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UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

Docket No.

Subcommittee on Extreme External Phenomena

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UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

Subcommittee on Extreme External Phenomena

Presidential Room
Airport Executive Inn
275 S. Airport Blvd.
San Francisco, California

Thursday, December 8, 1983

The Subcommittee on Extreme External Phenomena convened at 8:30 a.m., pursuant to notice, David Okrent, Chairman of the Subcommittee presiding.

PRESENT FOR THE ACRS:

D. Okrent, member
H. Lewis, member
C. Siess, member
J.C. Mark, member

S. Bush, Consultant
P. Pomery, Consultant
B. Page, Consultant
A. Ang, Consultant
J. Maxwell, Consultant
G. Thompson, Consultant
E. Luco, Consultant
J. Reed, ACRS Consultant
W. Hall, ACRS Consultant
R. Kennedy, ACRS Consultant
C.A. Cornell, ACRS Consultant
M. Bohn, ACRS Consultant

PRESENT FOR THE ACRS (Continued)

R. Savio, Designated Federal Employee
S. Seth, ACRS Staff

G. Lear, NRC Staff
R. Jackson, NRC Staff
V. Noonan, NRC Staff

ALSO PRESENT

L. Reiter
C. Stepp
P. Yanov
R. McGuire
S. Alexander
R. Budnitz
C.P. Tan
B. Henries
O. Kustu
P. Smith
C.W. Lin
D. Tang
D. Guzy
J. Kimball
G. Johnson
C.K. Chou
I. Wall
R. Thomas

P R O C E E D I N G S

MR. 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.

Other ACRS members present today are Drs. Lewis, Siess , and Mark.

We have a number of ACRS consultants in attendance. They include Dr. Ang, Dr. Bush, Dr. Luco, Mr. Maxwell, Dr. Page, Dr. Pomeroy and Dr. Thompson.

Have I missed anyone?

The purpose of the meeting is to conduct a workshop on the quantification of seismic design margins for nuclear power plants.

The main topics of discussion will be the adequacy of the methodology for quantification of seismic design margins and a discussion of ongoing NRC and industry programs in this area.

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

Dr. Richard Savio is the Designated Federal Employee for the meeting.

The rules for participation in today's meeting have been announced as a part of the notice of this meeting previously published in the Federal Register on Wednesday, November 23, 1983.

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

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

We have not received any written statements from members of the public or requests for times to make statements. However, I note that the agenda provides for many discussion periods and as with previously meetings of a similar nature held on the subject of extreme external phenomena, we would like to treat this meeting with some -- a considerable amount of flexibility.

We will encourage participation from the floor, just as to be acknowledged to participate in the various technical matters and so forth.

I will try to keep us approximately to the printed agenda, but we are here to learn and exchange ideas.

By way of just a bit of background, although I suspect most people here are familiar with the matter, in a considerable number of operating or construction permit reviews in the past several years, the ACRS has raised questions concerning aspects of seismic safety.

In some cases, the questions were specific to the particular plant. In some cases they were rather more general and for example, in its report on the Perry reactor, the ACRS stated its belief that it's important that there be considerable assurance that the combination of the seismic design basis and the procedures used to establish the design margin be such that the seismic risk represents an acceptably low contribution to the overall risk.

In January, 1983, the ACRS wrote a letter to the Chairman of the NRC. The subject was quantification of seismic design margins and in this letter the ACRS discussed the general subject, made some suggestions for how the problem might be examined on a broad basis rather than on a plant by plant basis and since then I guess we have been waiting to hear what the NRC Staff and the Commissioners think about the matter, whether they in fact expect to have programs which pursue these questions in some broad generic way or whether they expect to deal with these things on a case by case basis the time scale on which they will address these matters or perhaps whether they have information which deals with the question of just how much a contribution to risk the seismic part is.

So, one thing we would hope to learn from this meeting is where the NRC thinks it stands and expects to be moving.

Similarly, we are interested in learning how representatives of the industry view this matter and how they think it should be addressed, so there are these sort of programmatic kinds of questions that we hope are addressed directl or indirectly as part of the meeting.

I was trying to summarize what we might hope to learn from this meeting in a few short questions and what I wrote down was the following.

This is on the plane after one o'clock, Scott, so I don't know --

(Laughter.)

-- whether it quite meets the mark but anyway, how much do we know about the seismic contribution to LWR risk?

How much do we need to know?

Can we learn what we need to know and if so, how?

And what are the NRC programs and are they adequate for the purpose?

So, by way of a brief introduction, I will ask the Subcommittee members if they would like to comment on the agenda or raise issues that they would like people to particularly address or whatever.

Mr. Mark, Dr. Seiss, Dr. Lewis, any comments?

MR. MARK: I don't think it adds anything. I am wondering though if the position we would like to approach asymptoticly and in as short an asymptote as possible, would it be that of wishing to feel we had level of confidence in an earthquake with the appearance time of something in the few times ten to the minus fifth or few times ten to the -- let's see -- a probability of occurrance per year, let me try, of a few times 10^{-5} or some number less than 10^{-4} and we want to be as sure then as we can that the plant will be in a position to cope with such an event and shut down safely and that is what we lack, or one way of saying what we lack, and that is what we would like to get to as far as possible as soon as possible.

MR. OKRENT: I think you have put a very succinct measure of the matter. That was a helpful comment.

MR. SIESS: I am not sure it was that helpful without the uncertainties being defined. I am perfectly happy with the four questions you stated, Dave, if we can at the end of this session come up with any kind of answers to those four questions, I think we would have accomplished a great deal.

MR. LEWIS: I think you have stated the questions reasonably well.

I would like to come out of this whole thing better educated on my confidence on the estimates of recurrence times for given acceleration at a given site than I am now, so for my part I have more confidence in one's ability to predict the behavior of the structure than to predict the recurrence time for a given acceleration.

MR. OKRENT: I might note in connection with Dr. Lewis's last comment, we had hoped to have a considerable discussion on this matter with opening talks by USGS.

Unfortunately that part of the program seems to have melted away for reasons I am not sure I completely understand but the topic will remain and we have people here who have thought about the matter so I don't propose to lose the topic.Even if we have lost that presentation, it may be that somehow during either today or tomorrow we will manage a 90-minute interval or something like that where I think that people can discuss this I will say impromptu, but people shouldn't be surprized if I called on them for opinions.

Are there any other points?

All right, let's see if we can get ahead of the agenda at least once.

We will move into the item where Mr. Jackson will give us some summary from the Staff.

MR. JACKSON: My name is Bob Jackson. I am Chief of the Geoscience Branch at NRC.

Basically I have prepared with Mr. Knight a little written introduction which I will read and be willing to discuss afterwards.

Jim Knight has been tied up for the past six weeks in a site in California. He sends his apologies for not being able to attend this meeting. Instead he sent three of his four Branch chiefs and I am not sure we can fill in fully for him.

In response to question number two, I have a short introduction.

Basically the ACRS has requested that we provide this Subcommittee with some insight as to what is likely to come about or what is expected in the future by the NRC Staff in the area of Quantification of Seismic Design Margins.

As you are aware, we have discussed this issue with you a number of times during 1982 and prior to that.

As a result, the ACRS forwarded a letter to the Commission last January. We viewed this letter as a call for a consolidation and possibly rethinking of NRC programs to allow us to gain better confidence in the capability of nuclear power plants to withstand earthquakes greater than their design basis.

I also reviewed the request as a call for continued emphasis on a multiple approach including both deterministic analysis of seismic margins and a call for increased attention to seismic probabilistic risk assessments.

We forwarded our general response to the ACRS letter in an April 4 letter to Commissioner Ahearne, in which we indicated that we concurred in principle with the Committee's recommendation but also indicated that extensive discussions would be needed to define specific programs that are feasible considering the availability of data and resources, resources meaning both staff and dollars.

We are here at this meeting in this spirit, not only to contribute to the discussion but to hear suggestions from the Subcommittee, your consultants and other participants on how we should be proceeding.

We have available here for discussions members from the NRC Staff prepared to discuss a variety of topics from hazards to equipment qualification structural engineering and geotechnical engineering.

Each of these individuals will provide a separate summary or specific insights on the work they have been doing.

As you are aware, I have told you before, both Ted Algermissen and Jim Devine of the Survey are not able to attend due to the press of other activities. Jim Devine's press is that they just received their budget yesterday or the day before from OMB and suffered quite a bit in that and he felt it was more important to stay behind and try to recover some of that and that is a probably more important effort for him at this point in time.

Dr. Algermissen would have liked to participate but felt he did not have sufficient time to prepare adequately and as a result he will look forward to meeting with you in the future.

The USGS has however asked that Dr. Boatwright provide us with an analysis of information relating to the New Brunswick earthquake. I think you will find that interesting.

We have suggested he be put on the agenda for tomorrow around 10:45, I think in place of Mr. Devine. He will, I think, raise some stimulating discussion on observations of high frequency ground motion in that area.

Since forwarding our April 4 letter, we have made some progress in responding to your request but due to the press of other activities, we have not advanced as much as we would have liked.

As we previously indicated, it is our intent to proceed on this question with an existing programs and staff and resource availability. This is still our intent. That offers a severe impediment to our making a great deal of progress.

We have, however, made substantial progress in the area of how to deal with external events and PRA. A working group prepared a report which made seven specific recommendations in the external event areas including a view, one that external events should be included in PRAs. It may seem obvious that that should be the case but there is a fair number of personnel within the Staff who don't believe this should be the case. Until this working group met and over a period of several months had extensive arguments, this was the overall recommendation made to the Division of Engineering and has been embraced essentially by the Office.

The second observation was that a short term program to develop external event DRA procedures, guidelines and acceptance criteria be developed. This activity has been incorporated into the current operating plan and is scheduled for completion in September of 1984.

Essentially the idea there is to provide the industry and the Staff with both some guidelines as to how we should be proceeding and what they should do and how we are going to review that when we get it.

One other item was a need for research activities and the call for an assessment of a simplified external event PRA methodology and there were numerous recommendations.

That document I belive has been made available previously to the ACRS.

I might also add that one of the research recommendations indicates it is necessary to make an assessment of where the SSMRP, Seismic Safety Margins Research Program, fits into this program and how to go about improving prioritization of related PRA issues.

A second point: we have also made considerable progress in establishing the seismic hazard for plants in the Eastern US as a result of the Charleston earthquake issue.

This program, which is being conducted for the Staff by Lawrence Livermore National Laboratory is proceeding well and we have some preliminary results to share with you tomorrow.

We have also had a number of meetings with Atomic Industrial Forum and EPRI and their owners groups to provide further encouragement for their extensive new program on seismic hazards.

I understand that several people, Carl Stepp and others, from EPRI will be presenting that program to you and I think you will find it very interesting.

We have also had the U.S. Geological Survey through the Office of Research working on the effect of the seismic hazard assuming certain tectonic models for the Eastern United States, those causative models.

Based on these programs and our more deterministic geologic and seismologic research effort, we feel quite strongly we are making excellent progress in improving our capability of characterizing the seismic hazard for a given site. We look forward to some very interesting results in the next year or so.

Dr. Lewis's comments are well-taken in that I have been in a number of meetings in which the internal event personnel indicated that they know more accurately where they are going than the external events, which is unknown and vica versa, so it is a fairly strong argument that is continuing.

A third point: as we mentioned in the April letter, the Seismic Qualification Utilities Group has held a number of meetings. Additional meetings have been held and Vince Noonan, Chief of the Equipment Qualification Branch is here and following this introduction will present his findings.

There has been some discussion within the Staff of a call for an owners group for seismic design margins issue. That issue is not totally resolved within the Staff right now. There is some reluctance to encourage the industry yet to develop anothers owners group and I think we will be looking forward to your guidance on that, also whether you think there is such a need.

We are currently actively involved in the review of several PRAs which include the seismic considerations.

We have recently completed our review of the Limerick PRA and we will be discussing these results with you in the next few months on the Limerick docket.

In addition, we have initiated our review of the Millstone PRA and GESSAR PRA. These reviews now include a substantial involvement of inhouse NRC Staff, especially in the seismic area.

On this point, this was not the case for the previous reviews that were done for Zion or Indian Point, especially in the external event area.

In our reviews, we are finding we must specify the seismic be considered in a relative rather than an absolute manner.

MR. OKRENT: Excuse me.

MR. JACKSON: Excuse me.

MR. OKRENT: What do you mean and why is that a requirement?

MR. JACKSON: It basically means that the calculations of risk and hazard that are being determined have uncertainty and extensive assumptions made and them such that you can make comparisons between one site and another within the seismic area but that the actual number being assigned to that may be substantially in error, either higher or lower.

It is basically a way to deal with the uncertainty.

MR. OKRENT: And it is your position that you are not comparing two absolute things and calling it a relative comparison? I am trying to understand.

MR. REITER: Dr. Okrent, can I answer that question?

MR. OKRENT: Why not?

MR. REITER: Leon Reiter of the Staff.

I think the important thing we want to talk about the relative comparison is because of the uncertainty in making those comparisons we want to have comparisons which we think are comparing items of essentially the same, subject to the same sort of errors.

For instance, typically we like to compare apples to apples. Some of the ways these are being used are within say, a plant we have a sequence of seismic events. We find which seismic event, which seismic sequence contributes most to risk or to core melt frequency, something which may not be as appropriate as comparing seismic to nonseismic initiating events. There the source of uncertainty may be different and that kind of relative comparison may not be as appropriate, so the relative works the extent to which you can talk about comparing similar types of phenomena that are subject to the same sources of uncertainty and I think as we recommended in the Limerick study, we can see as we like to say "carefully constructed comparisons."

We have to think carefully about those comparisons and if we think it proper, draw the proper conclusions.

MR. OKRENT: Let me see, are you discussing the seismicity and geology, one side compared to another, or are you talking about what is happening inside the plant to components in what you just said?

MR. REITER: We can talk about whatever. In any aspect you want to talk about, we have to make the same judgments, comparing similar types of phenomena such that we are not dealing with essentially two items which in source of uncertainty are so different that the comparison may be meaningless.

MR. OKRENT: Let's stay with the site question just for the moment.

To my own naive way of viewing these things, if you are trying to suggest some kind of hazard curve in the Northeast and then another hazard curve in the Chicago area and a third one in Tennessee and then you say, we are getting relative numbers and we are really not comparing things that are basically absolute in each place by doing a comparison, but somehow the assumptions you are making all are interwoven and there isn't really a separate picture involved in one area that could be radically detached from the other areas.

Am I making the point?

MR. THOMPSON: (Nods affirmatively.)

MR. OKRENT: I hear you frequently talk about this relative thing and about somehow it leads to less uncertainty or less difference or however you want to put it than doing things in an absolute fashion.

I will refer you to a recent draft or final report by a former NRC employee in PRA named Vesley, in which he points out that you are as likely to have large errors in relative comparisons or larger errors than in absolute and by the way, I endorse that position.

MR. REITHER: I think that is correct depending on what relative comparison you are making.

I will give you an example. Let's talk about seismic risk from one place to another. If I have one group of analysts who analyzing, say, the seismic hazard in one plant, say in the Northeast and then I take a completely separate group of analysts who are doing something, let us say, some place in the Southeast or in the Midwest, that is not as appropriate as comparing the hazard curve generated for those two sites done by the same group of analysts involved in the same programs.

End 1.

For that reason, we have urged the utilities to come up with a coordinated pattern so that we could determine hazard risks from one large body. That is the program EPRI is undertaking and Lawrence Livermore is undertaking.

MR. OKRENT: But you have only responded to a part of the way in which I will call it differences or uncertainties may enter.

There are questions of what is going on. That may be misunderstood even though you have body doing the whole country.

MR. REITER: That is possible.

MR. OKRENT: Okay.

MR. REITER: In no way do you eliminate the uncertainty but our experience is in making these relative estimates and looking at different groups, we find the uncertainty is dramatically reduced when we have these carefully constructed relative estimates.

I see Dr. Siess is shaking his head.

MR. SIESS: I don't understand it, Leon.

It seems to me you might be more likely to get similar results by having two parts of the country done by different groups of analysts, each doing their first review or each doing their second reviews and by the same groups of analysts doing the Northeast then with that experience doing the Midwest.

I don't see on what basis you decide these things are the same or similar or less different and I really need a glossary to understand you. I am getting all sorts of words here -- "relative, " "absolute," "different," "uncertainly" and I don't know what they mean.

MR. JACKSON: Maybe I could make a suggestion.

We talked about this before we came. Leon has been recently involved in coordinating the review for the Limerick PRA and in that regard he has completed that review.

We talked about having him make a presentation based upon that and maybe if he did that in an organized fashion to you later today or tomorrow, we could then pick up on this discussion again and he could define a few terms.

Is that acceptable?

MR. OKRENT: Well --

MR. JACKSON: We have time to put it in, I am sure.

MR. OKRENT: Let's see how things -- I am not sure this topic will be sitting on the table quietly until then.

Did you have a point, Dr. Lewis?

MR. LEWIS: I wanted to obfuscate even a little further if I may.

(Laughter.)

It may be too early. I find this discussion extremely interesting and I have to say up until a few years ago I was a great "relative risk buff" myself but it went away.

(Laughter.)

Just like the common cold, it will probably come back. Let me pose it in a simple way without speaking about seismicity or anything like that.

I am trying to understand what is meant by conservatism and the way in which it affects our assessment of relative risks.

Let me pose a hypothetical case to illustrate my problem, Risk A and Risk B.

The point estimate for Risk A lower than for Risk B, but the uncertainty band on Risk A is greater than it is for Risk B. If I take the point estimates, I conclude that Risk B is more serious than Risk A. If I go to the 95 percent confidence level or whatever statistical measure you want use, then because the uncertainty in Risk A is greater, I am pushed up to make it a more serious threat to the plant than Risk B.

So the question of how you assess relative risks depends very much upon how much confidence you want that you have properly bounded the risk and I am worried that the track we run down is one in which the most uncertain thing always dominates because we are so obsessed with wanting to go out very far on the confidence curve and I would dearly love to understand that issue in the context of seismicity at the end of these two days if it can be, but that is my problem with relative risk.

In the conservative world in which NRC lives, it is so driven by the uncertainty that the actual point estimate almost becomes irrelevant when you have uncertainties of factors of 10, 100 and 1000 as you sometimes do. That is my further obfuscation.

MR. OKRENT: Mr. Reiter?

MR. REITER: Yes, Dr. Lewis, I agree with you 100 percent.

MR. LEWIS: That is overkill.

(Laughter.)

MR. REITER: I believe in the context we are talking about it may not apply. Simply stated, if we can compare two entities subject by and large to the same bias and that bias differing we can compare those assuming different biases and look at, assuming there is one state of bias that is correct, we can see how those results compare.

When we do that, those kinds of results we find are not completely stable but there are more stable than if we look at the absolute estimates of uncertainty alone.

In other words, if the sources of uncertainty is the same in two entities, we can make a more valid relative comparison than if the source of uncertainty is different.

Now many of the sources of uncertainty in estimating seismic hazard are the same. We can make those kinds of comparisons. It is not a statement of absolute truth. There are still uncertainties involved but we feel butter about those kinds of statements.

MR. LEWIS: Yes.

MR. REITER: The best example would be, supposing we have very competing hypotheses, true, about what is causing earthquakes in the Eastern United States.

We can see what the results of those varying, competing hypotheses are, what different experts might say and if we get a consistent message coming from that, no matter what the numbers are, if that gives us some sort of ranking about different areas being let's say subject to more hazards than other areas, we feel more comfortable with that conclusion than saying no, this is a $10^{-6.7}$ and that is a $10^{-4.3}$. It all depends on the ability to make those carefully constructed comparisons where the sources of bias are similar.

They will never be 100% similar, but the more they are, the better for comparison. Again, we feel, many of us feel, that the comparison between seismic and nonseismic risk contributors may be premature.

I thank many of the people involved in PRA have indicated that. I know Dr. Kennedy has indicated that. And maybe John later on - - I don't know what his opinion is, maybe he will be able to talk on that later on.

Also it is not a case of one is completely (ay in relative if one is completely bad in absolute; there is a gradation and one has to be very careful and look at that and when making those comparisons to see to what extent that conclusion is correct.

MR. JACKSON: This has created some problem. I notice there is one utility in the audience here and their consultants who feel strongly about the use of absolute probability and feel that the calculations they have done for their plant, the absolute probabilities, calculated is the correct one.

So from a regulatory point of view, we then get into a debate about whose absolute probability that has been calculated is correct or incorrect. It is in my view kind of arguing about the appropriate g value. I think we are heading down a path where this issue needs to be worked out.

Dr. Cornell is here. I think he probably has some strong views on that.

(Laughter.)

MR. CORNELL: He was a little late. I will wait until I have caught up.

MR. OKRENT: I am sure we will come back again to the question of should seismic probabilistic studies somehow not be mixed with other probabilistic studies?

Just for the moment let's leave it at that is not a dogma universally accepted.

I interrupted you in your statement. Please go ahead.

MR. JACKSON: One of the primary problems that continues to persist in doing seismic PRAs is the reliance upon subjective judgment, both for seismic hazard and fragility and the lack of data base in both of these areas.

From an administrative point of view, we recently formed an inhouse NRC Seismic Design Margins Working Group to assist in establishing our future directions with regard for the need to new work or the modification of existing programs.

The overall approach of the group will be work in the coming year to assess our progress in different areas.

One example, for instance, would be assessing what the seismic PRAs we have done have really taught us. The Office of Research will play an active role in this group. One end in mind is to help the Office of Research in identifying how fiscal year 1985 research resources will be expended to address the current problems so this meeting is timely.

The Office of Research in our April 4th letter to you indicated that NRR would assume the lead role for this issue and the Office of Research is looking to us to provide them guidance on how we should be proceeding.

I hope this meeting will touch on a number of areas. Should we be acquiring more data on fragility, it seems obvious the answer is yes. What new methodologies or simplified methodologies should we be developing and the like.

One issue that continues to present a significant problem from the regulatory side of the coin and it was raised as we indicated in previous meetings and was raised again in a recent Commission meeting you have with the Commissioners and raised by Dr. Remick was our ability as a Staff to implement the additional requirements on utilities to address the seismic design margins question.

This issue was not at all resolved. The NRC Staff currently does not perceive the seismic design margins issue to be an unresolved safety issue based on the general understanding that has been developed of inherent seismic capacity of nuclear power plants that has been obtained through extensive programs conducted to date.

Information --

MR. OKRENT: Excuse me, could you give me the technical justification for this point of view that would help me compare it with some other unresolved safety issues, in other words, what you think the risk from seismic is and what you think the risk from these unresolved safety issues is, so I can see that.

Do you think it is smaller from seismic and there-

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fore doesn't have to be called an unresolved safety issue?

MR. JACKSON: I cannot do that here but --

MR. OKREN: Then can you tell me on what basis Staff decided it doesn't need to be an unresolved safety issue if it is not a risk basis?

MR. JACKSON: It has been based on the engineering judgment by the reviewers and management there over the years, testimony that has been proffered in many hearings on seismic issues over the last 15 or 20 years and was a general understanding that it is not a priority issue relative to other issues.

I don't know. It's like many things the NRC does. It is a strong engineering judgment based upon years of trying to decide these questions in individual plants.

I am not equipped to sit here and argue point by point with you but this is essentially the position currently. Obviously that could change.

MR. SIESS: Bob, just to say engineering judgment doesn't tell me anything. I could make an engineering judgment as to whether something satisfied the NRC's criteria. I can also make an engineering judgment as to whether this is a large or a small risk.

Now are you making an engineering judgment about risk? That is what Dave was asking you?

MR. JACKSON: I think that judgment is obviously made as you go along, as this risk is different from other things. I have often been told that an earthquake is just one other load and there are many loads that contribute to it. It is, as an example, the Division of Licensing, who is responsible for the plants in responding to the letters that have come forward on individual plants has really not asked the Division of Engineering to worry about this problem in particular, so from that point of view it clearly did not float up as a significant issue.

MR. SIESS: From a nonlegal point of view, the Staff has reached a judgment that the seismic margin issue does not affect the health and safety of the public to the extent that the 25 or 30 unresolved safety issues do?

MR. JACKSON: Yes, that is true. Now I am not here capable of defending that.

MR. SEISS: I don't have that list of USI's in front of me but I am going to think of a couple.

MR. JACKSON: I think the question you are raising ties in with the overall question of regulatory authority.

The utilities and maybe some of them here today can discuss is -- feel they have met the criteria in the regulation and as a result something beyond that is a new question.

MR. OKRENT: Excuse me a moment. One could have taken the same attitude on station backout if I can take just one example, since you ordinarily do not postulate loss of all of your onsite AC power in the regulations.

I think we are hearing about a fundamental matter. In fact, what we have heard, as I understand it, is there are important sections of the Staff probably within NRR at the Office level and the Division of Operating Reactors and so forth who currently -- for whatever reason -- do not choose to really give this matter priority or perhaps to treat the matter and furthermore, I think we have heard that there is no what I would call focused effort within NRR to try to tackle this.

We have heard there are pieces here and there but there is not a person or a small group who is given the job of really putting it in perspective and defining a program when one is needed despite the reassuring memo that Mr. Dircks sent to the Commissioners in April that the Staff agreed with the January ACRS letter and were going to move along.

So, one of the things I think that the Subcommittee has to think about during this meeting is whether something considerably stronger than the January 11 letter is needed.

There are different kinds of ways in which things can be stronger. On the other hand, if we are satisfied that the seismic risk is really probably very small, we can just write the Commissioners to forget about our January 11 letter, but I am rather curious how the Staff arrives at this position on USI just from the Indian Point PRA alone. It seems a little incongruous to me.

MR. JACKSON: I don't want to give the indication that there is not concern about this topic within the Staff. I think th e is extensive concern. Of course, there is a lot of concern about a lot of other issues.

I think based upon all of the work done and money expended, this is just a judgment being made. Now I think there is a commitment to proceed. Along with the existing programs, there are a number of PRA programs ongoing.

I believe we have really done only our first PRA with Limerick and that may be a biased view because in the Indian Point and Zion, the Staff, which had experienced dealing with a deterministic judgment, was not extensively involved in those reviews so we are just beginning to find our way in this particular area, so there is concern and there is effort. There is quite a bit of effort at research.

I think there is a strong feeling that we need to go through with the current PRAs that we have just completed on Limerick 1 and we will have a meeting with you and the utility on Limerick and the Millstone PRA, which is proceeding well.

We will be completing that, I think, some time in the early summertime and then assess what that is really telling us about both plants and whether it is really helping us make the judgments we need to.

End 2.

Now Indian Point was one case. Zion was another case in which seismic was important and I think in Limerick it does not show itself to be an important contributor. Millstone, we are still awaiting the results.

Basically, any major program that would have to be implemented requires a lot of interaction with Research and with the utilities and this would require some formation of the utilities group.

We have had the Seismic Safety Margins Research Program, which expended many millions of dollars and allowed us to make some of the judgments we are making and I think we will hear from Mike Bohn and others about what that actually did contribute. That's all.

MR. SIESS: I think I missed the point, Bob, about how looking at the individual PRAs helps you decide something.

MR. JACKSON: I think if we look at a few we have actually done and tried to wrestle with internally in the seismic areas, let us say structural fragility seismic hazard equipment qualification, mechanical engineering type problems and now we look at a few PRAs where we have really become intensively involved, what does that tell us about how we do business and whether or not seismic is really a contributor to risk?

For instance, at Indian Point it was believed that seismic became a dominant contributor because of the conservative assumptions made in certain elements that were of the analysis made and I only know it in general -- there are others here, probably John Reed, who could explain in detail what those assumptions were.

MR. SIESS: Suppose I make six PRAs and in only one instance is seismic a major contributor to risk, now what does that tell me about the importance of the seismic margin issue? That it is only one-sixth as important as station blackout?

MR. JACKSON: I don't know. It is obviously a problem.If it shows it is the dominant contributor to all plants, it is obviously a higher importance than if it is for a few plants.

MR. SIESS: Would you be willing to take that one in six and say that is only seven plants or ten plants out of 60 or something so it is not important when you don't know which ones it is?

I was just wondering, once I find one plant where I honestly believe it is important, how many do I have to find where it is not important before I stop worrying?

MR. JACKSON: I obviously can't answer that question.

MR. OKRENT: If I pursue the question --

MR. JACKSON: Unless you guys want to try to answer it.

MR. SIESS: I am trying to make decisions.

MR. OKRENT: If I pursue the question and think about the results of the reviews of PRAs dealing with internal initiators, one tends to find considerable disparity from plant to plant as to which are the important internal initiators, when you look in detail so that it may be this particular set of transients or that particular system that is contributing to the dominant risk and so forth -- so it could again be that it is a one in six situation and if you would say it is only one in six, if we don't have to look at it, we could probably get rid of a lot of things you worry about internally.

Do I make my point?

MR. JACKSON: Yes. Okay, I think if it is all right we will proceed with Vince Noonan's presentation on the seismic qualification utility group.

Is this an appropriate time?

MR. OKRENT: I don't know if it is appropriate, but I understand he has an early plane.

MR. JACKSON: We don't have Mr. Devine here nor Mr. Allgermissen.

MR. OKRENT: All right, go ahead.

MR. NOONAN: Good morning, gentlemen.

I am Vince Noonan from the Staff. After listening to the previous discussion, it seems like I am a little bit out of place in this particular agenda because I am not here to talk about quantifying seismic margins. I am going to talk about what we have done using occurrence data to maybe demonstrate that we don't need to go back to the operating plants and require a lot of new seismic testing to bring the plants up to current criteria.

I would like to maybe address one subject. Before I got on here we were talking about what is more important, whether station blackout is more important. One thing I didn't hear said with this thing is when we talk about this kind of thing, we have done a lot of work in seismic and I don't think we are necessarily going to backtrack. I think one thing the Staff is really saying, as long as we keep doing the kinds of things we are doing in designing plants and piping and equipment, then we don't need to make it unresolved safety. If we back off of that position, then it becomes important.

Some of the things I want to talk about here embrace that point of view.

In this discussion, I will be talking about the feasibility of using experience data from preliminary conclusions based strictly on personal observations from the Senior Seismic Review and Advisory Panel, which I will refer to as the SSRAP panel, some restrictions that we can see being applied, a very brief discussion on the seismic input from extended scope NRC actions and impacted schedules.

There was a pilot study started maybe a year or a year and a half ago by the Seismic Qualification Utilities Group, which we refer to as the SQUG group. That utility group went out and started looking at experience data particularly in California based upon California earthquakes and looking at the types of equipment used in power plants, not necessarily nuclear power plants, just power plants in general and seeing what the effects have been on equipment that have gone through these kinds of earthquakes.

As that utility group and the NRC Staff started working this problem, It became obvious we needed a group of experts. We refer to that group as the Senior Seismic Review Advisory Panel. The panel was endorsed by NRC. It was picked by both the utility group and the NRC.

Part of this program is the active participation with the NRC and the utility group in monthly meetings to go through issues involving similarity and the seismic input and asking questions that normally the kinds of questions the Staff would ask in any kind of review.

The SSRAP Panel, headed by -- chaired by Dr. Kennedy and I will show a viewgraph listing all of the other participants basically is there as an independent panel. They are there to both give their recommendations and conclusions to the utility group and the NRC people.

Concerns raised by the NRC in these meetings have been addressed by the SSRAP Panel and on the 15th of this month in Bethesda we will have a meeting where the SSRAP Panel will sort of brief the utility group and the Staff on their conclusions.

The panel that we talk about is made up by this group by people. As I said, Dr. Kennedy is Chairman of the group. Dr. von Riesemann from Sandia, Mr. Wyllie from -- actually I think he is from Dagenkolb & Associates in California -- Dr. Schife from Purdue University and Dr. Ibenez, who is located in Los Angeles from ANCO Engineers.

This is a brief slide. Like I said, the SSRAP Panel will be giving their conclusions to both the Staff and the utility groups.Just based upon the meetings we have had to date, it seems that the seven classes of equipment listed there are mainly the classes of equipment we are talking about at this point in time.

We limited it to seven classes for one major reason. We were not sure that this approach was going to work and whether the NRC Staff would even go along with this type of approach, so we limited it, like I said, to those seven classes of equipment.

The Panel will give their recommendations and conclusions and brief us on what their final report will say. We expect that report toward the end of February. Those seven are motor control center, low voltage switch gear, metal clad switch gear, motor operated valves, air operated valves, vertical pumps, horizontal pumps.

I say it appears stable because of the discussions we had have in these meetings. I am sure there will be caveats and restrictions.

I have a slide here that will list a few restrictions we sort of see coming along.

The next step, and I will talk more about this later, is to start looking at other classes of equipment.

I am going to list a slide here that I call restrictions. What effectively has happened over the past eight years, at least from the Staff's point of view, looking at an approach of using experience data to say that we don't have a major serious problem in power plants, I would say a year ago I was very pessimistic that this would ever work with the Staff. I did not see this working.

Over the year and much to the credit of the utility group and their consultants, I have seen that position change considerably and the Staff -- one of my very conservative Staff members has told me recently, he said to me the other day basically that he is now willing to accept this approach where a year ago he would not even think about it. Part of the things we are seeing in this kind of going through and looking at experience, we see certain things you could say maybe are common sense things, but you have to make sure the equipment is properly anchored.

Every case we saw where equipment was moved or fell over, it was because it wasn't tied down properly. The

Staff in the last few months, also in looking at available test data and what we see here when we see test failures reported to us and shake table testing it almost always -- 98 percent of the time -- is associated with anchorage, either adding proper fasteners to the floor or beefing up the cabinets structurally to take the load's relays.

We will not accept relays on data. There are too many problems. We find operability problems in electrical equipment is almost always associated with relays. If relays is shattered, equipment trips off line. In some cases, that is probably no problem. In some cases we are not so sure.

What we will probably do is have a separate test of relays for all electrical equipment in the power plants.

One organization I know about, Westinghouse, has undertaken this. About five years ago they had a pretty expensive relay test program. They did change a lot of relays in their equipment so effectively at least from five years ago, the equipment was upgraded to remove a lot of the so-called "bad actors."

When we try to extrapolate this data to valves, we will probably use experience data based on eccentricity and pipe diameters. That is just an observation at this point in time.

Also the last bullet, we talk about vertical pumps restricted by pump height and lateral restraint. This is more or less a similarity argument. It is an argument to extract this kind of information from the experience data base and apply the equipment you have in your present plants today. They will probably use those types of restrictions.

I listed four. These are the four I can see coming out of this. Again, I must say we will know more toward the end of the month as to what the recommendation of the SSRAP Panel is regarding these kinds of restrictions.

Briefly, on the seismic imput, what has been done for the work here is basically that the data base plants are based on an average of two horizontal spectra and the seismic input for data base plants is to be applicable to equipment supported at elevations no higher than 40 feet.

I think that is the kind of thing coming on there although there can be additional caveats put on this thing. That is how the data began, from a seismic inputs point of view.

MR. OKRENT: Excuse me, is the seismic input something that will have been measured -- an input, an estimated input?

MR. NOONAN: In most cases, it is free field input that is measured. I believe that is correct, isn't it, Peter?

MR. YANEV:In a number of cases that is measured input at the site in the immediate vicinity and in a number of cases from records within let's say a 10-mile radius of the site.

MR. OKRENT: And is part of this then a translation of whatever information you have outside to a calculation of what is going on in the plant or what?

MR. NOONAN: You used the word calculation. We used calculations available today, those type of things. Is that what you are saying?

MR. OKRENT: I don't know that calculations were necessarily made of the type you are interested in and in any event, they may not be applicable for the particular earthquake that you now have data for.

It seems to me that we have enough experience in finding anomalies in data.When you get many measurements for a single earthquake, I will put "anomalies" in quotes here, people find reasons , qualitative or quantitative, for why something is high or low but I am trying to understand in effect by this question it is representative of a family of questions how you expect to exert what I will call a statistical control on the meaningfulness of the conclusions, how you will measure the uncertainties in the conclusion with regard to how well you knew at least the input to the equipment for the earthquake experience.

MR. NOONAN: I will say this. We will probably not do what you are suggesting. We will not relate it to uncertainties. We will not do a statistical -- we will use a statistical basis to do this kind of thing.

MR. OKRENT: How well will you even know what the shaking was on the particular equipment?

End 3.

