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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS (ACRS)

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548TH MEETING

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THURSDAY, DECEMBER 6, 2007

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The meeting was held in Room T-2B3, Two White Flint North, 11545 Rockville Pike, Rockville, Maryland, at 8:30 a.m., William J. Shack, Chairman, presiding.

MEMBERS PRESENT:

WILLIAM J. SHACK	Chairman
MARIO V. BONACA	Vice Chairman
SAID ABDEL-KHALIK	Member
JOHN W. STETKAR	Member
OTTO L. MAYNARD	Member
DENNIS C. BLEY	Member
MICHAEL CORRADINI	Member
GEORGE E. APOSTOLAKIS	Member
DANA A. POWERS	Member
J. SAM ARMIJO	Member
JOHN D. SIEBER	Member-At-Large

NRC STAFF PRESENT:

SAM DURAISWAMY, Designated Federal Official
ROBERT LEE TREGONING
LEE ABRAMSON
NILESH CHOKSHI
KHALID SHAUKAT
RICHARD DUDLEY
TIM COLLINS
GREG CRANSTON
TAI HUANG
ROBERT PRATO
FAROUK ELTAWILA
JIMI YEROKUN
MIKE CHEOK
JOCELYN MITCHELL
DON DUBE
RICH SHERRY

ALSO PRESENT:

DOUG PRUITT
YOUSEF FARAWILA
EDWIN LYMEN

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P R O C E E D I N G S

(8:28 a.m.)

CHAIRMAN SHACK: The meeting will come to order.

This is the first day of the 548th meeting of the Advisory Committee on Reactor Safeguards. During today's meeting, the Committee will consider the following:

Draft final NUREG-1829, estimating loss of coolant accident frequencies through the elicitation process;

And a draft NUREG on seismic considerations for the transition break size;

The AREVA enhanced Option III long-term stability solution;

The state-of-the-art reactor consequence analysis, SOARCA, which will be a part open and part closed meeting;

A draft ACRS report on the NRC Safety Research Program;

And preparation of ACRS reports.

A portion of this meeting may be closed to discuss safeguards and national security information related to the SOARCA project.

This meeting is being conducted in

accordance with the provisions of the Federal Advisory Committee Act. Mr. Sam Duraiswamy is the Designated Federal Official for the initial portion of the meeting.

We have received no written comments from members of the public regarding today's session. We have received a request from Dr. Edwin Lymen, Union of Concerned Scientists, for time to make oral statements regarding the SOARCA project.

A transcript of portions of the meeting is being kept, and it is requested that the speakers use one of the microphones, identify themselves and speak with sufficient clarity and volume so that they can be readily heard.

I will begin with some items of current interest. The members are scheduled to interview a candidate today during lunchtime. We'll be handing out a resume. It's one candidate so we'll do it as group.

Other information. Ms. Barbara Jo White, who has been with the ACRS office for almost 40 years is retiring on January 3rd, 2008. All of these years she has provided outstanding administrative support to the members. She has always ensured that the members have a good place to stay when they attend ACRS meetings in town or out of town.

She has been exceptional in assuring that the federal register notices for the subcommittee and full committee meeting have been issued consistent with FACA requirements.

Her outstanding administrative support to members, hard work, dedication, professional attitude in dealing with no only the members and staff, but also the public are very much appreciated.

Thank you, and good luck in your future endeavors.

(Applause.)

CHAIRMAN SHACK: In addition to a retiree, we have some new additions to the ACRS staff. Dr. Harold Vander Mollen will be joining the ACRS staff as a senior staff engineer on December 24th. He will be the responsible engineer for the Subcommittees on Reliability and PRA and Regulatory Policies and Practices.

He came to the AEC regulatory staff from the National Bureau of Standards in 1974. He spent 13 years in several technical branches in NRR working on reactor physics, accident and transient analysis, technical specifications, generic issues program, and PRA issues.

In 1987, he and his section were transferred from the PRA branch in NRR to the PRA branch in the Office of Nuclear Regulatory Research, just in time to work on the NUREG-1150 project.

When it was finished, he was put in charge of PRA methods development. In 1999, after 12 years in the PRA branch in RES, he took over the generic program issues program again.

Welcome aboard.

(Applause.)

CHAIRMAN SHACK: Ms. Kendra Freeland joined the ACRS/ACNW&M staff on October 22nd as an administrative assistant. She will be handling travel authorization, vouchers and compensation for the members, one of our most important concerns.

(Laughter.)

CHAIRMAN SHACK: Prior to joining the ACRS ACNW&M staff, she served as secretary for the Division of Contracts in the Office of Administration.

Kendra received a Bachelor of Arts degree in corporate and broadcast communications from Elon University, Elon, North Carolina, and a Master's degree in communications from Hawaii Pacific University, Honolulu, Hawaii.

She knows how to pick a graduate school.

(Laughter.)

CHAIRMAN SHACK: Welcome aboard.

Ms. Guita Irani joined the ACRS/ACNW&M staff on November 13th, 2007, as an information technology specialist. She is a new member of the NRC.

Guita started her career in information technology working as a DOD contractor for the Joint Spectrum Center in 2000. In 2003, she moved to the Pension Benefit Guaranty Corporation to support their federal contracts.

Guita holds a Master's degree in information technology from the University of Maryland and has been involved with software development and IT support throughout her career.

Welcome aboard.

MR. DURAISWAMY: Janet is not here. So you can do that tomorrow.

CHAIRMAN SHACK: Okay. We'll hold. Sounds good.

Well, then we can move to our business today, and our first item of business is the draft NUREG on estimating loss of coolant accident, LOCA, frequencies through the elicitation process, and Dr. Apostolakis will lead us through that.

MEMBER APOSTOLAKIS: Thank you, Bill.

We had a subcommittee meeting on the 27th of November when we heard from the staff on both studies. One is on the expert judgment elicitation process and results, and the other one was more focused studies on seismic issues.

There were no issues that were raised by the subcommittee. The members appear to be -- well, actually they were -- pleased with what they heard. The staff also presented their responses to public comments on the elicitation process. So we asked them to come back today and give a shortened performance so that the members will form an opinion.

And we are expected to write a letter at this meeting. So with that, I should turn to you, Rob?

MR. TREGONING: Yes.

MEMBER APOSTOLAKIS: Okay.

MR. TREGONING: Thank you, Dr. Apostolakis and Mr. Chairman.

My name is Rob Tregoning from the Office of Research, and to my right is Lee Abramson, and we will be leading you through the first abridged presentation on the development of NUREG-1829, on passive system LOCA frequency development for risk-informed revision of 10 CFR 5046.

Why did we get into this work? Well, our bosses essentially told us we needed to do this. So this work was done in response to Commission direction provided by SRM-02-0057, and a couple of quotes there. "The staff should provide the Commission a comprehensive LOCA failure analysis and frequency estimation that is realistically conservative and amenable to decision-making with appropriate margins for uncertainty."

So that was our edict. That was our direction. Also, in the same SRM, the Commission said the staff should use expert elicitation to converge whenever possible service data and PFM results. So those are our marching orders. That's what we set off to do.

And we're here today, as Dr. Apostolakis had indicated, requesting a letter or an ACRS recommendation to publish the study, NUREG-1829. Our opinion is it sufficiently meets the Commission direction, satisfies that and should be published as a result.

A brief executive summary. We used the formal elicitation process to develop estimates of generic BWR and PWR passive system LOCA frequencies associated with material degradation. We had a group of 12 panelists. They provided us with quantitative estimates supported by qualitative rationale. They did this individually in individual elicitations for underlying technical issues that were developed as a group.

We had very good or generally good agreement on the qualitative LOCA contributing factors. However, as you've seen in the report, there was large individual uncertainty and also large panel variability in actually quantifying the estimates. So coming up with frequency estimates associated with the phenomena that they were predicting.

That wasn't surprising, of course. We expected that, and that was the reason that we chose to do elicitation to begin with, to provide a framework and a mechanism for dealing with the expected large uncertainty in panel variability.

The bottom line, we developed group results. So we aggregated the individual estimates for the LOCA frequency distribution parameters. So we didn't determine distributions per se, but we determined certain parameters of the distribution, the 50th, 95th and the mean. We used a number of different aggregation schemes. One scheme we used was the geometric mean. We thought those results were consistent with the elicitation objective in structure and they're also generally comparable with the NUREG/CR-5750 estimates.

That study was the prior study that was used to develop LOCA frequency estimates back in the mid-'90s. It was not done using elicitation. It was done by just simply evaluating service experience.

As mentioned in this last bullet, we looked at other aggregation schemes and other aggregation schemes can give you quite a bit different results, and typically these other schemes that we looked at did result in higher LOCA frequency.

We show the results here. These are the bottom line results for BWR and PWR. Generic frequencies, you see three curves on each of those. The black curves are the medians, the reds are the mean, and then the green are the 95th.

The center points are what we're considering the best estimate, and then their confidence bounds, the error bars represent 90 percent confidence bounds. So a five percent and a 95 percent upper and lower confidence bound about that best estimate.

These particular results, we did a modest adjustment for overconfidence. We adjusted error factors or uncertainty ranges in some expert opinions to coincide with a well-known elicitation. I don't want to call it a fact, but a finding in many elicitation studies that experts tend to be overconfident.

We didn't see as strong a bias for overconfidence in these results. So that's why a correction ended up only being relatively modest in this case.

These 90 percent confidence bounds, it says 95, but it's really 90 percent -- they're used to represent or reflect the diversity or the differences among individual panelists' opinion, and then the difference between the medians and the 95th really reflect the individual panelists' uncertainties. So there's two types of uncertainty or variability that we're trying to capture.

MEMBER CORRADINI: Could you repeat what you just said? You said that the very -- could you repeat, please?

MR. TREGONING: The confidence bounds about any individual value here, either about the mean, median or 95th percentile, they represent the spread or the difference among the individual estimates that we got from the panel. For each panelist, we asked for them for each answer that they provided, we asked for a best guess, essentially a median value, and then we asked high and low estimates, which we interpreted as fifth and 95th percentile estimates about that mid-value.

MEMBER CORRADINI: Thank you.

MR. TREGONING: I guess this is the only new slide that the subcommittee hasn't seen. It was put in at a request from Professor Apostolakis at the subcommittee meeting. He wanted to see what the distribution shape looked like. So we did a very simple exercise to create these, and these are essentially -- all I did was take a simple, lognormal, not split or anything, just the full lognormal, and I fit them to the 95th and the mean because those are the two parameters that we're most interested in using.

So I forced it to go through the mean and the 95th, and then the question was, well, how well does it estimate the median and the fifth. And the fifth, really a lot of extrapolation to get down to the fifth.

And both Lee and I were quite surprised at how well the fits tended to be. So in the medians in all cases, there was less than 30 percent error, which again, for LOCA frequencies, considering the variability we have, is pretty darn good. And even in the fifth percentile it was less than 50 percent error, except in one case where we had a percent error of 200.

And I've picked four plots here. The blue plot and the red plot are actually the worst fit of all the distributions that we fit to this thing. So these are the worst, and the green and the black are more representative of the types of fits you would see.

And I just summarized the percent error there.

MEMBER APOSTOLAKIS: So what does this mean? Let me understand the curve. So the blue curve is for BWR-5; is that what you're saying?

MR. TREGONING: Yes. So that's the BWR LOCA frequency at LOCA Category 5. So --

MEMBER APOSTOLAKIS: So you are only showing mean curves. All of these are mean curves?

MR. TREGONING: No, these are distributions. So plotted on these are all of the percentiles of the fit distribution.

MEMBER APOSTOLAKIS: Oh, I'm sorry. Yes.

MR. TREGONING: And then the points are our actual values.

MEMBER APOSTOLAKIS: The means.

MR. TREGONING: Those are our values. I identified where the means are because the other thing you can see here is that the means in all cases are a relatively high percentile, not surprising, but the means vary anywhere from about the 70th to even as high the 85th percentile on the distribution, depending on which parameter you're looking at.

MEMBER CORRADINI: So what you're plotting here is a fit shape to the three points that we saw in the previous curve on some break sizes.

MR. TREGONING: Right, right. If I go to the previous one --

MEMBER CORRADINI: That's fine.

MR. TREGONING: -- where we didn't show the fifth, that was the four points that we developed.

MEMBER CORRADINI: Got it.

MEMBER APOSTOLAKIS: Now, the scale, I guess, distorts a little bit what is happening because they rise too steeply, don't they? I mean the curves.

MEMBER BLEY: Goes over about four orders of magnitude.

MEMBER APOSTOLAKIS: The green one is the only one.

MEMBER BLEY: And the other one goes over at least two orders of magnitude.

CHAIRMAN SHACK: There's a good spread in those curves, and the spread is sort of what you think. For small breaks it's narrow, where for big breaks, it's very wide.

MR. TREGONING: So the black one is the small break. So you can see they're in order of increasing break size, obviously.

