence it is blowing the historesis group around like this for damping as opposed to just adding in the small amount of damping in the small high-frequency group.

So, again, that relates to Dr. Luco's idea of limiting the damping as being a way to get some of the numerical problems out. But the method does have some numerical problems.

We -- Again, the degree of non-linearity has a great deal to do with how well the two compare. For very low input acceleration levels, and at shallow sites, we didn't have any problem. When we got to higher levels of acceleration and deeper sites, we did run into the numerical conversions problems.

Let's go to the next slide.

I can dismiss this one very quickly, in that we -- First of all, to briefly review it, what we mean by the plain strain assumption is, conventionally, you are going to look at one of these plains over here in terms of either vertical or horizontal, but you are not going to be considering, in terms of the soil conditions, the three-dimensional effect that you might get like this.

We had anticipated being able to make a maximum sheer strain type determination and go to a cyclic Lambert-Osgood type curve and try to pick it off, and we immediately ran into a problem that we probably should have known better than to ever try it to begin with, and it relates to something, a paper that was presented by Dr. Hardin at Pasadena about two years ago dealing with it.

We really are lacking in a good three-dimensional theory of soil, non-linear soil behavior, so our conclusion at this time is that the state of the art really doesn't permit us to make the assessment. When we think of it in terms of the way we traditionally do, one direction and then another direction, we offered an opinion that the effect is probably not large, because you get out to high enough strains, and the effect on the non-linear property tends to become flat.

But that is, again, an opinion statement. I think I just went through these, so we will -- Now, embedment effects, also -- the next slide, I think, is about the last one, or next to the last one.

This was a -- We went to the literature again, and

I just picked one out here of the many plots that we presented, and remember that the focus was on primarily what the standard review plan requires to do a soil structure interaction analysis considering embedment, and the work shown there was some work done by Jack Hall, I believe, presented at a SMRC conference some years ago.

But after reviewing all the work, if we can go to the next and final slide, we basically came to the conclusions given there, that both lump parameter and finite element methods can be used, but they can't be used in a cookbook fashion, when we say lumped parameter. You notice that the springs were included on the side of a foundation in the lumped parameter representation done by Hall.

So, there is nothing in here, we believe, in the mathematics that would automatically rule out the use of lumped parameters when you have embedment, but it is not just a matter of using the simple soil spring under the base of the foundation and forgetting about it. That would lead to some problems.

The other one is a quantitative assessment that indeed embedment significantly increases stiffness. It also has an effect on damping. And basically, finite element methods, while probably more expensive to use, allow you to do some analyses with making fewer assumptions than you have to make if you try to do it by lumped parameter.

DR. ZUDANS: When you are talking about stiffness here, you are talking about stiffness that the structure seeks?

DR. SHAW: Yes. Right. The effective soil spring, you might say, of the foundation.

Now, in reality, the soil stiffness is not a spring, it is a frequency dependent compliance function and there has been debate over that for a number of years in terms of the practical significance of using a compliance function as opposed to, say, a half space determined spring For the typical ranges of frqeuencies, it has usually been our opinion, and this was not one of the things that was addressed in here, this was just a matter of our opinion that over the range of frequencies of interest, the two of them turn out to be almost the same.

Jack Hall's paper addressed that, but the rest of them were drawn from other works.

Did I -- How much did I run over? I am done.

DR. OKRENT: We have five minutes left for questions.

MR. LUCO: Could we go back to your conclusions in Test 5?

DR. SHAW: The three-dimensional?

MR. LUCO: Plain strain.

DR. SHAW: You mean you want to physically see the slide?

MR. LUCO: Yes, I would like to see it.

DR. SHAW: This was 6. Back up a little further.

(Pause.)

DR. SHAW: This is highly opinionated, because there was no quantitative way to --

MR. LUCO: I read somewhere in these reports that you were getting a higher response using a two-dimensional model than a three-dimensional model.

DR. SHAW: Yes. Using a very contrived, if we back it up one more, using a very contrived method of trying to do an analysis, given that this factor here is moving around, and that you really say, when dces it reverse, and if you are going to go to something like a Lamb rt-Osgood curve, you have to worry about sine, and you lose the sine here.