MR. NOONAN: As far as actual values are concerned?

MR. OKRENT: Yes.

MR. NOONAN: We will not know. We will know where the plant is located and where the earthquake cinders are and based on that data we will make our judgements.

I will not be able to tell you though that in every case this particular equipment's are a certain peak value.

MR. OKRENT: And another question along that line: have you set up some kind of criteria that in order for equipment in an existing nuclear plant to be somehow qualified by by the shaking of a surrogate in a nonnuclear plant, how many successful shakings must have occurred with what kind of confidence that you have met or exceeded the spectrum?

I am trying to see whether you have any measure. I know that weeks have been spent at Diablo Canyon recently going in detail as to just how adequate is a sampling program and so forth and does it take a hearing in order to get the Staff to look through the thing and come up with what I will call a developed logic?

Otherwise, is it just seat-of-the-pants engineering judgment?

MR. NOONAN: No. I don't think so. We will be looking at at least what has been proposed so far today, is that we have looked at a number of earthquakes that have occurred in California, also the Alaskan earthquake. The seven classes of equipment I talked about there. We looked at those kinds of equipment that appeared in these areas where these earthquakes occurred and we looked at a number of plants.

In some plants the damage to the buildings was extensive. We saw a lot of cracking occurring in the foundation and so forth, but the equipment wasn't damaged.

In some cases the ceilings fell in on the equipment and it stood there. It was damaged by the falling ceiling but the equipment didn't suffer any damage.

The statistical value of this is you look at a motor control center -- motor control centers don't vary that much from plant to plant. You look at them and look at them but you don't see much difference, so you use that kind of statistical basis, given this is in a like area, this is in a strong area, the most thing we saw, most everything we saw was this thing slid across the floor or fell.

MR. SIESS: The motor control centers don't vary that much. It seems to me that a fair number of motor control centers have been testing on these shake tables --

MR. NOONAN: That's right. There is that data available too.

MR. SIESS: The same question Dr. Okrent raised about how much does it take to know that something is qualified, it seems to me would apply even to shake table tests.

Do you want to shake down 100 and see if 99 pass, or

I was going to remind him that his questions apply equally to what we have been accepting as shake table qualifications.

MR. OKRENT: I agree but in most cases you shake one kind of equipment. We seldom go in and test more than one in most cases. We test one piece of equipment and you say that is good for everything given that you have taken a conservative spectrum.

MR. SIESS: That answers his question, then: one success is extrapolated to some level of confidence except that you don't express it in level of confidence because this is current day licensing and it is not probabilistic, right?

MR. NOONAN: That's right.

I will talk about it a little later on, but we we be talking about test data to see if we can suppliment this prior to our final report on the A-46 issue, which I expect to be done around this time next year, but I want to reiterate there will not be a statistical number put on this.

MR. OKRENT: I really didn't expect you to come up with enough good data that you would be able to develop the statistics that would satisfy Dr. Easterling, for example, but I was wondering whether you were thinking about this issue in the sense of how much is the minimum I need to be satisfied and how can I quantify that and I am not sure that that kind of thinking has been written down and if it has, I would like to see it.

MR. NOONAN: We are talking about how much we need and what is the minimum number of things we are looking at. I am not ready to articulate that because we are still discussing it and we have a lot of different viewpoints.

I might add though you talked about the amount of data. Dr. Easterling has a lot of data, reams of it.

MR. KENNEDY: Dave, basically there are over 2000 pieces of data that have been well-documented for these seven classes of equipment in earthquake ground motions that range from certainly between .3g and .6g, at least the SSRAP is comfortable with the estimates of the ground motion team between .3 and .6g.

We have at all of the buildings --

MR. OKRENT: Excuse me, is that a peak acceleration or an effective acceleration?

MR. KENNEDY: The average of the two horizontal components. It is the average of the two horizontal components' instrumental --

MR. OKRENT: Instrumental?

MR. KENNEDY: Yes, but average of the two horizontal and these buildings that this equipment was sited in, at least the vast majority of the buildings the equipment was sited in, and the low portions of those buildings, which is where most of this equipment was located, it is estimated -- now there is no calculation behind this estimate -- but it is estimated that the motion that the equipment saw ranged from 100 to 150 percent of the free field motion. Probably in most cases pretty close to the free field motion there was very little amplification up to those buildings.

The SSRAP conclusions are going to be that because of out of over 2000 pieces of equipment with only one failure and the failure was due to impacting of a valve into a column because of too flexible a pipe, the conclusions basically are that we think that is sufficient to demonstrate a reasonably high confidence of seismic ruggedness in the .2 to .3g range.

We are very uncomfortable about extrapolating those conclusions beyond about .3g, but we think there is a pretty substantial data base with reasonable estimates of what the free field ground motion and the data is well documented by the Seismic Qualification Utility Group.

MR. OKRENT: And again this is a .3g peak instrumental?

MR. KENNEDY: (He shrugs.)

In every case we have look, you like to use ground acceleration because it is an easy single number to describe. In every case we have actually looked at the free field response spectra and we are really doing comparisons in terms of free field response spectra versus response spectra at the nuclear plants, so in all cases it is the acceleration from which the spectra is anchored and it is hard to discuss whether that is instrumental effective, et cetera.

MR. OKRENT: Okay.

MR. NOONAN: I might carry on here. The extended scope of this effort will be for other classes of equipment we have not conside d to date.

We will probably have a joint effort between the utility group and the NRC to define what of all of the classes of equipment left out there some people have judged there might be another grouping of as many as 40 to 50 classes -- we are not sure yet of those.

We feel that we did not necessarily need to look at them all and the approach, I will say, the way to approach this coming out of here, we'll probably look at critical equipment to be defined by systems and the systems really needed to, number one, trip the reactor and the systems needed to bring it into hot standby.

One other thing I have not put up there but is under discussion, we will probably insist that we have lost all offsite power to the station and we will proceed from there to define other classes of equipment and see if we can also come to the same kind of conclusion we have done for the seven classes we have looked at to date.

In this effort, we will be talking to the SSRP people to try to get them to help us in this area. We haven't done that yet but that is being proposed.

MR. OKRENT: If I could ask Dr. Kennedy a question, I think you gave a number like 7000 components, do I remember correctly?

MR. KENNEDY: That had been reviewed.

MR. OKRENT: Yes.

MR. KENNEDY: Over 2000 --

MR. OKRENT: 2000, I beg your pardon.

MR. KENNEDY: -- that have had some detailed review. There were more than that, components, but that is the total number in the seven classes. There is not 2000 in any one class. That is the summation of all seven classes. Also --

MR. OKRENT: I see, and I think you said there were no failures except one.

MR. KENNEDY: There was one failure.

MR. OKRENT: But you said, if I understood correctly, it was due to --

MR. KENNEDY: Impacts between the valve housing and the valve yoke in a structural column of the building because of insufficient clearances.

MR. OKRENT: Okay, fine. I guess what I am wondering is what is the experience in testing similar components if they are tested when you put them on the shake table?

Is the record that good?

MR. KENNEDY: I can't speak for the entire SSRAP on that subject. In my opinion, much of the equipment on the shake tables overtest the equipment, you test multiple times for longer durations. You get an overtesting in that you get too high accelerations. The testing techniques tend to over-test the performance and my judgment has not been -- you do find some failures of these classes of equipment when tested on shake tables, not very many but you do find some.

MR. OKRENT: It would be interesting to understand this difference, I think.

MR. KENNEDY: There have been a number of studies on that difference. Dan Kania of Southwest Research has done research on just that subject for the NRC.

MR. NOONAN: May I also comment on that? I agree with Dr. Kennedy on that issue, that you overtest on the subject, but assuming the NRC mail system is working, there was a letter that left yesterday to EPRI asking them to assist us in looking at testing or maybe even testing some pieces of equipment to help us in this area.

MR. OKRENT: From what I have heard, I must confess it is not completely clear that they are being overtested at the table in the following sense.

I was quoted numbers like .3 to .6g free field, which is usually much larger than the free field you postulate before you go into a shake table test, so in that respect, you were citing earthquakes that presumably were stronger than are used as the basis for the shake table.

Now in the shake table they may, I don't know, use multiple tests. I must confess, I am not put to great ease by the knowledge that multiple tests lead to failure when you are not talking about hundreds of thousands or thousands of tests.

MR. KENNEDY: Most of the failures on shake table tests of the seven classes of equipment of which I am aware anyway occurred for input motions that had zero period accelerations in the 3g to 10g range. It is not likely, at least in my opinion, for a .3g ground motion to result in a 3g to 10g input to this equipment.

In every failure I have seen of this equipment has had ZPAs in that range.

MR. NOONAN: May I make one other point based upon observation?

A test that I witnessed and I have witnessed a fair number, usually when they set up their shake tables they use the actual piece of equipment on the table to get the proper feedback characteristics in the table. Sometimes at what they call half level, they hit it maybe 50 times for 10 second intervals, shaking the spectrums. When they bring it up to the full level -- they want to get it at 15 to 20 seconds at full level -- it could very well be a minute by the time they get it to the proper level and the test engineer is satisfied. They run it and set it down.

So from that standpoint you do quite a bit of testing, particularly in the setup of the tests to the point when I personally get concerned about fatigue of the equipment, particularly anchorage, when you hit it and hit it and hit it repeatedly, I worry about that.

The NRC's basic action is to complete the so-called A-46 issue, which is the nomenclature we give it at the NRC and the implementation schedule is becoming a little hard to figure out here and the SQUG group, so we have about 17 utilities, we are not sure now to handle the other utilities.

There has been talk we might send out 50.54(f) letters to the other utilities asking them for the same kind of report and documentation we have received from the SQUG group.

The schedule I mentioned before, we will have a briefing of preliminary findings of the SSRAP people to brief the NRC and other utilities on the 15th. That is scheduled for Bethesda.

We are trying to get a preliminary resolution of the A-46 document by the Spring of 1984, resolution through public comments and the CRGR review by the end of '84 and at that point in time, we will then implement through individual plant schedules through the Equipment Qualifications Branch, Division of Licensing.

MR. SIESS: When you finish up A-46, presumably you will have or the Commission will have some assurance that the components, equipment in existing plants meet current seismic criteria. It won't provide you any particular assurance that there are margins beyond those current criteria?

MR. NOONAN: That is correct, sir.

MR. SIESS: So all of this effort is an unresolved safety issue, is getting us up to the current seismic design levels?

MR. NOONAN: That's right.

MR. SIESS: It doesn't tell us anything about what reserve we might have beyond that?

MR. NOONAN: That is correct, sir. One thing we will ask in addition to that probably is, I limit this strictly to the operating reactors, the things left to be resolved, and we will try to answer in the A-46 is the replacement of equipment replacement parts in the operating plants, how that is handled and also on plants and licensing, I do not propose at this time to extend this kind of effort into new plant. That is not being proposed by the Staff and I guess, gentlemen, that is really the end of my presentation.

MR. OKRENT: What does that last statement mean?

MR. NOONAN: I don't think we want to take the experience data into the new plants. I kind of refer to my original statement.

We have done a lot of work in the seismic area.

There are things, requirements, that have been sent out for new plants to sort of upgrade them, get them to the proper perspective. We are looking at the operating plants. It is is very difficult to go into a plant and take out equipment and replace it. Sooner or later they will, at least on a sub-component level. That equipment we would hope would meet our latest 19 -- what? 344 -- 1975 versions.

MR. OKRENT: Is there a part of your program that systematically identifies differences that may and do exist between nuclear power plants and the plants you are referring to here or the equipment you are referring to here so that you know just which components and possibly sections you have no direct experimental information for?

End 4.

MR. NOONAN: If I interpret you correctly, you are talking about the similarity elements. I have a component in a nuclear plant, how do I show it's similar to --

MR. OKRENT: There may be some for which there isn't a similarity. There is a separate question as to how far similarity extends in a thing like this but I have to assume that there are aspects of nuclear power plants involving components that are different.

MR. NOONAN: They are different and I can answer that, that we have talked about that. The resolution is not clear but we have to look at it on a case by case basis, I guess.

MR. OKRENT: For example, there are containment requirements that may introduce equipment you wouldn't ordinarily find.

MR. NOONAN: That is why we are trying to use EPRI and the data banks we have. We have two data banks really in this country that I know about. One is at EPRI and NUS data bank in Florida. The other one is the NRC data bank that we set up in Idaho. That data bank is -- we are trying to put all of the test reports we can lay our hands on. We are sending out letters to utilities asking them to cooperate in this effort and give us the kinds of test information where they have put it, so --

MR. OKRENT: You are asking a different question.

I am trying to find out whether there is some kind of systematic way of going through the thing to see what is covered and what is not covered and maybe the answer is no.

MR. NOONAN: I guess the answer at this point in time is that it is being talked about. I don't have a good answer for you. We are struggling with that question. I really don't have a good answer.

MR. OKRENT: Dr. Pomeroy?

MR. POMEROY: Perhaps I did not understand quite correctly but could you clarify for me the data base plants for which you have experience and the seismic inputs are derived for the Western United States and Alaska?

Is that essentially correct?

MR. NOONAN: Yes, sir, they are nuclear plants; they are nonnuclear plants.

MR. POMEROY: Then I wonder whether SSRAP or the NRC has looked at the question of the possible differences in the spectra arising from the Eastern United States events relative to Western United States events?

MR. NOONAN: This is being looked at, yes, sir, but I can't speak for the SSRAP.

MR. KENNEDY: The really well-documented data is basically from San Fernando, the Imperial Valley earthquake and the Coalinga earthquake.

We have spectra data provided to us close to these plants that are in the data base.For each of those three earthquakes any conclusions that SSRAP will reach will be anchored to those spectra -- i.e., if a nuclear power plant site has spectra that exceed those spectra the frequency range of interest, our conclusions are not valid and our report is going to be very clear on that subject.

MR. POMEROY: Thank you.

MR. OKRENT: Dr. Ang?

MR. ANG: I have a quesiton I would like to address to Dr. Kennedy.

In light of the work that the SSRAP is doing, do you see any developments on fragility clearance? If so, how might it affect --

MR. KENNEDY: Do you mean for PRAs?

MR. ANG: Yes.

MR. KENNEDY: In looking at the experience data provided to the utilities by SSRAP, the experience data is very confirmatory in my judgment of fragility curves that have generally existed prior to seeing that data; i.e., they show that below ground motions in the .3 to .6g range, at least for these items of quipment, the probabilties of failure of seismic induced failures are very low.

MR. ANG: To continue --

MR. KENNEDY: But they don't tell the rest of the fragility curve.

curve.

MR. ANG: Right. I was going to ask you whether you have any confidence in the level that leads to failure of the equipment that is beyond the design level?

MR. KENNEDY: The only data that I am aware of for fragility curves at higher levels and ground motions in the .3 to .6g range is fragility data generated by the Corps of Engineers in the Safeguards Program for Nonseismically Qualified (off the shelf) Equipment. Now that data does show ruggedness to higher levels.

There is some data that shows ruggedness to higher levels because the equipment in some cases has been overtested in qualification tests and did not fail so there is a little bit of data at higher levels but the data is sparse once you get beyond ground motions in the .3 to .6g range.

MR. OKRENT: Dr. Hall?

MR. HALL: Bill Hall from the University of Illinois.

Let me try another one. It doesn't have to do with the shaking and maybe there are some people in the audience here who are electrical engineers or geophysicists?

Peter, maybe you can shed some light on this and Vince, see what you would say about this.

As one who was raised on the West Coast and remembers dozens of earthquakes and the transients that goes with them who lives in the Midwest now, where there are lots of tornadoes with tremendous electrical discharges associated who works heavily in the missiles field where missiles go through various charged layers and ground is relative, and in another sense, working around people who work on new versions of airplanes where they worry tremendously about what is ground on our modern airplanes with regard to the computer systems and so forth and thinking back to a study Clarance Allen and I were involved in some years ago in the Academy involving earthquake prediction where we came upon this question of well, an earthquake, there have been reported tremendous changes in background, electrical state, I will call it, what is the feeling, what is know about this in the sense of near field earthquakes with regard to electrical systems?

This was been on my mind for some time. Do we know much about this? Is it something that can be ignored? Is it a real problem? How does it affect modern solid state circuitry systems? I would like to hear a little more on this.

MR. NOONAN: I am not the one to address that particular question.

MR. HALL: I know that, Vince, I am just stating the question.

MR. NOONAN: Someone else may be able to --

(No response.)

MR. OKRENT: Do we have a volunteer? I see a hand.

MR. YANEV: I am not answering the question.

MR. OKRENT: Please identify yourself.

MR. YANEV: I am Peter Yanev, with the consulting firm that did a lot of work for the SQUG group. I am certainly not qualified to discuss electrical discharges in earthquakes. I have read report of the Engineering Research Institute that alludes to such effects and there have been a few studies in fact done on that.

What we have tried to do in collecting the data is to document what happened to the power plants themselves, how they performed, given whatever physical phenomena were going on around in addition to the actual ground shaking and the response of the structures and equipment, so in all cases we tried to ascertain how the equipment or the systems or the whole power plant performed.

Now to the best of my knowledge of all of the records of the power plants we have reviewed, there is certainly no reference to such an effect causing a preturbance to operability. There is no mention of that sort of an effect being connected with whatever was happening at the power plant.

One of the advantages we have had with some of the more sophisticated utility systems is they maintain rather retailed log books on a minute by minute basis when there is some sort of occurrence that is unusual.

We went back to those records to see how the systems did and this is what I am basing my comments on.

MR. OKRENT: Dr. Bush?

MR. BUSH: This would simply support one of the restrictions established with regard to equipment on another committee. Out of the Pressure Vessel Research Committee, we were reviewing the business of piping and supports and also equipment supports and our conclusion there is indeed we should go back to Section 3 of the Code and specifically request a beefing up of the supports on equipment and at the same time a rèduction in strength, a deliberate weakening of the supports on the piping with the idea that under no circumstances do we want overturning moments controlling on the equipment and we would a lot rather have these supports on the piping fail rather than all the piping fail the piping.

I think in general this goes along with the restrictions established by SSRAP.

MR. NOONAN: I have nothing further.

MR. OKRENT: Okay, thank you.

Have you any further discussion on this point at this time?

MR. KENNEDY: I do not want to waste your time but if you want I can give you some of the concerns the SSRAP has and some of the strong recommendations of the SSRAP which will be given on next Thursday.

One is the SSRAP is certainly concerned that to use experience data base in a nuclear plant, we are going to strongly recommend a detailed and thorough walk-through has to be performed in the nuclear plant.

A bery thorough review of anchorage is going to have to be performed on this equipment in the nuclear plant. Both from a strength and a stiffness standpoint, we are concerned about just using screws through the base of sheet metal, you know, turned over sheet metal at the base of cabinets and calling that anchorage.

We are concerned about the problem of impact loads from nonsafety-related equipment falling on these seven classes of safety-related equipment and we are concerned about the lack of sufficient clearance such that valves might lay into civil structures and each of these items are going to be clearly culled out as areas that the SSRAP feels need to be evaluated by a walk-througg at a nuclear plant before using this experience data base.

I guess the other thing is the SSRAP has not felt that the experience data base is adequately documented to demonstrate proper operation of relays during the earthquake.

There does appear to be substantial data documented that they perform fine structurally, that they will operate fine after the earthquake.

End 5.

Those, I guess, are some amplifications of some of the reservations and concerns we have with this experience data base.

MR. OKRENT: Dr. Bush?

MR. BUSH: May I ask a question?

I think the remarks all dealth with the structural reliability. I presume that the same applied to functional reliability in all instances?

MR. KENNEDY: The observations from the data base seem to indicate that if the seven classes of equipment again, I guess that should be the restriction, our conclusions are only for the seven classes of equipment. For these seven classes of equipment it appears that if they structurally hold together, if anchorage or impacts are prevented other than for relay chatter, for operational or functional problems on relay chatter, there is no evidence in the data base of any functionality problems.

This equipment appears to be rugged enough that it will function properly other than for potential relay chatter in these ground-shaking environments.

MR. OKRENT: I wonder, is there experience in Japan with failure of any of this equipment in your seven classes due to earthquakes?

MR. KENNEDY: SSRAP asked the utilities to try to give us some better documentation of foreign earthquake experience. To date, the SSRAP is totally dissatisfied with the documentation that has been provided to us on foreign earthquakes.

In our meeting next Thursday, that is going to be brought up, something that our conclusions is still conditional on more data being provided on foreign earthquakes.

I can't answer that; possibly Peter can, but we are not satisfied with the data we have received on foreign earthquakes so I don't know whether there is or is not in Japan.

MR. YANEV: Perhaps I can amplify this further. About three weeks ago I had a meeting with Dr. Hiki Shibata in Tokyo to at least represent the group and get an idea as to whether or not we should get any surprizes if we went to Japan and looked at the data.

We have had teams in Japan look at some of the data especially with the Miyagi-Ken-Oki earthquake in 1978. We spent together with one of the members of the SSRAP a few hours with Dr. Schiefe with Dr. Heki Shibata and went over the data he had, which is rather an extensive publication, his own chronography and the chronography of others from those earthquakes.

We saw no instance of failed equipment that we could not explain from failure of anchorage or interaction such as an impact of a collapsing structure on a piece of equipment.

So at least we walked out with a warm feeling without certainly done any detailed survey of the equipment.

MR. OKRENT: Well, that is interesting, of course it is a little bit hard to tell sometimes if there were two causes of failure present at the same time and if you see one and assume that is it and there was no other it is sort of a hard question to answer, unless someone has looked the way a coroner sometimes has to.

MR. YANEV: We were concerned about that same issue quite a bit. In trying to ascertain, wanting to make sure there would be no surprizes in the future because we hadn't looked at everytthing, we conducted a poll of the most prominent engineers in the United States, Americans who have surveyed foreign earthquakes and United States earthquakes and we tried, we conducted a poll.

We conducted on the order of 30 telephone conversations with individuals trice to find out about such failures and what we asked especially was, if you were walking into this plant or into this facility, a highrise that has similar equipment, would you have seen collapsed equipment not adequately anchored?

Obviously they did. Obviously they reported extensively on those.

We also asked repeatedly if there was a massive failure internally of the equipment, is it likely that you would have known about it? The answer repeatedly was yes, we would have been told about it. Yes, we would have noticed had there been major failures internally of the equipment. This would have to be more obvious. I wouldn't expect someone to see the binding of the bearings of a pump but especially for the electrical equipment where you would expect to see a jumpled mass of wires and cases of plastic inside, it simply didn't happen, so again we tried to build up our confidence that there were no exceptions we were aware of in that fashion.

What we did find out that was rather interesting is that all of the individuals were repeatedly reporting failures of equipment due to anchorage to such an extent that all of us after looking at the literature simply wound up with the impression that the equipment is very weak. It has been an impression that has been totally wrong once you start looking at the details of the reported failures and trying to explain away what caused the failures.

MR. OKRENT: Well, just to give an example, maybe what I was alluding to before, you might have a major failure of anchorage but there might be an internal failure of anchorage that is now disguised, okay?

You don't know about it but it is equally --well, there, so I hope you are getting the idea that we are in favor of your --

MR. HALL: Dave?

MR. OKRENT: -- trying to extract as much as you can from experience with nonnuclear systems, but one needs to look at this information with a rather demanding point of view and not hoping, as it were, that this is like that and so forth, to make sure that indeed you have only covered part of the story.

MR. YANEV: For the several thousand pieces of equipment, this is indeed exactly what we tried to do.

MR. NOONAN: Dr. Okrent, may I speak to that point?

In A-46, we talk about anchorage. We will not limit our conversations or writings to only the mountings of the cabinets. We will talk about anchorage to make sure that circuit boards cannot slip out of their mountings. We will probably also talk about anchorage as having components inside cabinets even down to small components.

MR. OKRENT: I suspect anchorage has been identified well enough that it will, except in very subtle cases, be looked at but I am using it as an example.

MR. HALL: Let me elaborate on what you are saying, Dave.

MR. OKRENT: Go ahead, Paul and maybe Peter can speak to this again.

MR. HALL: This is first-hand experience. At the University of Illinois we are not a commercial testing lab but we do have shake tables and in two cases I know of, a large corporation which shall go unnamed has come over to us and had us test relays and relay time equipment, we don't do much of this, and go away with the data and we have asked some questions about why are you doing this and it turns out that these pieces of equipment, Peter, are in motor control units among other things and the statement that was given was something like this: well, our worry is that we don't know what the vendor that put this system together is going to do wit our small unit and we want to at least satisfy ourselves as much as we can that the design of our unit to the best of our ability is good enough from a liability point of view -- that is, something happens to it and it causes the total system to have troubles at some time that we are at least partially protected (end of statement).

Some of us have written papers on this sort of thing and I am encouraged to hear from you that you don't see evidence of this very much, I mean that is what I am hearing from you, to the degree of things as you look at it, right?

MR. YANEV: That is certainly the impression we have from the data we have collected. Part of the strength of the data is we certainly expect in a non-nuclear facility the quality assurance requirements to be much lower and we have allowed the types of construction errors, the types of maintenance changes that you see in conventional facilities affect the quality of some of this equipment so undoubtedly much of the equipment had partial anchorage missing. I am talking about the components within the panels.

Now we didn't go to a survey to make sure that is the case, but given the large number of pieces of equipment -- they are on the order of 3000, in fact -- we probably looked on the order of 10,000 pieces of equipment but not carefully enough but we have an idea of how the test was performed and very much the impression is that the safety factors are large and they certainly cover the acceleration levels we are concerned with.

MR. HALL: The only other point I would make, we have been involved in this rather heavily in some limited tests on our test tables with computer equipment in the last 10 years, system-wide as much as possible in which the computer equipment has been energized to going up steady state, going down unenergized with tremendous error programs involved, trying to look at these systems in pretty severe shaking situations.

When I come back to this relay businesss, I have some concerns in the sense that these were new relays. They were not relays that were worn and had been in service for 10 years. They were not energized. They were not not under this situation, so -- but I cannot believe that these types of people are not doing some of this type of examination too, but I have no evidence of this.

MR. YANEV: I would rather stay away from the computers and the relays since that is not a part of our data base. We do have literally thousands and thousands of relays.

One of the most surprizing aspects of the investigation was not to find broken relays.

Now we have data. We have indications that there were relay trips. We have not tried to determine what caused those trips, why they tripped, were they intentionally tripped, were they supposed to? We have not tried that out.

MR. OKRENT: I am going to call an end to this discussion.

We will use the 15-minutes later on, I am sure.

Let's take our break 15 minutes early and then continue.

End 6.

(Recess.)

MR. OKRENT: We will reconvene.

Gentlemen, will you please sit down and in any event don't talk on your way to your chairs.

We have roughly an hour on the agenda before we reach item B and I propose to use it for a discussion of what some people call seismic hazards and the uncertainties therein.

During the break, I have cornered a couple of people and in a moment I will corner one more and ask them if they would give five minute leadoff comments on how they view this aspect of the problem, how well do we know the seismic hazard? Where are the big holes, et. cetera, et. cetera.

Robin McGuire is going to lead off, after which we will hear from Paul Pomeroy and then I will ask the Staff to choose a presentor, so Robin, then we will have open discussion, so please start and by the way, if you can't hear in the back raise your hands or wave them so that the speaker knows or at least I know and can tell him.

MR. MC GUIRE: Let me start out by showing this slide which many of you have seen many times, which illustrates the three steps comprizing a seismic hazard analysis.

At the top left, we have to devide the areas concerned into so-called seismic sources. At the top right for each source, we describe the frequency of occurrence of earthquakes of different sizes with a probability distribution.

At the bottom right we have an attenuation function, which allows us to estimate ground motion as a function of the earthquake's size and the distance from that earthquake to our site. We have some dispersion in that function.

And then at the bottom left, after we integrate over all of the uncertainties in source zones in occurrences of earthquakes and the ground motion distribution, the arrow saying "by integration" represents an application of the total probability theorum.

What we wind up with is a description of the frequency of occurrence versus some measure of the ground motion.

Now the steps involved in that process and that step of integration doing the total probability theorum is now well established in the literature. We have done these things for over a decade now. We have experience in applying them at nuclear plants on the order of a dozen or 15 sites and I think most of the practitioners in the field agree that the methodology is a valid one and a strong one and the remaining issues are ones of uncertainty in how we apply each of those steps and I will give just a couple of brief examples.

This is taken from an application in New England. It represents one interpretation of tectonics in New England, a rather broad one as you can see.

It says basically you don't know what is causing earthquakes and we can assume they will occur with equal likelihood anyplace in the future.

A separate interpretation is one that says we are very sure about what causes earthquakes and we are confident that where they have occurred and have not occurred in the past is an indication of where they will occur, respectively, in the future, so we can draw clearer zones representing the seismic hazard of seismic occurrence.

(Slide.)

Illustrating uncertainty in ground motion is this slide showing several, five in fact, available functions which allows us to estimate ground motion as a magnitude of distance. As you can see at a given distance for a given magnitude, the uncertainty in the mean estimate in ground motion for Eastern U.S. earthquakes might by on the order of a factor of two or three total range.

Now the way we handle these uncertainties is to produce the seismic hazard results by applying the integration process and the total probability theorum for all possible combinations of all of those variables. That can be uncertainty in Zone A, uncertainty in activity rates in the slope. In the upper bound, all three of those, the last three, are parts of the uncertainty in the frequency of occurrence of earthquakes, uncertainty in attenuation laws and perhaps uncertainty in the errors associated with those attenuation laws.

That leads us to many, many sets of hypotheses, in fact all possible combinations giving us many, many sets of hazard curves.

Now we produced those and illustrate those by reducing them, perhaps representing them with fractile hazard curves, that is for given peak ground acceleration we can give you the median hazard curve and a fractile which you desire.

I will get now to the point of my making comments on my experience and observations and having applied these analyses in a dozen or 15 plants.

That is, first, no one of those sets of uncertainties governs the uncertainties in the results we have. It seems that not one is governing but all are contributing.In terms of the uncertainty in these results, if we produce maybe 100 hazard curves the total range is something like a factor of 100 in probability of excedence, that is for a given ground accelaration from one extreme to another out of 100 curves might be a factor of 100 in the probability of excedence

To give more quantitative numbers, they range from the 16th to the 84th fractile might be something like plus or minus a factor of three from the median.

If our purpose is to pick a target, a risk level, an acceptable risk level then the uncertainty shown by the top left distribution there is the appropriate one to look at and we should be looking at uncertainty in peak ground accelerations.

Given the target risk level, that uncertainty of course is much smaller. It is not represented by plus or minus a factor of three but maybe by plus or minus a factor of 50 percent perhaps.

As I say, no one of those uncertainties in the process governs that total uncertainty and we are in the mode now of looking at the uncertainties and trying to represent them, make sure that we represent them in an unbiased and documentable fashion so that researchers and reviewers can look at those and either ascribe to them or now and in the proces of doing this repeated times, we can learn from it and in our view produce better results the next time.

So, I think my perspective, number one, is that that process of producing an unbiased set of uncertainties for all of those input parameters is an important one that needs to be looked at very carefully and second, a second important point is we need to make sure that we have not made some pathological error in one of the assumptions which would bias our results one way or another.

There are several possible examples that come up. One is are Eastern U.S. earthquakes fundamentally different from California earthquakes in the ground motions they generate. If they are, this might produce a shift in this entire set of curves let's say five years from now when we have learned a lot more about those earthquakes from where those curves lie now.

Another example is, how do we interpret historical intensities? The largest, by far the largest amount of data in the Eastern U.S. is in the form of interpreted intensities of earthquakes, historical earthquakes. How do we translate those into a current quantification of earthquakes, for instance magnitude? If we are making an error in that translation, that again in the future might if we correct that error might produce an entire shift in those curves.

So, I think my point's conclusion would be (a) we need to document and consider very carefully the range of steps, the range of input parameters that are necessary for these curves and second, we need to go through that entire process very carefully to make sure we are not making some biasing error in one of the steps of the procedure.

MR. OKRENT: Thank you.

I think what I am going to do is have the three commentors and then we will have discussion.

MR. MC GUIRE: Sure.

MR. OKRENT: I will even hold my own questions.

MR. POMEROY: Since this is a somewhat spur of the moment presentation, I will use Robin's slides, since I didn't bring any.

I have just a few comments I would like to make somewhat provocatively in order to stimulate the discussion.

Robin has alluded to the fact that we do not know what the causative mechanisms of intra-plate earthquakes are in general and intra-plate earthquakes are those we are primarily dealing with in central and eastern part of the country so that in fact we may utilize a given hypothesis, you have to remember that that hypothesis is one of perhaps 10 hypotheses regarding the causative mechanisms and none of those 10 hypotheses may be correct.

In order to establish the seismic zonation indicated in Part A of this figure, that is in order to establish source zones, one either has to have some conception of the causative mechanism or one has to deal exclusively with historic seismicity in some manner.

I will touch on historic seismicity when we get to Item B here. I will just say in the few PRAs that have been generated to date, the Indian Point PRA and the Zion PRAs, reasonable source zone configurations have been selected but there are solely the result of expert judgment and a consideration of the seismicity goes into that expert judgment.

Nevertheless, the actual source zone configuration or lack thereof simply is an unknown factor at the present time in essence.

I would like to turn briefly to Item B here. This seems rather straightforward. This is simply a plot of the cumulative number or the number of events versus the size of an event. Now one would say that is a rather easy thing to plot for any given seismic zone but in fact that is not true. In fact, there is great deal of argument about the few events that in many cases control this slope (indicating) and those few events which are critical to controlling that slope indeed introduce a significant uncertainty into the later calculations, as Robin points out.

The uncertainties in the historic data base in my mind are significant enough so that you have some real question about this number. As Robin said also, our operating procedure has been simply to ask varying numbers of people what their opinions are with regard to these slopes given the historic data base and their interpretation of it and in general one gets a value or a grouping of values that doesn't vary too much from what most of us would say is a reasonable value.

Nevertheless, most of us have grown up in the same milieux and somehow we may have all talked to each other so often that even that may be significantly in error.

You also have to recognize that the historic data base for some of the source zones are the data base itself is very small and therefore you introduce an additional uncertainty in that slope value simply by that particular problem.

Robin has alluded to the problem of the attenuation and what we use for an attenuation, which is a critical input factor to this whole process. In the Lawrence Livermore studies that were done using an evaluation of expert opinion, they had to essentially establish a separate panel of experts simply to evaluate what function was the appropriate function to use. It is still not clear to many seismologists that those are the appropriate functions for the eastern and Central part of the United States.

Finally, as Robin said, I think the procedure here is fairly well documented in the literature and fairly accepted by most seismologists although the Academy of Sciences is currently talking about the initiation of a study -- incidentally the study will be undertaken at the request of the NRC to evaluate the use of PRA information in the evaluation of seismic hazards.

There is one other factor I have alluded to and that is in the eastern and central part of the country, depending upon your perception, we have at the most something like 400 years of historic data. Many seismologists in the country are very uncomfortable using that data base and in reality that is not a very complete data base except perhaps for the last 150 to 200 years. Anyway, using that data base in evaluating probabilities of excedence that are 10^{-5} and 10^{-6} the PRAs that I have reviewed, namely the Indian Point and Zion PRAs, have in the seismic inputs all of these assumptions made on the basis of either expert judgment, expert input of one or two people and they are contained in Step A and in Step B and in Step C.

Those judgments build upon themselves in a way that I question when you look at all of the probabilities as Robin showed in his next to the last slide and you evaluate all of those probabilities whether when you get done with this kind of an approach whether or not you have something that is meaningful.

I question its meaningfulness in terms of the decisionmaking process and we could discuss relative versus absolute steps.

I have one other comment. Many of these calculations that I have seen also involve the choice of an upper magnitude cutoff in terms of the maximum size of an event that is assumed to be capable of occurring in a given source zone.

There are various arguments about that subject but it is not at all clear that that parameter is even a real parameter that comes into these discussions. There obviously is some upper limit someplace in terms of the absolute capability of the rock to support a certain amount of stress.

Nevertheless, whether a given source region has a given upper magnitude cutoff does affect these final results also and that parameter is also not well established.

I think that PRAs are useful, extremely useful and I would like to see more of them done in this mode but I think the assumptions have to be clearly stated and if you once state the assumptions clearly the person who is evaluating these PRAs has to be extremely careful in taking those assumptions into consideration when looking at the final product.