MEMBER APOSTOLAKIS: So if I did a PRA tomorrow and I needed the frequency of various LOCAs, I could use this one, although this is based -- these curves are based on what you call a baseline approach, right?

MR. TREGONING: These were geometric mean aggregated results. Yes, they were.

MEMBER CORRADINI: So I guess since I didn't read in detail the report, I interpret the difference between the Ps and the Bs as primarily a pressure effect, not a materials effect and not a chemistry effect.

What do the experts say relative to that in terms of their -- because if I remember this process, you have to elicit not just a number, but a reason for the number.

MR. TREGONING: Yes, and I didn't overlay in this presentation Ps with Bs, but what you see is the Ps have higher small break frequencies.

MEMBER CORRADINI: Right. I was looking at that just from the numbers.

MR. TREGONING: Right. Well, it's not clear from this because I show two Ps. I show two small Ps and then I show two large Bs.

MEMBER CORRADINI: Right. It's just the previous one I was looking at.

MR. TREGONING: Right. So the Ps are higher at small break, and then they're actually lower in intermediate breaks, and then at the biggest breaks the Ps get higher again.

The Ps are higher at small break primarily due to the fact steam generator tube rupture failures and concern for PWSCC issues related to CRDM, other small tube piping.

The Bs tend to get higher in the intermediate break because of largely driven by remaining IGSCC issues just due to the fact that BWRs, a lot of the large piping still retains flaws that were generated earlier under normal water chemistry the documents you see.

And while the experts believe that they have been mitigated to the sense that the frequencies were relatively low, they still raised up or rose up to be one of the highest risk contributors. So even though they have been mitigated, they still were the largest risk contributor.

And then when you get down to the highest frequencies, PWRs dominate, again, and that's more of a population issue. PWRs have larger pipes, more, bigger non-piping components that could fail and lead to a LOCA. So there wasn't anything unique that was driving that other than the increased population.

CHAIRMAN SHACK: But these frequencies still include the steam generator tubes --

MR. TREGONING: Yes.

CHAIRMAN SHACK: -- for the PWRs which will --

MR. TREGONING: But even if I take the --

CHAIRMAN SHACK: It's still true.

MR. TREGONING: -- I don't show it here. If I take the steam generator out, Ps are still higher.

CHAIRMAN SHACK: Now, you said the lognormal plots were the baseline or are they the error factor corrected?

MR. TREGONING: These are error factor corrected.

CHAIRMAN SHACK: Corrected. Okay. That's sort of your best estimate curve.

MR. TREGONING: That's what we would call, yes.

MEMBER APOSTOLAKIS: Now, if you included in this the multiple distribution, what do you call that?

MR. TREGONING: The mixture distribution?

MEMBER APOSTOLAKIS: Yes, mixture distribution. How would these curves change? Would they be broader?

MR. TREGONING: Yes. Yes.

MEMBER APOSTOLAKIS: They would be broader on the high side especially or --

MR. TREGONING: They would be broader high, broader to the high.

CHAIRMAN SHACK: And the means would be higher.

MR. TREGONING: They would be broader high and low.

MEMBER APOSTOLAKIS: And low.

MR. TREGONING: And low, and then the means would be shift obviously.

MEMBER BLEY: Medians would be about the same probably?

MR. TREGONING: No. Again, it's a totally different way to aggregate. So, no, the medians would be -- I'm not sure how the medians would work out.

CHAIRMAN SHACK: They're higher. I mean, that's the way they work out when you look at the numbers.

MR. TREGONING: Okay.

CHAIRMAN SHACK: I mean, just looking I can read the table.

MR. TREGONING: Yes, I haven't looked at that.

MEMBER APOSTOLAKIS: You could develop curves like this using that other method.

MR. TREGONING: Yes. Yes, you could.

MEMBER APOSTOLAKIS: But then ultimately you might want to combine the curves.

MR. TREGONING: But the mixture distribution, you come up with the distribution itself. So you wouldn't turn around and fit it as we have here.

MEMBER APOSTOLAKIS: I understand that, but you can always change those distributions based on insights you got here. I mean, ultimately what matters is what you learn from the whole exercise, right?

I would make an analogy with licensing a reactor. We have been told many times in this room by the staff our decision is not based on a single analysis. It's the result of a process.

So here, you know, you might say at the end I want a distribution which says, you know, I've been through this. I've done it ten different ways. This is what I think it is.

Now, that takes guts.

(Laughter.)

MEMBER APOSTOLAKIS: Anyway, it's okay. I did want to inquire, but this is the biggest problem Bayesian methods have.

MR. TREGONING: Okay. I think George could give this presentation at this point. I think he already has. So we'll --

(Laughter.)

MR. TREGONING: -- spend the rest of the time -- we'll continue to move through.

MEMBER APOSTOLAKIS: And you still don't believe me.

MR. TREGONING: I believe you, you know, with all of my heart.

So we did a number of sensitivity analyses, and Professor Apostolakis alluded to one of those, and all I'm going to touch on is one because it's the one that's the most interesting. It's the one that's the most controversial, and that's looking at different ways to aggregate individual results.

So what I had shown before is essentially the blue curves here, and these are the means. So if I go back to this plot, those blue curves correspond to the red curves on this plot. I apologize for changing colors on you guys.

And the red curves here, they represent using either arithmetic mean to aggregate the individual expert estimates or analogously, at least for determining the mean, actually creating a mixture distribution from the result.

MEMBER APOSTOLAKIS: Wait a minute now. Isn't it true that they can be an arithmetic mean where the percentile is not the same as the mixture distribution.

MR. TREGONING: Right, but when you're looking at the mean it is.

MEMBER APOSTOLAKIS: Oh, you may be right there.

MR. TREGONING: Yes, yes. For that one

parameter.

MEMBER BLEY: I'd have to think real hard about that one.

MR. TREGONING: Well, the way we did arithmetic mean aggregation, we just took the arithmetic mean of all the percentile estimates of the estimates.

So for the mean estimates that they gave us, it's just the arithmetic. It's just the mean of the mean. So the mixture distribution, when you work through it, that mean is also the mean in the middle.

MEMBER APOSTOLAKIS: The mean is a funny quantity.

CHAIRMAN SHACK: It is.

MEMBER ARMIJO: I don't understand why there's such a big difference between the mean and the mean of the mixture for LOCA Category 4 on the BWR. I mean, those two curves are very different compared to the PWR. What go that?

CHAIRMAN SHACK: Essentially we had one of the panelists that if you look at the red curves, they're weighted by one panelist result. So essentially one panelist was very much higher than the rest in their predictions for BWRs. So that's why the curve shape looks like that, and that's why it's so different than the geometric mean aggregate.

Now, that one panelist, his model was -- again, his biggest risk driver was IGSCC, and this was a PFM approach, and the PFM model was essentially giving him the result that, you know, a large break LOCA has about the same frequency as a much smaller LOCA in that large piping.

So that's why his results look so flat, but they were very different. They were different than everyone else's results, and that's one of the reasons for the big difference between or the primary reason driving the big difference between the arithmetic mean and the geometric mean aggregated results.

MEMBER ARMIJO: But that same person, when you got to the Category 5 and 6s was pretty much consistent with the rest of the --

MR. TREGONING: Yes, they go back down, right.

CHAIRMAN SHACK: If you see a six it's not a pipe break anymore, you know.

MR. TREGONING: This isn't a pipe break. So it's apples and oranges, and five for BWR. You really needed a complete rupture of the prime recirc. piping to get that. So when it came down to complete, you know, these guys are balancing for all the pipes. They have to consider complete ruptures of smallest pipes leading up to that LOCA category, as well as partial ruptures of bigger pipes.

MEMBER MAYNARD: Well, they all didn't provide data for every category either, did they?

MR. TREGONING: They were consistent in that they didn't all provide us BWR and PWR information. Some of the experts only felt qualified to give us BWR information. But once they gave us information, they gave us information from all the categories from one plant type, and that was required because of the way we structured the elicitation. We needed that to be so that they could develop self-consistent estimates.

Now, some of their estimates were very like if their qualitative response said I don't think the pump casings are a significant risk driver, right, they didn't necessarily need to give us quantitative estimates at that point. You know, we can take that information and say, okay, I just need to make sure that these don't contribute to your final risk profile.

MEMBER ABDEL-KHALIK: What is the smallest size sample in all of these categories?

MR. TREGONING: We had eight BWR estimates and nine PWR estimates. So of the 12 we had one expert that didn't provide any estimates for anything. So then we had 11 that gave us estimates, and eight of those gave us BWRs, and nine of them gave us Ps. So we had two people that didn't give us Ps and three that didn't give us Bs.

I wanted to talk a little bit about the review. We've had quite a bit of review. We started with the panel itself. We did a lot of Q&A and feedback on the individual responses that they gave us. They gave us pieces. To develop one set of frequencies for an expert it took about 100, 200 questions that they had to answer. So they didn't necessarily see what their final outcome was when they were giving us a testimony.

So when they were giving us the testimony, we were checking to make sure their rationale and the numbers they were giving us makes sense, and that was actually the most extensive part of the process because quite often those things didn't match up. So we had quite a lot of feedback of each of the individual experts to make sure that their quantitative numbers did support their qualitative rationale.

They also reviewed the calculations and analysis that we did on their individual results to make sure it was accurate within the framework that we had provided them, and again, as I mentioned here, once the draft NUREG was put together, they also reviewed the general qualitative and quantitative findings and conclusions.

Did you have a question?

MEMBER CORRADINI: Well, I was going to -- I was looking through the -- so they provided their analyses or their bases for their judgments and they spoke with each other and discussed it as part of it. Was there interplay between the experts? I guess that's what I'm asking.

And then did they reevaluate it and give you another set of numbers?

MR. TREGONING: There was, and there were chances for them to do the reevaluation. The way we structured it is we brought them together as a group to develop all of the issues and brainstorm and identify the things they were going to be evaluating.

Then we did some background analyses which I'm not going to go into, but essentially the base case analyses, and we brought them together again and discussed that.

Then they went off and did their elicitations individually. Then, once we got all of the results, we brought them back together as a group again and said, "Here are your individual results and here are your results with respect to the group, and we focused on that meeting and that was about a three-day meeting. We were looking at differences because you're always looking for, you know, if one expert is different than the other. You're looking to see if there's qualitative reasons that the other ones hadn't thought about.

And they were given the opportunity after that meeting if they so chose to revise their estimates, but to be honest, nobody did. So even though they were informed, no one felt strongly enough about the new information that they thought they needed to go back and redo their estimates.

Again, we've had a lot of group exchange prior to that as well.

MEMBER CORRADINI: On the flow chart, I'm sorry. I didn't mean to take -- but that was very helpful. I was looking for the flow chart in the document.

MR. TREGONING: Okay.

MEMBER CORRADINI: I'm sorry. Thank you.

MR. TREGONING: So, again, we had a lot of feedback with the panel. We also had a small external peer review. We had two people with decision analysts and a statistician, and we asked them to look at the structure of the elicitation, the analysis procedure framework, how we did aggregation, and those review reports are publicly available.

The external peer review was quite helpful. It helped us refine our analysis technique. We've had a large number of ACRS interactions that we thank you. I think this is our 13th or maybe 14th at this point.

And then we've had internal staff review, NRR as well as people in the Office of Research, and finally we went through public review and comment.

I'll briefly touch on here in the next few slides the public comments that we got. We issued draft NUREG-1829 in June of 2005. We opened the public comment period, and then we closed it on November 2005.

We had 29 comments from the public. We had nice diversity of comments. We actually had one of the elicitation panelists himself that felt compelled to comment. That was interesting. We got some comments from academia and --

MEMBER APOSTOLAKIS: Did he ever explain to you why he didn't raise his concerns during the elicitation process?

MR. TREGONING: Oh, he did.

MEMBER APOSTOLAKIS: Oh, he did?

MEMBER CORRADINI: He just wanted to put it on the record.

MR. TREGONING: Yes, he did.

(Laughter.)

MR. TREGONING: You know these group dynamics. We discussed his comments and issues as a group, and then the group --

CHAIRMAN SHACK: Didn't see the light.

MR. TREGONING: No, no one, but that's okay.

MEMBER APOSTOLAKIS: That's good.

MR. TREGONING: And, in fact, I encourage them to do that. I said, you know, there is an opportunity and just the fact that you were an elicitation panelist, that shouldn't stop you from commenting as well as it shouldn't stop anybody from commenting. So he did that.

MEMBER APOSTOLAKIS: It would be funny if you commented though.

(Laughter.)

MEMBER APOSTOLAKIS: Right? Or Lee.

MR. ABRAMSON: We're a united team.

MR. TREGONING: I don't know if that would be unprecedented, but probably close to it.

And then we've got a number of comments from industry, owners groups, individual licensees.