We arbitrarily dreampt up some methods based upon what the incremental changes were from one state to another, and whether we called it a sine change or didn't call it a sine change, and then we ran some analyses with that, and -- I would almost have to look at those specific curves, which I didn't do, but your recollection is correct, but I guess I didn't for today feel that there was any credibility in those two contrived methods of trying to do the analysis, so I didn't talk about it.

MR. LUCO: But your conclusion seems to be just the opposite of those results.

DR. SHAW: That, again, I can't say. I remember the specific curves, and if I can take a couple of minutes, I can take a look, but that could be. In the preparation of the slide, it might have gotten turned upside down, and I might have said the opposite of what we meant to say. I would have to check it.

MR. LUCO: Well, I would agree with your conclusion as listed in your slide.

DR. SHAW: Okay.

I will be happy to check that and discuss it.

Any other questions?

DR. TRIFUNAC: I have a question for NRC. Do they still limit the standard review plan to shake, lush, and flush?

DR. OKRENT: No. Dr. Chan?

DR. CHAN: This is Sy Chan, Structural Engineering Branch.

I think I should start from the day, February 7, 8, 1977, the ACRS had a subcommittee meeting at the Ramada Inn, Bethesda, and during that meeting, some staff members of the Structural Engineering Branch had challenged this standard review plan position, which accepted the shake, lush programs as an appropriate means to calculate for the soil structure interaction of the structures.

After that meeting, I think it is the consensus that there exists a problem in using that shake, lush program for soil structure interaction because the solution is rather model dependent. That depends on how you model the soil media. For the one-dimensional wave propagation model, which is actually equivalent to a shear modeling, and it is quite different from a half space, or in those days we called it equivalent soil spring method, or compliance function method.

And after that meeting, the Structural Engineering Branch began to take a more cautious approach, and requested that the structural response, calculated structural response from both of these methods be compared. We cannot stick to, say, accept just one set of calculations.

In those days, two methods were acceptable. One is the old half space soil spring method. The other is the so-called finite element. Actually, it is one dimensional shear wave propagation method. Later on, I think it was around in 1978, the Structural Engineering Branch requested that the results by those two methods be compared.

Usually, the end product is the full response spectrum at different levels of the structure, and we require, in order to design for equipments and whatever piping systems are anchored on the floor, then the envelope of the response spectrum should be used, and that was their position after this Ramada Inn meeting.

Before that, and even as is now in the official standard review plan, we still list these shake, lush programs as being appropriate, but actually our branch position has been modified.

MR. KNIGHT: I might add -- This is Jim Knight. Any confusion between what is in the standard review plan and what we are doing, that is just a matter of not having the time and resources to go back and modify the standard review plan.

DR. TRIFUNAC: But that still generally means that if I submit design of a plant to you tomorrow, that if I do it with lush and flush and shake, it will be okay.

MR. KNIGHT: No.

DR. TRIFUNAC: I mean, as one approach.

MR. KNIGHT: Well, but you have got to submit that approach and you've got to look at it also from a compliance function approach, and to generate floor response spectra at a given location, you have got to envelope both of them.

DR. TRIFUNAC: I see.

DR. ZUDANS: Isn't it true that a new one that you are proposing now no longer makes reference to the specific code, only discusses two methods?

MR. KNIGHT: That would be our approach.

DR. ZUDANS: The one that you have now.

MR. KNIGHT: Well, we don't -- The only reason I am hesitating is, we really don't have -- oh, it is a draft. We don't have a plan out on the market, out on the street, so to speak. It is about time to do it.

DR. ZUDANS: Not on the market. It is on our street, but not on the public street.

MR. KNIGHT: Those are recommendations. They don't represent a plan endorsed by the staff and by management and published.

DR. OKRENT: Okay. I will hold you there, because we will get behind schedule. We are going to get back to these topics when we get to the recommendations, and I want to leave time for the recommendations, as Dr. Bagchi has suggested, so that is, in fact, when I will ask the consultants to come out as well as they can with any commens they are able to make today.

There will be another round arriving possibly, but I think at the time we have the recommendations, we would like to see what specific things you may have to say at this time.

All right, let's go on. We are on schedule one hour late.

DR. BAGCHI: I am sorry. The next speaker is Dr. Leon Reiter -- no, no, I am sorry. Larry Wight of Tera

Corporation.