MR. OKRENT: Okay, who is up for the Staff? Mr. Reiter?

MR. REITER: Just a few brief comments.

I wanted to mention again what Paul said. The NRC Staff has requested the Committee on Seismology of the National Academy of Science to take a look at probabilistic seismic hazards and as far as I understand the proceeding, I know Allen is one of the members who is supposed to get together with a group on that and I think we will hopefully get eventually some sort of statement, some sort of an evaluation of the use of probabilistic estimates of seismic hazard.

One of the reasons is there is a diverse range of opinion about the usefulness of such estimates. On the one hand you have people who have gone to court, to federal agencies saying probability is worthless. On the other hand, you have people writing regulations based upon something like 10^{-6} or 10^{-7}. NRC stands somewhere in between.

We think it would be worthwhile if the Committee with a group like the National Academy took a hopefully detached look into this to get some general insight into the process.

I want to mention a few things --

MR. SIESS: Leon, you say the NRC is somewhere in between. Is that on the average?

(Laughter.)

MR. REITER: Best estimate.

(Laughter.)

Now people on one side consider us extremists Depending upon which side you come from, we are having quarrels with two agencies at this point. One considers us extremists slaves of probability, and the other considers us absolute people who refuse to use it.

MR. THOMPSON: So we are probably just about right.

MR. REITER: Yes, it is always nice to be criticized from both sides. Tomorrow Jeff Kimball will make a presentation of some of the initial results of Livermore but Robin talked about the uncertainty and I want to show you some of the results we are coming up with These are very initial results. These are for one site. I don't reallly know which site it is.

This is an acceleration level (indicating) increasing this direction, this is 1 g and these are levels of annual chances of excedence. This is the 50th percentile (indicating) and these are 15th and 85th, 15th and 95th and at least according to this preliminary result you can see that there is a wide range of uncertainty, several orders of magnitude and what surprizes me is that this uncertainty is even rather large at some of the lower probabilities. Now when I think back about it, it is really not surprizing.

Recently I was comparing some of the estimates for sites. There are like three groups making estimates or have made estimates of probabilities -- the USGS, the Algermissen and Perkins group, had put together various maps not particularly of nuclear power plants but generally for building codes.

NRC through Lawrence Livermore has done some studies on the SEP program we are advancing now and the PRA studies done largely through the work of Robin McGuire is another source.

Eventually EPRI will also come up with some estimates.

We have noticed at very low return periods, sometimes 10^{-3}, the numbers coalesce and sometimes there is like an order of magnitude of difference. I don't know if that surprizing we should have this much uncertainty even at the lower probabilities but it is very large and the source of it comes, although we come from the same data, we all basically use the pioneering work done by Alan Cornell to arrive at the conclusions.

How do you put together, how do you treat your expert opinion varies greatly and what is your source of uncertainty and you can get different results.

End 7.

Bob mentioned I worked on Limerick. I was looking at Limerick and I did try to do some detective work and I found some interesting conclusions, one that was a little shocking to me.

Limerick had, if you looked at the numbers at Limerick, the seismic events supposedly contributed something like 13 percent to core damage frequency and something like 84 percent to early fatalities, these are utility studies -- these are not upgraded NRC studies, something like that.

It is interesting that the sequences that are dominant in core melt frequency are not the same sequences dominant in risk. One is essentially equipment failure but in the risk it turns out that most of the risk comes from failure of the reactor pressure vessel supports. What that has to do with seismic pressure -- I'm getting there -- the reactor vessel pressure supports has a seismicity of 1.2 g over 65 percent of the risk in these early fatalities, please excuse me if I don't use the right words -- man r-e-m comes in the range to 1 to 1 1/2 g and these numbers that are coming out from this, if I believe the numbers I have here, the early fatality risk at Limerick seems to be not too far off from the early fatality risk at Indian Point -- in the area of .2, so it not an outrageously small number and I have to ask myself, and that curve, that 1 to 1 1/2 g, comes from one curve in the seismic input, which I assume it is the so-called decollement model and I have to ask myself how valid? We have to make sure we take a step back and look at that before we draw conclusions on a sequence that assumes 1 to 1 1/2 g in southeastern Pennsylvania and this is causing a major contribution to early fatalities.

What that means is we talked about how various types of expert opinion are needed at various stages and we can't get away from these judgments but to me the most important expert opinion, the most important judgment applied is not during the individual stage but what you do with it after. If you lose sight of that engineering judgment or expert opinion and attempt to apply these numbers blindly, I think we are making a big mistake.

MR. OKRENT: I wonder if I could ask just a point in clarification.

Robin McGuire mentioned that something only had an uncertainty of 50 percent. What was that thing? I am still not clear.

MR. MC GUIRE: The point I was trying to make is based upon one study which we did, if you look at the uncertainty in frequency of excedence for a given ground acceleration from the median to the 16th and 84th percentile, that might be plus or minus a factor of three, an equivalent uncertainty in the ground acceleration for a target probability of excedence would not be plus or minus a factor of 3, it would be plus or minus 50 percent. That is the only point I was trying to make.

MR. OKRENT: And does that hold at 10^{-5} per year as well as 10^{-3} per year?

MR. MC GUIRE: Those are really ballpark numbers. The slope of these curves generally goes down when this is plotted on a log/log scale, so that would change as a function of the level, the frequency of excedence somewhat but my point was not to hone in on 50 percent exactly but to say it is not a factor of 3, I guess. The factor of 3 would be in the frequency of excedence but a factor of something like 50 percent would be in ground acceleration.

MR. OKRENT: Again, I am still trying to understand whether you feel that is, that uncertainty defined that way, is relatively independent of which level of annual probability of excedence you are choosing or it is quite different?

MR. MC GUIRE: That is a good ballpark rule of thumb for the range 10^{-3} to 10^{-5}. It changes somewhat in that range but it is a good ballpark.

MR. OKRENT: Okay, thank you.

The subject is open for comment or discussion.

Dr. Lewis?

MR. LEWIS: I just wanted to clarify in my own mind while you are standing there two things that you said. One is the term "uncertainty" has been used a great deal and

I am trying to understand the difference between uncertainty and dispersion which are, after all, two quite different things.

One can have, and I believe you showed a picture of New England with a lot of dots on there which I assumed were historic earthquakes, and that shows the dispersion of earthquakes in New England as an historical matter but the uncertainty in the future likelihood of an earthquake is not governed by that dispersion. That is, the dispersion in data may be due to real physical effects. There are faults in some places and not in others, although they may not be as relevant in the East as it is in the West.

The extent to which one distinguishes between uncertainty and dispersion is something on which I would like to become educated.

The second point, which is closely related to it, is that the sequence A, B, C, D that you showed is absolutely straightforward and I became a little confused at the point of integration because all of the 16th and 84th percentiles that you showed seem to have pictures of what looked like normal distributions on a log scale and therefore I assumed log normal distributions and I wonder to what extent the results become sensitive to that.

For example, if one is using expert opinion as we just heard from Reiter to decide to make as an input on the likelihood of a given grand acceleration, are there data that show if I study experts that their opinions are log normally distributed?

MR. MC GUIRE: If you will allow me a subjective response to that --

MR. LEWIS: According to a Bayesian algorithm --

MR. MC GUIRE: Let me respond to those two in order.

I am glad you brought up the difference between dispersion and uncertainty or what we might call statistical uncertainty and subjective uncertainty.

Statistical dispersion is represented by each of these people, A, B and C. Within a zone, a source zone, there is uncertainty even if that is the correct source zone which represents the tectonic structure, there is uncertainty in where earthquakes will occur in future within that source zone. Part B, given this frequency of occurrence of earthquakes and exponential distribution typically which is represented here, there is uncertainty on what the magnitude of events will be in the future. That is a statistical uncertainty.

Given that this is the correct, in Part C, the correct ground motion equation, there is still uncertainty of given the magnitude and given the distance of what the ground acceleration will be at any site. That is a statistical uncertainty and the integration of that to perform the hazard analysis is an integration over all of those uncertainties and that lead to one curve, one probability of excedence versus ground acceleration curves as illustrated there and that frequency of occurrence versus ground acceleration is a result of all of those dispersions or all of those statistical uncertainties where earthquakes will occur in the future, what their magnitudes will be and what the ground motions will be given the magnitude and location of occurrence.

MR. LEWIS: That is very interesting but I was really aiming it to -- there are two issues still fuzzy in my mind. One is everything is assumed to be log normal. I gather that is the case, otherwise you can't do these integrations. You have to have an underlying curve. These are integrations if you like uncertainties in probability, which are derived from many things.

The second point is I don't mean when I distinguish between dispersion and uncertainty to distinguish between subjective probability and objective probability. That is still another bifurcation in the statistical game but very often the statistical dispersion leads to predictive uncertainties, sometimes it does not but sometimes it does.

There are many sources of predictive uncertainty, one of which is statistical dispersion in the historical record but there are other sources of predictive uncertainty, some of which you have alluded to.

You don't know the magnitude of the historical earthquake. You only have damage data, so all of these things are folded together but they fold together in different ways and the impression I had when you first spoke was, I think you said this was a fairly cut and dried procedure agreed to by everyone in the community over the last 10 years and I wonder if they have so submerged these differences like, for example, the assumption of a log normal distribution, that people no longer fight about it but if so that doesn't make it true.

Is that the situation? I am merely looking for education here.

End 8.

MR. MC GUIRE: Well, maybe I misled you there in trying to summarize in a brief way.--

MR. LEWIS: Yes, I understand.

MR. MC GUIRE: -- what in fact are ideas which have developed over a period of time. All of these are not log normal. The integration is typically done numerically so we can accommodate any probability distribution.

There is some expert opinion and argument in the field as to what some of those probability distributions are or how they should be represented, at what distributions they should be represented.

Each of those pieces then I should not represent as being absolutely agreed upon by everybody in the field. What I was trying to insinuate was this process of dividing the seismic hazard problem into seismic sources and earthquake distributions and ground motion estimations and integrating over those using the total probability there is, I think, an agreed upon methodology and that is the way to make seismic hazard calculations to come up with these results of frequency of occurrence versus ground acceleration.

I didn't mean to imply that every piece of each step was totally agreed upon.

MR. LEWIS: No, no, I was not trying to read that into what you said, but you say that in some cases there are agreed deviations from log normality and that means there is a great deal in those cases known about the sources of predictive uncertainty for those links in the chain and I will simply have to study that at some later time. I only worry that when one has factors of 10 in predictive uncertainty, just to try to keep the terminology consistent if not right, that one can be drawn into error when one is dealing with very low probability events by being pushed way out on the tail of a curve for which there is no basis in fact if one is looking for certainty and I will just let it go at that.

You have clarified some of the issues I had in mind.

MR. JACKSON: Just a minor observation, Dr. Lewis. In working with the external event PRA Working Group, there were a number of people, earthquake and tornado specialists and other events, and the observation I made is the people who have a lot of data, like number of earthquakes, they go on and argue endlessly about whether the appropriate distribution; those who have little data assume a distribution and argue about other things --

(Laughter.)

-- so I think in terms of doing PRA, the observation that came away from that meeting was we are always going to argue about something and in this case when you have a little data, you have to make an assumption on the distribution.

MR. LEWIS: I can understand that phenomenon. I am worried about the specific distributions. They need to do those integrations.

MR. OKRENT: Dr. Pomeroy had his hand up.

MR. POMEROY: Robin, just to introduce one more factor into this, could you comment briefly on how you choose the weights you attach at each step in this process to the various source zone hypotheses and the various choices of the B values and the attenuation curves, how are those weights assigned in your methodology?

MR. MC GUIRE: They are expert opinion. The process we have used up until now has been a rather informal one where we query experts to try to obtain their unbiased response to a series of questions about what their beliefs are in certain seismic sources, in certain parameters of seismic sources and attenuation functions.

The EPRI Program will develop in a much more formal way a proper format and methodology, whether it is to extract expert judgment and put expert weights on various hypotheses for which we have no statistical basis to assign weights.

MR. OKRENT: Dr. Reiter?

MR. REITER: Dr. Lewis, the issue of distribution has not been a dead one. In fact, if Mike Trifunac was here, he had argued rather strongly against log normal distribution. I don't know whether it is of significance or not but there are people who question it and question is it significant enough to make a difference or not, so it has not been completely forgotten and put away on the side. People are looking at it but I don't know if anyone has come up with anything better than that.

Just a short response on expert opinion. There really exist different ways to look at it. In one poll you have people go out and get a bunch of experts and they pick one view or one particular representation and that is the way they do the map.

On the other end, they go out and ask the experts and include everything the experts would say and some were in the middle, is where you have several people trying to estimate what the experts might say and in some cases utilities have gone through literature to find various ways to do it.

So, there exist a whole range of ways of dealing with that expert opinion and I think that may -- I know certainly in the extremes -- account for some of the differences in results and that may not be consistent from site to site, it may vary.

MR. LEWIS: If I could just say one word on that. I never doubted you were on the side of truth and righteousness --

(Laughter.)

-- and that people were in fact looking at these things. The trap that I worry about that can be so deeply embedded in the methodology that people forget it is there is sort of the following.

If I asked 15 people for the height of the Emperor of China and get 15 results I will get a mean, median and mode, dispersion and all of those good things. If I take that dispersion and characterize it by a log normal distribution and calculate a low probability by going way out on that log normal distribution curve beyond any of the estimates I have and use that as part of a regulatory process, I am making a terrible mistake and you can do that without even noticing it and I am sure you are not.

MR. OKRENT: Can I ask the following?

Dr. Mark introduced in his comment an interest in knowing what would be the degree of shaking probability of excedence on the order of 10^{-5} per year. The Staff in prior meetings with the ACRS has said we cannot predict up to that level. I think that is a fair quote of the Staff.

On the other hand, Dr. McGuire suggested at that level he can predict with a plus or minus 50 percent if I understood his answer to my question.

MR. MC GUIRE: (Nods affirmatively.)

MR. OKRENT: Can we hear a little bit of opinion in this particular area?

(Laughter.)

MR. JACKSON: I will keep score.

MR. MC GUIRE: Could I respond to that first?

MR. OKRENT: Whoever. All are invited.

Dr. McGuire is first.

MR. MC GUIRE: This is in a sense rebuttal to Dr. Pomeroy and Dr. Reiter's presentations. My thought is let's not punish the messenger because he brings bad news. If we have great uncertainty in what the hazard is the eastern U.S. it is not because we are using formalized probability analysis, it is because we have great uncertainties in what causes earthquakes and how large they will be and what ground motions they will generate and how frequent those earthquakes will occur and that is the state of knowledge. It is not the fault of probability analysis, seismic hazard analysis.

MR. SIESS: Dave, would you repeat the four questions you posed at the beginning? I didn't write them down.

MR. OKRENT: How much do we know about the seismic contribution to LWR risk? How much do we need to know? Can we learn what we need to know? How? And then, what are the NRC and industry programs and are they adequate for the purpose?

MR. SIESS: (Nods affirmatively.)

MR. OKRENT: I am still curious to hear those who have opinions on the 10^{-5} per year area.

Dr. Luco?

MR. LUCO: I believe the statement by McGuire that a measure of the standard deviation on the order of 50 percent is too optimistic. If you look at the dispersion of acceleration versus magnitude and distance, the dispersion there for a given magnitude and a given distance is already on the order of 50 percent to 100 percent.

Campbell's configurations give a dispersion on the order of a factor of 1.4, Joyner's and Lorsley's, a factor of 1.9. Just that fact of change, there is a factor of an order of 1.5 to 1.9, so I find it really hard to believe that considering all the sources of uncertainties, you would end up with a factor of just 50 percent.

MR. OKRENT: Dr. Cornell?

MR. CORNELL: I think perhaps 75 percent of the debate which has just gone on has really been a problem in semantics. Dispersion uncertainties, the questions that were addressed about these definitions and terms were never really answered. Robin got started and was interrupted. Dr. Luco's comments just now that "dispersion that we see in ground motion" is indeed what Robin referred to as a dispersion in the attenuation law. It is integrated in by the total probability theorum. It is a part of the prediction process. It is contained in the curve. It is involved with everything else. It would be a wonderful service to this Committee and to this business if someone would write about a two-page definition of terms and then about a five-page definition of nonterms and improper interpretations that exist very strongly in the literature and by the literature I would include the minutes that are being recorded to this meeting. It is going to be impossible for anyone to read it and understand what is coming out of this unless we have a few precise things we agree on and use the terms, such as "uncertainty," such as "statistical uncertainties," always in the same ways otherwise it is total confusion.

I am merely concerned about this. I don't think we can proceed usefully unless we can make a few basic agreements on these terms. In PRA work, where this is thought about intensely, the unfortunate terminology has come up of probabilities of frequencies and as unfortunate as it is, it is at least a dichotomy that is made and everyone agrees to it and there what is referred to as probability most of us in this room refer to as "uncertainty" and there what is refered to as "frequency" most of us in this room refer to as "probability." That is just the beginning of the problem but in most PRA work, the dialogue is kept clean and clear and in this room it has not been and I am concerned about it.

MR. OKRENT: Dr. Kennedy?

MR. KENNEDY: I agree with Alan and I think there has been different usages of the term. I don't believe that Robin McGuire ever said that in the 10^{-5} annual frequency of excedence region that he was able to predict the annual frequency of excedence within plus or minus 50 percent.

MR. SIESS: No.

MR. LEWIS: No.

MR. KENNEDY: In actual fact, in every one of these hazard curves --

MR. SIESS: He said a factor of three.

MR. KENNEDY: Or hazard studies I've seen as a part of PRAs, when you got to 10^{-5} annual frequencies of exceedance region, your uncertains in the actual frequencies of exceedances are generally like three orders of magnitude and maybe even four order of magnitude. I think that leads to the problem Leon Reiter brought up and may be slightly exaggerated but he made a very important point: when you are dealing with three and four orders of magnitude of uncertainty in the annual frequency of exceedance, and then you try to express your results in terms of a point estimate or best estimate and in the numbers Leon was giving of point estimates or best estimate numbers, you get some very funny conclusions and the conclusion that seismic dominated early fatalities at Limerick is one of those funny conclusions and you get it by looking at the mean or best estimate of something that has in the case of early fatalities four to five orders of magnitude of uncertainty.

You would reach just the opposite conclusion if you looked at the median. You would reach the conclusion that seismic was a low contributor to early fatalities at the median. Now that is no better conclusion either, so whether you look at the mean or the median, you will reach totally opposite conclusions when you are comparing seismic with internal hazard.

The problem with the mean is that it is totally dominated by the most extreme hazard curves, in this case the decollement zone. It is dominated totally by how much weighting you put on that extreme hazard term. If you don't believe in that hazard curve you won't believe in that result. If you are a strong believer in that hazard curve you will be a believer in the end result.

We have somehow got to describe this process in a way that we carry through to the end results the fact that we have uncertainties of four to five orders of magnitude when we are out in this region.

End 9.

MR. OKRENT: Are there other comments?

Yes, Dr. Ang?

MR. ANG: I would like to ask for opinion from either Dr. Kennedy or Dr. Cornell with regard to the possibility of perhaps maybe even confusing the state of the art, but instead of separating between "uncertainty" and "frequency" or "probability" there is still a third alternative which has been adopted in civil engineering that may or may not have been discussed in nuclear and that is to fold the so-called uncertainty part and integrate it once more and come out with a probability of frequency which is approximately the mean.

By doing that you get away from the dichotomy of the conflicting situation between low risk and high uncertainty versus high risk and low uncertainty such as the site A and B that was mentioned earlier by one of the SSRAP members.

I wonder if Dr. Kennedy or Dr. Cornell wish to comment on that?

MR. KENNEDY: I would like to comment on that. I am very much opposed to folding them together. If you fold them together you have only a best estimate number when you finally come out. Then you would reach the conclusion, let us say at Limerick, that in your best estimate seismic dominated the early fatalities. I don't agree with that conclusion, but if you fold the uncertainties and the dispersion into one quantity, the tremendous uncertainties that exist in the seismic hazard curves and in the fragility curves, overwhelm the dispersions and they lead to conclusions that seismic is a very contributor to risk.

MR. ANG: May I add to that?

MR. OKRENT: Sure.

MR. ANG: Let me be clear. I am not necessarily in favor of that in fact in the early stages of its development in civil engineering I was also against it. Nevertheless, it seems to me it would get away from the dichotomy as far as relative risks.

If you are going to use it as a relative measure of risks, it seems to me you would get away from that problem of having a conflict between low risk and high uncertainty and high risk and low uncertainty and which figure to use. At least you would get away from that conflict.

MR. OKRENT: I must say I feel a little uncomfortable at discarding the use of the mean when it gives you the answer you don't like.

MR. SIESS: He didn't like either one.

MR. OKRENT: No, I'm sorry there are large uncertainties which are contributing to this and in effect if you say I will not take the mean calculated this way without justifying a reduction in the uncertainties you are now lending yourself a crutch that puts you subject to a third party suspicion of bias.

MR. KENNEDY: Why do we have to use either a mean or a median? Why can't we just simply say that our estimate and I am going to pull numbers out of the air, these are not real numbers, why can't we just say that our estimate of the range of possibilities on the annual frequency of seismic induced early fatalities ranges from one times ten to the minus five to one times ten to the minus ten and not have to describe a mean or a median in that range because I think that is where we start having our problems.

MR. OKRENT: That is a possible way of presenting information.

The people who make the decisions in the end have to decide in what form or forms, as they may prefer to have the information presented in multiple ways, that they want to see it.

I would like to go back --

MR. SIESS: Dave?

MR. OKRENT: Yes?

MR. SIESS: It seems to me if the uncertainties are made clear by one means or another the people who make the decisions may decide that the uncertainties are so great that they would rather not make the decision on that basis at all.

MR. OKRENT: Yes.

MR. SIESS: And I don't think we should conceal or try to hide the uncertainties from the people who are going to have to use the answer or choose to use the answer to make the decision.

MR. OKRENT: Yes? We have lots of hands.

By the way, there was a hand at the back that I said I would acknowledge on this topic. Go ahead, first.

MR. ALEXANDER: Shelton Alexander from Penn State University.

The discussion here has been focused on how well can we do presently and what are the uncertainties as we now perceive them and I would argue at least in the case of the Eastern United States that there is some hope at least to reduce those uncertainties perhaps significantly and you will hear tomorrow from Carl Stepp on one approach to that in which for example the various hypotheses have a very significant control over the estimated hazard at a particular point are going to be examined hopefully systematically and critically for the whole Eastern half of the United States, where the definition of those models on a physical basis is spelled out in the beginning and that I think will help elucidate the nature of the uncertainties and perhaps the number of candidate hypotheses down to a fewer number which can then be looked at in detail. That is one thing.

On this issue of attenuation, this clearly comes in heavily in uncertainty. The seismic margin network as it now exists and is being operated, there is every reason to believe that we can characterize the attenuation much more accurately and reliably than those curves would suggest and as well characterize the attenuation as a function of frequency, spectral frequency, which has not been done so far.

Third, the seismic monitoring efforts, for example, are beginning to give a lot of additional information as to the state of stress, which after all is the most fundamental parameter that we would like to know but don't know very well that can cause earthquakes in the first place so I think that the hope would be that many of these parameters that govern uncertainty can be narrowed in a time frame that is useful for this licensing and evaluation process.

MR. OKRENT: Mr. Reed?

MR. REED: I would like to muddy the waters a little bit more, focusing on the probability of the frequency disribution and whether we should look at the whole distribution of the mean or median or whatever.

I find myself being more concerned with what the heck contributed to the distribution and continually wanting the information on contributors to the distribution and in the discussion here on seismic hazard, I am always interested in what from the seismic hazard curves contributes to that distribution.

On the fragility side, I often ask myself which components and how are the components contributing to this?

What I sort of feel is in our information that we are trying to convey from these PRA analyses, I think the probability and frequency distribution in one sense is a very needed item and is missing the various attributes that ought to go along with it.

Explaining how this came about and I think an inte-ligent decision-maker wants this and wants to be able to understand so he can himself interject some judgment judging whether the distribution he is dealing with is adequate in making the decisions.

MR. OKRENT: Dr. Lewis?

MR. LEWIS: I just want to -- we have touched upon some fairly important things here in the last couple of moments and I must say I agree completely with Dr. Kennedy that the way to provide information to someone is to give them the full curve which contains in principle everything you know. I am not sure that giving information saying it is between .1 and .401 is the right way to do it because in my experience with decision-makers they focus on whichever end of that distribution suits their purpose and carry only that number around in their mind.

That's just a whimsical way of saying there are two things we have to do. One is to understand the nature of the risk and I agree with Dr. Reed, the ingredients of the risk and what they come from.

But in the end the other thing is to present it to people who make regulatory decisions in such a way that they make the correct regulatory decisions.

And the second one, which I have just alluded to here may be much more difficult because the full information really does contain all of the curves and encapsulating them in single numbers whether they are incorrectly means or medians or incorrectly modes, may be the wrong way to do it. I don't know an answer to the last problem.

MR. OKRENT: Other comments?

Dr. Pomeroy?

MR. POMEROY: I would just like to make a brief comment with regard to basically that.

In one PRA I have looked at, there were three hypotheses considered and each one was assigned. They were selected from a number of other hypotheses in the first place and each was assigned a weight based upon expert judgment and one of the sentences read roughly that other hypotheses could have been chosen which would have made a final seismic hazard but was much greater or much less, but these considered three representative examples in the PRA.

The point I think that I would like to make is that is probably a result of a very limited input from the seismic side to the total PRA effort.

I feel that a stronger PRA could be made by at least considering all of the hypotheses and perhaps those at the extreme ends, those that contributed to make a great hazard and a small hazard and that that consideration alone would give a decision-maker more to base his decision on rather than a statement that seismic hazard may be significantly greater or significantly less depending upon what hypotheses you choose.

MR. JACKSON: Could I ask Dr. Pomeroy two quick questions?

MR. OKRENT: Two short ones and then I am going on to the schedule.

MR. JACKSON: I will ask short questions. I don't know about his answers.

MR. OKRENT: I asked for short answers.

MR. JACKSON: Two questions, Paul.

One, in your comments you talked about the use of seismic networks to assist in determining zonation. The first question I have are how critical are those networks to proceeding with this kind of a study?

The second question is, you seemed to indicate very strongly we ought to do more seismic PRAs but can you tell me in your opinion why a utility should do a seismic PRA and why I should require a utility to do one.

MR. POMEROY: I would like to answer the first question briefly and abstain on the second question because I would discuss it with you but I don't think I would answer that in a short time frame here.

I do think seismic networks are important. We are beginning in the East and Central part of the United States to develop a good picture of the patterns of seismic activity and there is good evidence that in general those patterns of seismic activity are stationary in time, at least on the order of 40 years or 50 years or so. Therefore, that information gives us some predictive capability.

Unfortunately, it doesn't always give us predictive capability as perhaps in the case of the New Brunswick earthquakes.

I think the other information that is being developed by the network, that is some of the information on the attenuation, some of the information on the source characterization of the events, that information is critical and it is coming at a slow rate because the earthquakes are occurring at a slow rate and they need to be monitored, at least in the foreseeable future.

MR. OKRENT: Dr. Budnitz, did you want to comment, sir?

MR. BUDNITZ: I had a comment about the use of expert opinion. It goes to whether the expert opinion is somehow legitimized by the rest of the experts. Now that is a very difficult thing to discuss but in fact it's how the decision-maker or anybody really decides whether the expert opinion is worth including, if you know what I mean.

There might be 10 people considered experts in a room and one of them disagrees with the other nine and everybody else says I disagree, but his opinion is legitimate; in other words, I understand how he got there. I understand what the rationale is although I don't agree. There is a disagreement.

On the other hand, there are also situations in which the one in 10, the way that person arrived at it is not considered legitimate by anybody else and is discarded. We all know of situations of people who aren't called experts. There are people who claim all sorts of things are going to happen or use science or misuse it for other goals.

For example, there are people who claim there will be an earthquake and now and Saturday on the San Andreas Fault, near here, because their shoulder hurt and that opinion is somewhat less valid than a person who claimed that based upon some evidence in the earth sciences.

Now without arguing that either, it might end up being accepted but there are different reasons for expert opinion and I think that the crucial element here is an area where the science itself is not firmly based upon mechanisms and understands unlike if you are arguing about Newton's Laws or something.

In a situation like that, the most important information to reveal to the decision-maker is the technical basis for these disagreements and whether or not others in the community who disagree consider the deviant opinions or the ones out on the edges of the spectrum to be nevertheless legitimate. That is why I come back to Howell's thing. If you have 20 people who are experts who put a number or something that has a number there is a spectrum. If they all have a basis, they are all presumably worth some consideration. If there is one that has no basis whatsoever, darn it, you shouldn't include it.

End 10.

MR. OKRENT: Okay, thank you.

I am going to go on to the next item because we are fifteen minutes behind now. Dr. Luco is the speaker. We will come back to this matter in the panel, no doubt.

MR. LUCO: I will describe some aspects of the modeling and solution of the soil structure problem, which has effects on the calculated response of nuclear power plant structures.

Most of my comments will be related to the containment building and among the many factors that can be considered, I will look at the effects of soils, soil characteristics, the effects associated with a particular type of seismic excitation considered and the effects of embedment of the foundation and finally the effects introduced by nonvertical incident rates.

The basis mode for, well, let me say that the results that I present were obtained in the course of two studies.

The first one was supported by the Lawrence Livermore Lab as part of the Seismic Safety Margins Program.

The second study has been conducted by Woodward and Clyde under a contract from the Nuclear Regulatory Commission and has to do with engineering characterizations of motion.

There we have a model of the containment building. We are going to look at the response at the top of the mat at the top of the containment shell and also at the top of the internal structure and compare these quantities for different soil models and different types of seismic excitation. The foundation is embedded 20 feet in the ground, I'm sorry, 40 feet.

A number of soil models are considered. The first one identified there is one, well the figure shows the distribution of shear weight velocities in the soil with dep .s. One is a response to a uniform house base, a velocity of 7600 feet per second, which is the limit above which it would be considered rock by NRC and there are progressively softer soil models, 2, 3 and 4.

First, let me show the ratio between the response at the bottom of the foundation to the amplitude of motion on the free field.

If we assume that we have vertical incident weights at the site, this is plotted versus frequency, and a ratio equal to one would mean the motion at the bottom of the foundation, the translation of the bottom of foundation is equal to the amplitude of motion on the ground surface outside the building.

We can see the different lines represent the difference types of soil profiles considered. The hard layer of soil, 7600 feet per second, and the dotted lines would be the softer soil profiles.

You can see here there is a very significant reduction of the amplitude of motion with frequency even for the relatively stiff soil profile - - - represented there by the solid line. This effect is associated with the scattering of weights by a disembedded foundation and usually it is not allowed to be considered by the NRC Staff.

So, here we have a considerable factor that reduces the motion at high frequencies. Now because of the embedment of the foundation and again if it is cylindrical waves some rocking will be created even if we don't consider the inertia forces associated with the presence of the structure and the rocking is represented on these curves; .3 means the vertical displacement on the edge of the foundation induced by rocking, is 30 percent of the motion on the free field, so you can induce a significant amount of rocking due to embedment of the foundation and again if you don't include these effects, the embedment effects, you are introducing error.

This is to give you some idea of the differences in results that we obtained depending upon the soil profile.

FY denotes the base shear force at the base of the containment shell. We have five columns of numbers corresponding to rigid soil and the progressively softer soil profiles so depending upon the soil characteristics you can go from .349, .34, .574, .563, .369 and in this particular case, the largest base shear force is not obtained from the rigid soil profile but from a softer soil profile somewhere in here (indicating), so you can see that the flexibility of the ground has a significant effect on the response and should be considered.

This table illustrates the variation of the effects of soil structure interaction depending upon the soil profile and on the seismic excitation. The numbers shown here are percentage variation with respect to the results for a rigid soil.

If you consider a very rigid soil profile, which included soil restructure interaction, you get a five percent reduction. If excitation is the Parkfield record, you get a 6.8 reduction. If you have a particular component of El Centro, this is 1979, if you use an artificial that fits the Reg Guide 160 spectrum, the interaction effects introduce a 22 percent or 23 percent increase in response and so on.

For Melendy Ranch you get this reduction (indicating) but these effects of interaction are highly dependent on the type of soil profile.

For instance here for soil profile 3, which is a fairly soft, not extremely soft but you can get an interaction which increases the response by 66 percent. A reduction for one component of El Centro, 19 percent, 61 percent for Y component of El Centro and reductions for Melendy Ranch and artificial, Melendy Ranch are high frequency records for small magnitude earthquakes and there you can see the effects of interaction have caused a strong reduction in base shear force but Parkfield and one of the components for El Centro which have a low frequency component in the excitation you can get a 60 percent increase in the base year force du to soil structure interaction.

Here there are similar results for the effects on the peak acceleration at the base of the containment and in this case you can see that typically the effects of soil structure interaction for most soil profiles and for most records we have considered there lead to a reduction in peak acceleration so this is in a sense good news from the point of view of equipment.

At the lower level soil structure interaction effects typically will reduce high frequency components of motion.

A summary of the effects at the top of the containment shell is shown is this figure. These are the floor response spectra at the top of the containment shell for the Melendy Ranch record which again has a lot of high frequency energy.

The solid line is for a rigid soil and the following lines correspond to softer and softer soil profiles. You can see there is a very strong reduction on the response at the top of the containment at high frequencies and for all frequencies. That happens for Melendy Ranch, which was for a magnitude 4.6 earthquake at a short distance.

There are components of Melendy Ranch. You see the same type of behavior here. We have the comparison for the El Centro Station No. 5 record for the 1971 earthquake and this is a component of motion parallel to the fault.

Again you see a strong reaction as the soil gets softer and softer particularly at high frequency and then now if you look at the Parkfield record and the Y component for El Centro, these components are motion perpendicular to the fault, very close to the fault and perpendicular to the fault.

There you see the solid line represents the rigid soil and the other lines represent softer soils. In some cases the softer soils may lead to higher responses than the ones you get from a rigid soil when you exclude the interaction and here you have the results of an artificial record which fits the Reg Guide inspection and you see reductions in those frequencies.

So the effects of soil structure interaction depend very much of the type of soil profile and the type of seismic excitation.

This suggests that these analyses have to be performed considering a series of records. You should not do this for one simple record.

Results in this figure are for the motion at the top of the base mat, the comparison as before but now at the top of the base mat and in this case again the solid line is excluding interactions. That means assuming the soil is rigid for Melendy Ranch and one of the components of El Centro.

Typically we have reductions of high frequencies when we include soil structure interaction. We have a shift in the frequency at which the maximum response occurs but for Parkfield and the Y component of El Centro Station No. 5, we may have increases in response at high frequencies when we include soil structure interaction.

Also we can get increases at higher frequencies. An artificial record consistent with the Reg Guide 160 spectrum typically leads to reductions in response for most soil profiles when you include soil structure interaction.

The next aspect that I wanted to consider was the effect of excluding kinematic interaction. When the foundation is embedded in the ground you have this phenomena of scattering of waves in such a way that part of the energy is radiated back into the ground and thus goes into the structure.

In many analyses you are not allowed to consider this effect. When you do that you exaggerate your results by the percentages listed here.

For instance, if you take the Parkfield record and you consider the base shear forces for the containment shell, the error introduced by not accounting for this scattering is you have increased your response by 17 percent. If you go to artifical record consistent with Reg Guide 160 spectrum you have increased your response by 20 percent.

If you take El Centro's Station No. 5's record you have increased your response by 17 to 30 percent and then for high frequency records such as Melendy Ranch by excluding these kinematic interaction you have increased your response by somewhere between 50 and 70 percent. These are base shear forces and overturning moments.

You see it worst if you look at the internal structure here for instance for this high frequency, low magnitude earthquake. You have increased your response by 100 percent by a factor of two, so this, to my mind, indicates that for a proper evaluation of the forces and motion at different points within the structure we must include kinematic interaction effects, otherwise we are overestimating the response.

These effects are shown here on the floor response spectra at the top of the containment shell. The solid lines includes kinematic interaction and the dotted line excludes kinematic interaction.

You can see when you exclude kinematic interaction you artifically increase the response at the top of the structure.