At the same time that we went out for public comment we were anxious to get this out and get some comment, but the document was being reviewed internally by NRR as well, and so we got a large number of comments from the NRR staff, and in fact, the document we provided to you has the NRR comments commingled with the public comments, and we grouped the comments topically just so ACRS -- we would be able to avail you of that information so you could consider all the comments that we got, and in total we got about 101 separate comments.

So in general, to summarize the public comments, you know, public comments were generally useful. They identified some additions and clarifications, that we went forward to hopefully improve the exposition, as well as facilitate the use of these results. None of the comments certainly in the author's mind, and hopefully the responses document that, presented a significant challenge to the appropriateness of the objective approach, analysis or results.

With the public comments as well, you can see the most passionate controversy is still the proper method for aggregating individual estimates to produce group estimates.

MEMBER ARMIJO: Was there any particular -- and I would know this, I guess -- from the BWR owners on that discrepancy, was that a big, big issue?

MR. TREGONING: You mean on the discrepancy between the one expert and the others?

MEMBER ARMIJO: Right.

MR. TREGONING: No, we didn't get a comment on that. The comment that we got from the BWR owners groups or at least one comment, and you've heard these, is they were concerned that we didn't appropriately credit mitigation of IGSCC. And we did change some of the language in the report, but it didn't change any of the estimates, and you know, we documented in the report as well as in the response about how we considered mitigation, not just of IGSCC, but for all of these mechanisms that people were considering.

IGSCC was probably the most unique case because a lot of the service experience that you have for BWRs is colored by that IGSCC experience and then anything --

MEMBER ARMIJO: Anything prior to water chemistry, pre-mitigation and all of that.

MR. TREGONING: Anything before 1983, you know, and we had a lot of discussion with the group about that. In fact, a lot of the service history estimates showed pre-1983 precursor events, post-1983, and we actually then did sensitivity studies, both from a service history perspective and then a PFM perspective on the effect of different IGSCC mitigations on the failure frequency.

So it was something that we had discussed quite a bit in the elicitation.

So I just wanted to give you an example of one public comment here, recognizing that we don't have time to go into a lot of them. Of course, I'll be happy to take questions on any.

But there was one comment that our SB LOCA estimates were too high and that they weren't representative of operating experience. The comment said, you know, that approximately one order of magnitude and then the NUREG/CR-5750 results.

The implication is that we should be having one SB LOCA every four years and that using these estimates, at least the small break LOCA estimates in existing PRAs would lead to unwarranted impacts that are not supported by operational experience.

MEMBER CORRADINI: So your thought about that would be?

MR. TREGONING: Yes, so we thought about that and responded, and I think the main thing that we thought was a good idea is we didn't have a comparison within 1829 on how the results compared with service experience. So we added this section.

We had a section on how it compared with prior studies, and a lot of those prior studies had shown how they compared with service experience, but we thought a fresh look at service experience would be useful.

And when we say "service experience," we're really limiting it to the small break LOCAs because that's where we have -- you can actually argue that we've had a couple of events. Certainly we've had steam generator events, and we've had a few pipe breaks in Class 1 systems that border on the small break LOCA threshold. So we actually had some data other than zero events. So we felt most comfortable making those comparisons.

This is the basis or sort of the basic response or the fundamental points in the response that we made to this one. It's at least the author's opinion that the SB LOCA and the 5750 estimates are generally consistent. The steam generator tube rupture estimates are virtually identical. In fact, they're actually a little bit lower, and that mainly is reflective of the fact that we've had additional service experience since 5750 came out, but there are about --

MEMBER APOSTOLAKIS: What was the year of 5750? I don't remember.

MR. TREGONING: It was published, I think, in '97 or '98, but a lot of the events, most of the events were analyzed up to about '96.

MEMBER APOSTOLAKIS: That's about ten years.

MR. TREGONING: About ten years now.

The BWR SB LOCA estimates are actually quite similar to 5750, within about 20 percent. The big discrepancies are the PWR SB LOCA estimates. They're higher than the 1829 study, about a factor of five, and again, the experts supported that with, again, there was a lot of concern at the time about the effects of PWSCC on small break LOCAs. So their concern was reflected in this increase.

We also --

MEMBER APOSTOLAKIS: Excuse me, Rob. So this is the estimate that would lead to one small break LOCA per four years? I mean they made a statement of that nature --

MR. TREGONING: Right.

MEMBER APOSTOLAKIS: -- in your slide, right?

MR. TREGONING: Well, there were a couple of things. When we published the draft NUREG, we had the steam generator estimates separately. Then we just had the LOCA estimates that had combined the steam generator and the small break LOCA estimates. Okay?

So when they did their estimates, they did a simple subtraction, and the way we aggregated, you can't really do a simple subtraction to get the results. So what we did is we went back and looked at each individual set of results and for each of those individual results, we subtracted their steam generator risk contribution from all the others, and then we re-aggregated.

So we analyzed in a way that was consistent with how we analyzed the rest of the results, and because it's not a linear analysis, you don't get the same answer as you would if you simply subtract them. So in the new 1829, we actually published the small break LOCA estimates without steam generator contributions as well so that people can see what they are.

So that's in addition. We added those, as well as we did --

MEMBER APOSTOLAKIS: But what is the answer to this?

MR. TREGONING: What do you mean?

MEMBER APOSTOLAKIS: Is it a true statement that your estimate leads to an average of one small break LOCA every four years? A simple --

MEMBER ARMIJO: It doesn't make sense. Sanity check.

MEMBER APOSTOLAKIS: Are you still doing that?

MR. TREGONING: It's not quite as high as that, but you know, you're one in four, one in five, but, again, you have to look at -- these are not average. These frequencies are never intended to represent averages over the entire operating fleet, right? They were meant to be snapshots of where we are now, given concerns, and they were concerns about the effect of PWSCC and PWRs on what those frequencies were.

We looked at all of the pipe breaks that we had and Lee helped us. We did a Poisson-type of analysis, and you might want to, and you might want to comment on this, and showed that the estimates that we had as well as the uncertainty about those estimates, even though they were elevated, they were still consistent with operating experience or they weren't inconsistent with operating experience.

MEMBER APOSTOLAKIS: So is operating experience telling us then that we have something we can call a small break LOCA once every four or five years? Is that what you're saying?

MR. TREGONING: No, that's not what I'm saying. I'm saying the current frequencies that we have are higher for PWR SB LOCAs.

MEMBER CORRADINI: But if I could just read on page 750 of the report, specifically the paragraph here that you guys have is that you point out that for small breaks, the current elicitation is lower than the pilot, but it is higher than -- as you state, "However, the current elicitation concerns for PWSCC cracking and BWR CRDM nozzles results in additional increases."

Is that -- I'm looking for a physical reason why, if the expert judgment is larger than the service experience, does it come down to those sorts of --

MR. TREGONING: Yes.

MEMBER CORRADINI: Okay, all right.

MR. TREGONING: Yes, that was the qualitative rationale driving it, and the expert said even though, again, when we did the study, when we did the results or when we did the elicitation, it was 2003. So you know, we had had Davis-Besse. We had Oconee, We had V.C. Summer. We were still in the process of attempting to develop mitigation strategies or we hadn't even started it yet, to be honest with you, for PWSCC. We're really starting that now.

And many of those same experts said while it's elevated now, the expectation is that once mitigation has been fully implemented, that those frequencies will decrease again.

MEMBER CORRADINI: Okay. That's fine. I just wanted to understand because this is not my area. It's a material. So I'm always looking for the physical reason underlying why an estimate might be different than the service experience.

CHAIRMAN SHACK: Just Lee's Poisson

analysis gave him a resulting range of .7 to five breaks, and you've had one basically.

MR. TREGONING: Yes.

MEMBER APOSTOLAKIS: So the third red bullet, BWR small break LOCA estimates are higher by approximately a factor of five, but because NUREG-5750 is kind of old, they think this is reasonable. That's the implication there?

MR. TREGONING: Yes.

MEMBER APOSTOLAKIS: Okay.

MR. TREGONING: And it's not the fact that NUREG/CR-5750 is old. It's the fact that, again, the elicitation -- these estimates were supported by, you know, expectations for higher frequencies due to PWSCC cracking.

So that's the third bullet. You know, the differences that do exist are supported by qualitative rationale, and we made a number of modifications. We have provided the separate steam generator tube and small break LOCA estimates as I mentioned. We have a much more extensive comparison between 1829 and historical results, and then we also have these operating --

MEMBER APOSTOLAKIS: So that frequency then is roughly two or so, ten to the minus three, an average number?

CHAIRMAN SHACK: Five times ten to the minus four for small break LOCAs.

MEMBER APOSTOLAKIS: Five, ten to the minus four we mean by it.

CHAIRMAN SHACK: Which in 5750 is one.

MEMBER APOSTOLAKIS: Is what?

CHAIRMAN SHACK: A one times ten to the minus four.

MEMBER APOSTOLAKIS: So if you have five ten to the minus four and you have how many PWRs?

MR. TREGONING: Sixty-nine.

MEMBER APOSTOLAKIS: Multiply that by 69. Do I get this number of four or five per year? I guess I --

MR. TREGONING: No, no, no, no, no. You get one every four years is what the commenter --

MEMBER APOSTOLAKIS: One every four years. So 69 multiplied by four.

MEMBER MAYNARD: Well, another factor is you want this to be a tool that's useful in the future, not necessarily reflecting exactly where we are today. It completes aging, and so the numbers that you're giving and the tools that you're putting out there need to be a good five or ten years from now as well as today.

MR. TREGONING: Right. We did ask for estimates. We asked for three time periods. We asked for current snapshot. Then we asked for 15 years from now and then we asked for another 20 years past that. So we did provide multiple estimates, but you know, there's a realization, too, that you know, your epistemic knowledge state is changing as you go along, too. So certainly the further you asked people to prognosticate, you know, we had enough uncertainty with the current day. So when you try to prognosticate out further, you have more uncertainty, more variability.

MEMBER APOSTOLAKIS: All right, fine. Let's go on.

MR. TREGONING: That's it.

MEMBER APOSTOLAKIS: Any questions before we move on to seismic? That's the next one, right?

(No response.)

MEMBER APOSTOLAKIS: Okay. Nilesh, are you taking over?

MR. CHOKSHI: Yes.

MEMBER APOSTOLAKIS: Thank you very much, by the way, as an afterthought.

Tell us who you are and why you're

qualified to address this distinguished group.

MR. CHOKSHI: Yes, I will.

CHAIRMAN SHACK: He drew the short straw.

(Laughter.)

MR. CHOKSHI: Okay. Good morning. My name is Nilesh Chokshi. I'm Deputy Director of the Division of Environmental and Site Reviews, Office of New Reactors.

And if you wonder why I'm here giving this presentation, I was in Research when this study was conducted two years back. So that's the reason I'm here, and as you see from the list of names, this was an interoffice team, including seismic expertise, piping design, fracture mechanics, seismic risk, and also the people involved in the rulemaking. So this was, you know, a substantial and also very large contractor support.

In fact, Dr. Gery Wilkowski and I and his organization, EMC^2, made the floor piping analysis, and he was here at the subcommittee presentation, and I think he's available on the phone also. So that's good for me. He can answer some of the questions.

So with that, let me --

MEMBER APOSTOLAKIS: Who's the gentleman on your right?

MR. CHOKSHI: No, Gery is not here. Oh, sorry. This is Mr. Khalid Shaukat. He's from Research, and you'll see him again when we move forward with this study, but he was the project manager of this study.

Okay. So now what I intend to do is to cover the basic objective. At the subcommittee meeting we did discuss in detail the technical approach and rationale behind this. I think during the short time here I'm going to focus on some of the key research and findings and not as much on the methodology.

And then what I want to do is towards the end I'll summarize the response from the industry on specific questions where we are asking the proposed rulemaking and where we are and what factors we need to consider as we move forward in this rulemaking process, but as pertains to this particular issue.

So let me start with the objective. Instead of directly estimating the seismic and use break frequencies as it was done for the expert elicitation, we decided to concentrate on a different question, and the question was: what are the conditions and likelihood which would, under the seismic-induced loading, which would be incompatible with the proposed TBS?

In other words, would the seismic-induced breaks, would they be larger than the TBS and would have frequencies of ten to the minus five or more?

And I think that's a very germane question, given, though, that the object was to provide this information so people can comment and respond to questions. In the context of the proposed rule, this was a direct question and also within the time period it's something you can do, you know, estimating absolute frequencies given seismic events. It's a much larger undertaking.

In order to answer this question we took six activities. We looked at unflawed piping; flawed piping, piping that has cracks or degradations; indirect failures; review of past earthquake, past PRAS; and then there was a study conducted in the early '80s in connection with GDC4, which was to answer the question whether the LOCA and the earthquake load seems to be combined, and this was a full-blown probabilistic fracture mechanics analysis.

The first three are the different -- mechanisms, how the piping and piping system can fail and would have, you know, an impact on TBS.