I might add, I forgot to mention that there is a slight change in the presentation of the last topic, which is the summary. That will be presented by Dr. Paul Smith of Lawrence Livermore.

MR. WIGHT: Good afternoon. My name is Larry Wight, and I am with Tera Corporation. I was the project manager at Tera on a project that I would like to present to you today, overview the overall approach and present some of the conclusions that we've got thus far.

I would like to also say that this was a Lawrence Livermore Laboratory project, and Don Verner was the project manager at Lawrence Livermore. He could not be here today, and so I am speaking for him.

Many of you are not aware of the details of this project, and so I would like to go into its overall basis in a little bit of detail here this afternoon. Our objective in this project, which started about two years ago, has changed a bit with time, but it currently is to integrate all relevant and available data with seismic hazard models to provide the best estimate of the seismic hazard as expressed by the free field instrumental ground motion for the SEP sites.

As we stepped into this project, we faced head-on an issue that many other people preceding us had faced, that is, what is the basis for certain global decisions that have to be made in any seismic hazard analysis, where the data is incomplete, sparse, or uncertain, or inaccurate.

I am not thinking of such cases as trying to estimate the magnitude of a specific earthquake, or as we talked about just before me, what the effect of soil column response is on ground motion. Instead, I am really referring to much larger issues that are in the end judgment calls.

For example, how does one deal with earthquakes in New Madrid, that have occurred in the past down here, and how does one deal with the question of the likelihood of those earthquakes migrating up the Wabash Valley or covering the entire central stable region, in the same way as was mentioned earlier today, as the Charleston earthquakes, and what is the rationale and the basis for restricting the Charleston earthquake to that specific locality?

Identical global questions like this might include, what is the maximum possible earthquake that a particular region or, more uniquely, a specific fault might generate in the future? These are particularly in the eastern United States, but in general in any part of the country, very difficult questions to answer, given the length of recorded history.

We structured this program not only to try to develop the best possible basis for answering these global issues, but to also provide the maximum benefit to the NRC, and I would like to here think of that benefit in four categories, credibility, accuracy, quality, and relevance.

In terms of credibility, the seismic hazard model that we employed for the analysis was well tested. It had been peer reviewed both at universities and in the scientific publications. It was accurate in that we had tested this particular methodologies against others that were at the same time available, with favorable results.

In terms of quality, from the very beginning of this project, we attempted to make peer review a keystone, and I will talk more about that in a bit, but internally, just at Tera, we attempted to bring specialized experts in to assist us in dealing with very specific points, and I have mentioned a couple of those people on this vu-graph.

They included Harris Shaw, co-director of the John Bloom Earthquake Engineering Center at Stanford, Greg Baecker at MIT, a specialist in the application of probability to civil engineering issues, Tom McEvilly, editor of the Seismological Society of America bulletin, and Danielle Veneziano, also at MIT, dealing in application of probability to civil engineering problems.

In terms of formal peer review, a group of people were assembled to actually independently review the entire program and some of the preliminary results we had developed at that time. These people were, specifically, Professor Otto Nuttli, Professor Lyn Sykes, Professor Al Ang, and Professor Veneziano, who had also -- who has more recently assisted us internally.

I would like to just say a few words about how -- what alternatives one has in performing a seismic hazard analysis. As we initiated this project, we attempted to find ways that were as compatible and as consistent as possible with the Appendix A approach, and so on this vu-graph I would like to overview the different techniques that are available to any analyst performing a seismic hazard analysis.

Many of these have been mentioned earlier today, in fact. First, there is a deterministic approach, which is an approach based on first principles, such as is being applied at San Onofre, and it is being applied further investigated under NRC research grants. In this particular approach, one starts from the first physical principles of the process and attempts to model the rupture process itself, the fault kinematics and the material properties of the earth media, to synthetically predict ground motion.

Another overall approach that is available to analysts is an empirical approach, and for illustration here, I indicate that Appendix A is really an empirical approach. It is a response spectrum which has been defined from a sweep of accelograms anchored to an empirically determined pga.

Something closely related to that is the empirical site-specific spectrum, which is also being applied at San Onofre, as was mentioned earlier, which was direct averaging of spectral ordinates from a representative sweep of time histories.