This is a response of the bottom of the structure and there at high frequencies you have artificially increased the response by a significant amount. This at the top of internal structure, let me see, I'm sorry, this is at the top of the internal structure and this rocking at the top of the base mat so all of these response components have been exaggerated by this artificial condition of excluding the kinematic interaction.

The results that I showed were for the artificial excitation which fits the Reg Guide spectrum and this is even more significant when you look at high frequency excitation such as the Melendy Ranch excitation, you will see very large increases in response at the top of the base mat by this artificial condition.

The other aspect I wanted to discuss has to do with the effect of nonvertical incident waves on the response of the containment building and some results are presented in this table.

Let me concentrate on the line above this dark boundary, which corresponds to the base shear forces on the containment shell.

The first number is for vertical incident shear waves. The second number is for love waves with an apparent face velocity of four kilometers per second. The third number here is for love waves with an apparent face velocity of two kilometers per second and you can see that the difference is less than one percent.

So, in terms of the base shear force, these inclined waves have very little effect in this case.

The first three numbers here are for a plant which is embedded in the ground.

The next three numbers are for a surface founded containment building and again there is an effect of inclined waves that are less than one percent in the second case.

If we look at this operating floor which is on the internal structure, there we can see some differences due to the inclined waves. Here we have say 34 for vertical incident waves and 44 for inclined waves with an apparent velocity of two kilometers per second.

The internal structure in this particular model is not symetric. It has some eccentricity and that combined with the torsional response induced by the inclined waves lead to this increase in response.

It is a translation of the response on the operating floor, so there is an increase on the order of 30 percent on the response at that particular floor due to inclined waves but this effect could be accounted for by the accidental eccentricity on the order of five or six percent.

Okay. So to summarize, the effects of soil structure interaction are highly dependent upon the type of soil profile. The effects of a given soil profile are highly dependent upon the particular seismic excitation. The seismic excitation may be normalized to the same peak acceleration but the interaction effects may be very different depending on that excitation but we have found that for high frequency motions associated with low magnitude earthquakes and short distances, interaction effects introduce a very large reduction in response.

But we also have found that for some small magnitude earthquakes and for sations very close to the fault, interaction effects can increase the response by as much as 60 percent.

The effect of excluding kinematical interaction as a modeling decision induces an increase in response, an artificial increase in response which may range from a few tenths, from say 10 percent to as much as 100 percent.

The effect of inclined waves for realistic estimates of the apparent face velocity of these waves have small effects on the base shear forces and overturning moments, small effects on the translational response on the center line of the containment building but have some effects on the response of the internal structure if some eccentricity exists, however the effects of the inclined waves could be simulated by the use of accidental eccentricity and typically we have found the effects of inclined waves were not as large as we saw a few years back.

There are sources of uncertainties introduced in soil structure analysis. One is the method of analysis we use.

You also have the interaction through the soil when you have several structures in one plant and then you have the problem of nonlinear response of soils and the structures which is difficult to model.

That's all.

MR. OKRENT: Mr. Siess?

MR. SIESS: I have a couple of questions, the answers to which might help me understand the implications of this in relation to the margins for existing plants and I am not sure they are addressed to Dr. Luco or whether someone else can answer them, but you pointed out that for an embedded structure, as most of our containments are, that neglecting the soil structure interaction is conservative?

MR. LUCO: (Nods affirmatively.)

MR. SIESS: What is our recent practice, regulatory practice regarding soil structure interaction on embedded structures? Do we permit it?

MR. LEAR: With respect to embedded structures, it is in and of itself a part of the modeling which Dr. Luco said the development of the finite method element, the discretization of the soil and the structure itself in that model, it would in and of itself be considered.

MR. SIESS: Beginning at what point?

MR. LEAR: The matter of deconvolution, which is

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I think perhaps another subject that prompts what you say or prompts what you ask, has been cast about a number of times as to whether or not the actual foundation should be taken as the location for a deconvoluted response spectrum.

Because it has been found having done that you will find a response spectra at the foundation level less than the ground itself from which you started. That's not been allowed.

MR. SIESS: Let me be more specific. He says neglecting the soil structure interaction, he said it is not permitted to be included.

MR. LEAR: He didn't say quite that. He said neglecting the kinematic action of the vertical or incident, angular incident waves, is a conservatism that produces in the structure the shear moments and the likes and the additional stress.

MR. SIESS: Is that an additional conservatism that has been applied to structures we would be looking at, for example, the seven or eight structures that we sited in our letter, our collective letters.

Would those have that conservatism?

MR. LEAR: It is inherent in the actual calculation. I believe with Dr. Luco --

MR. SIESS: I am not going back to the SEP plant. I am talking about those of most recent vintage.

MR. LEAR: I'm sorry, I didn't hear you.

MR. SIESS: The more recent vintage plants -- this kind of conservatism would exist?

MR. LEAR: Indeed it would.

MR. SIESS: That gives me a little help.

The other point he made was making these calculations of soil structure interaction, they seem to be relatively sensitive to the particular spectrum, not spectrum but record you use. We have been talking a lot about using Western earthquake data for Eastern plants.

In the development of site-specific spectra, the uniform hazards approach, were we using California earthquake records or were we using records that would be more correct for the Eastern United States?

MR. LEAR: Bob could probably speak to that better but I believe the concept is the recent accumulation of information on earthquakes permit a more localized determination of a spectrum that replicates or attempts to replicate what would exist in a given locate as opposed to going to the California source of information.

We are drawing upon I believe the body of knowledge for a specific locale. That infers site-specific.

MR. SIESS: But there won't be many locations in the Eastern U.S. where we have a record for a .6g or 1g that Leon mentioned in Southeastern Pennsylvania.

MR. LEAR: That's correct but I don't believe we are choosing for a site-specific response spectra for a particular plant, say Limerick.

MR. JACKSON: Let me make a few comments if I could.

MR. SIESS: If we were evaluating Limerick for seismic margins, what could we use to take into account the type of sensitivity Dr. Luco mentioned?

MR. JACKSON: I would ask for one question of clarifiation from Dr. Luco first.

I get the perception from listening to him and from what I know from other reviews that his calculations are based upon a near field earthquake and not really, may not be appropriate for a far field earthquake say greater than 15 or 20 kilometers. I don't want to argue about that definition and therefore for the sight-dependent spectra, the site-specific spectra we are using in the East, this phenomenon may not be valid.

MR. SIESS: I would hate to think that 1.25 in Southeastern Pennsylvania was far field.

MR. JACKSON: The other problem I think we are having is I think a number, I don't know if the sites are soil founded sites, many of those are not soil founded and to go back to your first question, the site-dependent spectra records generated include some California records but also include records from Italy and elsewhere.

MR. SIESS: Inter-plate?

MR. JACKSON: Some but not much.

Could you clarify the near field, far field type question?

MR. LUCO: Almost all of the records used in this study are very short epicentral distances. In the Eastern U.S. the concentration would be different.

MR. JACKSON: Do you still think it would be a valid effect?

MR. LUCO: I think the basic conclusion should still be valid, that you should not rely on a particular record. There should be several records to obtain estimates.

The alternative is under certain conditions to use an artificial record which has a sufficiently broad spectrum. Under certain conditions that could be a good solution instead of using mini-records but that may be dangerous under our conditions.

Let me indicate once again neglecting the effects of soil structure interaction is not always conservative. For a particular situation, as we have in the Parkfield and Imperial Valley, one of the components of motion there which corresponds to motions perpendicular to the fault, if you do not include the interaction effects you get a much lower response, so interaction in that case increases the response but this is for a magnitude say 6 1/2 earthquake, 5 1/2 to 6 1/2 and very close to the fault.

For smaller magnitude earthquakes and records with higher frequency content, we see a reduction associated with soil structure interaction but on the other hand, excluding the effects of kinematic interaction always increased the response. It was always conservative.

MR. OKRENT: Mr. Reiter?

MR. REITER: I wanted to respond a little bit to the question Dr. Siess asked. Perhaps you can correct me on this, Paul.

What we have observed about the Eastern records we are beginning to get and these records are essentially small earthquakes nearby, less than five within 10 or so kilometers, that there seems to be an element of high frequency.

There is a dispute as to whether that is source related or site related. I think tomorrow Dr. Boatwright will talk about that but that is the kind at this present point. That is the anomaly we have. We may get further reportings later on. We may get some additional insights.In the past we have used all of the available records we have.

As Bob indicated, those are inter-plate, some of those are California so if we are talking about taking the information we have now and modifying the records, whether it is due source or site based on the information.

Now it might involve looking at the high frequencies of smaller earthquakes. I am not saying that is the final conclusion, just based on the available information.

MR. OKRENT: Dr. Thompson?

MR. THOMPSON: Does the fact that nonlinear effects are not utilized increase the degree of conservatism?

MR. LUCO: Yes. All of the results presented here were for linear analysis. We have done a simple comparison to indicate the effects of a soil nonlinearity.

A calculation was done for a peak acceleration of 5.25g.Consistent with that ground motion you have certain soil properties. The motion was increased. The peak acceleration was increased by a factor of 2.5 and the soil properties due to nonlinear effects changed.

When we looked at the interaction effects, considering the radiation in soil properties induced by these nonlinearities we see the response of the structure did not increase by a factor of 2.5 but by a factor less than that and in this calculation we assumed the structure remained linear, so as the excitation increases by a certain factor, the soil becomes softer, there is more energy dissipation in the soil and the response of the structure does not increase by the same factor.

So there is a source of conservatism there and this was particularly important again for high-frequency records.

MR. THOMPSON: Thank you.

MR. OKRENT: Mr. Siess?

MR. SIESS: Let me try something else. In looking at this question of seismic margins, that is what margin do we have to resist an earthquake greater than the one we have designed for?

One possible source of such a margin is that our conventional regulatory required analysis procedures yield seismic inputs to the structure that are greater than those that would be obtained from a more correct or more realistic or both analysis.

Now, I have the impression from what Dr. Luco presented, that our standard, whatever that may mean, Class 20 plants analysis procedures for seismic input, may in some cases be conservative and in some cases not be conservative depending upon the level of the earthquake, the nearness to the sort, the frequency content, the type of soil, the embedment et. cetera, is that a fair statement?

MR. LEAR: I would like Dr. Luco to answer that question.

MR. TANG: Maybe I can answer.

MR. LUCO: Excuse me, let me.

MR. TANG: I am C. P. Tang from the Structural Review and Technical Engineering Branch.

In answer to Professor Siess's question, I think the regulatory requirement for soil structure interaction is first we want the analysis to be on a fixed basis. Then we use, the other analysis uses the soil structure then you have to envelope that. This is very conservative so some of the utilities object to that requirement very much.

MR. SIESS: You think it is conservative?

MR. TANG: Because we were using the envelope.

MR. SIESS: Dr. Luco?

MR. LUCO: Yes, what I have seen for the Eastern U.S., if we assume that typically we will have moderate magnitude earthquakes or low magnitude earthquakes with fairly strong high frequencies, then I would say that the interaction effects would tend to reduce the response of the structure when compared with an analysis which assumes that the soil is rigid.

Now in many cases, soil structure interaction analyses are made and typically we are not allowed to include the kinematic interaction effects, so that tends to increase your response.

But on the other hand, the procedure that you may have used for the analysis, the actual method of solution that you use may introduce some conservatisms.

For instance, in many cases a two-dimensional model is used to solve the interaction problem and under certain conditions a two-dimensional model may underestimate the response by a factor on the order of say 30 percent.

MR. LEAR: Excuse me, Dr. Luco. What model are you referring to? What computer code?

DR. LUCO: If you use a two-dimensional model as opposed to a three dimensional model, if there isn't much material attenuation in the soil under those conditions a two-dimensional model may underpredict the response.

MR. LEAR: But what is that exemplified by? What computer code that you currently are using or know of?

MR. LUCO: Yes, if you used, well, Rush is a two-dimensional model. I have to be careful there. I don't want to say that anytime you use less you are underestimating the response. It depends on the depth of the rocks and other things.

MR. LEAR: There are a considerable number of factors you have to consider with the use, true.

MR. SIESS: I have two answers, Dr. Tang says it is always conservative and I think I hear Dr. Luco saying sometimes it is, sometimes it isn't, which is just where I started.

MR. LUCO: I think on physical grounds, it is conservative but sometimes the calculation of the interaction effects by use of a two-dimensional model may have led you to the wrong estimate.

MR. SIESS: I don't get the distinction. I am only interested in the physical phenomenon.

MR. LUCO: From a physical point of view, I think you will have in most case reductions.

MR. OKRENT: There was a hand in the back. Is he still there?

MR. HENRIES: I am Bill Henries from Yankee Atomic Electric Company.

I am wondering if your studies are adequate. There are a number of East coast plants that were bounded on bedrock but subsequently backfilled with 30 or 40 feet of soil not as stiff as the rock obviously and compounded by auxiliary building somewhat separated surrounding the containment structure of anywhere up to 180 degrees.

By ignoring the kinematic interaction, would you say in general the responses are quite conservative if you just did the fixed base solution and didn't include any radiational damage and by how much?

MR. LUCO: Yes, the results I have presented here assumed that the foundation of the containment building is surrounded by soil and that there is contact between the soil and the walls of the foundation and in many cases the compact between the soil and the foundation is only partial so to obtain an accurate numerical estimate of what happens in that situation is very difficult and probably the effect would not be as pronounced as I showed here assuming full contact between the soil and the foundation.

MR. HENRIES: But there would still be some conservatism because you would still have some radiation on the kinematic interaction?

MR. LUCO: Yes. I have not done a particular analysis to be able to substantiate that.

MR. OKRENT: Dr. Cornell?

MR. CORNELL: Addressing Dr. Siess's question, one of the analyses you can do, given the seismic hazard analysis, is to in effect run it backwards and ask, given that we have a .5g event in the East Coast, what would cause it? What is likely to cause it and the conclusions of those studies are inevitably that it will be a relatively close event and of small magnitude.

The implications of that are two it seems to me, with respect to conservatism.

One, in the moderate frequency, low frequency range, meaning say two hertz or so, those are records which are typically very not rich vis-a-vis design spectrum.

At the other end, as Dr. Reiter suggested, there is some indication there may be relatively rich, in the 10 to 20 hertz region vis-a-vis design spectra. On the other hand, what you have just said, it seems to me, is that is precisely where soil structure interaction will be the most defective in reducing responses, that is the high frequency end of high frequency earthquakes.

And secondly, in response to Dr. Thompson's questions, that the nonlinear effects would also be more conservative in that range, is that a fair assessment of the problem?

MR. LUCO: Yes. I think that is fair.

MR. JACKSON: I would just like to make a comment while we are looking at all of the conservatisms, Dr. Siess, that in many of the sites we have looked at the question of amplification of motion was not looked at in older sites and in some not-so-old so that if we are going to go back and look at conservatisms, I think you really need to look at the whole site effects problem, both amplification and reduction and I think there may be compensating factors there.

MR. SIESS: I figured I was confused enough at this stage that if we go into all of the effects I would -- I am just trying to straighten them out.

Right now I think I have had an unequivocal "maybe."

MR. OKRENT: I am going to make an unequivocal decision that we finish this topic and instead of taking the next topic before lunch we take it after lunch, so I would like to recess for one hour. I hope we can get in and out in that time but please, would the next speaker, Dr. Hall, and the recorder be here in one hour.

(Whereupon, at 12:45 p.m., the meeting was recessed, to reconvene at 1:45 p.m., this same day.)

End 12.

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AFTERNOON SESSION

(1:45 p.m.)

MR. OKRENT: I think our next speaker is here, so let's reconvene. If you make any major points in the first few minutes --

MR. HALL: I am going to; that is why I would like to stall for a few minutes.

MR. OKRENT: No, you can repeat them at the end of your talk.

MR. HALL: I would rather cut two minutes off of my talk.

(Pause)

MR. HALL: I will start off. The title of my talk is Engineering Design Evaluations. Dr. Okrent gave me a free license, is the way I looked at it, anyway.

MR. OKRENT: Right.

MR. HALL: This gave me latitude to do a number of things, and I really put a lot of effort in this, as you will see.

In answer to your question, to start off with -- How much do we know? -- my answer would be: quite a bit. With regard to how much do we need to know, I would say: a lot more. And with regard to can we learn what we need to know: yes, but it is going to take time and money.

So I am not going to answer the fourth.

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Anyway, I am known by most of you, I think, to be an optimist about these sorts of things, and all of us are in this together, and I want to say at the beginning, don't take some of my remarks as criticism. This is meant to be constructive criticism or introspective criticism because we are operating at the forefront of some of these areas we are working with.

In order to put my thoughts in perspective, I picked up about 50 of my NUREG reports that I thought had a bearing on this subject, believe it or not -- I'm sure the NRC will be glad to hear that -- and a lot of the old reports we worked on in the military field with regard to related subjects, and put together some of my experience on major projects and sat back and put on my tinted, snow-colored filter glasses to take one big look at this subject because it doesn't pertain just to this field. I am also involved in a similar problem in another field which I will relate in just a minute.

It is a great concern to me, frankly, that we are not doing a better job in evaluating the margins as they need to be in connection with these nuclear projects, and you say, margins of what, doing a better job of what? I would say that of quantifying our position on margins in terms of our understanding of what may occur and why it occurs.

So with that, let me start with the first slide.

We have a set of Vu-graphs. Some of these Vu-graphs I will go by very rapidly, and others I will stick to for a few moments, and I will make about eight or nine points. This is kind of an outline of the situation, and my little note up here of "what if" refers to, I think, the charge to the Committee of what if the earthquake that occurs is somewhat larger than that which maybe we have used for the SSE. I think I have the pitch this morning that that is on target.

These are the headings of my sections that I came down through, and Dick Savio has a copy of my Vu-graphs with the background material, and I do have some very pointed summaries and conclusions to make at the end of this particular talk.

When we talk about design, I think about it somewhat differently than perhaps a few of you do, but to me, design means the whole thing, from the standpoint of the concept, purpose, economics of whether you are going to have a product, the marketing, where the raw materials come from, the manpower to make your product, and so forth. And then you will come down to the conceptual form that has to do with somebody worrying about what it is you are trying to build, trial designs to achieve a function of performance, and I have listed a number of other things here with regard to nuclear plants that would be of concern in designing a nuclear plant. These are things that are obvious, and I won't

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even spend the time to go through this except to point out in each case I list components and systems; I don't just talk about components. I think we have spent way too much time in the past dealing with individual entities and not enough time dealing with systems.

Now, at this point I am going to digress for a moment off of my prepared text, and at the risk of sounding a little bit self-serving, I would like to make some observations, and these are not meant to be the way they sound. I think we can be accused of talking to ourselves too much, and I talk to All Ang about this from time to time in the sense of talking to ourselves.

I personally have had the opportunity over the last several decades to work with a very outstanding group of engineers, some of whom are in the room here. Others are dead. Others aren't here today. Some of these people I consider to be among the forefront of designers of the world today, and my experience, as some of you know, has dealt with things like the Alaskan pipeline, nuclear plants, industrial facilities and gas centrifuge projects which are in those type of projects and the military.

And at the risk of making a point -- and I mentioned this military connection before. I will go a little deeper this time. I mentioned this last December, Dave, you may remember, when we were there, and without just saying off-hand

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that I have been a party to a group of designers, a team that over the last 13 years and particular over the last seven years has worked on the design of silos and protective, hardened systems for the MS missile system and related systems, and I can tell you that the design levels that we are now working at range between 5 and 6 kilobars of overpressure. That is about 80,000 psi. And to give you some idea about this -- this is a public number, so I'm not telling something that isn't public. To give you some idea about this, this is about 1000 times greater than the numbers we were working with 20 years ago. Three orders of magnitude.

Of course, associated with that, in terms of talking about the overpressure, are all of the other associated phenomena of thermal, EMP, radiation, on and on and on. You say what does this have to do with seismic margins? Well, my first answer to that was: a lot. Then I said to myself: everything. And you say, what is this all about? Because it has to do with the philosophical basis about how somebody designs something and how somebody looks at margins.

And in that particular sense, I would comment that the designer looking at a system like this there is absolutely no experience at all goes in after getting the criteria and the definition of the environment that is involved, looks at the various trial forms and does nothing but really

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study behavior at the outset, for one simple reason: there is no basis of calculation. We are in a domain where understanding constituitive relationships and other things doesn't exist, so how do you calculate something when you don't even know how to model it?

This is really pure design in the sense of watching these people, the one or two people who are key people, actually formulate what it is you are after and then slowly, from the research and testing program that go forward and the advances in analysis techniques, you start to calculate to check, use this as a tool to get at these adequacies and to ascertain what the margins are.

Now, I don't want to spend a lot of time on margins of military systems except to tell you it is slightly different than what we are up against here in the sense that you pair them closer for the simple reason that your philosophy of what you are after in terms of an operating system is somewhat different.

Now, I know some of you are going to say, well, you are talking about something that is unreal because they have unlimited money; and my answer to that would be I think really in the things I have been involved in and the really cost conscious designers that I have been with, I think maybe they get a lot more for their dollar than maybe we do in the field we are working with here; and that is all I will say

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about the milita / field.

But it does have a bearing on what we are after, and it makes one appreciate what this is all about.

Now, I have a pattern to what I am going to present. I sat back -- and I hope you can bear with me on this -- and I said, well, I am going to go through this thing in kind of steps, and I hope my logic will come out as you listen to what I have to say and the six or seven points I am going to make.

The first thing I said to myself is: why so much attention to seismic, because it is just a subset of the total design process? This is the point I'm trying to make, it is just one subset, and sometimes in my mind it is way overblown in terms of its importance in the total set of things we should be considering.

As I said, it is an unpredictable transient, the responses of which are only relatively well-understood in the case of very strong excitation, possible damage to components that might place the system in a condition associated with potentially adverse consequences with respect to radiation release, effects on health, population activity and so forth.

You said, well, what is this all about? And I say: component design with careful, detailed attention to system design, and I underline "system" again for proportion and construction. Behavior. And a big theme I will go on

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today all through my talk has to do with behavior, because I think this is where we are missing the boat, and the performance in achieving the desired operating safety functions. I gave this definition a year ago December. I said it has to do with the margin of strength with resising and accommodating the overloading, unexpected or expected.

All right, next. So, routinely you say, what is this seismic design process? Let's hone in on the subset I keep talking about? What is it? Usually it involves the simple thing of trying to decide what the loadings are and looking at the resistances -- we will look at this in a second -- and looking at the load combinations, picking out the form, the materials, proportioning, analyzing it, repeating this and going through the normal process. Maybe that is okay and maybe it isn't.

Next, the scope. Examples of this. I have some pages out of my talk a year ago. These are examples of the types of loads, on the left-hand side, that you would be concerned with. I won't go through all of these. I like to think of it as load and resistance, but it can't be kept that clean.

On the right-hand side are examples of things we think about in the resistance field, and we have some of these interaction aspects which are very, very important that Dr. Luco talked about, and I will address this at the end a

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little bit in the sense of soil/structure interaction and how it affects things, the uncertainties or confidence limits. I like to think about certainties instead of uncertainty. I get increasingly interested in certainties, and I think maybe we need to concentrate on that word a little bit if we are going to deal with the public and our knowledge base in codes and so forth.

Next. Then I put up a little picture I also had a year ago about the loads and looking at the combinations of loads the way we put things together, and I have a note that says this is necessary but not sufficient. In other words, in my mind for a really good designer to work for a structure or a piece of equipment or something or even an electrical system, just to take the combination of the loads by itself may not be sufficient. You need to think about the other things that go with it, especially as we go into the sensibly elastic, non-linear region. You just can't treat it that simply. You have to think about what the function is and how the performance is that takes place, what the margins are.

A good designer has margins on his mind all the time. He never stops thinking about margins. Then we go through some of the other process involved, and at the bottom I list some of the stress and strain rations that are often used in the sense of evaluating margins. But again, I think these are too simple, too, in terms of convincing me that the margins are adequate.

Next. Now, I will concentrate for a few moments on the loading column in my simple-minded way, and I need some input from a few people in the audience to finish this, I think. I said, well, let's look at the loading business for a minute. We work with acceleration, velocity and displacement routinely, the peak values or the effective values, and then we turn around and look at response spectra, which we will address a little later, and we try to do something about how this fits in with damage and field observations. We do not do a very good job on why things have not been damaged.

Of course, some of us have been hinting at this for some time, that we should be doing a lot more examining of what it is that makes things work, and incidentally, this is one of the keys in the military field. They spend a lot more time thinking about why things work than I think we do.

Now, the bottom part we will come back to, Bob. This is the one we are going to switch. So I will go through these very fast.

I said, well, because a lot of my research deals in this margin business and I am working on the seismic and military area, I thought I would go back and take a little revisit at some of these expressions about peak acceleration versus modified Mercalli intensity. I see a plot like that,

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and then I look at the next plot, and I look at that and I say, of course that really comes from something like this, which is Ambrasseys and all of the earthquakes through 1973 that were of any significance in the world, and that makes a believer out of you.

You say, well, there is kind of a problem here.

(Laughter)

You have got it, job security.

Here are some plots out of the Corps of Engineers waterway experiment, the station report that Perninsky has drawn for someone that plotted Drafinian breeze curves and some more modified Mercalli data for acceleration and velocity displacement, and you come away with the same feeling, that there is a lot of spread, although admittedly the modified Mercalli scheme for measuring the damage is not the greatest, for a lot of reasons that we have put down in writing many times. It doesn't differentiate between in Boston whether you are talking about three buildings with chimneys down or 3000 buildings with chimneys down. It doesn't tell you whether the chimneys were built in 1870 or 1970. It doesn't do a lot of these things.

But nonetheless, when you look at these spreads you see factors of 10 or more in these spreads. And then I thought, well, I will go to some of the western states. Now, this is an interesting plot. This is modified Mercalli across

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the bottom and magnitude up the left-hand side. And I said, wow, this is very interesting because down here at -- if you go up here about magnitude 5 or 6 in here, you go from, like, MM-5 all of the way up to an MM-11. So that somewhere goes -- let me look at my plot for a moment -- that might go somewhere in terms of acceleration, which I don't care particularly to use, but it might go somewhere from, like, .02 g up to more than 1 g, like a factor of 50.

Oh, that's okay. That's almost two orders of magnitude. And the next one. And then I thought, well, I will look at an eastern earthquake, and I looked at this New Hampshire series and I get the same feeling, this is a little different. This has to do with modified Mercalli on the left, and then we will go up around things that are 5 or 6 in there, and we see all of this. Look at this variation, in 5 or 6 or 4, even, that goes across from 10 kilometers all of the way out to, goodness gracious, you name it, out to at least 400 or 500 kilometers, a given damage that goes across the board.

Well, you come away with a funny feeling from this. You say, this is not going to help too much, perhaps. So I said, well, let's go another route. So I went back to the NUREGS and I went into one by Vern Reiter. This is Volume 4 of the series -- what is it 1582? In the back is the TERA plots. I think this particular thing is out of a TERA plot. I took this plot and I had some stuff from Oak

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Ridge and I had a few things on the West Coast. Al, you have got to live with me here. And I said, let's look at these for a minute and see what this is all about.

I said, for example, if the return period for the SSE is somewhere between 1000 and 2000 years, that is maybe. Somewhere up in there -- the central value is the center line, and this is somewhere at about, maybe it is about one-tenth of a g, and this is for Dresden now. This is the interesting thing. I've got a couple of things for Dresden, which is near home. See, I'm sticking near home. And I said, well, that is interesting.

The median value is about .12, and that's not news to those of us who have worked on it over the years, and the design for the SSE, as we know, is up around .2. So it is on the upper side.

And then I said, interestingly, well, what happens if the return period is 10,000 years? I've got my snowglasses on, now, you know, and I look at this 10,000 years. Why did I pick 10,000 years? Because I'm not necessarily a believer in 10,000 years. I want to go on record right here being sure that everyone in this room realizes I'm not sitting up here preaching that 10,000 years is the return period we should use.

It turns out that Dr. Okrent is the one who started me on this.

(Laughter)

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And at the Vallecitos hearings some x years ago, three, four or five years ago, and it has been a long dialogue which is still going on.

But anyway, let's use 10,000 as an example. So you say, what happens at 10,000? I say, well, that is interesting because it goes from roughly .12 up to about .22 on .8 times or something like that. So with that piece of evidence in mind, I said, now I am going to go back and look at some of the tables in the front of the same NUREG report where the experts had given their opinions. And I looked at these things that had 200 years, 1000 years, 4000 years.

Now, I have kind of cut off the left-hand side, which talked about the 2 sigma truncation and the 3 sigma truncation. It's all in there and you can go into it if you want to. And I said, look at these numbers; from 200 years to 1000 years it looks like it roughly doubles. It depends on where you are in here. That is the acceleration. The velocities look like they go up two and a half to three times, on the average.

I said, well, that is very interesting, and then I looked at the next jump, from 1000 years to 4000 years, and you see things again and accelerations that are on the order of 1.6 to 2, something like this, down through here. And you can see where I am coming from, I think, already, and

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down in the velocities it is more.

So, on the next plot I said let's go back and look at some of these attenuation expressions, and I picked at random one by Donovan and Bornstein, which was in the IDRISS paper in 1978. It already had the numbers and I didn't have to calculate very long. So I looked at magnitude of 6.5 and 7.5, and I'm looking at these accelerations on the right-hand side, and I see that somewhere out here, 10, 30 and 50 kilometers, these numbers go up about a factor of 2.

So I said, well, that is kind of confirmatory too. In other words, to get up to this big return period, I kind of go up one magnitude and the accelerations kind of double. In other words, what am I doing here? I'm trying to see how big is big.

You know, we talk about margins. I am trying to get some feel as to what kind of numbers we are talking about.

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Then I looked at the energy down here. I looked at that for the simple reason that in the work I'm doing in Illinois in my research -- I won't take the next two hours to tell you about this, but we are looking very heavily at some of these damage mechanisms, trying to unravel what it is that might take place, and it's predicated in large amount on this energy concept. You know, we can do a great job in kind of estimating the quasi-sensibly elastic range. We can make some estimates of the collapse range, but quite frankly, in between we are in no-man's land.

So I look at these energies, and there is nothing new here because I use one of the later expressions that Richard pointed out, that the factor 1 magnitude, of course, is a factor of 30, which we all know. But that doesn't help us much in the sense of going ahead.

So we come back, then, to the last slide, Bob, the bottom part of the last one, which says what did we learn out of all of this? What I learned was the following.

I learned if you go from an SSE that might cover around 1000 to 2000 years in return period, and I went up to 10,000 years -- and I'm not subscribing to that number. Again, I wanted to get some idea of these margins, what happens. I see the acceleration approximately doubles. I see that the velocity approximately triples, and this bothers me a little bit. That is what the question mark means, because I don't

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quite understand why the velocity would triple. It tells me that there may be some problems in the modeling or something is going on that I don't quite understand, and I think this needs to be unraveled a little bit.

At the moment I have a funny feeling that there is a consistency problem. I can't identify what it is, but I point this out to you experts here, and I found out the magnitude went up +1, and it kind of gave me a feeling as to what we are talking about, and that is the first major point, maybe the second major point I want to make, and I will stop right there and let you think about that as we go on.

Different subject. New subject. I go into seismic excitation effects, and I say what are these effects, now, that we are interested in from a design point of view? Well, first the usual inertial effects, transient, translational and rotational effects, relative motion, faulting if it affects things we are concerned with, and then we have subsets looking at strain and local yielding and significant deformation.

Local yielding does not mean we are in trouble. That's the point I'm trying to make. I'm not even sure how we measure significant deformation. Then we get into crushing. As we start to get into the crushing domain, we are probably in trouble. Stresses are inferred, and as a research type in a university where we work with laboratories and experiments and calculations, I get a little paranoid about stress at times because I can't measure stress. I can measure strains, I can measure deformations, but I cannot measure stress. And this business of using stress all of the time as an indicator to me. So I look in here, trying to rationalize also what is a primary stress in the sense of a direct loading as defined by some of the documents, and secondary stresses, which could be self-relieving and so forth, to try again to measure these margins because in some cases, if they are really self-relieving or of a secondary type, then you can think about them differently than you would if they were of a primary type. And of course, this is a major part of arriving at some judgment about margins.

And then I have listed down here some of the other things, about the gross things I would worry about, buckling, fracture, rupture, pressure and temperature transients, thermal shocks, connection failures and so forth. I underlined electrical down here, structural, mechanical and electrical, because I made a few statements this morning about some of my concerns in this field. Through one of my daughters marrying a power engineer and some other connections in the family recently, I think I'm in a situation of learning more about power engineering than I did before. In fact, maybe I have learned a lot of things I don't want to know.

So I am getting increasingly interested in the electrical field as regards vulnerability and margins also.

Some other examples. I will go through some examples here of this. I picked this out of a NUREG report. These are the kinds of things that interest me. All of these are referenced, incidentally. I have the references cited here, and anyone who wants to know the source of anything I have shown, we can get it to you immediately. I don't know what the design pressures were for design at Indian Point. That's what the question mark is there, but I don't know.

MR. SIESS: 50 to the 60 psi.

MR. HALL: They are 50 to the 60 in both cases? That's what I kind of assumed. And it was interesting to me to find the first concrete cracking and predicted failures and so on. These numbers were not as high as I might have expected. That's why I put this table up here. I looked at it with my filtered glasses and I said, my golly, these margins, if these were real, what was the mode of failure, what was the behavior that led to these numbers? And as far as I can tell from what was written, it was reaching some critical shear stres at the junction of the side of the containment wall with the base, and that discontinuity would be the one I would expect would have some difficulties, so

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it's not surprising.

But these factors are perhaps a factor of 2 to 2-1/2. They are not as much as I might have thought. And the point I want to make here is I wonder if as engineers, the type of design engineer I described here ten minutes ago, if we started from scratch and were designing this today, would we use that geometry? That's the question I'm asking, because with our concern about margins, maybe we would use a different geometry.

All right, next, quickly.

We go back to some of the old tests run in the mid-fifties when I was a graduate student looking at things with impulse machines and so on on beams.

Next slide, Bob.

And we find results for steel beams. That shows you some of the rapid load tests on steel beams, predicted within reason various frames. These are very simple frames that gave us some feeling that we knew a little bit about what we were doing.

Next, a plot I showed a year ago a little bit about some of the concrete, reinforced concrete beam tests, in which Professor Siess was eminently involved, showing you the effects of the reinforcing ratios and so on and what very large differences these can make in very simple elements like reinforced concrete beams.

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This is an eye-opener. It shows you that you really do need to understand the behavior if you are going to get differing degrees of deformation in the energy source. You say this is all so simple. Of course it is simple, but it is the only kind of stuff we have available on which we make up our codes, our rules and our regulations, and it is this kind of stuff we use in the design process. This is the basis for what we do in design, especially as we go forward into the non-linear field, which is not easily handled.

All right, next.

Here is a plot out of a 1972 report by Boris Bresler detailing some of the shapes of concrete and compression, depicting what happens in terms of the overall behavior and depending upon the confinement of the member. We say, well, we know something about confinement. We know what confinement does. But sometimes it gets difficult to define exactly how it is handled.

Next, and another one out of a report, a paper that Loring Wyllie up here in San Francisco wrote talking about some of the observations in earthquakes and some of the details on shear walls, how important the edge effects are in terms of making shear walls work properly. Again, some very important critical details that dictate the behavior of the element and the system under earthquake excitation. The designer that doesn't understand the behavior and merely

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goes at it with a cookbook is, in my mind, in deep trouble, and we see evidence of this in earthquakes constantly.

Next I went back to the NUREGs again, the good old NUREGs, and I picked out -- I think this is out of Bob's shop, in fact, a couple of them, and I picked this out for description in the sense -- I think this is Zion, if I'm not mistaken. It has to do with the types of things, and I won't go all the way through this, but it has to do with the types of things that were identified as being problem areas, and from the margin point of view, you look at it and you look at the damage described on the right side. The damage is listed as damage. It doesn't say failure. It doesn't say collapse. It says damage. And you just have to say to yourself, how significant is this? Is this something that you can tolerate? Is it localized or is it something that's going to render this plant inoperable?

That is a very good question, and the PRA studies that are going forward attempt to identify which of these things plays the major role in the process. As I recall -- Bob can correct me, and John Reed, too -- one of them was this service water pipes, was one of the major factors, wasn't it, Bob, in terms of dictating the margins in that particular system?