The review of past earthquake experience and past PRAs were to get additional insight; also to calibrate system level analytical study CRD, what we see in the earthquake experience, and what we find analytically; is this consistent or not; and then this, the last, was an important study, and a lot of decisions were based on this study. It also provides a direct way for us to calculate the mean direct failures.

So we used the modified history to build a short current-day hazard and use that for the indirect figures.

Now --

MEMBER ARMIJO: Were these analyses only done for the transition break size pipes?

MR. CHOKSHI: No.

MEMBER ARMIJO: Just for that size or for --

MR. CHOKSHI: No. How long we'll talk about that, the next slide, the scope of the study.

And we used basically a combination of deterministic and probabilistic approach. For the unflawed piping and indirect failures, it's pretty much probabilistic approach, and we did not estimate the four distributions, but it was a probabilistic approach.

On the flawed piping, the seismic loading was based on the probabilistic hazard, but the rest of the calculations were deterministic.

To address some of the variabilities, we did a large number of samples, and I'll try to tell you about how many piping systems we looked at, and we also conducted some additional sensitivity analysis to look at the effects of key assumptions.

Here is, I think, to answer your question, I'll move on to this viewgraph.

One of the biggest challenges in performing this type of analysis is the availability of the design information because that was our starting point. We needed normal operating stresses, seismic stresses. I'm talking about design stresses, material properties, and a few other things so we can do our calculations.

One of the databases which had captured this information is the leak before break application database, and which basically applies to PWRs. So we had these data available for PWRs, and that's why one of the reasons was material evaluations for BWRs.

Having said that, there's nothing inherent in these matters or conclusions which does not apply to BWRs or other situation.

Now, the tool I mentioned about, that we wanted to look at a large sample to cover the range of materials properties, range of the seismic stresses, and the site conditions. We selected 27 PWRs, 24 on the large side, three on the small sides. Large size from the seismic perspective is not critical. We generally get higher stresses.

The second issue was what hazard curve to use, and this was a question because, two years back, this is when the Early Site Permit applications were coming in, and they were using new estimates. Without considering all factors, we thought that for the 27 sites the research we had available was that Livermore has those, and we will study the sensitivity of alternate hazard in a different way. So we decided to use the Livermore hazard curve.

Now, I think to answer your question, because we see what's the effect on TBS, we selected piping systems larger than the TBS. We did examine one or two cases with the TBS diameter, but more as a calibration, but which meant that we were looking at hot leg, cold leg, and crossover leg, and we selected 52 systems from 27 PWRs. We tried to capture the highest trace locations and materials. Okay?

Now, one of the key, in this kind of

evaluation, you need to do a realistic estimate of seismic stresses. The design stresses is a starting point, but as you go up in the earthquake, it's difficult; to seismic PRA, you have to do more realistic estimate of seismic stresses at the higher level, and we used basically an approach commonly used in the seismic PRA and seismic margin to estimate those spaces.

So that was a common approach, I would say, in all three, that we were trying to estimate realistic estimates at higher level of earthquakes, and for the flawed piping we selected two discrete levels, ten to the minus five probability of accidents and ten to the minus six probability of accidents. For direct and indirect, we can basically use, then, the entire probabilistic hazard code.

So this is what I'm talking about matters and not too much more. In flawed piping and indirect, I'll do a little bit more, but at the subcommittee we showed some quantitative research on the unflawed piping, but I think other cases are a lot more interesting. So I'm going to --

The key finding is that unflawed piping, in order to get a seismic-induced failure, you have to have a lot of flaw. It just doesn't happen. In fact, our numerical results would be an order of mean failure ten to the minus nine or less, okay, for the cases we looked at.

I think it's important to a little bit talk about now earthquake experience. We have looked at a number of industrial facilities and fossil power plants, most of the data come from, but all of the welded piping systems which are engineered actually behave very well in the earthquakes. We are looking here at experience data up to .5g ground acceleration, and where we see failure, there's a severe degradation, either support failure, again, associated with severe degradation; there is missing anchor bolts or corroded plates.

We see relative motion. When you have an inflexible pipe and there is a support, and in fact, at a recent earthquake in Japan, we saw, I think, all seven plants. There was a vent. Vent was connected to a stack, which was in a different foundation, and other support was in a different foundation. All six identically failed because of this anchor motion.

And then things falling over the piping. So I think this result is consistent. So I think this case, that unflawed piping, unflawed piping is basically a piping which meets the assumptions used in design and would fail by a particular shutting down than a collapse or a tear-through.

Okay. Now, the flawed piping, I think that's the more interesting. Again, I have been conducting, you know, crude probabilistic analysis. We decided to look at design conditions and the conditions at the higher level. We wanted to look at what are the critical flaws at ten to the minus five or ten to the minus six seismic range, and either they're large or small compared with the crude allowable flow evaluation.

So we performed all the normal operating conditions and earthquake, ASME inspection/evaluation criteria for circumferential surface flaw, and we also used the LBB procedure. What would be the through-wall flaws and how they would compare with the critical flaws at ten to the minus five up rate and ten to the minus six up rate?

And this was basically to answer two questions. Will ASME surface flaw criteria at normal stresses find flaws that are smaller than the ten to the minus six or ten to the minus five? Because this implies some inherent safety.

And would the LBB procedure find the through-wall flaws that are smaller than the particular flaws?

And also I think, as I mentioned with the experience data, the question itself, how large these flaws have to be, is important. So that was the parameter, you know, how large these flaws have to be. Because if you were really looking at the total probabilistically, then you will have to look at probabilities of existing flaws, probabilities of detecting flaws were they to link before. So I think this information was very -- that's why I said that in the right context, the proposed TBS, this information was germane.

So I'm going to now go to the resource. All right. Let me first -- these are the two results for the surface flaw evaluation. This is the two systems from the 52 systems we examined, large, and the plot on the left, I believe it's in a hot leg looking at a ten to the minus five earthquake stresses from a Westinghouse PWR. But let me first explain what you are looking at.

The X axis is the flaw length. Okay? And as you go from the extent of circumferential flaw length. On the ordinate is the flow depth ratio, is the ratio of through-wall to the pipe thickness. So as the flaws get smaller, circumferentially you have to have a larger depth for them to become critical.

As you move toward the right with the larger flaws, then you know, these critical depth ratios are becoming smaller. An interesting thing is that you see that after a certain .5 or something or .6, you start approaching basically asymptotic value.

You are also seeing the ASME code limit, that basically this requires evaluation of flaw. If it's smaller than that, I think you can continue operation. In no case, you can go tolerate more than .75 here.

This is a typical case. In the report this is called Category A. The red line is the critical ten to the minus five critical flaw length. Yellow is using the ASME code strength and procedure, and this is the same as the ASME procedure, but using actual strength. The code allows that. In this case the critical flaws associated with the earthquake, large earthquake traces, is much larger and this is a typical ten to the minus five.

Now, we also have what we call Category C, a few cases, and I'll show you the overall. In this case, the critical flaw is smaller than what the ASME code would allow, but I think the one important thing is that in all cases there are very high -- issues.

The cracks have to be pretty great.

Now, what I'm going to show you now, the results for the ten to the minus six in a little slightly different form. This is the calculation performed for a large earthquake. The ten to the minus five was sort of a starting point for the TBS, but we also wanted to look at what happens at larger earthquakes. One of the things, it answers the question of an alternate hazard as well as whether there is a sharp transition somewhere, you know.

And what you are seeing here is that upon a very large flaw, for a different seismic -- for ten to the minus six seismic stresses, what are the critical flaw depth values? And you see that .3 is the smallest value. It's somewhat material-dependent, but in many cases, you wanted this stress level is much larger. So you have to have a very significant, large cracks in the pipes before you get to the seismic in these breaks.

Here is the summary of the 52 cases, and as I mentioned, for eight times ten to the minus five, yes, 48 cases the critical crack sizes will be larger than the ASME code. In one case it was larger than the core evaluation using the core values, but smaller using the actual strength values, and in three cases was the second case I showed that's called Category C.

But, again, I think to me the -- well, I'll come back to the final message. So this was the results of the surface flaw evaluation.

The second thing we wanted to look at, the leak-before-break behavior, and we wanted to see that if you applied LBB analysis as currently with the factors of safety of ten on the leak rate and on the flow size factor of safety of two, and how would that compare when you do the same calculations, but using higher seismic stresses and also examine the sensitivity to different factors of safety and also maybe different assumptions on the leak detection capabilities or the leak rate.

So I'll show you the results from the sensitivity studies. This is also a factor of the crack morphology, and I'm going to -- so we looked at three crack morphologies, one with a very smooth crack, a PWSCC type crack, and corrosion-free.

Okay. So here are the results. On the X axis side is the ratio of normal to normal plus higher side mixed traces at ten to the minus five. So as you go left implies higher seismic stresses.

This is the leakage over critical flow size, the leakage flow size using the current procedures, and get the design basis stresses, SSC stresses, and this is the critical flaw sizes at ten to the minus five.

These are the results of fire systems for all different plants. What it shows, that if you consider the factor of safety of 1.5 instead of two, in the report there are other results, but if you do these calculations, you keep the liquid requirement the same as factor of safety of ten on the detection capability of one gpm. You will find some cases where the critical flaw size would be smaller than the LBB.

But if you look at an alternate leak detection capability, and I understand that some LBBs use this, of .5 gpm, keep the same factor of safety, you can see that there.

And you know, this was not to draw the conclusions, but to provide information so people can evaluate in all of the proper contexts.

MEMBER ARMIJO: You said .5, but the chart says five.

MR. CHOKSHI: Well, yes. The five gpm, you calculate your break size and the flaw size based on the certain rate. Okay? Five gpm is used in this calculation, but the current procedure requires that if you have a flow liquid of five gpm, your detection capability should have a factor of ten safety, safety factor of ten. So detection to be .5 gpm. Okay?

Basically this is a sensitivity study on two different detection capabilities. So this is the results from the leak before break. So here is the summary of the flawed piping. I think to me these two viewgraphs really are critical. The critical crack slips are larger than 40 percent for the ten to the minus five and larger than the 30 percent thickness for ten to the minus six. So you're talking about substantial, large flaws. Again, I think to me it seems to be consistent with what we are seeing.

On the LBB flaw size, again, we see in many cases that the LBB flaw size will be smaller than the critical, and for the better appreciation of under what conditions the LBB could be, you know, at least a viable consideration, we need some sensitivity studies.

Now I am going to move to the indirect failure, and this is the failure mechanism which PRAs include. Seismic PRAs traditionally, and I think for good reasons, have not included piping failure as an initiator of LOCAs, other than small LOCAs, but some of the PRAs have included this, and to give an example, the 1150 study plant, there was a failure mode where the steam generator support failed, and what happens is that those sequences occur in very large earthquakes. They dominate the release contributions, but they are not dominating the contributing core damage because you make an assumption that if I'm going to feel a support of a steam generator that is going to allow movement, I'm going to fail containment also at the same time, and I'm going to have a LOCA which probably is not possible to mitigate.

And so this is the assumption also. The assumption is that the failure frequency of support is the same as if we left concealed break.

Now, let me now talk a little bit about the original Livermore study we just conducted in the early to mid-'80s. They grouped the plants in various renderings. They are Westinghouse, CE, BMW, and then they also looked at one BWR plant.

They selected the one pilot case, and then they looked at data across the fleet. They used the generic hazard curve for east of the Rockies. What we had to do was to primarily update the hazard information.

Now, we have a plant specific or site specific hazard information. So that was the major modification to the work they did. They had a number of studies. We selected two cases. One was what was in the bounding case in their analysis of a Westinghouse, and another was CE, and I'll show these in a minute, but in Livermore study they had estimate of the fragility of the support, and we modified that to reflect the site specific information.

CHAIRMAN SHACK: Now, did you look at IPEEE-2 to see if those fragilities from the Livermore study were still -- you know, when people looked at them presumably at their individual plant they did a little better job.

MR. CHOKSHI: And I think what happened when IPEEE, that seismic sequences were basically governed by other failures. So there was little inside, but when we did the seismic margin development, we had looked at this, and I think only two components from the seismic margin you examined below .5g is the pressurizer support and the vessel support for the BWRs.

Most of the other components are very high capacity, and the results reflect that. So basically we completed the failure probability of the support with the site specific hazard and modification of the Livermore study.

MEMBER SIEBER: How detailed is the calculations involving support fragility for large components like --

MR. CHOKSHI: It says --

MEMBER SIEBER: -- steam generators?

MR. CHOKSHI: It's fairly significant because Unit 2, you know --

MEMBER SIEBER: Not just examination of the beams, cradles and so forth, you know. My experience is that bolting is a critical issue in those large supports.

MR. CHOKSHI: Right. Yes, we need to look at a variety of failure modes and see. You know, they have combined them so that you could arrive at those.

MEMBER SIEBER: I guess you can draw the same conclusion about pipe supports. It's the bolts that fail first.