Finally, there is the probabilistic approach, and in fact, it was the principal approach selected for this project. It is distinguished from the other two in that it considers all possible earthquakes at all possible distances, along with the probability of occurrence. I would like to go into a little more detail as to the distinction between these different approaches.

But just to cut these techniques one different way, I would like to say that every seismic hazard analysis consists of four distinct steps, represented as four quadrants here in this vu-graph, one being zonation, another being representation of seismicity, a third a ground motion model or an attenuation model, and finally, some sort of synthesis or integration.

The deterministic approach starts with the hypothesis that a given earthquake, say, occurring in an arch structure in the base underneath Anna, Ohio, postulates the character of that earthquake, and models the ground motion resulting from it, and it thereby has somewhat arbitrarily selected the particular size of the earthquake, and it models internally the ground motion model that is built into the physics of the code.

On the other hand, the empirical approaches all say that the hazard at a particular site is dominated by earthquakes coming from a particular region, and it, too, therefore, is making a statement as to the nature of activity that contributes to the exposure of the site, and it bypasses this step completely through this hypothesis.

However, the probabilistic approach considers all possible zones around the site, along with the probability of occurrence of all sized earthquakes, and includes with that a ground motion model and formally integrates out over all sizes and distances.

Each of these techniques, then, deterministic, empirical, and probabilistic, represents a very different perspective of the seismic hazard, and therefore the reslts are best used comparatively. I would like to just quickly review again the distinction between these three approaches.

The objective, first of all, in the deterministic approach is to model a specific earthquake at a specific location. In an empirical approach, it considers a sweep of earthquakes over a very tight magnitude range and distance range, while the probabilistic approach considers all earthquakes and all distances.

The advantage of the deterministic approach is that it allows modeling outside the data, with the credibility associated with the quality of the modeling, of course. The advantage of an empirical approach is that it is very simple, very direct, one answer. The probabilistic approach we favor strongly in that it allows a decision-maker to trade off other variables in the problem, such as in the case of evaluating the safety of old reactors, the radio-nuclide inventory, that has been in issue at the GETER, for example. The dispersibility, the population, the structural resistance, all the things that would ordinarily be part of a formal risk analysis.

There are disadvantages of each of these, and I want to acknowledge them. In the case of a deterministic approach, it is a very complex model, very, very complex, and there is a lot of uncertainty as to how one should attack certain components of that problem.

The disadvantage of the empirical approach is that it has an underlying hypothesis as to what dominates the seismic load. A given size earthquake at a given distance.

The probabilistic approach has a big disadvantage, an that is that you must model tails of distributions derived from rather incomplete data, and the specific modeling of those tails can for certain risk -- hazard levels or return periods drive the results, and I will give you some examples of the extent to which it can drive them a little bit later.

I would like to develop a theme starting with this vu-graph that will better introduce one aspect of this project. And what I am saying here is that any seismic hazard analysis is only as good as the judgment and the experience of the analysts involved in the project, no matter what approach is applied, whether it is deterministic, empirical, or probabilistic.

And so, in the case of a deterministic approach, the analyst must model the fault rupture kinematics. He must pick a stress drop or a magnitude for the earthquake. He must model the earth media. A very complex question.

In the empirical approaches, again, its underlying hypothesis. Somebody must decide the magnitude range and the distance range that drives the seismic hazard, whether it is an Appendix A approach or a site specific approach.

In the probabilistic approach, there are basically four components that an analyst must judge, and they are the four quadrants, basically, of that earlier vu-graph, that is, the zonation, the region in which earthquakes -- future earthquakes will occur, their frequency within those regions, characterized by a size distribution, the maximum possible earthquake in each of those regions, and finally, a ground motion or an attenuation model.

These are all fairly difficult questions, and depending on one's success in dealing with them, one can produce quality results or not.

NUCLEAR REGULATORY COMMISSION

This is to certify that the attached proceedings before the

____

in the matter of: ACRS - Subcommittee on Extreme External Phenomena

Date of Proceeding: June 4, 1980

Docket Number: ______________________

Place of Proceeding: Washington, D. C.

were held as herein appears, and that this is the original transcript thereof for the file of the Commission.

Suzanne Babineau

Official Reporter (Typed)

Suzanne Babineau

Official Reporter (Signature)