But the point to this, standing back with my filter glasses on, was that there are a whole lot of things.

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And you say to yourself -- and I don't mean to be critical, now, this is the whole point -- you say to yourself, did the designer or the designers of this plant stand back with their filter glasses on and look at this whole system and try to balance it out as best they could in terms of where they were and understanding what is going on?

The next plot is a similar plot -- I'm not going to spend any time on it at all, in fact, I will go right by it -- having to do with the turbine and auxiliary building. You see the same sort of things, only they are different, and this makes good sense, this is helpful, but it doesn't exactly quantify the numbers.

Then we get down to some equipment. In the equipment area, I took out of another -- it could be one of your reports, Bob, I'm not sure. What I did is I took the list of the items, and on the right-hand side I had my secretary pick the words that I underlined in the sense of what was the problem or what was the fix or so forth, and I come down with a list of things in the sense of for the particular evaluation that was made of some of the problems, some were insufficient information -- interesting -- some were "okay." "Okay" doesn't tell me much about how much margin was there, but that wasn't the point to the study, so you can't really fault it for that.

But again the point here is it lists a whole lot

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of things which may be separate items, they may be connected in a system, and the point is that from a margin point of view, we really should look at these from systems, multiple systems, redundant systems, and decide, of course, whether they are really important.

I mean some of these things probably are not important because they are not, probably, in the mainstream of things that are inside the pressure coolant boundary that would be required for safe shutdown and cooling, but again, it is an indicator.

All right, let's go on. The same thing. Let's go right by it, Bob, and go to the next one. We are getting toward the end here.

So we come back, then, to one more subject. That was to kind of put that to rest. We talk here about seismic loadings, again talking a little bit about transient motions. The transfer function, of course, we use is response spectra. It is a transfer function because it works around the earthquake citation and works with structural elements, so it is clearly a transfer function, and response spectra have many limitations. Unfortunately, a lot of engineers don't realize what the limitations are. A lot of us are working on trying to do a better job of reflecting duration, energy input, dissipation, yielding, reversals in motion, number of cycles, the types of cycles. We know that all of these things affect the damage and the margin that is left. We don't understand these things.

We go to the next one, examples a little bit about elastic spectra. We so blithely work with, first of all, magnitude as a God-given number. I don't know how many hearings I have been in other than NRC where the hearing judge treats the magnitude as a fixed entity. It could be 6.12 and that's it. It's not 6.4, it's not 5.8. The magnitude is 6.14, and yet he gets into this part of it, well it's all fuzzy. You have to be a realist about some of these things, and when you know where spectra come from, here is a case of what is this. Kern County fills out the Newmark-Hall spectra a little bit. Here is the same spectra. Here is another earthquake. It doesn't fill it out at all. So you have to be a realist and say, oh, well, these spectra are something on the average, of course, and sometimes they have a margin and sometimes they don't.

And then we go to the next plot, which is the last spectrum plot, and we look at this depiction of an elastic spectrum, and we look at what you call, some of you call the inelastic spectra, which I prefer to call the modified spectra to take care of inelastic behaivior, and you treat that as if it is something that is really well-understood. That is the bottom dark line here, a spectrum plot for acceleration and yield deformation for a ductility factor of

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3. I see these things used blithely in practice, and I asked a few questions and I went into class four weeks ago and asked my class of very bright students, of course, to design me a little system that had a 40-pound weight on the top of it and was 24 inches tall. I gave them the shape of the yielding mechanism, and they quickly found out that the frequency shifted and then they began to worry about the fact that they really had a problem of defining what the yield level was because -- and a lot of engineers don't realize this -- when you use these things, unless you really know what the yield or course limit level is -- if it's too soft, the deformations are going to be 20 times what you estimate. If the system is too stiff, the forces will be 10 times what you estimate there. These things are only useful when you really know how the element behaves.

Very few engineers understand this. Believe me. I can attest to this from being around the companies where they use these in practice. It scares me to death, and I am one of the ones who helped originate this.

So we go on here looking at some of these seismic resistance parameters. We have some of our evidence that has to do from past testing with monotonic-type loadings, whereas in earthquake effects, of course, we have reversal loading. I have listed here some of the other things that enter into the process here. I have fatigue listed, which we have

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discussed briefly today. Then we get down into this middle part where I talk about damage or lack thereof. And for those of us who work with the behavior of systems that undergo large deformations and so on, you go through a regime that is characterized by sensibly elastic up through something which yields. These may be compounded by the fact that the shear deformation in this element occurs first, followed by flexural yielding at a later time. They don't occur together. They are out of phase. Followed by a loss of strength, the dip on the right-hand side, followed by membrane action, which takes over if, indeed, the structural system is made in such a manner that it can take membrane action. This is important. It can't do that unless it is made so it can do that. Unless you understand the mode of the behavior, the point all of this is about, you really cannot calculate, you really cannot predict what is going to happen.

This is typical, incidentally, of some of the things we work with in military systems. They go out to these very, very large deformations, and the predictions of where they go are absolutely ridiculous unless you understand this type of behavior.

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The next slide has some funny pictures on it to remind me of some things. Let me give you one more example of the funny types of things. This is closer to home. We go down here with components, structural systems, mechanical systems and so on. And then I am listing things down there, and I have columns and walls and floors. And I show two wide flange sections.

This comes about from some studies that I have been involved in recently at an unnamed location having to do with some very heavy floors and load drops, and you read in the textbooks and look in the codes and you read about plastic design and limit stress and limit analysis, and everything assumes that the beams can develop their fully plastic moment.

And then for one who worked in this field a good part of his life and wrote his doctoral thesis in this area, I said, wait a minute, hold tight, I said. All of the research that was done from 1946 through '77 deals with wide flange shapes of a geometry in which the flange width is approximately .7 of the depth of the beam, and here we are working with beams in a plant that is a built and exists in which the flange width over depth is, like, .2. I said, what makes you think that all of this textbook stuff works for the right-hand beam? Oh, well, nobody said it didn't, and you just very quickly look back and you say, well, you

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see, you have to know what you're looking at, and you say all of the behavioral information in the books deals with the left-hand beam, and it is only in the last two or three years that Dr. Popov at Berkeley, in connection with his eccentrically braced beams, has started to with with sections that are of the right-hand portion.

As you expect, all sorts of things come into effect in terms of non-linear behavior, local buckling of the flanges, local buckling of the welds, tension fields, et cetera if you are going to have any significant deformation at all.

The point to this is if you don't understand the behavior and you blithely go through the calculational basis, you are in deep trouble.

With that, let me look a little further here. This leads next into the analysis business. I am a big believer in analysis. I have great reservations about it. At times it is a very valuable tool, but if you analyze things in a manner in which the input and iterative parameters are not meaningful, you are obviously not going to get things out that mean things. I will comment on this in a minute.

All right, we have two sheets to go. We come down, then, to the bottom line, and I have this as kind of a summary now. You say to yourself, margin of safety

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evaluation, what are the things that we really need to think about among some things that are being discussed here, and I left out -- well, I started at the beginning. I said, you need to be very careful about which the critical sections are. If you are going to look at a system, you have to meticulously find out what the system is and all of its elements. You don't just look at the pipes and pipe supports. You also have to find out whether there are sensors in the pipe and whether the welds are of good quality and what the connections are and what else hooks into it; if you get a leak in the pipe, will you lose pressure at other places, and so on. You need to look at the redundancy of the system.

I think a lot of our examinations of systems have not really been done that thoroughly, and perhaps some of the IDVC studies that are going on at the present time will help in unraveling a few of these systems. I have talked about unloadings. There are ways of testing systems.

Now, this comes about out of NUREG reports and so on. I have listed in my collection of Vu-graphs a lot of references that have to do with, for example, testing of systems, a very valuable part of drawing some judgment about margins, field observations and measurements. We will talk about this in just a moment more.

We don't have many of these. Those we do have are extremely helpful. We have large-scale tests. Some of you are aware in Japan at the present time we have a seven-story

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reinforced concrete building that has been tested by pseudo-dynamic force technique and a six-story steel building, and these will shed a little bit of light, not a lot, just a little bit of light. They are very expensive, very expensive. We should do more on laboratory tests and models. We just have let that go in recent years pretty much.

I put the soil/structure interaction under analysis. Dr. Luco has talked about this. I consider this to be a very, very important thing, and I am hoping that measurements that are made in the field, perhaps in Taiwan and other places, will help us shed more light on this.

And then, of course, contrary to what you may think after all of this, I do have a great respect for the risk analysis material. Now, for the last page, what do I think about all of this. Well, hold on. I need another page here.

I have listed four major things that after all of this synthesis, it seems to me how do we do better, what can we do to quantify these things in a better form than exists? Maybe we can't wait. Some of you sitting here say we have got to do it now. Well, if we have got to do it now, we have got to use everything we have available and we will address this.

So the first item I have -- you see, this is a university professor talking -- item number one is research.

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It has to be. There really was no order to this. I just listed them, one, two, three, four, after much thinking. On the research aspects, I would offer the following observations.

I think that we need a range of studies more than we have at the present time. I think some of these might well be under joint sponsorship. I am talking about like NRC and NSF, or NRC and whoever, industry related and so on, keeping them as unbiased as possible. I think there should be more small studies. I know the research group has several very large studies that I know of under way. Hopefully, these will have and will point up very valuable information, but I am kind of a believer in making small steps with a lot of people, some cross-checking. And if it is carefully managed and carefully overseen by the proper peer process, it seems we get further faster in a given period of time.

And my observation is that there has to be some really careful insight as to what is sponsored, the interpretive aspects. That's what I've listed in there. I think we need a little more of the interpretive aspect, but we should not be too, too fixed on everything being applied at the moment it is done. Again, falling back on the military field, it is really peculiar, but the things that have paid off, in many cases, it seems like every five years something pays off and it is something that was not specified in the

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research that was undertaken previously five years before, it was a spin-off, something basis, something fundamental, something different that came out of it. Bootleg research, in some cases.

Well, what are the types of things I have been talking about. I will rattle them off to you in order. Of course, I think there should be more research on loading, partly of the type we have been talking about here in terms of what makes up the excitation and the total environment, and the combinations of loadings that go into the excitations that have been discussed in various talks.

I think the soil/structure interaction needs a lot of work. Dr. Luco's work is indicative of the type of work needed, and I think it needs a lot of work because it is a very intrinsic part of the process that goes on in an earthquake. There needs to be more work on modeling. We blithely model things and rarely do we study the modeling process. We just take it as a given fact that you model it this way. I am not so sure as I get older that we are as smart as we think we are.

Floor response spectra. I personally have big questions in my mind about some of the floor response spectra that I see, the makeup of them, the shape of them, the amplitudes of them. I can't resolve in my own mind that we have done enough experimental work to even verify in the

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simplest cases that they look like we think they do, and this is an area in which I personally may become involved very shortly. I will just tip you off to this.

Field observations. We have a certain number of instruments out there. Are they in the right place? Will they measure the right thing? Many of us who work around the earthquake engineering field struggle with this constantly, and I will come back to this in a moment. This is a very vital point and the Academy is facing up to this, and I will come back and talk about the Academy of Sciences in one second.

I think we need more lab tests and full-scale tests. Now, full-scale tests eat up lots of money. They are very limited in what they can produce, but we need some of these. I think we need a lot more laboratory tests to build upon to get to that particular point.

In the fragility area, I think we have talked about this this morning. I won't take time on this. I think we have identified a whole lot of things that we could afford to explore additionally in fragility, and my thought here is it isn't just a matter of looking at each entity by itself. I am becoming increasingly concerned about looking at the fragility on a system basis. I think we are maybe overlooking some very important things.

The key to all of this, the bottom line that I am

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getting at has to do with behavior, understanding behavior and looking at individual systems and the system as a whole. The two words are "behavior" and "systems." Now, let me come back to item 1 for a second. Let me come back to the Academy of Sciences.

I may address this more in some comments, but most of you know in the National Research Council there is a committee on seismology, of which Paul Pomeroy is the chairman at the moment. There is a committee on earthquake engineering just formed, of which George Housner is the chairman. I am a member.

We are charged with various things, and it is a little bit awesome. The committee on which I am, and I am chairman of a group to look into how can we do a better job of selling Congress on the fact that we need money to do research, not just in structural engineering, but we are working with all of the agencies just as much as the other committee is. How can we keep those instruments out there?

Many of you may think that these earthquake-recording instruments are just going to stay out there being available to record earthquakes and we will get records routinely. Fellows, it is in trouble. The programs are in trouble, and the trouble is money. I could list off here a dozen things like this, and dollars are the factor, and I would be interested at your convenience in talking to some of

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you at some length about what are the things that we can do to convince the attorneys that make up Congress that this is an important field and that we need funds and why we need funds and so on, and I can't think of a better group to say this to because in my estimation, many of the advances in earthquake engineering have come about from the application of research in the group sitting in this room today. I mean this is where it is. This is where a large part of the stuff at the forefront has occurred.

Second item. Synthesis studies. I think we could afford to, in terms of margin studies, do some things we haven't done. I think we could afford to have some contracts let for small synthesis studies made up of a broad spectrum of people at universities in practice, testing laboratories, non-profits and so forth, to keep it unbiased, to look and try to describe in a better way the things that we know about the behavior of certain of the elements and systems, the analysis techniques for those elements and systems, the design aspects of those, and put it in a form in which it can be read by the average practicing engineer.

In other words, it doesn't only have merit with regard to those of us sitting in this room. These things should be of a form that practicing engineers can read and understand, and I emphasize "understand," and maybe in this way we could influence practice as we go along. I really mean

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it. I am getting concerned about what I see out there in the real world.

Item 3. Engineering design and evaluation. This is where I told you it will be a little bit introspective. As a professor and a consulting engineer, at times I think -- I will put it this way. In the last several years there are some cases in which I have really been embarrassed, I am embarrassed for my profession, in the sense that the quality of the work in some cases that I see, the nature of the way the calculations and designs are carried out and communicated on paper, the way in which calculations are communicated leave me cold, and if it affects me that way, how in the world does it affect other people?

I think that all of us could really afford to think a little bit about the people who work around us and how we do our own work, how we interpret it to people, and I have listed management at the bottom in the sense that my observation -- I'm not talking about top management, necessarily. It starts at the top, but it has to do with the middle management and layer two above layer one and so forth.

If management doesn't really understand what is going on, how do you expect the worker to understand what is going on? What I am getting at is in many cases I see very good work. I want to paint the other side. And in all cases when I look into it, I find out it's not just the engineers who understand what's going on in terms of the behavior and the components of the system; the managers do, too. And in those cases, the products are beautiful. Some of them are really beautiful. But there are other cases that, frankly, embarrass me, and that is all I am going to say about that.

You say, well, what is he talking about? He just completely ignores the cost of all of this. Well, no, we don't. Those of us who are out here doing some of this are very cost conscious, and I think that in this particular field, we need to keep cost at the top of our minds at all times

The last thing, very shortly, and I could spend the next hour talking about this, is construction practice. What I am trying to get at here is if the subsequent construction and the quality of the materials and the work that is put up doesn't meet all of these other things we talk about, all it takes is a few days of the constructor to undo everything we have talked about.

I don't care how well it is on paper, how well we have conceptually formed it up, how well we have designed it; if it is not constructed properly, it is all for naught. And I mentioned cost control because that is a big factor there as well as inspection and so forth. Well, I haven't put any numbers on a lot of these things, Dr. Okrent, but my mind has gone through a logic here and I have tried to paint in my picture each time I go a little deeper about what I think the process is, and I think if we understand the philosophy and the process a little better, maybe we can quantify some of these things in a better way.

Thank you very much.

MR. OKRENT: Comments or questions?

(No response)

MR. HALL: Well, let me ask a question.

MR. OKRENT: That's fair.

MR. HALL: Of Bob Jackson and some of your colleagues. Let me go back to the loading business, with regard to how big is big in terms of earthquakes.

Had you looked at these numbers? I mean I haven't seen it ever put up quite this way before, basically. Do these ratios that I have listed up here, like doubling acceleration and so on, make sense, roughly in terms of the things you have thought about with regard to the levels? See, even if you don't pick a 10,000 year return period, suppose you said it should be something bigger. Is it good enough for something bigger?

Well, let's don't predicate it on a return period, necessarily, but maybe we could. Suppose you said it should be something larger than the SSE. Maybe it should be 1-1/2 times larger than the SSE, whatever that means. I don't know

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at what level you gain the confidence that you really need, and it occurs to me -- I guess let me make a comment, and some of you probability guys can comment to this. It occurs to me that one of the things I didn't mention at all, of course, is in the 50-year life of a plant or something like this, if you go to this larger earthquake and you really knew you had a margin to achieve it, do you get the probability of exceedance down to a small enough number that you are willing to live with it? And again, I don't know.

As Dr. Mark was alluding to, I don't know how small that number should be. If it's on an annual basis, something like 10^{-5}. Then, of course, on a 50-year basis it is something bigger than that. But it is hard for me to quantify what is okay, and I will stop there. I mean it is a question. That is just one little point. If an engineer is going to design something, you really have to have something to target to, and we are talking here about a target in terms of numbers.

Okay, that's it. Maybe Dave wants to comment on it. I don't know.

MR. OKRENT: Mr. Jackson, go ahead.

MR. JACKSON: I don't have a response. I'm not sure how to respond to it. I think the initial observations you have made are correct in terms of the ground motion. There are other people in the room who know more about the

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velocity. Leon, do you know the velocity question he raised?

MR. REITER: In that particular study, estimating velocities is one of the more difficult things we have. Some people are predicting very large velocities of distances at least in the U.S. It is problematic.

MR. JACKSON: It is a subjective judgment.

MR. HALL: I think it was biased by the eastern experience, probably.

MR. REITER: Robin, what is --

MR. HALL: Alan has a comment.

MR. CORNELL: What you say about the doubling or tripling is, roughly speaking, right generally. This results from the fact that no matter where you are, east or west, the rate of attenuation of velocity is slower than that of acceleration. Therefore, more seismic sources are contributing to the risk of exceeding a particular velocity level than they are to exceeding a particular acceleration level, and the net impact is that you don't go up proportionally, you go up in terms of acceleration velocity, you go up by -- you increase your return period. You tend to go up faster with velocity.

Another way of saying it is the shape of the uniform hazard spectrum changes with probability. You tend to move up faster in that velocity.

MR. JACKSON: Bill, in past hearings and the like, you have been a strong advocate that substantial margins do exist in these nuclear power plants, and you have made a lot of observations today that would tend to erode that, I think.

MR. HALL: I didn't mean to, particularly. Well, go ahead.

MR. JACKSON: Is that the case or is what you are really doing a call for, say, a more focused, synthesized description of what we do know?

MR. HALL: I think that is right. I think you said it. It was that business, item 2 on my last there. I think we could afford to try to -- in some cases, I don't think we can put numbers on some of these things. We can infer them from things we have done in the past that have been done for other applications and so forth. I think it is possible to infer what some of these margins are. It is very hard to be clear about this.

I remember Nate Newmark in some of his papers with Alan, I think, you fellows made the pitch that the safety factor -- you used the word "safety factors" in terms of buildings in the simplest form were something like 3 or 4, if I am not mistaken, Alan, in terms of what the designs were versus where you thought you would get in trouble.

Is that a correct statement?

MR. CORNELL: (Nods affirmatively)

MR. HALL: I think those things still obtain. I

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am not arguing that these things are not correct. That by itself, to me, is not a complete descriptor of where as engineers we would be willing to decide that something was getting marginal in the sense of it's okay or not okay. It might not be you go all of that way before you were concerned that you had some problems.

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You see, I am not sure. I think that may be a little simple as a descriptor, but I realize it was done for analysis purposes, and it is a fair observation. Let me back off and say one more thing.

We find many, many times, in terms of things that we design, coming back to the military field again -- that's where this came from, in part -- in the military field we go back, we have some criteria, we design something, we go test it. We will find in many cases that the strength -- and I use the word "strength" -- the strength is three to four times that which we thought we were targetting when we went into this, for many, many reasons. It has to do with the materials are stronger. Things can take more strain than we assumed. Membrane action comes into effect, and we hadn't counted on membrane action, et cetera, et cetera, these types of things.

But you cannot always count on these sorts of things.

MR. SIESS: Well --

MR. HALL: Wait, let me go further. At the other extreme, I was describing this problem with floors with I beams, et cetera. The weak link in that study, which was obvious five seconds after you look at it, was the connections. The connections had been designed properly. I don't want to make any statements other than that. The connections were absolutely properly designed to carry the dead and live loads that were anticipated at the time the designs were carried out. Absolutely no question.

On the other hand, when you started to talk about extreme loads, they were inadequate and you had problems.

Okay.

MR. OKRENT: Dr. Siess.

MR. SIESS: Bob just asked a question about substantial margins. I think we could start out by agreeing that substantial margins exist, and then we could spend the next two days discussing what we mean by substantial, and what the margin is against. So a statement that substantial margins exist goes into the glossary, or the non-glossary. I don't know which one.

MR. JACKSON: (Nods affirmatively)

MR. SIESS: We don't know how big they are, we don't know where they are, and we don't know what they are protecting us against.

MR. OKRENT: Is this a round? I guess that's it. Thank you.

Yes?

MR. LIN: C.W. Lin from Westinghouse. I would like to ask Bill: You mentioned you have some concerns about floor response spectra. Are you concerened about the magnitude being too conservative or less conservative or what?

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MR. HALL: I frankly don't know. I get nervous about -- well, several things make me nervous. The first thing I get nervous about is the very strong frequency dependence of some of these, too narrow-banded, I guess is the way I would put it to you. In terms of the way I think things can happen, if the whole structural mechanical system is excited, I think that the calculations -- this is just a feeling -- I have a feeling that the calculations pinpoint the frequencies of response too finely. That is just a sense from working in dynamics all my life.

As far as the amplitudes -- look, I'm the first to admit that some of these amplitudes can be very high. I think we have a little bit of evidence that they can be high. Now, I would counter that by the fact that even though we look at these fragility studies of the type that we were discussing this morning and others that Kennedy and others have looked at and so on, and the military has looked at and so on, I didn't show this plot. I have one, a sheet that Bob used last year, which I looked at. It was very interesting. The breadth of some of these fragility levels on some pieces of equipment is incredible, as I would expect. I can remember it was, like, half a g up to 60 g, something like that.

You will find a class of equipment that has this kind of breadth (indicating) if you measure it by

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acceleration. I personally have said many, many times I don't think acceleration is a very good measure. I get in this constantly with hearing judges, particularly, who keep asking me about this, and I take my pen knife out and say, isn't 1-1/2 g pretty big? And they say, well, what's that, what do you mean? And I said, well, that's probably about 50 g. Nothing happened, did it? I make a point. That is kind of silly, but the point is that acceleration by itself doesn't tell you the whole thing. That's what I'm getting at.

I think my thinking through a lot of this process, to answer your question, is it's kind of like the modeling thing I identified. I'm not sure we have our handle completely on the descriptor for the behavior. I'm back to behavior again. And that is what I am really alluding to.

And maybe we are being, to answer your question, it just might be that we are being -- I don't like the word "conservative." It might be we are being too safe. I don't know. We may be, frankly.

MR. LIN: In terms of the frequency being too precise, but on the other hand, in our soil/structure interaction analysis we have to cover the subject of the soil, and on top of that, we have plus or minus 15 percent brodening. Don't you think that should be more than sufficient to cover that?

MR.HALL: I don't know how to answer that question.

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You see, the thing Dr. Luco was presenting this morning, which interested to me to no end -- I will see part of the study later -- I was very interested in the very narrow frequency response of the systems response he showed you this morning. I mean if you looked at those and remember them, they are very specific and very focused, and I don't know -- he identified this correctly and I'm sure it is a subject of study. I can't answer your question directly. I understand what you are after, but I don't know the answer to it.

MR. LIN: Okay. In your study you more or less mentioned you are going to do in this area --

MR. HALL: I hope to do.

MR. LIN: Okay, you hope to do. What are you planning to do, test the model or what?

MR. HALL: Maybe some of you could elaborate for me a little more. As I look through the literature, I don't find from what I have looked at very much experimental evidence, observational evidence, a lot of it to support a lot of things that are given through analysis as being the way it is. Am I wrong?

Let me ask a question. Is there more evidence out there than I think there is, or am I wrong?

MR. LIN: You are correct. We really don't have that much evidence to speak of.

MR. HALL: And I guess I was trained by a great engineer who was a believer in simplicity, believe me. His idea was if I can't understand something in a simple manner, I'm sure I can't understand it when it's complicated. That's what I was raised on, and it just seems to me that we could afford to run some relatively inexpensive, duplicated in a way -- several people do it -- simple experiments for us to gain confidence that even in simple cases, we can model and calculate what we see. That is what I was getting at.

MR. OKRENT: I think we will have to go on now. I'm sorry. We are running late. Thank you, Bill.

MR. HALL: Thanks.

MR. OKRENT: Spencer Bush is next.

MR. BUSH: I hope to address Bill Hall's item on systems, with which I agree completely with regard to the handling of an item such as fragility. And two, I think I will make a disavowal. I feel like I am completely out of place. By no stretch of the imagination have I related to seismic at any time, and as a result, I am simply cribbing mostly Bob Kennedy's data, as you will see, and I guess what I will try to do at the end is make a message with regard to the possibility of sources of information which probably aren't commonly know to this group which might be applicable with regard to certain components.

Well, there are some of these sources of the information I would hope to use, and of course, the first

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thing you ought to know is what you are talking about. I guess this would be one definition. I suspect if we were talking about glossaries, I have a suspicion one wouldn't have to look too far before you could find other definitions with regard to this. This certainly realistically describes what we will be using in there, and of course, the item that I think would interest me as an individual, of course, tends to be the particular application which we would see in here (indicating). We are talking about either the component as such or the behavior, either in a failure context or in a loss of function context. Again, it is simply a reiteration of the other with regard to the frequency of failure.

These are simply an expression, againn of the fragility curve, indicating at least some of the parameters with regard to uncertainty, with regard to failure frequency. And of course, this morning we were talking about such factors as median in developing the composite curve, and of course, Bill mentioned whether acceleration is the logical thing to use. It's hard to say. Obviously, one can use the other context, and instead of worrying about the one variable, you can worry about the variation of frequency as indicated here in considering the sigmoidal curves, so you will have both parameters, again, common knowledge to most of you, I suspect.

This is a statement, I might indicate, I probably would tend to disagree with with regard to the electrical.

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It is the old business of the less familiar you are with it, probably, the more you might disagree. On there, this is a specific statement with regard to the, you might say the probability that they will actually survive at particular acceleration levels.

I think I would agree with the statements made this morning on several items on mechanical equipment. I guess I personally have at least a number of reservations with regard to electrical equipment. It would be interesting to see, indeed, if a study such as was done or is under way were applied to the electrical equipment, just indeed what the probability of survival would be under these circumstances.

One can argue, of course, the last statement, I guess, is closer to home in my case with regard to safety factors and codes which are generally more applicable in this context, mechanical equipment, to the electrical equipment with regard to loads, et cetera, and I will hope to show a few examples that extend beyond the codes, at least discuss them in this respect, that relate to what I would call this probability of survival aspect. This was mentioned this morning.

When we get into fragility as such, one has to ask a couple of questions. What are the so-called hard data? How much information do we have that we can establish with a high degree of reliability that is essentially applicable? And the answer is: not very much. Corps of Engineers data probably represents the most representative in this instance and probably represents just about the total that you could call directly applicable. I think one could take some of the shaker table tests, et cetera, and say that to a degree, at least, they are applicable. And as soon as you leave that one, you get into the next category, how much can you infer indirectly either from the actual behavior in earthquakes, which will have another degree of subjectivity, or from other sources?

And then you lead into the third one, which, of course, is always interesting. It introduces a rather large factor of subjectivity, and that, of course, is the business of individual judgment by a large number of individuals as to what there is in this respect.

And this, of course (indicating), relates to the one variation in material properties failure modes, load distributions, response variables. I think we are beginning to get a little more information, and I would like to touch briefly on it perhaps a little more tomorrow on the panel, but in programs that I would say are unrelated, essentially, to what we are discussing here, we are beginning to get a somewhat better handle on these; and I think by indirect inference, perhaps we can get a grip. And I think some of these data, at least in a systems context, permit us to infer fragility values. At least that is my hope.

Here we get into the expert opinion business. These words were taken directly out of there. What it says, basically, is that there is a body of data out there and if you could really get ahold of it, it might expand your horizons considerably. I know, for example, that that is certainly true in Japan. There is a substantial body of data. I would assess -- talking about probabilities -- I would assess the probability of its availability in this country as down on the very low part of the sigmoidal curve. In other words, I would put it down as something like one percent or less, for obvious reasons. It is trade information and it is intended to continue that.

For example, we have been working on one aspect of this now for two years, and I would assess our net gain in that respect is essentially zero. We have gone through a random walk process and are essentially back where we started. However, we have some very interesting discussions with Professor Shabata that simply end up with, yes, we have discussed many things and come back to point zero. I think several others on here may have gone through the same thing.

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Here are some generic categories for components of fragility assessment. I was intrigued by the discussion this morning. You will see a number of items on the first line that relate directly to what is being done by the group that Bob Kennedy is chairing. You get into the valves and the pumps on this one. Particularly I am a little intrigued, and I guess I didn't raise the question -- I would have thought myself that there are certain classes of pressure vessels, and I am not talking about nuclear the reactor vessel, necessarily, but as a first approximation, could have fit in the same one with a fairly high reliability or probability that they would have met the same criteria.

I think, though, it is an interesting point that by inference from this fairly large number of data points and by the, I guess, positive action in the sense of very few failures, that one can at least write off a number of these. Now, that doesn't say you end up with a fragility curve. All that really says is as an upper bound in certain cases it hasn't failed, but it doesn't tell you much else.

This category (indicating) I suspect is somewhat less defined, and it will be interesting to see. Again, in the categories in the reports I have cited that are desirable from a fragility point of view, one sees, of course, a substantial number of electrical components in here, and of course, there was a discussion this morning of at least one,

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and I think the business of battery racks, et cetera, has come up endless times over the last period. And of course, then, the miscellaneous items such as ductwork, instrument racks, et cetera that we have heard.

On the one item, on hydraulic snubbers and pipe supports, that is an item, I think, nearer and dearer to my heart than most of the others on here, and I am inclined to think that positive actions are proceeding. The most positive of all is to get rid of as many as possible, which immediately reduces the possibility of failure because you have a lower population. That is what we are attempting to do at the present time.

On the basis of what was discussed this morning on expert advice, I thought you might find this slide intriguing. Incidentally, I don't claim that I generated it myself, and I would classify myself in the lower category with regard to expertise. I would describe myself as a subset of a university professor only. Nothing better than that.

One could argue, obviously, because I think I would agree with Bill Hall, that in some areas the military experts might well be given a factor of more like 4 than 2, depending upon the source of information. This is presumed on the basis, you might say, of hidden information in the files that would be unearthed and in a confidential manner could be

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presented and would be more applicable in the upper groups.

MR. SIESS: What about university professors and ACRS members?

(Laughter)

MR. BUSH: I would classify that as probably about .5.

(Laughter.)

Incidentally, based on my personal experience, I can think of certain categories up in the ones given a 3 that I would possibly classify as a subset of 1 or less than 1, too, but I won't name any names in that respect.

(Laughter)

Where are we on some of this? To you, I recognize, this is old hat; to others, depending on how closely you are separated into subsets, it may not be so old hat. Here is an example that summarizes what might be said to be the available information on component fragilities in the reasonably near past in these particular reports. It gives you some of the information that might be of interest in that particular category, as well as listing the failure modes on these particular circumstances on such things as the reactor core assembly, or more specifically, the guide tubes, which represent a very special subset, and the reactor pressure vessel, RPV, output nozzle, which I would consider as a rather more improbable event than might be here; but that is

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a highly subjective judgment.

Note the support failure mechanisms, which I believe I agree completely with the presentation this morning. This is a personal judgment. And in fact, we had a discussion of this not too many weeks ago in a meeting I am chairing on piping, and there was essentially complete consensus around the table at that time on two things: that we have overloaded our piping and they are too strong, and at the same time, we do not make our supports with regard to equipment, and I will name such equipment as steam generators, strong enough. I believe that is the message that came through fairly loud and clear this morning in that discussion.

Here is a continuation of some -- in electrical, of course, the last item here on relay chatter gets back to the item discussed this morning, both in "new relays" and also in used relays, as being a very significant item in there. And obviously I would say it does not really help to assess the reliability of pumps and valves as was done this morning unless you can assure at the same time that the reliability of the electrical system under the seismic event is appropriately high also.

In other words, if that doesn't go, you haven't accomplished that much.

This (indicating) was simply an application which was an interesting one to me. It is an application of some of the fragility data. In this instance the concern was the examination of the pressurizer enclosure. You see, it is high enough off the floor so you would consider it gets fairly substantial loads. There was an assessment under those circumstances which, at least from my point of view as an engineer, was quite interesting to me, on what were the probabilities, basically, of the collapse of this enclosure around the pressurizer.

Now, the pressurizer turns out to be a pretty critical component in a pressurized water system, and I would say if the enclosure per se might collapse, you could still survive, but if it takes the pressurizer with you, that represents, in my mind, at least, a major failure under which the system as such would probably not recover. That would be my assessment. So values such as this become extremely important.

Again, one might argue about that use of floor acceleration, but it does show the kind of fragility curves, and one could infer from such a collapse on the possible behavior with the regard to the pressurizer per se.

This one gets a little closer to the heart of my interest, and I want to deveote a few slides to, I guess, the kind of problems one can get in and the use or misuse one can arrive at with regard to fragilities on piping.

Over the past 10 or 15 years there have been a

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variety of failure modes suggested. One is plastic collapse. I am not a proponent of plastic collapse. I agree in certain systems I would expect it to occur, but in most piping systems, because of the D over T ratios that you have there where wall thickness is relatively large -- a lot of this stuff is Schedule 120 pipe -- the probability of plastic collapse strikes me as being extremely low. However, there has been enough belief in it, there have been enough true believers that we have gone from essentially what I would call flexible systems in the context of their flexibility only limited by the piping to rather stiff systems. And when I say stiff, I mean really stiff. There are supports every time you turn around.

That moves you toward the next mechanism of low cycle fatigue, although that tends to be more controlled by mechanisms other than the loads per se. It tends to be controlled by thermal radiants or transients more so.

The next step is crack growth and fracture by ratchetting. That is the end of the line. That is when you get really stiff systems, and we are mighty close to that already, because when you get into a ratchetting mechanism, the one thing you don't see is shakedown, and we are getting perilously close to that.

In passing, I might show this fragility curve. I am not a proponent of it. I am sure you could draw, one could argue, on the absolute value of such, but at least it indicates the data available under these circumstances.

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Here is a definition that I came upon, something that Bernie Langar did, and Bernie is, one might call, the father of the pressure vessel on the ASME Code and, to a degree, the piping systems. He made a comment here about variable loads in time causing dangerous damage such as fatigue cracks.

We can analyze such, but these are only applicable if ratchetting doesn't occur. If the system shakes down, you are fine, but if it does not, you are not. And Bernie's words said that the foregoing discussions -- and he was talking about shakedown there, are applicable only to situations shake down to essentially elastic behavior, where plastic strains occur only in small, isolated regions. If ratchetting allows permanent strain to build up cumulatively, the fatigue life will be drastically reduced.

What that says is, instead of a couple hundred thousand cycles, it may be a couple of hundred cycles to failure.

To put it in perspective, these are the types of curves, and if you took these as fragility curves, when you get into the unstabilized load condition, it wouldn't take very long, so if you were to look at the piping as a system and you had a ratchetting mechanism that would control, what you would be into, essentially, is a low cycle fatigue control just by heatup and cooldown, and you would have a lovely

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fragility curve that would indicate probably 10^1 or 10^2 cycles. You could expect the system to fail, which would be interesting.

I would like to touch on something that I believe has value in fragility and is generally outside the scope of a group such as this. I personally feel that quite a lot of data that could be relevant is outside the seismic arena and which could be used in the development of fragility curves, generally applicable more to systems than to specific components.

Let me cite a few. I might mention -- and I am not suggesting that these are valid as such, but there are methods of analysis now that have been used for faulted loads, specifically seismic, where they have been looking at piping and actually have a place in the Code now dealing with cracks that are as much as 50 percent through-wall. That is a fairly good-sized crack. It can indicate what the life is and the number of cycles.