MR. CHOKSHI: Yes, yes. And in the Diablo Canyon PRA, that was the mechanism they included, and it's more than one support in order to fail a pipe. You need to fail --

MEMBER SIEBER: You get a cascading effect.

MR. CHOKSHI: Cascading, right. You have to have at least I think, if I remember right, in the

Diablo, we looked at five supports.

MEMBER SIEBER: Thank you.

MR. CHOKSHI: Now, maybe if I show you the results from the Livermore regional study, and here you can see that -- let me concentrate on, let's say, median values. These are ten to the minus six, ten to the minus seven order, and what they had on the mean perspective, this was the lowest capacity plant. Ninety percent was two times ten to the minus five.

Making corrections to the fragility and using the site specific casuals, two times ten the minus six. That's so we wanted to compare that, and when I look at the -- and I believe for the Westinghouse, the lowest capacity, three time ten to the minus six. So it was still an order of less than ten to the minus five.

Now, EPRI, as a part of the response to questions, did some additional calculations using the EPRI latest hazard coverage, and they examined three cases, and I'm going to report on the results, but we haven't reviewed. They're basically the same approach, modified fragility, but they do add some additional factors, and their results range from six times ten to the minus six to five time ten to the minus eight, and one of them was a BWR. I think five times ten to the minus eight was BWR.

Anyway, so the bottom line, I think is that you still are away from the ten to the minus five type of pressure. So now let me go to the overall summary of the story.

Unflawed piping systems have very -- you know, seismic frequencies are small. Critical suppressed floor and through-wall, you know, LBB, you have to have large flaws to have seismic induced failures.

And then indirect piping failure, the things we looked at, it still seems like an order of ten to the minus six per year. So this was the overall summary.

Now, I'm going to switch to the approval and questions associated, which are included in the draft code, and responses. The proposed rule contained extensive discussion. You know, it observed that the expert elicitation had not included explicit consideration of seismic induced failure, and here is a large uncertainty, and there was still a question whether a plant specific assessment would be required or not, and there were three specific questions that were posed. One was to comment on the evaluations of the study and, you know, if any comment they had on that; effects on the five-day degradation on seismic in the LOCA frequencies and, you know, from their own information; and then also the other one was to potential policies and options to address this issue other than what, you know, we have put forth.

The comments primarily were from the industry, and I'll summarize quickly. Basically they had no really comments on the study itself. They said we agree with the study's findings and that TBS is not adversely affected from the seismic consideration.

And I'll go to the next slide.

This is important. This data risk or the change in the risk due to seismic is considered low, and our basic argument was that components in the piping in the primary loop and supports generally have a much large capability or capacity. It's a lower fragility compared to the rest of the plant, and so the risk is general dominated by the other previous scenarios, and so the seismic to be that.

And then I mentioned within that failure, EPRI gave us additional resource which we'll have to look at, and then their bottom line conclusion was that plant specific assessment cannot be required.

This is now my last slide. I think moving forward we have an ACRS recommendation. There is an

SRM requirement, and to address this issue we need to consider a number of things before we you know.

First, I think we need to look at the response to questions, and I think my three bullets actually. The next two of those, one of the response only.

Changing the risk, I think probably that's important, that we fully understand that.

One of the important, I think, considerations will be from Commission SRM and ACRS recommendations, how this will get -- addresses the defense-in-depth and mitigation recommendations. Because that will have an effect on any of the risks under any risks.

We need to understand fully whether the seismic -- is that under what conditions the seismic risk could be affected, and I think it is my -- this is mine now -- that if the seismic risk comes from the structure type failures, unless plant modifications are made.

Now, what the rule and mitigation plays and nonseismic failure plays, we need to look at and we need to better understand what we do here, but I think unless, you know, there is a significant change to the supports or something, I think I -- and then --

MEMBER APOSTOLAKIS: I didn't get that.

MEMBER CORRADINI: We don't understand you. I 'm trying to understand you.

MEMBER APOSTOLAKIS: Your conclusion you don't state.

MR. CHOKSHI: Okay.

MEMBER APOSTOLAKIS: Unless -- keep going.

MR. CHOKSHI: Okay. What I was trying to say, that unless somebody modifies the supports or something, unless there are physical changes to the seismic capacities are less. You know, I don't see that occurring, you know.

Now, there may be a system and operating condition changes which we have to evaluate after we understand what defense-in-depth and mitigation.

Structural changes I think is, you know, somebody's postulating. Then it will have an impact on seismic risk.

And then finally also I think, you know, this has to be reviewed to understand what the Commission has asked that we develop guidance on the issue of applicability of 1829, and I think to me this has some of the things which we may think independently in the seismic, you know, some of the regulatory considerations and things like that.

So at this point I think we have to wait and see how these things go before we make the decisions or, you know, how do we deal with these issues. So that's the end of my presentation.

VICE CHAIRMAN BONACA: Yeah, I repeat what I said during the subcommittee. You know, I would like to see sensitivity of the results that you are presenting to that multiplier you used to eliminate the excess conservatism, as they call it, associated with the design stresses.

I agree that we have to use a reduction, and I'm not proposing that you would use the design values, but that's a significant multiplier. I mean you are using a .6 or something like that if I remember now that was in the report, and I would like to understand the sensitivity and its conclusions to that multiplier.

MR. CHOKSHI: I think it's a good -- you know,, and what drove us to the looking, also the ten to the minus six, you know, a number of things, what happens with automatic hazards, what happens in the seismic stresses, you know.

It does not answer fully the question I think you asked, but --

VICE CHAIRMAN BONACA: Well, I mean, you

made the point that, you know, you cannot tolerate the design values. They're excessive.

MR. CHOKSHI: Right.

VICE CHAIRMAN BONACA: So there is some place between the design value and the reduced value, and the approach you're using to scale it down seems to be pretty empirical. I mean, it just --

MR. CHOKSHI: I think maybe the one thing I didn't mention, I think I agree with you, but there are a number of factors one can consider, and we basically selected adjustment of the seismic spaces using the concentrator on the site specific hazard information.

We were, I would say, considerably biased in that selection. We did not use all of the factors, but you are right. It was qualitative, but we could have examined it quantitatively.

VICE CHAIRMAN BONACA: Yeah, to get a sense again of the sensitivity, what is the margin it should have and --

MR. CHOKSHI: Absolutely, we can do that. Thank you.

MEMBER APOSTOLAKIS: We were also told at the subcommittee, a member of NRR, that this rule, the priority of this rule has been reduced, right?

MR. CHOKSHI: Well, then you have person.

MR. DUDLEY: Yes. When the Commission reviewed the staff's paper that addressed the ACRS recommendations, the Commission agreed with the staff's recommendation that the priority of this rule should be reduced from a high priority rule to a medium priority rule.

So we are proceeding forward with a rule, but on a little slower basis and our next due date is to provide a schedule to the Commission for completing this rule, and the schedule is due by March 31st, 2008.

MEMBER APOSTOLAKIS: At the subcommittee you told us that the reason or one of the reasons, I guess is that there are insignificant safety benefits of this rule, but the benefits really are negativities because they won't be able to raise the power. Is that a correct statement of what you said?

MR. DUDLEY: Well, that was the ACRS, included in the ACRS' letter. The staff pretty much agreed with that, and that was included in the Commission paper.

MEMBER APOSTOLAKIS: But isn't part of risk informing the regulations to remove unnecessary regulatory burden?

MR. DUDLEY: Yes, it is.

MEMBER APOSTOLAKIS: And wouldn't this rule remove such burden?

I mean we are not looking for safety benefits that haven't been risk informed.

MR. COLLINS: This is Tim Collins of the staff.

We agree with that, and that's why I think the rule was not killed as a whole.

MEMBER APOSTOLAKIS: Oh, okay.

MR. COLLINS: I mean, it was initially considered high priority because of the potential for safety benefits. That's got its high priority. Now, when we seem to come to the realization that there wasn't a whole lot necessarily there, then its priority got reduced to medium because there was still the potential for reducing unnecessary burden.

MEMBER APOSTOLAKIS: Yeah, that's helpful.

MEMBER ARMIJO: If I recall, some of the industry people were negative because the transition break size values were too high and implied that they wouldn't use a rule or they didn't expect too many people to want to use that rule. Is that still the case?

MEMBER SIEBER: It doesn't make any

difference.

MR. COLLINS: Well, I think the industry folks are here. Maybe they could answer that for themselves. I think that's still true.

MR. DUDLEY: Right. We haven't heard any indication from industry.

MEMBER ARMIJO: So the priority, it's not high priority for the Commission for safety benefit, and it's not much value to the industry. I think it's a good study, but I think the medium of priority for pursuing it is probably the right thing to do or even less.

MEMBER MAYNARD: I think the value to the industry or to a reduction in burden depends on what the form of the final rule comes out to be. What are the transition break sizes and what are the mitigating requirements?

I think it's something that can be of benefit and a reduction, or it can be something that provides really no benefit or no reduction, depending on really those two primary things, transition break size and what's required for mitigation.

MEMBER SIEBER: Well, it allows more realistic calculations in 5046 space, and to me that's a significant benefit.

MEMBER APOSTOLAKIS: So if it's of medium priority, that means when are we going to see you again.

MR. DUDLEY: I guess it will depend on the schedule that we provide to the Commission on March 31st.

MEMBER APOSTOLAKIS: Okay. Your schedule.

MR. DUDLEY: That's correct, and some of that depends on the work that you're hearing today. So we really can't provide you a schedule at this point in time.

CHAIRMAN SHACK: Nilesh, did you think of asking the NDE people for what they think the detectable crack size. You know, you've given me ASME code limits. You know, that's wonderful. I can't find a ten percent crack very reliably.

MR. CHOKSHI: I'm looking, but what I can tell you is that there was extensive discussion about that issue.

CHAIRMAN SHACK: I would expect there would be.

MR. CHOKSHI: And a number of people, including NRR resources. The best way to summarize is that we were starting basically can we put it, say, probability of detection, dealing directly.

CHAIRMAN SHACK: And so what you're really relying on is the probability that you're not going to have a 30 or 40 percent deep, long crack, which, you know, is pretty small.

MR. CHOKSHI: Implicitly I think that's why I think it's a good way to present this information, so people think about those factors. You know, what's the probability of having this size? How will it grow into the service, you know. So I think to me it brings the focus.

We were trying not to draw conclusions because it's hard to come to without any kind of probability.

CHAIRMAN SHACK: You still have to make that judgment on how likely those cracks are.

MR. CHOKSHI: And the report says that, you know, that given this, you know, that's why we looked at ten to the minus five but actual probability of failures, you know, but if you consider all of these factors, you know, it's obviously small.

MEMBER SIEBER: I think there's some comfort if you take something like centrifugally cast, austenitic stainless steels, it's pretty hard to find; it's not as easy to find flaws in that as other --

CHAIRMAN SHACK: That's a mild statement.

MEMBER SIEBER: It's not a curiosity. On the other hand, for the large pipes made of that material, it usually before a break, which tells us you something, too, and so I don't think that we are left without assurance.

CHAIRMAN SHACK: Oh, no, no, no. Those are big cracks under any circumstance.

MEMBER SIEBER: Absolutely. Well, this is what you want to avoid.

MR. CHOKSHI: And I think then there are many recent studies will even lock in a few more insights into what's more likely, but when we were doing it, I think, this was to present the information so people can make an informed judgment.

MR. TREGONING: This is Rob Tregoning from staff.

I would almost view those as a demonstration requirement. They tell you the performance that you have to have and then it would be up to maybe the reg. guide or even licensees that want to use 5046 to provide some sort of demonstration that their piping will meet that performance. It's not flawed, you know.

CHAIRMAN SHACK: But I think they're going to have to make that argument not on NDE, but on the fact that they have water chemistry and materials that are not suspectable to this kind of --

MR. TREGONING: Well, there are a variety of ways that you can make that argument.

CHAIRMAN SHACK: But I agree. They have to make that.

MR. TREGONING: NDE is a piece of it, but I would agree that you probably don't want to -- that's not your sole argument.

CHAIRMAN SHACK: I keep losing my argument that you actually have to demonstrate leak before break for these piping systems before you can take credit,b ut I still think that's a good idea.

MR. CHOKSHI: But to me I think for a risk informed rule, this is really a key question, and we need to understand the potential changes and will they have an effect on seismic risk.

Seismic risk is different than anything else and its common cause effects and are you really affecting this, you know? It may be dominated by some other things, and may not have a really -- you know, the redundance doesn't have the same effect from a mitigation point of view of difference in depth point of view on the seismic. If you put tow identical systems it doesn't buy you much.

So we, I think, need to ultimately be able to answer this question, you know.

MEMBER APOSTOLAKIS: Any other comments or questions from the members?

(No response.)

MEMBER APOSTOLAKIS: Well, thank you very much.

MR. CHOKSHI: Thank you. Thanks, Gery. I hope he's there.

MR. WILKOWSKI: Yeah, I'm here.

(Laughter.)

MEMBER APOSTOLAKIS: And, Mr. Chairman, 25 minutes early.

CHAIRMAN SHACK: Twenty-five minutes early, George.

MEMBER APOSTOLAKIS: I want to use that up in future meetings.

CHAIRMAN SHACK: A credit.

MEMBER SIEBER: Eliminate one of them.

CHAIRMAN SHACK: I think we will take a break now until 10:45.

(Whereupon, the foregoing matter went off the record at 10:05 a.m. and went back on the record at 10:46 a.m.)

CHAIRMAN SHACK: It's time to come back

into session.

Our next topic will be on the AREVA Enhanced Option III long-term stability solution, a topical report, and Said will be leading us through that.

MEMBER ABDEL-KHALIK: Thank you, Mr. Chairman.

The Thermal Hydraulics Subcommittee held a meeting on November 14 to review AREVA's detect and suppress stability solution and methodology. We heard presentation by AREVA and the staff regarding two licensing topical reports, ANP-262P, Rev. 0, entitled "Enhanced Option III, Long-term Stability Solution," and BAW-10255P, Rev. 2, entitled "Cycle Specific DIVOM Methodology Using the RAMONA5 Code."

Subsequent to the subcommittee meeting, the staff issued revised draft safety evaluation reports on November 27th.

At this time we will hear presentations by AREVA and the staff. Parts of this presentation will be closed because of the proprietary nature of the material to be presented, and at this time I'd like to call on Dr. Tai Huang of the NRC staff to begin the presentation.

MR. CRANSTON: Let me interject just

quickly. My name is Greg Cranston, Reactor Systems Branch Chief.

Before we introduce Tai, I also want to point out that Jose March-Leuba from Oak Ridge National Laboratory also participated in preparing this, is unable to attend today, and Dr. Tai Huang will be making the presentation.

Thank you.

DR. HUANG: Okay. I'm Tai Huang from Reactor System Branch, and I'm the original reviewer, technical reviewer for the AREVA BWR Owners' Group long-term stability solution, including ATWS LOOP and instability, and like today the Chairman says that we have two topic reports, and these regarded to stability. One is Enhanced Option III and second would be the cycle-specific DIVOM methodology using RAMONA5-FA code. These two topical reports are really interrelated, to support each other.

And as you see today, because the industry demand on that extended operating domain, so you see these three because of this demand for this extended operating domain which pose new challenges to stability as shown in this power flow map there. In this, back in the old day, we starting with the original licensing thermal power, and now into the

MELLLA EPU condition, and beyond that, they have a MELLLA+ region here because this stability boundary over here, and during that, the two pump trip situation, either here or they end up at these endpoints. In this region it would be up there, and this would be much thicker beyond this stability boundary region.

So that instability, why they post these, the new kind of instability, as you see in this power flow map, and then what to do then. You know, the staff and industry has developed and reviewed, and under this committee approved that they are generic solution for the BWR Owners" Group solution and to handle this region, and then after review, extended good up to the region here they're called BW owners group long-term stability solutions.

However, in this region there are two measure authority. One of them has been approved. The other one is today's, the under committee review. So you see this is a BW owners group approved solution right there, that neither Document 319608, and give us all kind of solution.

There are three options, E1A, 1B and Option 2 and 3, and these are approved and documented in this document there or the U.S. BWR reactors have implemented one of these solutions depend on their need today.

And now because, like in the previous slides there to handle the MELLLA region, there are two methodologies. It's under review, and one of them, GE DSS detect-suppress solution, density has been reviewed and approved for MELLLA+, and today one of their topical reports on AREVA, they're called EO-III, under review right now.

So what is what they call EO-III and what is difference between EO-III and enhanced Option III, and as you see previously, the owners group provision, they have an Option III. So the difference would be still keep the Option III features and plus some are different from AREVA so that they become enhanced Option III.

So enhanced Option III really is an evolutionary step, rely on existing methodology and hardware for Solution III and what the difference is that EO-III introduced measures for addressing the review of stability associated with extended flow in all conditions and the higher probability of single channel hydraulic instability excitation.

So the enhanced Option III have this kind of features over there. So the new element to use enhancement to existing Option III solutions are such as they introduced, introduction of a calculated exclusion region on the power flow mat designed to preclude single channel instability.

Also, they have a calculation procedure, how to do it. So this is different from the regular Option III.

Yes.

MEMBER CORRADINI: Can you show us on the diagram where the exclusion region is?

DR. HUANG: Okay. That would be in the closed session.

MEMBER CORRADINI: Okay.

CHAIRMAN SHACK: Can you show us on the cartoon though?

DR. HUANG: You want to show on cartoon?

CHAIRMAN SHACK: Yeah. I think that's all he's asking.

MEMBER APOSTOLAKIS: Two slides back, three slides back.

DR. HUANG: This one?

MEMBER APOSTOLAKIS: Yeah.

DR. HUANG: Basically I would say most likely similar with this concept, but the detail be in the process, you know, to show you one the slides.

CHAIRMAN SHACK: I think we just need to deal with this in the closed session.

MEMBER CORRADINI: Okay. Thank you.

DR. HUANG: Okay. So now because you have EO-III and you have to have the way to apply it, EO-III, so they need something they call Option III. If you're aware of the Option III, they have OPRM system using the OPRM input to get the set point. So they need a DIVOM curve.

So the second topical cycle specific DIVOM methodology from AREVA, and this time on curve really is a relationship between the hot bundle relative oxidation magnitude and the limiting fractional change in critical power ratio, and this is really a document in BW owners group solution, Needle 32465 document, and details go in there.

And our review will be a trace, you know, like capability of the RAMONA5-FA system core to model neutron oxidation of the regional mode pipe and also that range of input data defined that set points within the reload cycle for which diamond curve is generated, and to the end they have to summarize what is the result of these calculations and come out with a time on curve. So that would be, you know, the start review coverage area for that time on methodology.

And our conclusion under EO-III, we said EO-III is an acceptable authority to detect and suppress oscillation should they occur, which means a DVC-12 design criteria, design criteria 12. So the EO-III solution features provide protection up to and including the end of MELLLA conditions. The detail will be included in cross-section.

Now, let's go into the conclusion for the time on curve. The time on category called there, this is AREVA mass authority, is consistent with previous approved BWR owners" group mass authority document in Needle 32465 document.

RAMONA5 is an integral part of AREVA time on methodology, and they're using RAMONA5 and the staff review, and RAMONA5 is capable computing power flow and void oxidation with consistent phase lag and of a frequency that presented the unstable oxidations, and they can estimate the loss of critical power radio induced by this oxidation, and also AREVA has commit to support the staff review of RAMONA5-FA for time on calculation, and on top of this because the staff only make these limited reviews for this limiting application for time on calculation, the detailed review will be filed in the future.

And because this, so there is staff in the SER that has revised. They say RAMONA5-FA limitation there. The first was in the MELLLA+ region, if you want to apply this mass authority. One condition is the application of RAMONA5-FA to calculate time on curve under extended flow window operating domain, such as MELLLA+, it restricted true stability solution, having a scram protected exclusion region that substantially reduced the potential severity of power oxidation and why they have relieved that one there. In the cross-section we have a curve which shows that region always protected, you know.

And also, there's a penalty of ten percent must be added to time on slope calculated by RAMONA5-FA for extended flow window operating domains, and this penalty is equivalent to penalty of ten percent added to calculated relative CPR response for even power oxidation magnitude, which means that they put more margin there. You cannot rely on this, you know. The endpoint would be the calculation of whether your final MCPR compared to the stability limit. So there's penalty like equivalent to about close to ten percent because here is, say, from here and ten percent on top of that calculated there, and you put the same oxidation magnitude. You come out with CPR over initial MCPR. So there's a penalty over there and ten percent would be penalized for that.

Then, you know, like a reason for this being important is because today's power operation, you need a higher radio power peaking. Also, your power flow ratio is higher. So that means in the MELLLA+ region you're exposed to this and start really quicker and then the probability is higher. So that's why, you know, start will be review these and fit these.

If they want to get this ten percent penalty out, you have to review this line by line for the core.

MEMBER ABDEL-KHALIK: I'd like to point out that these two conditions were imposed by the staff after the subcommittee meeting on November 14th. So these were two new conditions that were included in the revised safety evaluation report that was issued on November 27th.

MEMBER SIEBER: Let me ask a question.

MEMBER ABDEL-KHALIK: Yes.

MEMBER SIEBER: Has RAMONA5-FA been approved by the staff on its own merit as opposed to in conjunction with this application?

DR. HUANG: Actually staff haven't

approved this. However, in that application portion of the staff review, we review some of this, but not fully review for this RAMONA5-FA.

MEMBER SIEBER: Okay. So RAMONA5-FA just as a computer code has not been staff approved.

DR. HUANG: Yes.

MEMBER SIEBER: And when you use RAMONA5-FA for this application without that blanket approval, what alternate methods did the staff want to assure us and everyone else that RAMONA5 will give reasonably accurate results?

DR. HUANG: Oh, okay. Ask staff.

MEMBER SIEBER: You don't have a code that will do that as far as I know.

DR. HUANG: Yes. Staff really looked at the RAMONA5A, what it can do for this limited use for the time on calculation, is try to learn that where the time on -- RAMONA5A, they can confirm that oxidation, you know, to that extent. Also they can confirm what the loads of CPR are, you know, how they're protected within the range of the uncertainty there. So staff looked at that and see this can perform this limited application up to the MELLLA region, not MELLLA+, yeah.

MEMBER SIEBER: Now, the restrictions that the staff has recently proposed, ten percent and so forth, I take it somewhere along the line you're going to explain how those penalties somehow are related to the use of RAMONA5 and why that penalty is good enough to say that stability can be detected and suppressed.

DR. HUANG: AREVA can support this one. The staff looked at proposed idea. They say five percent, for example, at beginning, and we say, well, this five percent penalty probably not good enough, and then we say, well, twice this five percent -- if we draw that line from that generic time on curve slope, it's about .05 slope.

Now, we say ten percent penalize that one. We see about .5. You know, it's ten percent. If .5 sit up over there, equivalent to about ten percent of energy released and ten percent of CPR margin you lose. That's a lot of penalty. You know, you look at and you compare that initial MCPR versus later CPR to come out with the set limit. It's kind of a big penalty from this operation.

So staff say, well, ten percent should cover these conditions.

MEMBER SIEBER: I presume that some place in the presentation you will elaborate on that.

DR. HUANG: Yes.

MEMBER SIEBER: Because it looks to me like there's some pulling of numbers out of the air and saying we don't think this is good enough, but there is no basis. But this ought to be okay.

DR. HUANG: Yes.

MEMBER SIEBER: And that may be because of my lack of full understanding. On the other hand, that's the way it appears.

DR. HUANG: Yeah, okay. I will think about --

MEMBER ABDEL-KHALIK: This remains as a major concern inasmuch as it appears to be -- you know, the adequacy of this penalty has not been fully justified and/or documented, and hopefully we'll hear some information as to why this gives us adequate assurance that this is okay until the staff completes its review of RAMONA5-FA.

MEMBER SIEBER: Well, the staff doesn't have the analytical tools to do that right now I don't think.

MEMBER ABDEL-KHALIK: You know, we would like to wait and hear what they have to say as to justification for the adequacy for such a penalty.

MEMBER ARMIJO: But in effect, if this goes through, this would be a limited approval of that code with some penalties that we get to determine whether it's justified, which seems to be a little bit backwards. It seems like you're going to approve the entire code and then address its applicability to different problems. We're doing it backwards.

MEMBER SIEBER: The problem you've solved now in the total review has to wait until additional analytical tools are available.

DR. HUANG: Yeah, we're taking into consideration it's ten percent penalty equivalent to MCP and they say .02, .01, some kind of number like that. So we justify why this ten percent is, you know.

MEMBER ABDEL-KHALIK: We'll probably get more information in the closed session. So perhaps what we ought to do is just move on with the presentation.

Thank you, Dr. Huang.

At this time we'd like to move on to the AREVA open part of the presentation before we get to the closed session.

(Pause in proceedings.)

MEMBER ABDEL-KHALIK: Let's proceed with the hard copies until visual aids are returned.

MEMBER ARMIJO: We're working off of this for this session?

MEMBER ABDEL-KHALIK: Yes.

MR. FARAWILA: And I will be giving you the slide numbers.

MEMBER APOSTOLAKIS: Sure. We can manage that.

(Laughter.)

MEMBER APOSTOLAKIS: Well, you know, it's single digits.

DR. FARAWILA: Okay. Chairman, members of the ACRS Committee, my name is Yousef Farawila. I will be presenting an overview of AREVA's Enhanced Option III long-term stability solution and associated DIVOM methodology using RAMONA5-FA.

Slide 3.

Just a quick road map of the presentation. First, I present a quick overview of the original Option III detect and suppress solution and talk about Part 21 report against it and the recovery from the Part 21 both in the short term and in the long term.