There is another program, again directly relevant to systems, where we have done an analysis on damping, again on piping systems, which clearly indicates that by enhanced damping value, of course, the probability of failure of the pipe changes dramatically. For example, if one goes from 2 percent to 5 percent, the actual amplitudes are dramatically changed, and in fact, you see it two ways, in removal of

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supports, et cetera, and also in what I would call the probability of failure being dramatically reduced. A couple of others that may be common knowledge or may not is that the Shippingport program might well give us information that would be coupled between fragility and degradation because that is looking specifically at aging degradation, and the data, I think, could be used to infer fragility.

Some of you are probably aware of it, and some not, that they are going to take Shippingport apart piece by piece and look at the pieces with regard to degradation mechanisms, which could be used to infer what I would call probability of failure under circumstances.

One other is the NRC piping program, which actually will look at the failure of piping under severe conditions in the near future.

Getting back to fragility per se, I guess I would have to agree with the comments this morning that there is a definite lack of fragility data. Some of the things I mention might provide some additional information, at least with respect to specific components, and I really feel that there are data that would be directly applicable to the fragility field but they are so far outside the envelope of the seismic studies that there are probably very few people aware of it.

Thank you.

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MR. OKRENT: Are there questions or comments? Yes, there is a hand in the back.

MR. HENRIES: Bill Henries from Yankee Atomic.

Has the NRC given any thought to the latest findings of the Air Pressure Committee on dampings?

MR. BUSH: Yes. I used a very tricky technique on this. I went directly to Joe Palladino, which is cheating a bit, but it is more effetive than some other things. Actually, I think the time was ripe, and we sent the information without backup, and by backup, I am talking about the technical document a short time ago, and we had a response from Howard Denton, who heads up the Nuclear Regulatory portion of it, who accepted our suggestions with regard to damping values, which essentially take you up to 5 percent at 10 hertz and then slide back down to 2 percent as an interim position, and I think there is a general feeling they could well be higher than that.

They also accepted the spectrum broadening aspect that was added, which would have an impact -- not very much, I would say, and in fact this was also accepted by the code. The main committee accepted this at the last go 'round. So both aspects have been accepted. They haven't modified the Reg Guide yet, but I suspect if someone wanted to bite the bullet and go in with a suggestion, it would probably be accepted. That is my reading.

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I might indicate in passing the impact on the reactor, and I realize that nobody wants to go through the amendment process, but the computer simulations on this indicate in a reactor that one of the newer breed that has more supports on it, that this change in damping alone will permit you to remove about one-half of your snubbers and one-half of your supports, which, in my mind, represents a substantial change.

That is not really fragility other than indirectly, but I think from a systems point of view, it gets to the system reliability, and as far as I am concerned, I equate fragility to system reliability, to unreliability.

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Yes, Dave?

MR. OKRENT: Do you know whether anyone has systematically looked at how aging might interact unfavorably with a severe earthquake late in the life of a reactor, and if it has not been done systematically, does your intuition tell you that certain components are the ones to look at hardest in this regard?

MR. BUSH: I think the answer to a systematic one is a clear "no." I think the Shippingport study is an effort which I would not call a systematic one but it would answer some of the questions. I also believe that some of the data that they are attempting to generate indirectly in the U.K. in answer mainly to the nuclear installation inspectors question will, again, respond to some specific components.

If one were to go piece by piece, which I think is what you are concerned with, let's say safety-related components only, there are inferential data on certain blocks that I, in my very personal and subjective judgment, would consider probably adequate, not enough to really refine it, but I think there are quite a few out there where there would be what I would call a gross lack of information.

That's my personal opinion on the thing. One that has been an item for argument, probably grown up extraordinarily, has to do with the cast stainless piping late in life. The argument is it might embrittle because you could

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sigma phase formation, and if you get sigma phase formation, how much loss in properties are there. Therefore, if you had a good shake, would it stand up or not? That is the kind of thing that is in there, and one can bound the values, that's about all. I would say there is a very definite lack of information, definitive information with regard to degraded materials, degrade components.

MR. HALL: Let me make another comment about that from another field. Let me be a little vague about this.

MR. BUSH: I know the one you are talking about.

MR. HALL: No, I'm talking about another one.

MR. BUSH: Another one?

MR. HALL: Electronics, modern solid state. Is that what you were talking about?

MR. BUSH: That was part of the thing, yes.

MR. HALL: Tests of hundreds of thousands to millions of cycles of fatigue, I will call it, on an electro-dynamic shake table of a component system, another component system put them in the entity you are interested in, and you have to connect them together so you put a bundle of wires in it, and then after x years, you get some big excitation, and BANG, just like that.

In other words, again it is the old system thing. Everything works fine as well as they can tell on the subsets, and yet they didn't pay enough attention to the bundles of

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wires between the modern solid state electronics. This is the real world.

MR. BUSH: Yes, that is right.

MR. HALL: Just as an example --

MR. BUSH: I am aware of some such data. It tends to be negative, unfortunately. That doesn't say you couldn't do it.

MR. HALL: No, you can do it. It can be done.

MR. BUSH: Yes. Well, there are some others. There's a large number of things out there that can age and have an adverse effect, but generally they are looked at only cursorily until they have trouble, and then they get more careful -- gaskets, things of that nature -- which you would as a first approximation thing didn't play much of a role. But you find they play a bigger role than anticipated.

MR. OKRENT: Dr. Cornell.

DR. CORNELL: Dr. Bush, let's say you are in a ratcheting situation or a situation where there is no ratcheting but you think something like a CRAC code model is the best representation we have of what is going to happen. Is it likely, in your opinion, that it will be the earthquake that is the straw for this camel's back, or will it not be the thermal cycle?

MR. BUSH: I would say the earthquake is not. I would anticipate, obviously, one can postulate that you are

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on the ragged edge. Everything has gone wrong, but you don't know about it, and you get to the ragged edge and the earthquake takes you over. I would say on purely probabilistic grounds, particularly for several of these mechanisms, you would have gone over the edge of the precipice quite a while before that. Certainly in ratcheting, I would be extremely surprised if you didn't find it out the hard way.

MR. CORNELL: So vis-a-vis seismic fragility, these are probably not critical problems; is that what you are saying?

MR. BUSH: I guess so. What you would really find is you are in trouble and you had better back off on it. These were kind of a warning-type thing, the things that if you go the wrong routes -- and I think we have, that's my personal opinion -- then you can get into these troubles regardless of the seismic.

Now, the seismic has one big difference. It tends to be global rather than local, and that, to me, makes a lot of difference. If I fail a lot of things, I would be much more worried than if I were to fail, say, a single system.

MR. CORNELL: Are there examples in a nuclear power plant of piping that might, let's say, be more sensitive to seismic problems, or let's say really critically sensitive to the seismic problem as opposed to the thermal load; that is, somehow it is protected from thermal problems so that if

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it will break, it will be the earthquake that does it, or what combination of stiffnesses, supports or whatever.

MR. BUSH: Well, there are certainly some, and the ones I think I might worry about, except I'm not sure -- the safety implications are kind of second order -- there is quite a bit of Schedule 10 and Schedule 20 pipe out there that is fairly good-sized diameter wise, and I would be very surprised in a seismic event if that stuff didn't buckle badly, and I'm not even sure after a couple of cycles whether it wouldn't go beyond the buckling mode and tear.

The thing about it is, it doesn't have that much safety implications except I haven't seen any assessment, if I fail seven or eight of them in a seismic event, what would be there. For example, some of the ones they carry are your void gases solutions are in there. What happens if you lose all of your boric acid solutions? I don't know. It may be no safety problem at all. Those are the places I would tend to expect it.

Some of these ties where you go from a big pipe to a small pipe and there is a rigidity change there, I think if you had this type of motion (indicating), which is quite possible -- in other words, it would flex -- I'm not sure how those smaller lines would stand up. I wouldn't be too surprised if they cracked. I don't know that they would break, but they might crack. Unfortunately, some of those are

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in primary systems.

Yes, Bob?

MR. KENNEDY: I think those are the kind of failures you have seen in past earthquakes most prevalently. This kind of piping where you have a small diameter branch line that is a straight run between two larger diameter pipes which are not supported where that branch pipe comes in. That failure mode disappears if people would think about flexibility in the small diameter branch pipe, too. But sometimes they don't.

MR. BUSH: Exactly. If the small pipe could go with the big pipe, you are probably not in such bad shape.

MR. OKRENT: Let's see. Dr. Ang has one or two slides, I think.

MR. ANG: Thank you, Mr. Chairman.

I would like to take a few minutes to present some material that has to do also with fragility. However, this is fragility of structural components, in particular. This is actually a part of our current study, research study supported by the National Science Foundation in an effort to develop damage functions, and our current status is only limited to reinforced concrete structural components.

Moreover, the components are only limited to beams and columns of conventional buildings, and the study actually is -- let me show you one. The particular damage

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function that we developed is expressed in this Function D. We call it a damage index. Let me explain how this particular function is developed.

First of all, we recognized, and others have also done this, that damage to structures under seismic loading really is made up of two components or two parameters: the maximum distortion, or ductility factor, if you wish, and, of course, the absorbed hysteritic energy. The first component here effectively reflects the effects of the ductility, the maximum deformation, whereas the second factor here is the total energy that is absorbed, hysteritic energy that is absorbed.

What we did is examine over 400 test specimens. In fact, 403. These are test speciments subjected to simulated earthquake loadings, dynamic tests of reinforced concrete beams and columns that have been tested up to failure. The data includes both U.S. data as well as Japanese data. In fact, the student who did this comes from the University of Tokyo and is quite familiar with Japanese test data also.

So what we have developed here is an empirical equation, the function of damage. The only objective we tried to achieve is that the collapse would be defined such as the value of the damage is equal to 1, greater than or equal to one; and the second part, of course, or the second

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objective, of course, is to express damage in terms of those two parameters I just mentioned.

It turns out that if we trace this damage index of every test spec, trace it all the way up to failure -- and by tracing, I mean calculate the maximum displacement and also the hysteritic energy absorbed following the actual earthquake motion of the test, we came out with a rather wide spread of this local damage index at the point it collapsed. So in other words, this represents really the distribution. In this case it is plotted on a log normal probability curve, and it turns out the mean value according to this function -- and that is why we use this -- the mean value of the damage index at collapse is approximately 1 -- 1.008, as you can see -- and the coefficient of variation is fairly large, 53 percent.

Now, this 53 percent is simply only for the definition of collapse expressed in terms of this damage index, of course.

The other result I guess you can see from this is it is fairly linear on a lognormal probability curve, so this might give, I suppose, some support to the assumption that fragility curves could be assumed to be log normal. The lower curve here --

MR. SIESS: Excuse me.

MR. ANG: Excuse me.

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MR. SIESS: I don't understand it completely. How did you define "collapse" for a beam, say?

MR. ANG: The beam just collapsed during the test. That is the ultimate test.

MR. SIESS: At zero load capacity?

MR. ANG: Correct.

MR. SIESS: It factored?

MR. ANG: Right.

MR. SIESS: Can you take one point on that curve and tell me what the probability means or what the damage index means? That is for a beam. Suppose it falls at .5. What does that mean?

MR. ANG: It means, then, that that particular beam has a damage index value of .5 at collapse.

MR. SIESS: That is the delta. It reached something less than its ultimate deflection. What is the $delta_u$.

MR. ANG: Well, this is delta ultimate here. Some of these parameters I didn't define. Delta ultimate here is the ultimate deformation under a static load.

MR. SIESS: That is a static load in that test?

MR. ANG: Right.

MR. SIESS: And delta is what it reaced in the dynamic test.

MR. ANG: Yes.

MR. SIESS: And the other terms?

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MR. ANG: The other term is hysteritic energy. It is a little more involved.

MR. SIESS: Was reached?

MR. ANG: The total hysteritic energy, because this is under cyclic also.

MR. SIESS: It's not a ratio of something to static?

MR. ANG: No. This is integrated. This is the total hysteritic energy as a specimen degrades. All of that is included here. It is a little more involved to explain that.

MR. CORNELL: In the same context, it seems to me that in monotonic loading, by definition, delta will equal $delta_u$ at failure.

MR. ANG. Right.

MR. CORNELL: So you have to get index one. How can you get .5 or 2?

MR. ANG: This is, of course, under dynamic loads. This is under oscillatory loading.

MR. CORNELL: So the index does not work on monotonic loading.

MR. ANG: The index is not monotonic.

MR. CORNELL: What if we have only one cycle?

MR. ANG: Then it will fail by simply the ultimate displacement.

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MR. CORNELL: And then delta will equal $delta_u$ by definition?

MR. ANG: Exactly. That's what I mean. $Delta_u$ is static loading, monotonic loading. So this is just -- this equation is purely empirical, obviously, but the point I am trying to stress here is although it is empirical, it does contain the two parameters, the maximum displacement as well as hysteritic energy.

MR. SIESS: Static tests would give you one for the source term.

MR. ANG: At collapse.

MR. SIESS: With no hysteritic energy.

MR. ANG: No hysteritic energy. But under earthquake loading you have hysteritic energy, and the two components have to be included. Here it is a simple combination. It is a linear combination, as you can see, of the two terms. Anyway, I have already mentioned that this is the distribution with the mean value equal to 1, but that is only the mean value.

If you express collapse in terms of the damage index, some of the beams could collapse with damage indexes of .2 or .3 instead of 1.

MR. SIESS: Why?

MR. ANG: Simply because -- remember, this is calculated from the data. The test collapse at --

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MR. SIESS: Is this due to variations of geometry, material properties?

MR. ANG: There are many things, simply one of which is the inaccuracy of this particular measure. Also remember it is an empirical measure. We are looking for something, you see, so we try to develop an expression where the mean value of the damage index has to be equal to 1, but of course, the coefficient of variation may be above that.

MR. SIESS: But for all you know, that large variation may be in your choice of a formula.

MR. ANG: It could be.

MR. KENNEDY: How do you find $delta_u$ for any specific beam in your test data? Do you do static tests as well?

MR. ANG: There are static formulas for calculating that, so that we just use that.

MR. SIESS: That would be fractural reinforcement?

MR. ANG: There are several modes we consider.

MR. SIESS: Shear?

MR. ANG: Shear is also included, right, yes. So as far as static ways of predicting the delta ultimate, there are available expressions. That is the one we use.

MR. SIESS: Yes, but they have a variation, too.

MR. ANG: Of course. That's what I meant. All of those variations are included. But we are looking for some expression. First of all, it has to be relatively simple, and this is the expression we came up with. This expression here, the lower curve is a little more accurate, but you see, the coefficient of variation is no better than the one up here, so we abandoned this particular one.

We also apply this to calculate the damage index of buildings, and the coefficient of variation indicated in this curve is the coefficient of variation of the building stories, and it's also right about 50 percent. These are buildings subjected -- there are three types of buildings we assume, and all subjected to earthquakes of different duration, one with a strong motion duration of 5 seconds, another 15 seconds, and the coefficient of variation seems to be all right around 50 percent.

This has been used, now, to calculate -- or has been calibrated, I should say. By that I mean that the damage index is calculated for those buildings that have been damaged during previous earthquakes, two earthquakes in particular, the San Fernando earthquakes of '71, the buildings that were damaged in that earthquake, and the other one is the Miyagi-Ken-Oki. Several buildings were damaged. Some of them, in fact, had to be abandoned. Those we also calculated, and by these means we give some meaning to the value of the damage index, capital D.

It turns out, then, that this part here (indicating),

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of course, are damage inspected. This has been categorized into five categories of buildings that have been damaged during earthquakes. The right-hand part are the calculated damage index for something like nine buildings that we did, both in San Fernando or the Miyagi-Ken-Oki earthquakes, and you will note that those that collapsed are up here (indicating), which is greater than 1. There were three buildings that collapsed during those two earthquakes. Some of them have very low damage, which is under "slight." These are less than .1, the value of damage index. The others, like E&I, are right around less than .4, and those apparently were felt to be repairable. You could restore the building.

So, according to this calibration, then, we have sort of tentatively come to the conclusion that the damage value, capital D, the damage index, less than .4. could be called a building that can be repaired, whereas if it was higher than .4, it most likely would be abandoned.

On this basis, then, we generated a fragility curve for concrete buildings, and the value R. Each one of these, now (indicating), is a lognormal distribution of the fragility curve probability of collapse versus characteristic intensity. This is just a measure of the damage potential of the earthquake.

What we defined as characteristic intensity includes the amplitude, the peak acceleration, as well as the duration of the strong motion part of the earthquake.

If I may turn back to another side here (indicating), the relationship between the damage index for the complete building and the characteristic intensity will have some linear relationship. You will see that the plot is very good for two durations, 5-second and 10-second earthquakes.

In this case, this is, then, the kind of fragility curve that can be modeled by a lognormal distribution.

Thank you very much.

MR. SIESS: What is R, Al?

MR. ANG: R is one of those parameters we defined. What it means here is the higher value R means that it has less resistance to ground motion, or less resistance to damage, I should say. A lower value of R means that it has a higher resistance to damage. That is what the parameter R is.

MR. SIESS: That is a characteristic of the structure?

MR. ANG: Yes, it is.

MR. SIESS: The geometry or --

MR. ANG: Well again, it is tied into the capacity in terms of the damage index.

MR. SIESS: Okay.

MR. OKRENT: There is a question.

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MR. KUSTU: I am Onder Kustu from EQE.

I may be misinterpreting your damage function. If I understand right, your damage factor or damage index is nearly proportional to hysteritic energy and your hysteritic load. In a well-designed ductile beam with lots of shear reinforcement, you will have sustained flat hysteritic cycles, and the same beam with the same strength but not so well designed in the shear, you will start pinching your histeritic group. So the area inside will be smaller and smaller.

Does that mean that a well-designed ductile beam after an equal number of cycles of the same amplitude will have a larger damage index because the area is larger?

MR. ANG: I think that is reflected also in this curve. I would imagine -- off-hand, I wouldn't be able to say exactly what causes this up here, but I would suspect that those that are heavily reinforced and have high ductility would probably be up here, although I could not swear to that. I don't know. Those that probably failed prematurely, maybe perhaps by shear, would be those that were down here (indicating).

I can't really be sure because of these two terms here. I imagine that those that are weak in the shear would probably not have very much of this term.

MR. KUSTU: But sometimes you may have conditions

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where the weakness in shear, let's say, is not so clear, so it couldn't really make much difference in static loading without cycling. You could push the member, two members. One is heavily reinforced and the other is barely enough to carry the bending motion, so they would fail under static loading at the same amplitude.

In that case, your formula is actually penalizing the ductile member.

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MR. ANG: I would say it is neither penalizing nor being in favor of it. All we did is analyze, examine all of the best data. Of course, there will be an analytical model to generate this function D. We subjected that particular test specimen with its geometry, its reinforcement, et cetera, and also using existing equations from reinforced concrete technology and calculated the value of D by tracing through both the displacement as well as the energy absorbed subject to that particular time history which the test, the specific test specimen had been subjected to and calculated the vale of the damage index B here when up to the point it collapsed.

MR. SIESS: Al, I think to get a damage index of 3, for example, dynamic or static, most of that has got to come from the hysteritic energy. You will not get it in deflection.

MR. ANG: That is what I say.

MR. SIESS: And I think that most of your decrease is in deflection under reverse loading, and most of your decrease is in the delta term. Most of your increase is in hysteritic energy.

MR. ANG: That is probably correct. Another way of saying it is the ones down here are most likely due to premature failure by shear without ductility. Exactly.

MR. SIESS: Can you separate them out?

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MR. ANG: Well, I will have to look into that. I cannot answer that.

MR. OKRENT: I think we are going to have to end the discussion at this point now or we will get too far behind the agenda. It is time for our break. Let's take ten minutes, and would the next speaker please be back at that time?

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(Recess)

MR. OKRENT: Please be seated and quiet, or at least quiet.

(Laughter)

Dr. Reed.

MR. REED: Thank you. The title of my talk is uses of seismic PPA. When I got assigned this, I looked through the list of the rest of the speakers and I noticed none of them specifically have a title "PRA" in there, and I thought to myself, I felt a little queasy about really talking about uses. So I said to myself, why can't I use this as an opportunity to say a few things that have been on my mind for some while.

(Laughter)

But I tried to organize it so it would end up properly in uses.

I should say a couple of words about my background. I have been involved in the review of seismic PRAS,

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specifically in the review of Zion and Indian Point. I am currently involved in Limerick, Millstone and GESSAR, and the comments I am going to make are basically from my experience and what I have come upon during this review process.

The topics I wanted to discuss today, the first is characteristics of current seismic PRAs. Some of these things we talked about a little earlier today. I think it might be useful to look at some of the important fundamental characteristics of what seismic PRAs really are.

If we talk about deterministic evaluations versus seismic PRA -- and what I mean by deterministic seismic evaluation is what I refer to as the design process, deterministic ways of sizing structures and components to resist seismic loads.

A topic for this third one is the issue of absolute versus relative results, which we talked about a little bit this morning, and I just happen to have a transparency in here. I would like to give a few comments on that.

Finally, before I get to uses, I will talk about state of the art, and where we go from here to improve our capability of doing seismic and better PRAs. And with that as a background, I will talk about potential uses of seismic PRZ.

There are three basic elements in seismic PRA; the hazards analysis, the fragility analysis and the systems analysis. Under the hazards analysis, we talked about this this morning. My first point is one which I think has been made already; that is, the methodology for performing seismic hazards analysis is fairly well established. There are questions in quantifying the various parameters that go into the analysis. One of the results of the hazards analysis is the very large uncertainlty.

This is an example of a family of hazard curves from one of the PRAs, and this particular family has some characteristics, and I think you are probably all aware of it. They might be worth repeating.

At the low acceleration levels, the uncertainty is much less than the higheracceleration levels. I think this is extremely important. When we talk about the strength of a structure and where it falls relative to these curves, it has to do a lot with the final uncertainties we end up with in our core melt or our other release categories we are

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concerned with.

I will come back to this transparency a little later.

In the fragility analysis basically what is used is a simple lognormal model characterizing the various steps involved in fragility analysis, starting with cell structure interaction, building response up to equipment response capacity, et cetera. I think this is state of the art and I think it is appropriate. The reason I feel this way is I think we have so very little data at this point that a more sophisticated model is really not warranted.

So much of our judgments as expressed in this model are subjective, and to use a more complicated model at this point, I don't think will give us any new informaiton or better information.

The data used in seismic fragility analysis can be characterized as either generic or plant specific. Generic data is often used, particularly for mechanical and electrical components. Plant-specific data is used more for the structures and larger equipment items.

I think there are definite uses of each of these classes of data, and as I get into my talk a little bit, I will try to give you some feeling of how I think those two types of data should be used.

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In commercial seismic PRAs, the analytical process depends very heavily on the original design analyses that have been conducted. In other words, they start with the analysis that was performed during the design of the plant, and because, as we all know, these analyses are done conservatively, they are adjusted parameters, are modified to come up with median centered along with the variabilities.

I think this has some important implications, and I will say a few more words about that later. In general, design and construction discrepancies are not included. I prefer to call these discrepancies rather than errors mainly because they do not always have to be bad. Many of the so-called discrepancies can be on the conservative side, but in general, I do not believe these are systematically included in PRAs. I think this does present some problems.

Sort of the bottom line, keeping design and construction discrepancies to the side for a moment, my feeling is the median parameter values that are currently being produced for various components and structures are on the conservative side and that the uncertainties that are assigned to the component fragilities are low, and I will try to give you an example or two in a moment to give more flavor to that comment.

The third area involved in a seismic PRA is, of course, the systems analysis. Fault trees are developed for the seismic systems analysis that are generally different than the internal event fault trees. The fault trees look much more coarse. Events are much larger in the sense that a shear wall failure of a structure usually means the failure of the entire structure. There isn't like a branch of the fault tree that has one structure with various combinations of failures of the different components.

I think this has a tendency to cause the definition of failure to err on the conservative side. The process of hazard fragility integration is fairly straightforward. There have been several methods that have been used: the discrete probability distribution method, also simulation methods using Monte Carlo simulation have also been used.

The results of seismic PRAs that have been submitted to date have, of course, dominant contributors, and the things particularly of interest are these contributors are different for each of the plants.

I have here the five PRAs that have been submitted: Zion, Indian Point 2 and Indian Point 3, Limerick and Millstone. This is the commercial PRAs. Under Zion the contributors were the service water pumps, the auxiliary building interconnecting piping, the crib house, which was the crib house roof failing, the batteries and racks.

At Indian Point it was a single component, the impact between the Unit 1 and Unit 2 control rooms. At

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Indian Point 3, it was basically the dominant contributors were from two components, the diesel generator fuel oil tanks and the control building. At Limerick, interestingly, it was a series of electrical components. And finally, Millstone, which is a recent one, which is currently being reviewed, has sort of a mixture of components, both electrical components, structures and also equipment.

Notice also what the mean frequency of core melt is if we use that as a gauge here. They are quite a bit different from each other.

One trend here that I would hope would be occurring would be that as we come into newer plants, we would expect the mean frequency of core melt to go down. There is a slight trend. At the older plant, Indian-2, there is a higher value, but the value for Millstone, which is a newer plant, is also high.

I think one of the points I am going to come to today is if you just take these values as they are and compare them to other contributors, fire and internal events, seismic is significant. One of the questions in my mind is, is this really true or is this because we are being so conservative in our analyses that we have created a situation here that makes seismic look like it is the dominant contributor?

My last item on this transparency is it is

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important to realize the results of seismic PRA in the sense of looking at the contributions from fragility and hazard. It is the median fragility that generally controls, in conjunction with the uncertainty or the range of hazard values, but it is not as important as the uncertainty in the fragility.

I would like to show a couple of examples that might demonstrate that. This is taken from Indian Point 2. What this is is the Indian Point 2 seismic core melt probability distribution, and also the density functions shown. Indian Point 2 had, as you remember on that previous transparency, a mean core melt of about 1.4×10^{-4}, and if we flash back here to the hazard curves, we can kind of see what is happening.

It turns out that the capacity of Indian Point 2 is dominated almost entirely between the impact between the Unit 2 and Unit 3 control rooms that had a median fragility capacity of about .27 g, so if you come up here at about 2.7 g, you can kind of see why the mean frequency came out to be about 10^{-4} and why the spread is something on the order of 2 to 3 orders of magnitude across here (indicating).

Now, if we go to Indian Point 3, which was different in that the systems fragility when you put in the individual components, which for that particular plant consist of primarily the control room shear wall and the buried diesel

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fuel oil tanks, the median capacity was, like, about .8 g, and so what you have got was a probability of frequency density function that was much more spread out; and the reason it is much more spread out and uncertain is, if you go back and look at the hazard curves, it is something like .8 g. You can see there is a tremendous uncertainty.

So what you should expect is, as plants get stronger and stronger, the uncertainty is going to get larger and larger. The saving grace will be that the whole curve will shift, and you can kind of see this if I put these two curves together.

It's difficult to do because they are not on the same scale, but you can see at Indian Point 2 the tail slaps over past 10^{-3}, but in this sense, the tail here stays to the left of 10^{-3}. So the curve for Indian Point 3 has shifted. The point I originally started with was not only to show you the kind of values and results that were obtained, but to make this point once again: that what is important from the hazard curves is the uncertainty, but that is not the only thing that is important; what is also important is the median fragility. Where is it? Is it out here at .8 g or down here at .2 g? And that is important to find exactly what that is.

Now, I would like to say a few words about deterministic evaluation versus seismic PRA. As I said

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before, I am sort of visualizing deterministic analysis as being a process we use in the design of a plant, and as you know, we are conservative at each step. This is what we attempt to do. We use conservative input, we assume conservative properties.

One of the problems, however, with deterministic analysis is at the very end when we are all done, what sort of design margins do we have? It is very difficult to use just deterministic tools to evaluate what kind of design margins we have.

Also, when a problem arises -- somebody finds that there has been a mistake made -- the question is what is the implication of that mistake? Again, deterministic tools do not help us very well to try to see if there is truly a problem. Of course, as you know, the goal in deterministic analysis is to be conservative, and, I think, properly so. But this leads to seismic PRA and this leads to some of the uses of seismic PRA.

First of all, on seismic PRA we have new analytical dimensions. When we are talking about the design of a plant, we are comparing the demand to the capacity and being sure the capacity is many times greater than the demand to have adequate margins. When we are talking about seismic PRA, we are talking about failure. We are talking about frequency of failure. And because we are not certain what this frequency

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will be, we talk about uncertainty on this frequency.

What is useful here with seismic PRA is it can be used to quantify conservatism in the deterministic analysis done for the design of a plant. Also, when problems arise, PRA can be used as a tool to put those problems into perspective.

Now, one of the things I see happening, and which disturbs me somewhat, is that there seems to be a tendency to try to approach seismic PRA with the same sort of philosophy as in the design process, to produce conservative PRAs. I don't think PRAs should be conservative. I think PRAs should be accurate. The use of conservative tools can be used in performing a PRA.

For example, in a screening-type process you have certain components that you suspect are not going to be major contributors, so you might assume that they are -- you may assume conservative properties for them, run them through your fault trees and your integration with your hazard curves, and if you find they are not contributors, that is a proper use of conservatism in PRA. But once you have come to the dominant contributors, you shouldn't be using conservative values for them, you should be trying to get as accurate values as possible; and I think this leads back to a question I posed here a little while ago: Are we possibly being too conservative in seismic PRA at this point, to the end that it

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MR. REED: ... is becoming a contributor more than we think it is?

MR. OKRENT: Are you going to answer that question or do you just ask that question?

MR. REED: I am asking it at this point. I have a bias and I will make that clear as I go on, but I really don't know completely the answer to that question.

MR. LEWIS: Is a bias another word for an answer?

(Laughter)

MR. REED: Let's say I have an answer but I need to qualify it. Okay?

MR. LEWIS: That's what I thought.

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MR. REED: I would like to spend a couple of minutes on this absolute versus relative question I heard discussed this morning. I think I am probably where Dr. Lewis was a few years ago, so bear with me.

I think the way Leon was trying to express it this morning is the same way I like to look at it, and that is, what I think is going on here in the minds of the engineers and seismologists and so forth is, there are these funny uncertainties that are running around that we can't get our hand onto, and it is because of these that it tends to lead us to want to deal with seismic PRA in a relative sense, rather than an absolute sense, and I would like to give you some examples of some of these uncertainties we are having a hard time getting out hands on.

Before I give those examples, another way to look at this relative versus absolute question is, if we had one million years and we could wait around, and if we built our plants the way we are building them today, and if we did our PRAs the way we are doing them today, if, in fact, we found that our failures, our core melts for example, fell within the probability distributions we are doing today, if we knew that a priori, then I would say that we have results today that we could compare on an absolute basis.

The problem is, at least in my mind, I have this feeling that as time goes on, the plants might not change,

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but our probability distributions would change. Therefore, on an absolute basis, the numbers are going to change.

But I think we can still, in a relative sense, if someone came in and shifted the hazard curves up by half a magnitude for example, I think we could still make decisions and conclusions, but we would do that on a relative basis.

MR. OKRENT: Excuse me. But you assume in that that all of the hazard curves shift the same way.

MR. REED: Yes. But they may not, of course.

MR. OKRENT: Exactly.

MR. REED: I appreciate that. I mean, it is sort of in a hierarchy. I'm not sure we can do the relative perfectly, but I think we can do the relative better than the absolute.

MR. OKRENT: My next question is, what is a relative seismic PRA?

MR. REED: An example would be to do a seismic PRA for one plant and then do a seismic PRA for another plant and compare those two results and try to draw some conclusions.

VOICE: Like Unit 2 and Unit 3 at Indian Point?

MR. REED: Yes, absolutely.

VOICE: What did you learn from that?

MR. REED: I very definitely learned there was

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an outlier in Indian Point 2, and just that very simple thing, we obtained from a relative PRA.

MR. LEWIS: Let me make a plug for relative PRAs just to confound the issue that, given what you said about realism versus conservatism in PRAs, with which I agree so wholeheartedly that nothing else you could say would make me feel mad at you --

(Laughter.)

-- the relative measure isn't subject to the problems I mentioned at the beginning, having to do with different error bands. If you really take the middle, which is doing a realistic calculation and doing the uncertainties as well as you can, then the relative position of two things does not change as you shrink the uncertainties. It is only when you insist on doing this bastardized -- forgive me -- conservative end of the error band as part of the PRA, that you can shift the relative positions of two things by changing the error bands without changing the mean or median or whatever it is.

MR. RFED: Yes, right.

MR. LEWIS: So I am with you on that.

MR. REED: It's a good point.

MR. BUDNITZ: John, let me disagree with that in part. Put the hazard curve back up there.

Now I think that is the IP hazard curve that I think

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I recognize, right?

MR. REED: I'm sorry. Say that again?

MR. BUDNITZ: That's the IP, Indian Point?

MR. REED: Yes.

MR. BUDNITZ: Part of what you see up there is differences of opinion. There is the Woodward & Clyde and Dames & Moore (phonetic)

MR. REED: I'm going to get into that in just a minute. Don't steal my thunder.

MR. BUDNITZ: I don't want to steal your thunder. But to the extent it is a difference of opinion, one of those opinions might be right.

MR. LEWIS: Or neither.

MR. BUDNITZ: Neither of them might be right, but one of them might be right, and provided they are respectable, if the tail of the distribution is, in fact, a respectable opinion, then it is worth giving it its weight.

MR. LEWIS: I understand.

MR. BUDNITZ: And that's a problem.

MR. LEWIS: I understand. But as long as -- I don't think we should argue as between ourselves on his time, so I won't say what I wanted to say.

MR. BUDNITZ: Whose time is it? Isn't it all of our time? Go on.

MR. REED: I knew I was going to start something.

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MR. SIESS: You gave an example of a relative PRA. Can you give me an example of an absolute PRA?

MR. REED: Sure. Take a PRA that has been conducted, and say the number of the mean frequency of core melt is exactly what it is going to be, take that and compare it to highway deaths and draw some conclusions.

MR. SIESS: That is an absolute use of a PRA. I see.

MR. REED: Right.

MR. ANG: That is still relative.

(Laughter.)

MR. LEAR: Country to country.

MR. REED: But on highway deaths, you know with fairly high confidence what highway deaths are.

MR. ANG: Nevertheless, you still don't know what the real risk is in the case of the nuclear rector. The highways you can do, but not the nuclear reactor.

MR. REITER: But he had a specific safety goal. He said the safety goal was 10^{-6}, and he used the PRA to say, "I mee the goal; I don't meet the safety goal." That would be an absolute, perhaps.

MR. REED: That's exactly an example.

MR. LEWIS: It could also be a mistake.

MR. OKRENT: Dr. Stepp wants to use your time.

MR. STEPP: Thank you.

I am Carl Stepp of EPI. Am I to understand by Leon's comment that thereason we cannot make absolute comparisons to PRA is we do not have an acceptable safety goal?

MR. REITER: That's kind of backwards.

MR. OKRENT: I'm sorry, Leon. If you are going to talk, you have to speak in a way that the recorder can here you, and also the Chairman.

MR. REITER: I think I would infer that we might have problems using a safety goal because of the lack of ability to endorse or sufficiently endorse absolute numbers.

MR. STEPP: May I just add to this one more comment?

I guess I am still having a lot of problem understanding why one cannot make absolute comparisons and yet make relative comparisons. If we are confident enough in the numbers to make relative comparisons, why are we not confident enough to make absolute comparisons?

MR. OKRENT: I'm with you, but I'm not sure we're a majority. But we're right.

(Laughter.)

Please proceed.

MR. STEPP: I feel like I am in good company now.

MR. REITER: 10^{-5}.

MR. STEPP: Myself, personally, I fit in a relative sense. I think we can do better with relative than absolute,

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but probably not as well with either one of them at this point.

Let me go back to my train of thought here, and what I would like to do is kind of give you some feelings from experience of why I have this sort of uneasiness about the amounts of uncertainty that are currently included in both the hazard and fragility characterizations.

First, starting with the seismic hazard, I will put back on here again the family of seismicity curves that you saw before. In fact, these curves did not come from one consulting group; they came from two.