And after that in closed session we will present enhanced Option III solution, which depends on excluding single channel hydraulic instability, and we will also mention the codes and methods that support that option, and then welcome your questions.

In page 4, you will see a sketch summarizing the original Option III, which is detect and suppress solutions. So if you look to your left where the core sketch is, you will see a closely spaced LPRM strings and signals coming from them at different elevations.

MEMBER APOSTOLAKIS: LPRM, OPRM? I don't know.

DR. FARAWILA: Oh, OPR, local power range monitors.

MEMBER APOSTOLAKIS: Say it again.

DR. FARAWILA: Local power range monitors, LPRMs. They are closely spaced so that they can detect regional oscillations, not only core-wide, and for the sake of redundancy, you have several of these composite signals, and for each one of them, you collect signals from different LPRM elevations.

When you sum them up, you get a signal that is called OPRM for oscillation power range monitor. That signal can be oscillatory, noisy, and before you process it first, it is filtered to remove high frequency noise, and it's also normalized. And the filtered and normalized signal goes to a period based detection algorithm, the PBDA.

The function of the period based detection algorithm is to examine the periodicity of the signal. If the periodicity is confirmed by having several successive periods within a tolerance range, then you suspect that you have an oscillation and you want to examine the amplitude of that oscillation against a preset set point.

If that is the case, if passed that test as well, that means you will get a trip signal. There is a trip logic that requires more than one trip signal order to actually scram and protect the reactor.

Next page.

MEMBER CORRADINI: Let me make sure I understand. And then not only is it the amplitude, but the number of times it crosses, right? You look for a number, not just --

DR. FARAWILA: Okay. I have two tests here. One is the periodicity, and so you see a number of confirmations. It could be 12. It could be --

MEMBER CORRADINI: That's where the N is.

DR. FARAWILA: Right.

MEMBER CORRADINI: Okay.

DR. FARAWILA: The second one is an amplitude, not periodicity.

MEMBER CORRADINI: Don't worry. You're

fine.

(Laughter.)

DR. FARAWILA: A big effort making this part.

(Laughter.)

MEMBER CORRADINI: Easier to see than the fancy new ones.

DR. FARAWILA: Okay. Next page.

All right. Because the system is designed to suppress the oscillation to protect the CPR safety limit, so inherently there is required a relationship between that oscillation and the CPR response. That relationship is called the DIVOM curve. It is based on time domain code calculation of a regional mode oscillation and the output is closest -- for each oscillation you get the relative oscillation magnitude, and you see the corresponding loss of CPR margin, and you plug these against each other to generate a DIVOM curve.

Originally, in the original Option III, that DIVOM curve is generic. It's calculated once, and it covers all plans and all cycles and all field designs.

Next slide.

We come to mention, as the agenda

requires, the Part 21 report against the Option III solution and how it was resolved. General Electric filed for a Part 21 report in August of 2001, which states that the generic DIVOM curve is not always conservative, and not conservative meaning higher than stated CPR response, which is equivalent to saying that it has higher DIVOM slope.

And that condition occurs at high radial peaking or high power-to-flow ratio, and the change was not always in the smallest steps. It can be sometimes rather high, up to probably doubling the generic value.

The way this issue was resolved in the short term was through the BWR owners group collective efforts and the procedure was revised in order to prescribe cycle specific DIVOM calculations. So with DIVOM being cycle specific, if such higher slopes are present, they would be taken into account.

However, this short solution was not good enough for most severe conditions that could be expected from MELLLA+ and also the accounting for much higher DIVOM slopes would result in low setpoints, which makes the system more susceptible to noise, and you have a probability of spurious scrams. That's a very undesirable thing.

So in the long term, a solution that takes care of this DIVOM problems that's not susceptible to it would be also applicable to MELLLA+. So if we focus on application to MELLLA+, it automatically covers the other operating regimes that are minor in comparison.

AREVA's long-term stability solution is enhanced Option III. I will just give you a quick example of what a well-behaved DIVOM curve is. You see it's fairly linear and very well defined slope. This one we calculated with RAMONA5-FA. We wanted to examine what is that elevated slope DIVOM curve. We'll go in closed session very shortly.

So if you want to look at the other side, the ill behaved ones, you probably want to borrow progressing by focus from your neighbor. Then you will see like on the next page, you will see how a not well defined DIVOM curve could look like, and in the next two or three viewgraphs we are looking at the same exact reactor state. Just we changed the initial perturbation to show you that the calculated DIVOM may not just simply have a sometimes higher slope, but it's not really that well defined.

Can we show the next one? Another one, another one.

Okay. So you could see there that DIVOM -- last time when we were here addressing the subcommittee we presented something similar with a reduced order model. This one we are actually showing the RAMONA calculations. The rest of the presentation should we go to the closed session.

MEMBER ABDEL-KHALIK: Okay. For the reporter, we are now switching to a closed session.

(Whereupon, the foregoing matter went off the record at 11:23 a.m. to reconvene in closed session and went back on the record at 1:15 p.m. in open session.)

CHAIRMAN SHACK: We can come back into session. Our next topic is the State-of-the-Art Reactor Consequence Analysis, the SOARCA project. We met with the staff in a subcommittee meeting and discussed, essentially, their approach to the problem, and some preliminary results that they'd received. And they're now going to update the Full Committee on the process, and their current status.

MR. PRATO: My Division Director will open up. Farouk.

MR. ELTAWILA: Good morning. I have a slight cold. I got it from Bill Shack, that he invited us to Oregon, and just blasted us with the winds over there that we could not survive.

As you know, the State-of-the-Art Consequence Analysis, I'm going to call it SOARCA for brevity from now on, is a voluntary effort, so we have to rely on the involvement of the industry. So I would like to start by thanking Surry and Peach Bottom for their cooperation, and providing us with the information that enabled us to do this analysis.

We met with the ACRS in the summer of 2006, and we met with the Subcommittee last week. And we have completed the baseline calculation for both Surry and Peach Bottom.

We started the SOARCA, because as you are aware, that has been the -- the Sandia Siting Study has been called into a different arena, and people are using it out of context, so we decided to do this analysis to try to update and replace the Sandia Siting Study. And we were motivated by a lot of things, among them, improvement in plant operation and maintenance; all the accomplishment that has been gained, or the insight that has been gained over the past 20 years from severe accident research, and additional regulatory requirement that was either imposed by NRC, or voluntarily implemented by the utility that improved plant operation and performance.

If you look at all the study, you find that there were many driven, particularly the early fatality, mainly driven by scenario that lead into early containment failure. And if you look at our history of dealing with this issue, we have resolved the main two or three issues that deal with early containment failure for pressurized water reactor containment heating. We're concluding that's a very low probability. Alpha mode failure, we concluded that is low probability, and physically impossible. So if you look at it from a phenomenological point of view, we have eliminated all the early containment failure. By that, that by itself, you can conclude that there will be no early fatalities, because there are enough time to allow for the evacuation and implement emergency preparedness. So we are -- although, the analysis, we are still doing the sensitivity analysis right now, we're confident that the result at the end, there will be no early fatalities. And I venture to say that even for latent cancer fatality, the result will be significantly improved over previous analysis.

With that, I would like to ask Bob Prato to start the discussion. Thank you.

MR. PRATO: Good afternoon. I'm Bob

Prato. I'm the Program Manager for SOARCA, and I want to thank you for this opportunity. As Farouk mentioned, the last time we were in front of the Full Committee was more than a year ago, and a lot of work has been accomplished over this past year and a half. And the team is looking forward to your feedback.

As we discussed with the Subcommittee we are basically going to be covering process. But as requested by the Subcommittee, we do have a demonstration, a high-level demonstration of a sequence that we began the SOARCA process with, exercising the process itself.

If you turn to the agenda, we're going to start with a project overview, and one of the slides for the project overview is a full diagram of the process, and we're going to get into great detail for each one of the boxes in that flow diagram. So we're going to cover accident sequence selection, containment system states, mitigative measures, MELCOR, MACCS2, emergency preparedness, and peer review in relatively good detail, hopefully to give the Full Committee an understanding of the SOARCA process, itself. And then we're going to cover a sample sequence, and we're going to update you on the status of reporting latent cancer fatalities.

The SOARCA objective; it's really two-fold. We are developing a State-of-the-Art, more realistic evaluation of progression, radiological release, and off-site consequences for frequency dominated core damage accident sequences. And we are going to provide a more accurate assessment of potential off-site consequences to replace previous consequence analysis, such as NUREG-2239, which is entitled, "The Technical Guide for Citing Criteria Development", which was issued more than 25 years ago, in November of 1982. That cite is more commonly referred to as the Sandia Siting Study.

MEMBER APOSTOLAKIS: You did change the first objective.

MR. PRATO: Yes, sir, as you requested.

MEMBER CORRADINI: Suggested.

MR. PRATO: Suggested.

MEMBER APOSTOLAKIS: Suggested.

MR. PRATO: And is it more accurate?

MEMBER APOSTOLAKIS: I think it's more accurate.

MR. PRATO: Anybody have any questions on that specifically?

CHAIRMAN SHACK: Just you are still moving ahead now with the study on Sequoia as the next step in the process?

MR. PRATO: We are going to be contacting -- we've contacted them previously. They have agreed to volunteer. They went into a refueling outage, so we are going to contact them again next week and set up a schedule, and we plan to start somewhere in the February time frame, interacting with them. Okay?

MR. ELTAWILA: Again, it's a voluntary effort. We don't know if they are going to -- so we are negotiating with different utilities, too.

MR. PRATO: Severe accident -- sir?

MEMBER APOSTOLAKIS: I guess we have discussed this ad nauseam, but why aren't you doing a Level 3 PRA? Is there a short answer for that?

MR. PRATO: There isn't a short answer to it.

CHAIRMAN SHACK: We keep asking it again and again.

MR. PRATO: As you well know, Charlie presented the staff's view on that. Charlie has the most integrated knowledge of all the pieces. Charlie, unfortunately, is not here, but he did make several key points.

MEMBER APOSTOLAKIS: What's his last name?

MR. PRATO: Tinkler.

MEMBER APOSTOLAKIS: Yes, we know him.

MR. PRATO: Okay? The key points, and I'll cover the key points first. We believe that the Level 1 PRA has done an outstanding job at this point of identifying what is important with regards to sequences, both from a CDF perspective, and from a LERF perspective. Second, and one of the underlying premises of the project is that the Level 2 and Level 3 deserve more attention, and more rigorous quantification.

It is also our view that the use of an integrated method, such as MELCOR and MACCS, together with an uncertainty analysis, was a better approach for this application, versus trying to quantify thousands of sequences, and it would help to shed some insights on risk.

The other thing is, is that with MACCS and MELCOR, if there is a problem with the analysis, we can attack the particular model in a more direct manner. And in Charlie's words, "the information is no buried in a sea of numbers for which it is difficult to extract this kind of information."

So why are we using CDS as our screening criteria? Well, from the start, there is a historical emphasis at the NRC on CDS, as well as an abundance of information on CDS. We have our updated benchmark SPAR models as an internal source for CDF information. And, remember, we have a high confidence in the Level 1 PRAs, as well, so --

CHAIRMAN SHACK: Internal events.

MR. PRATO: Excuse me?

CHAIRMAN SHACK: Internal events.

MR. PRATO: For internal events, correct.

CHAIRMAN SHACK: At full power.

MR. PRATO: Yes, sir.

MEMBER APOSTOLAKIS: Too many.

MR. PRATO: In addition, the NRC uses CDF as its criteria for risk-significance in Reg Guide 1.174. This Reg Guide uses a CDF of 10 to the minus 6, and a LERF of 10 to the minus 7. We use the same Reg Guide 1.174 criteria for CDF, and if you believe that the conditional containment failure probability is approximately 0.1, then we meet the criteria for LERF, as well. And, therefore, we captured the risk significance based on that criteria.

The only other question remaining is, are we capturing all the significant contributors to LERF by using CDF, as opposed to using LERF. Again, for PWRs, there really shouldn't be any significant dispute that early conditional containment failure probabilities are less than or equal to .01. As for BWRs, in other studies initial results for station blackout events indicated that vessel failure does not occur for more than eight hours into the event. And the customary definition for early is four hours, so we believe that we're on the right track for BWRs, as well. And although it's site-specific and sequence-specific, we are paying very close attention to the timing of the release, and we are making sure that it is beyond the early criteria.

MEMBER APOSTOLAKIS: Now NUREG-1150 and some other studies that are done by the industry, they did go all the way to Level 3. Have you compared what you have found with the findings of those studies?

MR. PRATO: Not yet, sir.

MEMBER APOSTOLAKIS: But you will do that?

MR. PRATO: I'm not sure if that's our plan right now, but the results are relatively preliminary.

MEMBER APOSTOLAKIS: It would add confidence.

MR. PRATO: Mike Yerokun.