This isn't quite right in the way I have broken these down, because there was some process that I never quite understood from the two individual reports. The curves were somehow collected, and I never did quite get the formula for collecting them. But in general, the curves to the left were from one consultant, and the curves to the right were from the second consultant.

The implications of all of this, the practical implications are as follows:

The curves from each consultant were weighted equally. The curves, the lower set of curves, when integrated with the fragility curves, produced, say, a mean frequency of core melt that, when compared with the similar operation for the second set of curves, was basically

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insignificant. So all you were doing was taking the results of the second consultant and cutting them in half by virtue of the fifty-fifty weighting. And one of the difficulties with this, as an example of uncertainty that leaves people feeling a little bit uneasy is that this is kind of an unresolvable thing at this point in that the two consultants will argue until the sun rises in the west that their curves are the best, and there is no reality and very little way we can find out what the reality is. But this uncertainty is here, and I wonder, if there had not been a similar process conducted for other PRAs, if we would not have had a similar situation.

MR. SIESS: How about a third consultant?

MR. REED: Excuse me. What?

MR. SIESS: What if you had a third consultant redo it? We would probably have a third set of curves.

MR. OKRENT: Well, there is a third set of curves further over, isn't there? It's my impression that the USGS predictions sit to the right of Consultant No. 2.

MR. REED: That's correct.

MR. OKRENT: Just so we understand the situation. And for Dr. Lewis' benefit, that's not necessarily an extreme position or a real tail-end of the tail.

MR. REED: That's right.

MR. SIESS: Nor is it necessarily correct.

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MR. REITER: It is only at a relatively high probability. The return theory is on the order of 475 years and 2500 years. Two points, I don't know if it's appropriate, but we should only use that in a very limited capacity.

MR. LEWIS: For the record, I never said the tail of the curve needn't be legitimate. I have often been a minority of one on ACRS.

(Laughter.)

MR. REED: I think this gives you a flavor from the hazard standpoint. Now let's take a look at fragility.

This is taken from one of the PRAs where there was a containment analysis in the published PRA, the median capacity -- this is in terms of the so-called damaged effective ground acceleration -- was given as 1.1 G with a logarithmic standard deviation of .26. That was the uncertainty value.

Now there was a general feeling among the reviewers that this was probably conservative, and although none of the reviewers attempted to do much with it, the NRC requested the utility to go back and have a harder look at this particular capacity and develop a new fragility estimate, and the answer that came out was 2.9 G with a beta of .38.

Now the thing that distrubs me about this is, if you take this a priori at the time the first one was published, what this says to me is, if I really truly believe the uncertainty of .26, then the chances of getting a value either in reality -- and I would hope also in terms of analysis -- that is equal to or greater than 2.9 is something like 1 in 10,000. This says to me that the uncertainty of the original estimate is very, very low.

The other thing that bothered me is, I would hope, as we spend more and more effort in trying to obtain an estimate of fragility, our beta should go down rather than up.

Another way you can look at this problem, if you look at it a posteriori if you believe the 2.9 value, now what is the chance that someone would come along and predict a value that was 1.1 G or less? Something like 3 times 10^{-3}. I think my conclusion from this is, one, I don't think enough effort was put in PRAs in quantifying fragility, and this is an example, and also in quantifying fragilities, I don't think there is a sufficient amount of uncertainty. There is this level of uncertainty that goes beyond the single engineer, although in this particular example, this is not two different engineers; it's the same engineer, so we don't have the problem we have with the hazards curves of two different groups.

I will go back and say a few words about discrepancies. Again, I put in paranthesis here, "errors." Some people call them errors, but I prefer to think of them

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as discrepancies, because they can be both ways.

As I have said before, I guess I have sort of a general feeling that PRAs, as being conducted, are on the conservative side, and that is, however, not considering design and construction discrepancies. The comments I am going to make are without considering design and construction discrepancies. And I think if we look at the process a little bit, we can see why.

First of all, if you look at the nature of engineers, engineers who do PRAs are the same engineers who have been involved in designs, and there is a built-in tendency to be conservative. Engineers are by nature conservative.

Secondly, the analyses that are used for the basis of PRAs are the original design analyses, and there is not a lot of time spent in going back and going through every detail of those. That tends to encourage conservatism.

Finally, the general effort that is put into seismic fragility analysis, to my thinking, is not large enough. The effort is a minimal effort. It is the three things together which tend to create conservative fragility values.

Now the other half of this coin -- and this is where I had to add my caveat -- is my concern for what I would call design and construction discrepancies. And when

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I say "design" in this category, I include discrepancies not only in the original design calculations, but also discrepancies in the PRA.

I would consider as evidence that example I gave you, that first fragility estimate for that containment was a discrepancy, because of the fact that during the second analysis done for that, they went back, they found that there had been -- I wouldn't call it an error in the analysis procedure, but a conservatism that had not been discovered the first time.

Second of all, they found there was more steel in the containment.

The third thing they found was that the soil loads were not as severe as originally thought.

But I think there are other classes of design and construction discrepancies that we don't even know about at this point, and things that sort of loom out there and sort of, to me, pollute this uncertainty picture and make it difficult for me to think that the answers we come up with are of value in an absolute sense.

I think, however, that absolute values are desirable, but I don't think they are necessary. I think we should strive toward them. There should be effort put forth in the design and construction discrepancy area to try to resolve and incorporate them. But I think we can

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go on with our work, making relative comparisons.

MR. BUSH: Could I ask you a question before you take that one off?

MR. REED: Sure.

MR. BUSH: One thing I don't see up there, which is a natural fact of life, is, the cited properties, often they have very little relationship to the actual properties.

MR. REED: The which properties designed?

MR. BUSH: The cited properties bear little relationship to the actual properties.

MR. REED: You are talking about material properties?

MR. BUSH: Material properties. Invariably they will pick them out of a standards book, and if you measure them, you will normally find a substantial difference between that and the true value, a very large value.

MR. REED: This is built into the seismic PRA design process. For example, if you are talking about a steel member, for example, something like that, if you use a median yield, usually the median yield is a factor of 1.2 or 1.3 larger than the code value. I mean, there is an attempt to try to adjust out these sorts of things.

I am more concerned at this point with things we can't get our hands on.

MR. SIESS: How would you propose to go about getting a handle on design and construction discrepancies?

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MR. REED: Why don't we hold that question? Could we hold that and get to the end?

Let me get to the end, and then I will give you a thought.

MR. OKRENT: Before you leave that, though, if you were a Commissioner and you had to make a decision concerning the need or not to provide some additional safety measure, whether it is an actual piece of hardware or some other kind of thing, that takes effort and money and so forth, how do you do that with a relative seismic PRA?

MR. REED: Well, you have a PRA intact at that point without the particular measure incorporated, right? You do a value impact.

MR. OKRENT: But how do you do a value impact if you are not able to put an absolute value on the risks involved and in some way compare it against the cost? Are you just going to have two columns that are in different units? Is this what you are proposing?

MR. REED: No. I am sure there is grey, but there are extremes here. If you incorporated the particular modification and cost one million dollars to do it and made absolutely no change whatsoever in the risk, I don't think you would do it. That is easy, okay?

MR. OKRENT: Sure. The problems arise on the ones that are one to one, but again I think, although it is clear, you can identify differences between Indian Point 2 and 3 on a relative basis. There are some decisions that require at least a feel for the band in which the absolute number falls.

MR. REED: I have another comment I will make a little later. Let me move on to it.

When I get down to this item here, "Interpretation of Results," maybe I can say a few words, because I think that is what the problem is. It becomes interpretation and what you get out of a PRA analysis, what you do with it, with the state of the art, as I have described it. Where do we go from here?

My personal feeling is, based on the reviews that I have been involved in, I feel that greater resources need to be spent on fragility analysis. I cite, as an example, I have had some experience in recent times participating in and observing the monumental amounts of analyses and re-analyses being conducted for the Diablo Canyone facility. It is just mind-boggling. And I compare that mentally to the amount of effort put into a typical fragility analysis for a PRA, and it is a peanut, and I see that as an extreme discontinuity. I think there need to be additional analyses as a part of fragility. I think it would be

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money wisely spent, not just money and effort thrown at the problem.

Some of the areas in which I would look for improvement, I feel that, as I explained before, the analyses are very much dependent upon existing plant design analyses. I think when a seismic PRA is performed, there should be seismic fragility benchmarking analyses. And what I mean by that is similar to what is sort of done in the SSMRP, where they repeat analyses many, many times in an experimental format. I think as a very minimum there ought to be one or maybe two analyses where possibly you start with the models, if they are salvageable from the original design analysis and correct the properties, the damping, the stiffnesses and so forth, and try to run, say, a median analysis to get calibrated, because in many cases we tended to, in our adjustments of the original design analyses, move a great deal away from the original analysis was.

With regard to the use of generic versus plant-specific data, I think generic data has a very definite use in PRAs, particularly in a screen process, as I said, but when it comes down to a series of components that are controlling the analysis, the possibility that these may be conservative or unconservative because they are generic. I believe plant-specific analyses and/or testing should be

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conducted in which to quantify those dominant important components; not all the components, just the important ones.

Finally I think there should be more effort spent in reviewing the design analyses. My sense in reviewing the calculations and reports, I don't get the strong, warm feeling in my stomach that a lot of time is spent really going back and tearing into the original design analysis to pick through the various assumptions that may or may not have been made in actuality.

I think it is important that the philosophy be in PRA, seismic PRA, to produce unbiased estimates of the seismic capacity.

Keep in mind what a median value is. A median value says there is a fifty percent chance that the true value is higher and a fifty percent chance that the true value is lower.

The definitions of failure, I have this feeling they are probably conservative, and I think this is partially connected with the definition and characterization of the fault trees. I think there can be a considerable amount of research effort in trying to really define what failures are for structures, in particular, and also in the areas of equipment operation versus structural types of failures.

One area that I think would be kind of interesting is, as other people have advocated, we should do more testing on a generic general scale to get a better feeling of how structures and equipment respond. I think we could parallel that kind of effort with what I would call prediction experiments, because that is really what a PRA analysis is. It is a game of prediction. What we should do is, for example, test an assemblage of concrete elements, a couple of walls and slabs, maybe two stories, set up the experiment and then invite different people in the PRA game to estimate at what level this will fail, or if it's going to be a fixed level of motion, will it or will it not fail, or what will be the amount of damage? Let's see what sort of results we get from an experiment like that, and how would you use the results of that test?

MR. ANG: Excuse me. And how would you use the results of that test?

MR. REED: I don't know. Maybe I am being skeptical. I would be surprised if everybody was real close.

(Laughter.)

Let's wait until we get the answer. I am sure there is a myriad of statistical tools that could tell how good we are at our predictions.

MR. TSAI: You would be surprised and so would they.

MR. REED: Well, let them crawl all over the thing. Tell them the ground motion going and let them crawl all over it. Look at the construction drawings. Put their

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fingers in the ground, anything they want, to come up with what they think is going to happen.

I had an experience with a four-story test structure on the Nevada test site. We put a shaker on it to try to destroy it. We all tried to predict at what level we were going to be able to destroy this thing, and we just couldn't do it. We got the shaker on there, and every time we would get on residence, the thing would start to crack and we would lose it, and we never were able to really destory that structure.

VOICE: It's still there, John.

(Laughter.)

MR. REED: I understand they passed it a few times and tried it again.

Profession-wide hazard data, this is something that is being done, the L-cubed program that was alluded to this morning, also the program that will be conducted here by EPRI. One of the things about fragility, we at least have a chance to invent experiments to test equipment and structures. We don't have quite the sama luxury with the hazard side of the thing. The best we can do there, I think, is to get as wide a contribution of opinion and to try to eliminate as much of the unwarranted uncertainty, that uncertainty which is present because of misunderstanding or lack of communication between people. I think this is being

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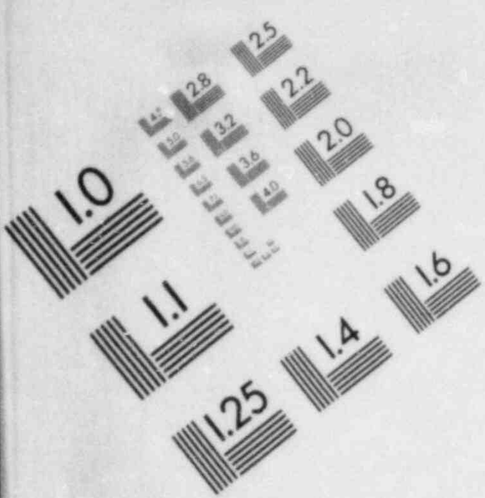

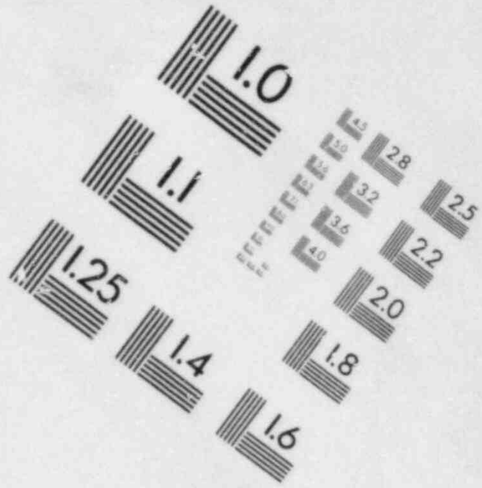

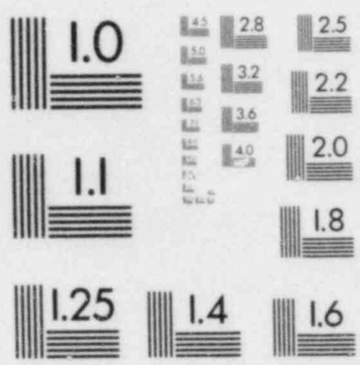

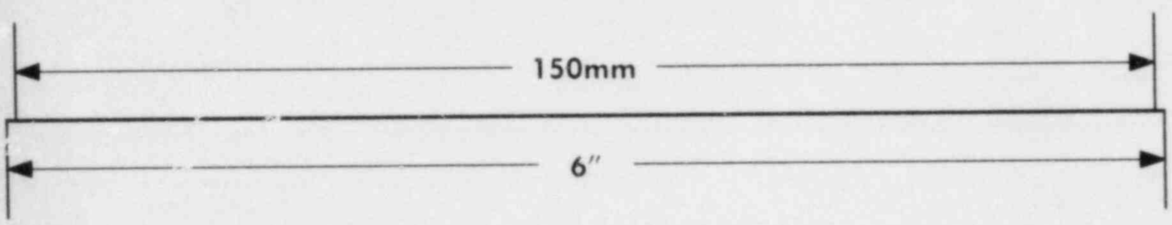

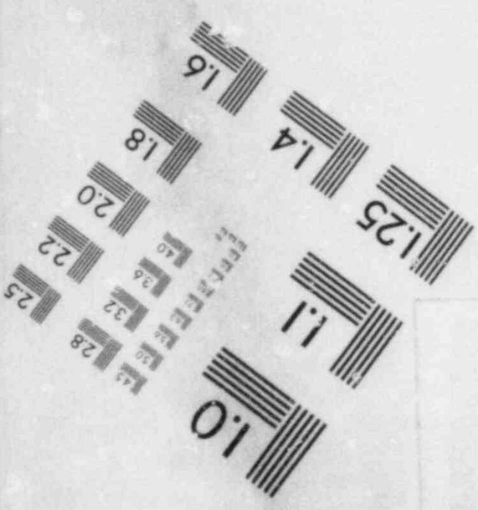

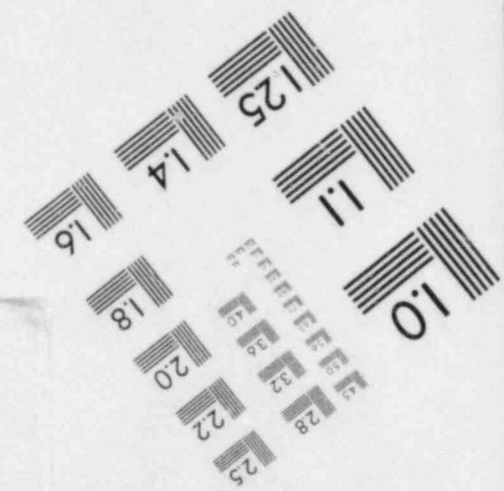

IMAGE EVALUATION TEST TARGET (MT-3)

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done.

One area that hasn't been touched on toda; is the business of the relationship between the hazard and the fragility. We have this thing called a ground motion parameter, which has been defined in many different ways. I think most of us feel that, although we have done well, it is not the most satisfactory way of going about this, and I think this is an area of uncertainty that is floating around that we need to try to tie down.

Now on this issue of interpretation of results, a comment I made this morning, kind of also in response to Dr. Okrent's question here, I feel left somewhat cold by only dealing with probability of frequency distributions of things like core melt and so forth. I think whatis also important and which I think is valuable information that comes out of a PRA analysis is, what is it that contributes to the results from the hazard perspective? Which hazard curves are the dominant contributors to the, say, mean frequency or other points on the probability distribution from the fragility side? What are the structures and components? Not only that, but at what acceleration level is this taking place, because I think that is important, too.

If you have a situation where the mean frequency of core melt is dominated by an acceleration that is at .3 G, that is much different than a situation where the

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mean frequency of core melt is dominated by an acceleration which is 1.5, because if you think of it as an incremental contribution to risk, to the background risk that is already there, if you, in fact, have a truly 1.5 G earthquake in an area, you are going to find that there is going to be a large amount of destruction to facilities and the additional effect of the nuclear power plant is much different than the case where the nuclear power plant fails and the rest of the environment around is sound.

So I guess what I think needs to be done -- and in a sense, I don't know the answer to your question, Dr. Okrent -- in many cases of how you even perform these so-called relative analyses, but I suspect that what needs to be done here is a better understanding of what you are going to do, what is the meaning of the results that come out? And I think it is more than just a probability of frequency distribution. I think it also has to do with the things that go into that. And along that line, what I would like to see more of in PRAs is a bit more clear presentation of results. I think a reviewer finds that when he gets a PRA, he immediately wants to tear into the results and try to find out what is contributing and also to try to perform some sensitivity analyses to see what is sensitive and what is not. And I think that sort of information needs to be provided as part of PRAs.

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Now with that as a background, I will get into the assigned topic of my presentation, and that is, What about potential uses of PRA? And what I have attempted to do here is list some different uses, some of which you are familiar with, some of which you may not be, and some of the comments I have made may also shed some light on the validity of each of these uses.

We have, of course, the full-blown seismic PRA. This is what we are doing. From this, we can identify the various seismic risk contributors. Once you have a PRA, you could use that as a basis, although maybe not absolutely clearly, but certainly in the sense of extremes, to try to identify cost-effective modifications. Indian Point 2, I think, is a very good example. It didn't take very much money to fix the situation between Unit 1 and Unit 2 control room roofs. I think the PRA is not a static, once-performed study. It is something which should be performed throughout the life of a plant.

As we all know, as time goes on, safety issues will arise, and the question is, what are the implications of the safety issues? And by having a PRA sitting there waiting, these issues can be incorporated and the analysis rerun to find out what the implications are.

I think there is a potential use in making relative risk comparisons between plants. A simple-minded

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example is the list I showed you earlier. I see Millstone being a very new plant, and I see a mean frequency of core melt near 10^{-4}, and my immediate reaction on seein that is, how come it is so high?

Well, that is a use in a relative sense of PRAs.

The next example is total risk to all plants. Here we are sort of leaning toward the more absolute use than a relative use. I think because of the fact that on the East Coast we have plants that are very close to each other, there will be dependencies, and the question is, is, in fact, the risk independent or dependent, and what is the risk of probabilities of core melt to one or more plants or to several plants?

This could be done using a PRA.

The next example is using the PRA as a tool to decide which components should be modified if a safety issue comes up concerning equipment or something. The question is, should that equipment be modified? But first, a PRA could be used to determine whether, in fact, there are real serious safety implications. In a sense, an example of that is the study that Lawrence Livermore has recently done on the low fracture toughness of steam generator cooling pump supports.

Another use is to quantify conservatism in regulatory requirements. You could very well take the PRA

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as a tool -- not easily, but you could certainly try to understand what conservatisms generically exist in the current regulatory requirements, or the next item, quantify changes in the requirements. If you decide to change the requirements, what are the implications?

Again, a recent study by Lawrence Livermore Labs, the A-40 value impact study, is an example of where an attempt was used to make a PRA to try to understand the implications.

I repeated the next one, which we've already talked about.

The following one is a cost/benefit tool for earthquake preparedness. I see this as a use for the utilities. I see in reality, in a practical sense, a greater problem with earthquakes -- not from the big ones which cause core melt, but from small earthquakes, earthquakes, say, of the size or larger than the OBE or less than the SSE, which cause the plant to be shut down and money lost because of the fact that the plant is shut down. The PRA could be used as a way to identify critical locations or components which to monitor immediately after an earthquake. The PRA could be used as a way of determining where to put instruments in a structure to develop an argument to restart the computer immediately after the earthquake.

22pb1

There are certainly uses of PRA in load combinations, very difficult to get a handle on load combinations or make a deterministic viewpoint.

If you have an SSE with and SRV, should they be combined absolutely in phase, or how should they be. A PRA could be used to address that sort of question.

And some of the little more general runs, such as categorizing research needs, investigating risk characteristics of alternate plant concepts, thinking of this in terms of plants that have not been designed or are in the process of being designed. Trying to decide what are the best configurations of components, PRA such as for GESSAR could help guide modifications, before in fact, the plants are even made. Prioritize the safety issues, and finally, the last one which I consider the weakest from my perspective is compare seismic PRA results to other risk contributors. The idea of comparing the results from earthquakes with, say, highway fatalities or airplane fatalities.

Thank you.

MR. OKRENT: I wonder if I could ask a question. I think I have heard you this time, and once before indicate that you thought the uncertainty in the fragility was not a big contributor to the overall risk. That it was the uncertainty in the seismic hazard and the median of the fragility that were important.

22pb2

MR. REED: Yes.

MR. OKRENT: What I have in mind is, one of my students did a few calculations taking Zion as an example, and it seemed, from his calculations that in that particular case the way the seismic hazard curve in the PRA and the fragility of the important components happened to fall, there was very little overlap leading to a low total contribution of seismic to core melt.

But in this case, an increase in the uncertainty or for the more important components with regard to their fragilities was a sensitive parameter. And he could calculate by postulating an arbitrary increase in the uncertainty, a very large increase in predicted core melt.

Now, it was also clear, by taking a different hazard curve, the same effect did not occur by changing the uncertainty and the fragility.

MR. REED: Is he using one hazard curve or a family of hazard curves?

MR. OKRENT: Well, no, I have to think now. I am not sure whether --

(Pause.)

I am not sure whether he took a distribution on the seismic hazard curve or a single curve.

MR. REED: I think that is the point.

MR. OKRENT: That may be.

22pb3

MR. REED: For a single hazard curve you are absolutely right. But the point is --

MR. OKRENT: I will have to check into that.

MR. REED: Looking back at this again, it is just that Zion was similar. There is so much uncertainty, such a spread that whether you have the Beta u, thinking of it on a component level here, of being .4 or .6, it gets sort of swamped.

MR. OKRENT: I have to check that. That may be the difference. Thank you.

MR. SIESS: You were going to tell me how you thought we could get a handle on construction design discrepancies.

MR. REED: Well, I think it is an extremely difficult problem. One think that has been of curiosity to me, though, it seems like it would be a worthwhile effort to take a look at ordinary industrial or just buildings in general, and study -- someone try to make a study. Because we really do have a database there. We know how many buildings have been built. And we know how many buildings have fallen down.

MR. SIESS: But we don't know how many mistakes have been made.

MR. REED: That's right.

MR. SIESS: But how about taking a plant that has

22pb4

undergone a fairly extensive independent design and construction verification program, of which there is at least one.

(Laughter.)

And there are two more, I believe, underway now. How extensive they are -- and do a PRA before and after, before the changes are made and after they are made. Would that help?

MR. REED: But what the problem here is, whatever you can identify is no longer a discrepancy anymore.

MR. SIESS: I know, but let's assume this has fewer residual discrepancies.

MR. REED: But you know what they are, right?

MR. SIESS: (Nods affirmatively.)

MR. REED: The problem is trying to get a handle. Once you know what a discrepancy is you can incorporate it into the analysis. The problem is, you want to somehow account for discrepancies that you don't know what they are, but you know they are there by virtue of human nature.

MR. SIESS: But if I take a plant that had not undergone this program and had done a PRA on it and got a certain answer, now I take a plant where I made a thorough investigation and found several hundred design and construction discrepancies, and I made changes to the plant. Whether it's several hundred or not, I don't know.

22pb5

MR. REED: You could run those through the analysis.

MR. SIESS: And if I don't get any difference I could forget about design.

MR. REED: That's one data point, but there may be some design and construction discrepancies that are not -- There's another thing I didn't say that I wanted to put in perspective here. We have QA programs, supposedly there are not discrepancies.

MR. BUSH: What's that?

MR. SIESS: No, no.

MR. REED: The goal.

MR. SIESS: I see the two statements but they are not connected.

MR. BUSH: They are not valid statements.

MR. LEWIS: He said supposedly.

MR. REED: The goal of the QA program is to try to eliminate as many errors as possibly can occur. Okay? Now, as far as the SSE is concerned, I think we are being very successful. But the problem is, when we are developing fragility data, we are way above the SSE level. And here a design and construction discrepancy, that might not be important for the SSE, because of the fact there is a large design margin built into the design process. May become important for developing a fragility value that is maybe two or three times the SSE value.

22pb6

MR. OKRENT: Dr. Tang?

MR. TANG: Dr. Reed, you are not implying that for instance, the Indian Point hazard curves are generally of the same degree of uncertainty exists for other sites, such as for instance, those in California.

MR. REED: For the east coast plants.

MR. ANG: For the east coast plants.

MR. REED: Yes.

MR. TANG: That would not be true for others.

MR. REED: I don't know. I cannot -- I really have to just make that comment for the PRAs that have been performed to date. And they have all been east coast plants.

MR. ANG: On the same point that Dr. Okrent alluded to, the uncertainty in the fragility part, in view of the fact that we have been extrapolating fragility way beyond the range where we have calculated data. I would think the uncertainty there may also be very high.

MR. REED: It is hard to -- it is not that the uncertainty for fragility is unimportant. What I am trying to say is what is important is we get that median value right.

MR. ANG: But that is the point.

MR. REED: The median value has a distribution about it. Okay? We want to get that right. And we want to spend the effort and time so that if we have a containment, if it is really 2.9 g, we don't want to call it 1.1 g. But once we have it, no matter, it is still a median value, and therefore, there in an uncertainty about that median value.

Now, if you look at the data that have been given for components, say, range between something like .3 to maybe ... igh as .4 or 5, something like that. So, with those values, it is not as important to spend a lot of time spending your effort, trying to fine tune that data value.

It seemed to me, the effort ought to be spent fine tuning where that median value is. And I understand the two are not independent of each other. That is the problem. Bob?

MR. KENNEDY: I would like to make a couple of comments. We've tried doing some sensitivity studies. And I do not agree that the median value is the most important parameter for the fragility curve.

Based upon our sensitivity studies, the most important portion of the fragility curve is in the 10 to 20 percentile frequency of failure range. And that is the portion of the fragility curve you would like to do best.

And that depends on both the median value and the uncertainty value.

Now, a couple of other points -- I am not familiar personally with this containment problem that you are mentioning because I wasn't -- I'm just not familiar with it. But I am familiar with some similar problems, although

22pb8

that appears to be very extreme compared with the other problems.

One of the things that happens in my opinion in fragility estimation is design engineers can feel comfortable estimating fragility levels up to some kind of ground motion level. When they get beyond that ground motion level, the whole thing is a game. And frankly, I believe that ground motion level is somewhere, probably if I had to pick a number, I would pick the number of about the .7 g range. And beyond that it doesn't make a lot of sense to be talking about fragilities.

At least on projects I am familiar with, what is often done is to look at those components in which there is at least -- there is estimated to be at least a 5 percent probability of failure, up to .7 g. To try to do a decent job on those.

And those where the 5 percent failure frequency or probability is at values above .7 g, not to do a very careful job. That may be wrong.

That came about from the belief that earthquakes beyond that level in the east were incredible events. And at least in some of the early PRAs, and therefore, really were not going to have a big influence on the solution, on the end results.

Now, sometime after the end results were found, they did have a big effect. Whenever they do have a big effect, I have a total lack of credibility in the end results.

You're containment, the containment problem you mentioned there falls into that range. In my judgment, who cares whether its median is 2.9g or 1.1g? They are both so high that I don't think various engineers would agree. I don't think there is any possibility that at those kinds of ground motion levels you come close to having an agreement between engineers as to what the median was.

MR. REED: The problem is that this particular containment was a major contributor to early release and early fatalities. And going from the 1.1g to the 2.9g, literally eliminated seismic as a contributor to early fatalities.

MR. KENNEDY: You are making too much out of the analysis, then because we don't have a single earthquake record up in that kind of range.

We are getting probabilities out of mathematics.

MR. REED: But this is the reality that is existing.

MR. KENNEDY: In my judgment, once we start getting to earthquake levels beyond 1g, we should cut our -- I don't know what we should do, but we should certainly asterisk any of our end results. Because our end results are highly suspect if they are being governed by fragilities up in the

22pb10

1.2g, 2g, 3g ground acceleration range. They don't have any meaning anyway.

MR. REED: Take Limerick for example. There are roughly four or five electrical components that are dominating that result. And their median capacities are, I have forgotten now, but it's like 1.3 to 1.5.

MR. KENNEDY: They have big betas on them. All the ones on Limerick, because now you are in what I am very familiar with, all of those have a big enough betas that they have probabilities of failure greater than 5 percent below the .7 to .8g range.

And therefore, they have a portion of their fragility curve down in a region that, I think, we ought to be trying to estimate the fragility curves. And I think it is that portion and not the median that is really important.

MR. REED: What would you say if the median was twice as big?

MR. KENNEDY: I would say I don't care. If the median was twice as big, they wouldn't dominate.

MR. REED: That's right, exactly.

MR. KENNEDY: And I wouldn't have any idea what their importance was.

MR. REED: That is the point I'm trying to make. Are we sure this thing we are calculating in the game we are playing is correct, or is it possibly twice as big.

22pbl1

MR. KENNEDY: Well, that is a really difficult problem. John, I have heard you a number of times say you think the medians are too low, and the uncertainties are too low. I personally agree with you. But I can list a whole number of other people for whom I have a great deal of respect who think just the opposite. They think the medians are too high and the uncertainties are too high.

I think this is a battle that is between people with a very strong probability background, yourself and myself, and people with a very strong design background. And at this stage, I don't know who is right. I think the industry can best say that probably the whole industry can do its best job down there in about the 5 percentile to maybe 20 percentile range of the fragility curve.

And thank goodness, that is the region that appears to be the most important region. And what is happening is that those people who believe the uncertainty should be bigger, want the medians higher, because raising the median, raising the uncertainty still keeps this 5 to 20 percentile region, generally about where it is.

I think that a fairly broad portion of the industry will agree, in that portion of the fragility curves. And there will always be, at least at this time, very large differences of opinions at the median levels and at the beta levels.

22pbl2

And they all seem to go the same way, in my opinion. In my observations those people who like big betas, big uncertainties also like bigger medians. And you and I both think the betas and medians ought to be raised some. And others don't agree.

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MR. REED: Yes.

MR. KENNEDY: I don't know what we are going to do about that portion of it. I think you're wrong. I hope you're wrong about the importance of the medians, because I don't think we're going to solve them.

MR. REED: What I see is the analysis being extremely sensitive to the median, and what I see is the values of the median jumping around a lot, depending on how much effort you put into trying to determine what they really are. And I really feel very strongly that if we have to spend more effort -- and I think we should -- we should be trying to perform our seismic fragility analyses to try to get as good an accuracy on that median value, and in t'e process of doing that, the probablistic aspects of it, namely the degree of uncertainty should come down. The more effort ycu spend, you would hope the uncertainty would come down.

MR. KENNEDY: The more you learn about the uncertainties, the higher up it goes.

MR. REED: I know.

MR. KENNEDY: Do you see that jumping around in the medians' cases where the medians go below about 1 G, because I haven't seen that. I have seen where various people have seen that in different medians, that much, when the medians are down in the range where they have some

mgc 23-2

meaning.

MR. REED: I would have to agree with that.

MR. OKRENT: Dr. Bohn?

MR. BOHN: The question of medians and uncertainties is tempered, I think, by the consideration of the correlation, because if you have a correlation between failures or a lack of correlation, you can have wider changes in probabilities of failures of several components. An example would be the six service water pumps in Zion. For the commercial PRA, they were assumed to be completely dependent, so that the failure of one implied the failure of all. So the responses were fully correlated, but the fragilities were not necessarily correlated. You have a one out of six success criteria, and the service water pumps probably would have dropped out of the analysis at any degree of correlation, and then you would have gotten an entirely different number in the Zion PRA.

MR. KENNEDY : That's a different problem. The problem of correlation, that is a very tough problem.

Yes, a lot of the commercial PRAs have assumed that in a seismic event, you lose the benefit of redundancy because you have identical items of equipment at identical locations, and the assumption has been, they will be all knocked out at approximately the same ground motion level. That's really tought, to know whether that is a reasonable

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assumption or not. It is certainly a critical assumption.

MR. REED: And it is a critical assumption. I mean, Mike, you can carry the argument even to the crib house roof, the assumption that there will be the failure of the crib house roof means there will be the failure of all six pumps.

MR. KENNEDY: That was also a major contributor at Zion. The failure of the crib house roof was assumed by the systems people to automatically mean failure of all of the components.

MR. BOHN: We did a sensitivity study on that. We found it made about an order of magnitude difference if you assumed the crib house failed, but didn't knock out all service water pumps. So I think we bounded that effect.

MR. OKRENT: Mr. Reiter?

MR. REITER: Yes, John. I wonder if you, Bob, Mike or someone else would comment on which measure we should use in our comparisons. Some people talk about core melt frequencies. Some people talk about early fatalities. Sometimes you get different messages, depending on which measurement you use.

I wonder if you or others would comment on that.

MR. REED: I think you would have to look at all measures of importance. The nice thing about core melt was, when the PRAs came out, they gave you nice plots of core melt, and you had something there to work with. The other release categories, there was nothing there to work with. That is the reason that everyone focused on core melt.

But I agree, if other release categories are important -- this is back to the thing I said about more information being given in a PRA. I think in a PRA, not only should the family of fragility curves for core melt be given, but also the family of fragility curves for each of the release categories that are of importance.

MR. KENNEDY: Seismic is going to have more effect on early fatalities. If seismic is important on core melt, it will be even more important on early fatalities, because again it is a common mode damage mechanism, so it has more effect on fatalities than it has on core melt. But they are both important measures.

MR. OKRENT: May I ask Drs. Reed and Kennedy whether they think aging has been included in the current estimates of fragility?

MR. KENNEDY: I don't think they have been very well included, because I don't -- it is not clear to me that we really know the effect of aging on seismic capability of equipment. If you look at the experience data from past earthquakes, you don't get the feeling, anyway from experience data, that there is a major aging problem, because some of that equipment has gone through earthquakes in the .3 to .6 G range was twenty years old at the time

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it went through the earthquake, and it didn't seem to result in failures.

Now I am not convinced that aging is a major problem. In my opinion, it is not as big a problem as design and construction discrepancies, but I don't think it is being very adequately covered in fragility work.

MR. OKRENT: Questions?

MR. SIESS: Can we come back to that example you gave with the containment?

What was being calculated?

MR. REED: The fragility curve for failure of the containment.

MR. SIESS: What does "failure" mean?

MR. REED: Some sort of deformation that was beyond a ductility limit, beyond which --

MR. SIESS: But the concern was leakage, was it not?

MR. REED: Absolutely.

MR. SIESS: They didn't ask for that, did they?

MR. REED: No.

MR. SIESS: If they had asked for what they really wanted, I would have been interested in seeing what the spread would have been. That was a mistake.

MR. REED: I would like to make one other comment on this question of median versus the uncertainty. It is

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sort of a practical comment.

I guess one of the ways I look at this is, in this benchmarking analyses is, I think benchmarking analyses ought to be done to try to get that median, but I don't think you have to do a whole bunch of benchmarking analyses to get the uncertainty, that you can use the approximate methods that have been evolved in the commercial PRAs to go after the uncertainty part of the problem.

I certainly would not advocate throwing away the uncertainty in the fragility. It is just that you do not have to do a lot of sophisticated analyses to fine-tune what the value is.

MR. SIESS: Could you tell me what you mean by "commercial PRAs"?

MR. REED: PRAs that have been done for the plants, as opposed to the SSMRP.

MR. SIESS: As opposed to SSMRP?

MR. REED: Right.

MR. OKRENT: Dr. Tang?

MR. ANG: I would like to pursue this business of uncertainty and fragility a little bit more.

Dr. Kennedy indicated that the main contributor to risk is at the 10 to 20 percentile level. I would agree, if we indeed have the margin of conservatism that is designed. I think that is the basic assumption.

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MR. KENNEDY: Sure.

MR. ANG: In light of the fact that you indicate there is uncertainty, an expert's opinion would differ as far as the median resistance is concerned between the designers and the people with a background in probability, should that particular type of uncertainty or difference in opinion be cranked in, just as you do in the hazard area?

(Pause.)

MR. KENNEDY: I would answer that I think -- well, it's a hard one to answer. In the ideal world, it should be cranked in. In other words, all of these fragility curves should sort of have the same slope. You should have a whole spread of them.

MR. ANG: Put it just the way you did with the hazard?

MR. KENNEDY: Just like on the hazard curve.

On the other hand, it has also been my observation from sensitivity studies that in a typical seismic PRA, the 90 percent bounds on core melt frequency from the 5th percentile to the 95th percentile, or maybe is should say the 80 percent bounds from the 80th percentile to the 90th percentile, the 80 percent bounds on core melt frequency, are typically like four orders of magnitude, which by sensitivity studies, three of those four orders of magnitude are due to the hazard curve.

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When you use a knife-edge fragility curve, no uncertainty whatsoever, you only lower that uncertainty bound in your end results one order of magnitude.

It seems to me that unless we can get a considerable reduction in uncertainties in the hazard curves, I am not too enthused about the idea of making more -- I don't know what the right word is -- but more complexity to the fragility curves by having different slopes.

MR. TANG: Well, calculationally, I don't think it would be any more complex than what you have already done with the hazard curve.

But what concerns me, suppose you do a PRA, let us say, for a site in California. So far all of them that have been done have been in the East where I can understand there would be considerable differences of opinion, expert opinion, as far as hazard is concerned. But if you do it for a site in California, probably the expert opinion will have very little difference. The difference in opinion on the fragility, in fact, may dominate.

MR. REED: Is that really true?

MR. JACKSON: No. The difference in hazards calculations is worse on the West Coast than in the East.

MR. KENNEDY: One of my problems, Al, is, if we are going to have a whole family of fragility curves with different slopes, and we are almost at that stage, getting to uncertainties on uncertainties, i.e. to the next level. It's not just a small additional effort. I believe to do it right, you would have to have many different people generating the fragility curve for that one component. You couldn't have one person generating the fragility curve of the component and have him honestly account for all of the diversity of opinions.

MR. ANG: But I think the game is no different from that of the hazards side. After all, we are after realism, and in the real world, there is, in fact, a considerable difference of opinion in fragility curves. It seems to me, that should also be included.

MR. OKRENT: Dr. Bush?

MR. BUSH: My experience in PRAs tends to be in other areas, so I would apologize in that respect. But the basic assumption in common mode failure redundancy, I don't think tends to be supported in other PRAs. That assumption here, it seems to me, has a tremendous impact on the values you come up with.

MR. REED: Yes.

MR. BUSH: And I really wonder how valid they are, quite frankly. I really think that is a weak link in the assumptions.

MR. KENNEDY: I don't know how you would be able. In the current state-of-the-art, it's very difficult how

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you would be able to really evaluate what the cross-correlation on capacity of two identical components, two identical pumps mounted side-by-side. They will both see the same input, i.e. there is a very high correlation on the input. The industry is struggling hard enough to come up with the fragility curve estimate for those pumps, and now we have to struggle to come up with a cross-correlation on those fragility curves. It is a tough question to answer, and the PRAs that have been done to date have taken a conservative view.

MR. BUSH: I agree that that would say, if I had components lined up and they weren't on the same shock wave, they would all fail, and they don't do that.

MR. SIESS: They don't know the frequency.

MR. BUSH: I guess they don't. That may be the problem.

MR. OKRENT: Dr. Smith?

MR. SMITH: Paul Smith, Lawrence Livermore Labs.

I think the point missed in this discussion of how much effort should go into fragilities is, while it's true, say, in getting the bottom line number -- and that's almost a mistake sometimes -- the fragilities aren't as important as you might think.

Now if you come up with a decision based on that, you say, "I think that plant needs to be strengthened," that

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is a decision, okay? It somehow needs to be strengthened.

Now the fragilities become very important, because you have stated your state of knowledge on fragility curves, and advanced, as Dr. Bush has shown, but the reality may be you are attempting to strengthen either a component that is really strong or one that is weak, and your definition in getting to the bottom line is not adequate for you to distinguish between those two components. So in the decision as to which components and how to strengthen the plant, now fragility has become much more important than they are in coming up with an estimate of core melt or things of that sort, redeeming uncertainties.

MR. SMITH: You have an uncertainty. You say, "I don't know my fragilities within a certain range, so they are anywhere in there." Well, where they are is very important as to whether or not, if you strengthen one, it will really reduce the risk. And in that context again, for decisions on strengthening plants, whatever the hazard curve is -- and we may not know what it is -- it is the same for everything at that plant, so for that type of decision, the uncertainty and the hazard curve are not nearly as important as it is in these others.

So these kinds of decisions as to where the uncertainties enter in and where they don't have to be kept separate in what kind of decision you are trying to make.

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MR. JACKSON: I have one or two questions, depending on how you split it up.

MR. REED: One at a time, please.

MR. JACKSON: I think it's one question.

The ACRS in its letters, I guess over the past two years and on more recent OL reviews, has requested some sort of margin analysis. That is a generalized comment. And the observation you have made is, in newer plants your expectation of the risk would be lower from seismic.

MR. REED: Right.

MR. JACKSON: Then what criteria, if we were to implement a program of requiring more seismic PRAs, what criteria would we go about using to select where we would require that they be done?

MR. REED: Do you mean which plants?

MR. JACKSON: Yes. It seems from the inference you were making, the plants we should be doing them on were the older plants, the SEP plants.

MR. REED: Right.

MR. JACKSON: Not the newer ones. Yet the concern seems to be generated, maybe out of necessity, on the newer OL plants. So based upon the experience you have, what kind of criteria would you go about using to select those plants?

MR. REED: One of the problems is, my expectations

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haven't been realized. If Millstone is an example of a newer plant and a very high risk, that certainly validates what I was expecting.

I think PRAs should be done for all plants. That is really my feeling. I think it should be a living document, something like you have control room simulators to train control room operators how to control the plant. I think in parallel there ought to be a PRA for a plant, because I think you will find as you go down the road here, there will be safety issues which will come up for the years to come, and every time one comes up, you want some basis to resolve it. And I think trying to resolve it in a deterministic manner is not fruitful.

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MR. KENEEDY: I think you have to be very careful when you compare results from different seismic PRAs. The Millstone is a case in point. I don't want to make comments misconstrued. I don't know which of the various PRAs is correct.

(Laughter)

But I know there is a tremendous difference in the methodology and details used on Millstone versus the other three. and to that extent I would never make a comparison of the results of Millstone with any of the other three.

Again, I have no idea which is correct, but the differences in end results come about because of the differences in the way the results were calculated. And one should not reach off to the conclusion that maybe newer plants have more risk than old plants without going in and looking at how the work was done.

MR. REED: I agree. The vote is not in yet.

MR. OKRENT: Well, I think we had better go to the next agenda item. I am sure we have not heard the last of the subject of seismic PRAs.

MR. LEWIS: But we can hope.

(Laughter)

tale?

MR. OKRENT: There goes the tale of ACRS again.

MR. LEWIS: Hey.

(Laughter)

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MR. OKRENT: Go ahead.

MR. LEAR: The next item on the agenda is the last item of the day. That is a requirement which was put to us at the NRC to come up with a statement on the results of existing studies, and I interpret that to mean results of existing studies in the context of the design margin determinations that have occurred at the behest of the NRC over the years, and in that regard, it is a rather open-ended topic, so I narrowed it, and we have heard that narrowing going on throughout the day in the areas of deterministic and also probabilistic concepts on my area of licensing actions and also on operating plants.

So to start out with, I have a slide which shows plants which have had a seismic reanalysis over the years, and I have listed those which have shown to have had an analysis for the new site-specific response spectra that was determined a few years back as a requirement on OL applications. Also, as you can see there, Summer has a unique requirement, not necessarily site-specific response spectra but the fact there was a Monticello reservoir nearby which caused induced seismicity in the area and they had to reevaluate the seismic design from that point. Also some shallow embedment effects that were stemming from that reservoir which changed the spectrum.

So in that context, we had some seismic design

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margin evaluations going on in a deterministic fashion. We all know about the occurrence of the Hosgri fault. That is a typo there. That doesn't mean there was one. That's the first one, and second and third is anticipated. Hopefully, that is it.

Then we had all the seismic --

(Laughter)

We hope the systematic evaluation plans, of which we have had ten over the past few years designed to early criteria, some of which didn't include earthquake design, we evaluated those against the current criteria.

MR. SIESS: George, on seismic reanalysis, do you mean they went in and did a complete new dynamic analysis, or is this looking --

MR. LEAR: Selective features, critical component structures, systems.

MR. SIESS: Because I have seen a seismic margin report recently. I think on one system in Midland.

MR. LEAR: I have a further breakdown on this.

MR. SIESS: Is that something different than you are talking about here?

MR. LEAR: No, not really, in the sense that these are reevaluations based on a new spectra of selected systems.

MR. SIESS: And they looked at the margins against the same basis.

MR. LEAR: That brings up another question. The definition we need to put in that book you are talking about is seismic design margin. I think there are probably three or four definitions.

MR. SIESS: But only one good one.

MR. LEAR: I am looking at one particular one at this moment in time, the seismic design margin, at this instance being within the deterministic realm, i.e., the earlier determination of stresses in a structure due to an earlier concept on an earthquake input motion. Sometime later the seismic-specific response spectra came along, and we have a new requirement for another stress calculation.

Comparing the two, we have a determination of a seismic margin there.

MR. SIESS: Okay.

MR. LEAR: So that's one definition. As I said, I was going to run through a few of these plants that were under the seismic reevaluation deterministically. The first one is Clinton. This was the early CP stage criteria for the seismic design. It was a peak acceleration, .25 g, at the foundation level, the design response spectrum from the Reg Guide 1.60. They used deconvolution to the bedrock and then to the foundation level of the plant, and also an FEM, finite element method, for the soils/structure interaction solution.

The next slide shows the reduction of seismic input motion via deconvolution. The Reg Guide 1.60 spectra is the solid line. It was deconvolved and we get a free field foundation spectra which has this dip in it for subsequent analysis. This is at the CP stage.

Along came our regulation for the criteria for soil/structure interaction, and this slide shows the current position that applied subsequently to Clinton.

Dr. Tan earlier mentioned that particular position, i.e., that we have to do the finite element method plus the elastic half space analysis to come up with two spectra, both of which are then enveloped in the first test, and if that isn't a successful desired route, we fall back on one of the second ones, which we consider the as-built stresses, and I believe Dr. Bush was talking about this a little while ago, where he said if you go to the mill strength of the rebar and such on the site and run the site, you will find there indeed is a higher as-built stress capacity than was originally contempated at the design stage. So if you have a component with a fundamental frequency lying within a band that has been exceeded, you can go to that out, so to speak, to find a relief.

Thirdly, if you do have a component that has its fundamental frequency within the bands of exceedance, you can perhaps analyze it in some other fashion to show it can

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withstand that load.

Next slide.

MR. LEWIS: What is "adequate conservatism," George? What does adequate conservatism mean?

MR. LEAR: That is anything above a safety factor of 1.

MR. LEWIS: That is adequate?

MR. LEAR: I can't give you a better one than that.

MR. LEWIS: That's not a Staff position, I take it.

MR. LEAR: No, it isn't. I think you can probably define it as well as I can what conservatism means.

MR. SIESS: To hear you say that, I would say the margin is zero.

MR. LEAR: For that particular definition of adequate conservatism, that would be correct, the margin would be zero.

MR. SIESS: I'm not sure that it is.

MR. LEAR: You're not sure that it is? Let's go on and see what we got when we did these three analyses.

The need for the seismic reevaluation, as we mentioned, is the fact that they used the deconvolution method, and only one of the two selected methods, i.e., the finite element method or the elastic half-space method. So they

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were required to go back as a result of these needs to do the reanalysis. Also, there were no consideration for soil property considerations, and I have mentioned the other two already.

MR. SIESS: In each case you are checking the plant against design conditions and factoring loads and so forth.

MR. LEAR: Yes.

MR. SIESS: So your margins are not zero when you are right on the nose. The only margin on design basis, margins against failure are built in to that.

MR. LEAR: Well, in the original design in the early days -- you know better, probably, than anyone here, once the design was completed, it was compared against code allowables or something comparable to that, and there would alwas be a difference between the two, and surely from that you would get -- I am facetious in saying it's a safety factor of 1.

MR. SIESS: When you meet code allowables, it doesn't mean there's a probability of 1 it will fail when you get to the SSE.

MR. LEAR: That's right.

MR. SIESS: All right.

MR. LEAR: Then, as I said, there was a requirement for reevaluation, and this was at the OL stage, a new magnitude

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of 5.8 for the rthquake, with a Reg Guide 1.60 spectra anchored at .2 g in the free field at the foundation level specified, and also a soil/structure interaction analysis, including soil property gradation and no deconvolution to evaluate the plant structures, the piping and equipment for the new site-specific spectra.

The SRV was safety relief valve and LOCA loads as well. This (indicating) shows the spectra which was used. It was a consolidated spectra, a response spectra for the site, and it combines the site-specific response apart from the Reg Guide 1.60 curve and although the section which was developed from the time history at that location for use in evaluating the structural response.

This shows a curve of the floor response at the base mat, a spectra comparison between the original and then the reanalysis. As we mentioned before, there was a criteria established for determining whether or not the structure or component would be acceptable under the new spectra.

You see here the new spectra, which is the one with the triangle, does fall below the original, so in that area it was found to be a very low frequency and therefore not of significance for the base mat.

The next one is comparable in that it is 100 feet above the base mat, and I am showing, just to illustrate rather

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than do a de novo design for the plant, certain features were picked. We will quickly flip through these, noting this was 100 feet above the base mat. Again, the same concept of the reanalysis enveloping -- excuse me, the original still enveloping the reanalysis. It was still within a safe margin.

And this, again, was floor response spectra in the main building operating floor.

MR. HALL: Let me ask a question. This is Clinton, which is 40 miles away? I'll have to admit I've never been there. Why are these so broad?

MR. LEAR: Why are the spectra so broad?

MR. HALL: Right.

MR. LEAR: Broad in this domain?

MR. HALL: Yes.

MR. LEAR: Well, I guess I would have to ask any one of you who are more familiar with the computer codes and the range of variables with which that is input to give a response to that question. It would appear to me simply a matter of what your input data reads.

MR. LIN: Is it possible, because you have two different kinds of models, one is finite and one is a spring-constant model?

MR. HENRIES: In the end these varying soil properties also --

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MR. KENNEDY: Enveloping a bunch of soil properties? Okay, that explains it if you're enveloping a bunch of soil properties and different modeling.

MR. TANG: Analyses, a number of analyses, the results of a number of analyses.

MR. KENNEDY: This is no single analysis.

MR. LEAR: The results of the stress evaluation for shear all forces in KIPS as it is shown here, you will notice the new stresses are shown here in the third column. They are greater than those developed originally in the design and increased value. And here again, we see that the results were evaluated taking into account what Dr. Bush was saying earlier, that indeed the actual yield strength at the test set site were much higher than those used for their initial design. And from that available knowledge, they were capable of stating that this was sufficient acceptable.

Okay, this is also another indication of the containment critical stress summary, wherein we have the new loads and the stresses as shown with the allowables. And in every instance except for the last, we find it to be within the allowable on the last. The code calls it for 143 psi for concrete tangential shear. That's at the dome spring line, A-53, whereas the allowable was 60 psi. That is for the standard review plan. And they calculated 72 psi, but this was considered to be a relatively small difference. And in view of the conservatisms inherent in the design itself, this was acceptable.

This next slide is sort of a summary of what I was saying throughout the last few moments, that the new response spectra are higher than the design basis in certain portions of the frequency range; but as I mentioned before, in the incidence of the base mat there is a low frequency area of no significance, and to evaluate it they did a thorough analysis on selected structures and some internal structures as well, and they are all within design allowables based upon the actual measured values at the site. We did have a presentation with the ACRS, and I am sure you probably all remember that, and certainly you did write a letter on this.

The next plant is that of FERMI-2. Again, there was a site-specific response spectra required. The plant was founded on competent rock, so there was no soil/structure interactionproblem. We went directly to the lump mass model. The base mat is fixed base. The results are shown in the next slide.

Next slide, please.

MR. OKRENT: Can you show that last graph again?

MR. LEAR: These are a comparison of the spectra. Reg Guide 1.60, the site-dependent response spectra, FERMI-_,

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and the earlier existing FERMA-2 design spectra.

MR. OKRENT: The original design spectra preceded Reg Guide 1.60; is that it?

MR. LEAR: I believe it did, yes. It would have definitely, because I would assume if it did not, then we wouldn't even be seeing that curve right there for FERMI-2. The point here is the site-specific response spectra is beneath Reg Guide 1.60, which is shown there.

In taking the new spectra and inputting into the codes, again, for the containment, a shear and moment diagram, this being the shear and the moment diagram, and in the brackets we see the ratios. Dr. Tan, do you want to comment about those ratios?

MR. TAN: This ratio is between the new value and the old value. The old values are in the bracket. The ratio is in the square brackets.

MR. LEAR: And those are found to be within acceptable ranges, are they not?

MR. TAN: We have to look at these stresses.

MR. LEAR: The next display is where they calculate stresses within the steel containment.

MR. TAN: The maximum is trend E.

MR. LEAR: If you look at this, you see under the new earthquake the value of 20,933, and the old one is 18,225 maximum.

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MR. TAN: And the allowable is 3,300.

MR. LEAR: Right, and the other is 13,900 versus 12,150.

END 24

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MR. LEAR: Are there any questions on that one?

(No response.)

MR. LEAR: Sequoya was another one that was reevaluated based upon a site-specific response spectra, and I won't bother to read those in detail, but the methods and assumptions are as shown.

I guess the key point is Item 10, "Few locations were evaluated as exceeding the code allowables by approximately ten percent" -- excuse me -- "five percent," which was less than yield.

Summer, as I mentioned before, was found to be acceptable -- Summer was reevaluated, based on reservoir-induced seismicity and adjusted spectra and found to be acceptable as well.

Okay, Slide 19, I guess it is, Midland. Midland, the next one you have on there. And the one I have on Midland -- yes, that's all right -- the ones we have talked about until now have been completed, and we have here a slide which pertains here to the Midland seismic reevaluation.

In this instance, another site-specific response spectra has been prepared and a certain category of structures selected for reevaluation. It is not a complete, one hundred percent redesign or reevaluation, but unique category structures were chosen for reevaluation,

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and the Applicant, Consumers Power Company, has submitted a number of volumes for this seismic margins design study, which I guess, Dr. Siess, you were talking about earlier on.

MR. SIESS: Yes. I have Volume 4. I would like to get the others.

MR. LEAR: You ought to ask the gentleman who wrote them. Perhaps he can get some for you. We will see if we can get some otherwise, but there were probably sixty of those things sent in. I don't know where they went. I had trouble getting one myself.

At any rate, the first volume is the methodology which Dr. Kennedy probably could elucidate on to some extent, and then each of the other volumes deals with each of the structures. It is still in the process of review by the Staff, and there are some plans for meetings with the Applicant, and this is with the consultant as well as the Applicant. So we have mentioned some of the things we have done, some of the things that are going on right now.

MR. OKRENT: Excuse me. When did the Midland volumes come in?

MR. SIESS: On seismic margins?

MR. LEAR: It was in 1983, I know.

MR. SIESS: The one I got was in the last three weeks.

MR. LEAR: I have February '83 on this copy. I don't know when it was sent in.

MR. SIESS: What volume number?

MR. LEAR: Volume 1: Methodology and Criteria.

MR. SIESS: I got Volume 4 within the month.

MR. LEAR: Was that Borated Water Storage Tank? I got that one.

MR. SIESS: It is a structure.

MR. LEAR: I will try to remember to get copies to you.

MR. SIESS: It's part of the aux building.

MR. LEAR: If Dr. Savio will write that down --

MR. SAVIO: I will remind you.

MR. LEAR: Okay.

I just wanted to mention in passing, since you were talking about completed work or results of studies, there have been over the years a number of computer codes developed, and the people who developed them are most likely in this room and know a great deal more about them than I, but I would like to at least mention, since that is a part of this topic, for response to soils and embankments, we know of SHAKE, which is a program for analyzing one-dimensional seismic wave propogation through various layers of material.

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We have FLUSH, a program for analyzing two-dimensional wave propogation through various finite elements of a mass, representing soil, rock and embankment configurations. You get nonlinear soil response considered, as is done in the case of SHAKE.

There is another one, QUAD-4, a finite element program for two-dimensional wave propogation.

We have LUSH, which Dr. Luco mentioned this morning, and I was hoping to hear from him a little more on what code he was using for the incident waves, other than vertically propogated waves. I, myself, have not yet heard of a code specifically that treats that. Perhaps that is what you were aiming at, further development of that concept. I am not sure.

And there are a couple in the area of design of slopes, SLOPE itself, and T-LUSH, a three-dimensional program for seismic analysis of earth dams.

In the work that Dr. Kennedy has been doing on Midland, there are some codes mentioned in that program. There is STUF, which creates a synthetic time histories through an iterative process.

CLASSI, that came out of the SSMRP programs. It develops frequency dependent soil impedences, both real and imaginary, for a structure.

Then we have SOIL ST, which computes composite

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modal dampening, using the Tsai approach and MOD SAP, which I am not at all familiar with. But that is an updated version of SAP used to develope the flexible base modes required for the response spectra analysis.

I mention these only to indicate that these are some of the many things that have been done in this field over the years, so we would have a snapshot of what some of our seismic design studies have produced.

Now having gone through the deterministic approaches rather rapidly, hitting the tips of the icebergs, we come to PRA and how that influences another definition of seismic design margin. We have accomplished a number of PRAs -- industry, contractors, consultants and the NRC itself -- and on this next slide we have a view of plants having PRA evaluations as shown here, and the seismic portion or cut of the PRA was not accomplished up until we did Big Rock Point, Limerick and the rest. So we are having, at this point in the development of a PRA, a focus in on the seismic aspect as well as the internal PRA.

MR. OKRENT: Excuse me.

MR. LEAR: Yes?

MR. OKRENT: If I see a plant mentioned, does that mean the PRA exists.

MR. LEAR: Let me slide this up. This was taken from a PRA fundamentals course. I can only say that that mgc 25

is a question interpreted here. There must be a PRA, from what I have been informed, although I, myself, have not seen these physically.

MR. OKRENT: I see. Okay, thank you.

MR. MARK: What is the meaning of the EPRI for Limerick?

MR. LEAR: EPRI for Limerick?

MR. TAN: Do you mean about the "No" and "Yes"?

MR. MARK: Yes.

MR. TAN: In the first study, they didn't include the seismic. In the second study, they included the seismic.

MR. LEAR: There were two branches. First was the internals, and then second -- right, okay.

Okay, the next slide gives a snapshot of the results of the review of recent PRAs in structural fragility. The structural fragility area, after the very thorough discussion by Dr. Reed, this is hardly worth spending too much time at all on, because of the details he provided, but we did have published three reviews -- Zion, Indian Point and Limerick. His presentation was more comprehensive.

Nevertheless, there were some findings that came out of those that distilled these that I have shown here. Seismic risks, dominant contributor and those various features of each of the plants were identified as failures potentially, and a very, very cryptic, short indicator of

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what we might use PRA for, hardly comparable to the extensive list Dr. Reed had, but nevertheless, it would provide us some insight into what we might expect beyond the SSE, in the range of two to four SSE, and also identification of sensitive components risk, sensitive components.

This next slide is an attempt to state what we are trying to do in some of our studies that are going on, both as a technical assistance contract under NRR sponsorship, the nuclear reacto regulation sponsorship, and also what is going on elsewhere through the Office of Research.

The cryptic comments here don't tell much of the story, but at least it does give an indication that there is something going on. It is not a quiescent area at all. There are various levels of support for these programs. There is certainly a need for more, based upon what I have been hearing today and based upon what I, myself, think. Getting attention, getting resources, giving it a high priority is another matter which neither I nor my compatriots here can really address in any forum such as this, perhaps.

At any rate, we are not sitting back doing nothing.

I guess that's it for me. I will take any questions if you have them.

Yes.

MR. HENRIES: Bill Henries, Yankee Atomic.

I didn't hear you mention the results from the SEP

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plants. Do you have any comparisons of the befores and afters on it that you can talk about generally?

MR. LEAR: No, I do not.

Yes?

MR. LIN: C. W. Lin from Westinghouse. I just want to make a comment.

Earlier, Dr. Tan from NRC mentioned that there were two different types of models, the finite model and the elastic model, that the results are more conservative, but if you look at the Trenton results, you will find that the response spectra has an extremely wide pipe, which means for the piping system, there is no way to get away from the pipe, and you w'will have to allow more support than usual, and by adding more supports in the piping system, you will wind up with a more rigid system, which may not be more reliable. And in fact, in the normal operation, when the temperature becomes more of a problem, you will encounter more problems than usual, and I don't think that's going to increase the reliability or safety at all, and I think it is on the contrary.

I also want to comment that NRC, -- I think Dr. Bush is involved in having a task force looking at different issues, but I don't think this is one of the issues. Maybe you should include this as one of the issues, to look at whether you should decouple the different models

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and not include all the models in the one analysis and try to force the pipe and equipment design to have a much more rigid design?

MR. BUSH: I can comment that I think the change in the damping factors, which will be in the appropriate range of hertz, takes care of the problem, because it essentially says that you don't have to put the extra supports there if you use it. But unfortunately there is a difference between a practical application and the necessary approach, because you have to amend the documents, and that is a very lengthy, painful process. It can be done, but it may not be done. In other words, the easy answer might be to put the extra supports there -- not the safe answer; the easy answer.

MR. LIN: But the damping issue and this response spectra issue are two different issues.

MR. BUSH: Yes.

MR. LEAR: Yes?

MR. TANG: Dave Tang from Westinghouse. I want to follow up a little bit more on what C.W. Lin just mentioned.

In another area, in equipment qualification, for example, the wide band response spectra you just presented really creates a tremendous burden as far as a laboratory's facilities are concerned. Laboratories usually cannot handle that kind of response spectra, Finally they enforce various

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changes, what we call a test response spectra, that may not be that realistic at all.

If you look into the subject carefully, you will find the philosophy of performing that many analyses to develop is such that the requirement cannot be that realistic and may not be that easy to enforce.

That is my comment.

MR. OKRENT: Excuse me. Let's assume for the moment that what you are saying is correct, that this wide band spectrum produces some undesired effects.

Have the various groups in the industry provided a sufficiently good defense for a proposed method of analysis such that it can stand up under scrutiny and not be subject to really major reservations and be one that gives what you would call a more realistic prediction, because if you try to be realistic but you miss it, you are in trouble, and you are not doing, if I understand it correctly, tests in situ to check all of your frequencies and so forth to find out where the calculations were wrong.

Do you see what I am trying to say?

MR. TANG: Yes.

MR. OKRENT: If the industry would come in with a good solid position, it seem to me you would have a better basis for getting a change in whatever the Staff is requiring now.

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MR. LIN: Dr. Okrent, one of the ways that can be used to remove this problem here would be to go through the kind of analysis, ASME analysis, is recommending you don't do an analysis based upon the entire response spectrum. Rather, chop it into pieces and say under certain conditions your spectrum will come within a certain band and you will do one analysis on that, an' in another condition your response spectra will come in another band, and do another analysis, but each one actually satisfies one reality, but you don't have to cover all of the realities.

MR. OKRENT: If there exists a really defensible position, then you should put it forward and be prepared to defend it thoroughly and argue for the change, is what I am saying.

MR. TANG: The IEEE 344 commentees are looking to this problem, but as far as I can see, you might analyze your test response spectrum and your job is done. What that amounts to as far as equipment performance or adequacy, seismic adequacy. That is still an open question.

MR. SIESS: I think Dr. Reed pointed out we didn't really have any good physical evidence on what the floor response spectra looked like, so I don't know how we can test any of these theoretical methods, the Staff's or anyone else's. I'm not convinced that the Staff's method of calculating soil/structure interaction, putting in a range,

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gives a good answer. They think it does and that is their basis for licensing.

What Dr. Okrent has said, come in with a good argument, actually, if someone had good data, they could probably convince us.

MR. TANG: As Dr. Lin just pointed out on the frequency band by frequency band basis you really have a case. Well, that is subject to further --

MR. SIESS: But that is just more calculations. We haven't labeled it reality.

MR. LIN: I would say not more calculations. In terms of software it may cost you a little bit more, but in terms of reliability and safety, I think it will improve the situation.

MR. SIESS: You can't prove it unless you have physical evidence to show you are getting a better answer.

MR. LIN: Maybe not, but I would suggest in reality you will not come up with any response motion to do any earthquakes with that kind of broad band. I have not seen any evidence.

MR. SIESS: That's not the problem. The problem is to get the equipment that will resist the earthquake.

MR. LIN: If I can show you my equipment will withstand an earthquake corresponding to the assumption that the finite model represents the situation. On the other

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hand, using a semi-permanent I can also show that I will satisfy the model with a stick model, which means I can satisfy all of the assumptions you have made on a piece-wise basis, on one single model basis, not on every model at the same time. There is no need for it.

Just like if you assume I have a case of a near-field fault, I have a high frequency content, and in the meantime I assume the earthquake comes from far field, which has a low frequency content with equal magnitude, they come at the same time, you can assume that each will come independently and analyze the situation independently.

MR. SIESS: It sounds reasonable, but that doesn't make it right.

(Laughter)

MR. OKRENT: Are there other questions, comments or jokes?

(Laughter)

(No response)

MR. OKRENT: If not, I will recess this meeting until tomorrow morning at 8:30, and I hope the panel is all set to give us a stimulating time.

(Whereupon, at 6:20 p.m. the meeting was recessed, to reconvene the following day at 8:30 a.m.)

CERTIFICATE OF PROCEEDINGS

This is to certify that the attached proceedings before the NRC COMMISSION

In the matter of: Meeting of Subcommittee on Extreme External Phenomena

Date of Proceeding: 12/8/83

Place of Proceeding: San Francisco, California

were held as herein appears, and that this is the original transcript for the file of the Commission.

Sharon Filipour
Official Reporter - Typed

Sharon Filipour
Official Reporter - Signature

Jackson
Tapes 1 & 2

Regulatory Prospective on the Quantification
of Seismic Design Margins and
a Summary of Ongoing Programs

You have requested that we provide this subcommittee with some insight as to what is likely to come about or what is expected in the future by the NRC staff in the area of the Quantification of Seismic Design Margins. As you are aware, we have discussed this issue with you a number of times during 1982 and as a result the ACRS forwarded a letter on this topic to the commissioners in January of this year. We viewed this letter as a call for a consolidation and possibly rethinking of NRC programs to allow us to gain better confidence in the capability of nuclear power plants to withstand earthquakes greater than their design basis. We also viewed the request as a call for continued emphasis on a multiple approach including both deterministic analysis of seismic margins and a call for increased attention to seismic probabilistic risk assessments.

We forwarded our general response to the ACRS request in an April 4, 1983 letter to Commissioner Ahearne in which we indicated that we concurred in principle with the committee's recommendation but also indicated that extensive discussions would be needed to define specific programs that are feasible considering the availability of data and resources. We are here at this meeting in this spirit not only to contribute to the discussion but to hear suggestions from the Subcommittee, your consultants, and other participants as to how we should be proceeding.

We have available here for the discussions members of the NRC staff prepared to discuss the variety of topics that you have requested and each of these individuals will be providing either summaries or specific insights that they have gained in their specific area of expertise. As you are aware, Ted Algermissen and Jim Devine of the USGS are not able to attend due to the press of activities in that agency and they offer their apologies and will look forward to future meetings. The USGS has asked Greg Gohn to provide us with an overview of future programs.

Since forwarding our April 4th letter we have made some progress in responding to your request but due to the press of other activities we have not advanced as much as we would have liked. As we previously indicated, it was our intent to proceed on this question within existing programs and staff and resource availability. This is still our intent.

I. We have made substantial progress in the area of how to deal with external events in PRA. A working group prepared a substantial report which made seven specific recommendations in the external events area including:

   - External events should be included in PRAs

   - A short term program to develop external event PRA procedures, guidelines, and acceptance criteria.
     (This activity has been incorporated into the current operating plan)

o The need for research activities and the call for an assessment of a simplified external events PRA methodology as well as several other recommendations.

I might also add that one of the research recommendations indicate that it is necessary to make an assessment of where the SSMRP fits into the program and how to improve the prioritization of related PRA issues.

II. We have also made considerable progress in establishing the seismic hazard for plants in the eastern U.S. as a result of the Charleston earthquake issue. This program by Lawrence Livermore Laboratories is proceeding and we have some preliminary results to share with you. Mr. Kimball and Mr. Reiter will provide a presentation tomorrow. We have also had a number of meetings with AIF and EPRI and the owner's group to provide further encouragement for their extensive new program on seismic hazard. We have had the USGS survey through the Office of Research working on the effect on the seismic hazard of assuming certain tectonic models for the eastern U.S. Based on these programs and our more deterministic geologic and seismologic research effort we feel quite strongly that we are making excellent progress in improving our capability of characterizing the seismic hazard for a given site. We look forward to some very interesting results in the next year or so.

III. As we mentioned in the April letter, the Seismic Qualification Utilities Group had held a number of meetings. Additional meetings have been held and Vince Noonan, Chief of the Equipment Qualification Branch will relay the findings to date of this group and what it means to the staff for the future.

IV. We are currently actively involved in the review of several PRAs which include seismic considerations. We have recently completed our review of the Limerick PRA and we will be discussing these results with you in the next few months. In addition, we have initiated our review of the Millstone PRA and GESSAR PRA. These reviews are now including a substantial involvement of in-house NRC staff especially in the seismic area. In our reviews we are finding that we must specify that the seismic aspects be considered in a relative rather than absolute manner. The primary problem that persist are the substantial reliance on subjective judgement to develop the seismic risk. The primary reasons for the existence of large uncertainties continues to be the lack of a data base for both fragility and hazard.

V. We have recently formed an inhouse Seismic Design Margins Working Group to assist in establishing our future directions with regard to the need for new work or the modification of existing programs in the seismic area. The overall approach of the group will be to work in the coming year to assess our progress in different areas, for example, assessing what the seismic PRAs have really taught us. The Office of Research will play an active role in this group with one end in mind to help in identifying how FY 85 resources will be expended to address current problems. Dan Guzy and Andy Murphy are present to address the research activities. We will be working as a group to try to formulate a meaningful and responsive program to address this ACRS issue.

One issue that continues to present a significant problem, as we have indicated previously and as was also raised by Dr. Remick at the recent Commission meeting, is our ability to implement additional requirements on utilities to address the seismic design margins question. This issue is not resolved. The NRC staff does not perceive the seismic design margins issue to be an Unresolved Safety Issue based on the general understanding of the inherent seismic capacity of nuclear power plants that has been obtained through the extensive programs conducted to date.

In summation, we are proceeding with a program to assess the directions we should be going with regard to the seismic design margins issue. This effort will necessarily require extensive interaction with the Office of Research and the utilities. In addition, we feel it important to assess what we learned in the current seismic PRA effort before proceeding with any significant new programs.