MR. YEROKUN: Let me try to -- a direct response to the question of do you plan to compare your results with NUREG-1150, I mean, yes, whatever comes out from SOARCA, we definitely will see what insights we can derive compared to all the previous studies. Obviously, the Siting Studies, but also NUREG-1150 to see what knowledge we gain from the approach we've used for SOARCA, and what that really means for the risk approach that was used for NUREG-1150.

MEMBER APOSTOLAKIS: Level 3 results.

MR. YEROKUN: I'm sorry?

MEMBER STETKAR: Well, even full scope Level 2.

MR. ELTAWILA: I'm going to jump here and say I don't know what benefit we will gain out of comparing the SOARCA study with NUREG-1150 study. I think we believe that these previous studies are very conservatively done, and did not represent the plants as operated, and design, and improvement that have been to the plants, so we will not be comparing apples with apples. I think that -- I appreciate your question, but I will prefer to do a Level 3 for a plant and compare it to a SOARCA study, but to try to compare the SOARCA with the NUREG-1150, it's not going to be a viable comparison.

MEMBER APOSTOLAKIS: It would be nice to know why there are differences. If you find different

-- if I go to the Peach Bottom evaluation in 1150, they give me -- well, all five plants, actually. They give me fatality curves, latent cancer curves, and so on, and they give me the dominant contributors. I mean, even if it's not part of your objective, wouldn't you be curious to know whether your results are different? And if they are different, why they are different? You may come back and say because we did a better job, but to say I'm not even going to look at it, it's kind of -- doesn't make sense to me.

MR. PRATO: Well, there was one other point Charlie wanted to make, or Charlie made at our last meeting. And he said, "With MELCOR, we do believe that additional large benefit is derived in looking at mitigating measures that has not yet been addressed in PRA, such as SAMGs, and other severe accident mitigation guidelines."

MEMBER APOSTOLAKIS: Well, you can always say those things when you compare. I'm not saying don't say it, but at least, I mean, tell us how the results are different.

MR. PRATO: And I think as you see -- as we go through the sample analysis, you'll see how it becomes obvious how considering the mitigative measures, all the mitigative measures have a significant benefit to the outcome.

MR. CHEOP: This is Mike Cheop. Let me try to address that. I think as part of the peer review process, as we are looking at accident sequences, we do ask ourselves why are we different from, let's say, 1150. And if you're different, what the reasons are. And we will convince ourselves what the differences are. And as we go forth into the Level 2 and Level 3 space, again, we do introduce a lot more, as Bob said, mitigative equipment. And we can't explain a lot of the differences through the different strategies that we're using, and the differences. We may not make a formal comparison, but we do, as part of the peer review, and our internal review process, try to convince ourselves as to what the differences are, and what's causing the differences.

MEMBER CORRADINI: Could I just ask for clarification.

MS. MITCHELL: This is Jocelyn Mitchell from the Office of Research. I just wanted to remind you that the Level 2 part of 1150 was done using the EXOR codes, like the PBSOAR, and the SRSOAR code, where they took a tiny handful of source term code package runs and spread them out into hundreds of sequences, and then subsequently collapsed into 17 for Surry, and I don't know the number for Peach Bottom, of release categories. I think it would be an exercise in futility to try to go back and say here is this integrated MELCOR analysis, and why did it change from expanding, collapsing, and basing on just a few runs.

MEMBER CORRADINI: Just to expand, I guess, what George is saying, make sure I understand the staff's position. So I think my way of saying it in some sense coming up with the same result that George is, if you took, and I'm going to pick Peach Bottom and Surry because they have an interesting historical, you can essentially take that and explain the differences. And I think that's kind of what I get from George is after, is explain the evolution of your insights, both in terms of modeling, in terms of additional measures that have been taken care of, and you can go all the way from WASH-1400 through 1150, through - and I was going to ask something about that, through a current, if they had, or if they do have a Level 3, and really then show what you've done, both in terms of methodology, models, and improvements. And that, I think, would help drive home the improvements that you have with SOARCA. I guess that's the way I view --

MEMBER APOSTOLAKIS: That's part of it.

MEMBER CORRADINI: That's another motivation to do it, as George is suggesting.

MEMBER SIEBER: But how does that benefit the overall science of what it is they're doing here? For example, methods and codes have changed, reliability data has changed, assumptions have changed, scope has changed. And to make the comparison, you're going to list a lot of changes. And it's not going to -- you aren't going to be able to draw a conclusion from it, other than this one is liberal, this one is conservative, not liberal, realistic versus conservative, and methods have changed over the years.

MEMBER APOSTOLAKIS: Well, there are two questions that come to mind. First of all, I don't even know why we're discussing this. Is this such a big effort that the staff is resisting, too many resources? It wouldn't look like that to me. But second, and what you're saying is that the curves that I see in terms of public consequences in NUREG-1150, and other Level 3 PRAs, have been completely invalidated, that this SOARCA thing now says don't believe any of that any more?

MEMBER SIEBER: That's how I took it.

MEMBER BLEY: If that's true, I guess I'd really want to understand why.

MEMBER APOSTOLAKIS: Me too. I really want to understand why. I mean, they have very nice curves there, kind of smooth. They tell you what dominates. It would be nice to say yes, we are consistent with those guides, but we're doing a better job.

CHAIRMAN SHACK: I mean, we do have this bifurcation where we select what we examine by looking at frequency, and then we examine the risk-significance of what's left. Why don't we just look at risk-significance in the first place?

MEMBER APOSTOLAKIS: Right.

MEMBER STETKAR: Well, that has to do with ----- having gone through a few recent, not 25 years old, not 15 to 20 years old, but within the last 10 years, full scope Level 2 risk assessments sponsored by the industry, not the NRC, that have included things like SAMGs, that have concluded that the most important contributors to off-site releases, and I'll stop it there, because these were not Level 3 risk assessments, are Level 1 core damage sequences that are in the noise level for core damage frequency. The most important contributors to off-site releases are sequences that are a small percentage, very small percentage in total of the core damage frequency.

In other words, it probably wouldn't even make the pie chart when you look at contributors to core damage. However, that has been the result of detailed analyses using reasonably refined Level 2 codes, taking credit for existing SAMGs, existing whatever you want to call them, beyond core damage operating procedures. And the concern, I think -- part of George's concern is, is the 10 to the minus 6 screening criterion basically missing most of the things that current studies, current industry-sponsored studies show, indeed, are most important to the issues that, indeed, you're examining, the Level 2, Level 3-type issues. In other words, are you missing those sequences by your screening process?

MEMBER SIEBER: Well, if you divide it up enough, you're going to miss a lot in the aggregate that are going to mean something.

MEMBER APOSTOLAKIS: That is additional evidence from that EPRI report that concluded that you go to very low frequencies in order to --

CHAIRMAN SHACK: Well, even if you look in 1150 and you cut it off at 10 to the minus 6, not a whole lot happens.

MEMBER APOSTOLAKIS: It's an unusual situation in the sense that you have -- say you went to go to some end state, and you have in-between some important stuff, like core damage frequency and release. In traditional PRA when you say dominant contributors to something at the end, you calculate that something, and then you identify the dominant contributors. Here we are using an intermediate state, core damage, to identify what we call dominant contributors, and then we see what their consequences are. It's a little different thing, which is useful by itself, by the way. I'm not saying it's not useful. It's very useful, but the question remains, why not go all the way, and bring into an additional dimension. When you communicate to the public now, when you say there are zero deaths, what does that do to your credibility?

CHAIRMAN SHACK: It's no worse than a bus accident.

MEMBER APOSTOLAKIS: You do have deaths. So what you are doing is fine. It sheds a lot of light into what can happen for these sequences, and so on. But it's a mystery to me why there is such resistance to go all the way. I mean, are we talking about doubling the effort or what?

MR. ELTAWILA: I don't think it's an effort, George. I think it is just if you keep refining the sequence further and further, and go down to a lower frequency event, I think that all what you're generating is number, but they don't mean anything. And because in most of these situations, you know what you can -- how we can deal with these scenarios, so it always will become to an accident management and improvement in evacuation, and improvement in the plant operation. So by just going down in the frequency domain to a very low frequency, yes, you can get an answer, yes, you can get -- that answer might show you that it's risk dominant, but what is the meaning of that?

MEMBER STETKAR: The meaning is that at those lower frequencies, there may be initiating events and consequential failures that also completely disable all of those mitigating systems, and operator actions that you're talking about. The nature of the consequences changes at those very low frequencies.

MR. ELTAWILA: Very low frequency, 10 to minus 9, but are --

MEMBER STETKAR: Or 10 to the minus 7.

MR. ELTAWILA: No, I don't think you -- we looked at 10 to minus 7, and we --

MEMBER STETKAR: In the context of the limited models that you had to deal with, in the context of that, those limited models, but did not look at very clearly external events, seismic events, things like that.

MR. CHEOP: Well, I think that's not quite correct. I mean, in the 10 to the minus 7 range, we did look at the external events, the seismic and the fire events, so in that sense, those are included in our 10 to the minus 7 look. And I guess in addressing Dr. Sieber's earlier comment, we actually have not tried to parse out the sequences to such a point where we can eliminate them from the screening process. We did try to keep groups together, so that we do not parse them out so that they are below the screening criteria, but I guess, to answer the broader question as to why we don't do a risk analysis versus a frequency dominant cutoff, that would be, I would imagine, changing the objective of the study. I mean, the objective of the study is to look at CDF dominant sequences, and that's the way they're going at this point.

MEMBER CORRADINI: I mean, if I could just - if I might. But I understand you've been directed a certain way. And I understand when one is directed, one follows the directions, but I'm asking --

MEMBER APOSTOLAKIS: Did the Commission say this?

MEMBER CORRADINI: Yes.

MEMBER APOSTOLAKIS: The Commission directed you to look at --

MEMBER CORRADINI: Yes. So that I understand. That's why I guess in some sense why I'm just suggesting for you to consider the staff to think of it from the historical perspective, and use the fact of the historical perspective to at least lay the explanation out as to what you're seeing, and the insights you're getting.

MR. ELTAWILA: Yes. If my answer at the beginning sounds like we're not going to do that, I apologize for that.

MEMBER CORRADINI: I sensed that you wanted to.

MR. ELTAWILA: We are going -- but if anybody asked me to try to quantify every single differences, this will be impossible, but we will try to -- what are the plant improvement that led into the lowering of the frequency? What is the phenomenological understanding that help us addressing this issue? That's part of the SOARCA report, so there is no doubt about that.

MEMBER SIEBER: I agree with Dr. Apostolakis, that this has to be -- this comparison needs to be done to a certain extent because this will be an important public document, and a lot of questions are going to be asked. And for sure, they will point out the differences, and if you aren't prepared to answer that in a public forum, then you haven't done the job right.

CHAIRMAN SHACK: I think we'd better move on. We have other things to cover, which will undoubtedly lead to discussion.

(Off the record comments.)

MR. PRATO: We got past this question. I think we can move forward in a reasonable --

MEMBER SIEBER: Do every other slide.

MR. PRATO: Okay. We're on slide, severe accident improvement, slide 4. Severe accident improvements that is, in part, the motivation behind SOARCA project, included improvements such as the 25 years, and literally millions of dollars that have been spent on national and international research that provides a better understanding of severe accidents, and the basis to conclude that some presumed early containment failure modes have been shown to be phenomenologically not feasible, or other severe accidents that are feasible, but that have been demonstrated to be preventable by accident mitigation.

Second item, regulatory improvement that reduced the likelihood of severe accidents, rules such as ATWS, Station Blackout Rule, and the Maintenance Rule, all of these have contributed to improved accident management, improved computer modeling capabilities, such as MACCS and MELCOR. Keep in mind that in 1982, when the 1982 study came out, there was nothing like MELCOR that was used or available at the time. And for MACCS, there was a much more primitive model, and there has been significant improvements, not only in the modeling, but in the computer technology that allows us to use computer modeling.

Enhancements in plant design, such as the TMI initial modifications, and the modifications that continued beyond the post-TMI modifications during the early 80s and late 90s, things that resulted in the installation of additional emergency diesels, for example.

Other plant improvements that have contributed to reducing the likelihood of severe accident include general improvement in plant performance. In the early and late 80s, capacity factors were in the 60s and the 70s, today they're in the 80s and the 90s. Emergency preparedness guidelines are available. They've been developed, and evolved, and tested very frequently. And mitigative measures, as you will see, will play a big role.

This next slide is an overview of the process. We're going to cover each one of the boxes in a lot more detail, but this just shows how the process flows, and how we come to -- how the SOARCA process works, in general.

The SOARCA approach. SOARCA is the only kind of accidents we're considering of full power operation. We are not considering low power, shutdown, or spent fuel pool-type of accidents. We are using a plant-specific sequence truncation of CDF of greater than or equal to 10 to the 6^{th}, and a CDF greater than or equal to 10 to the 7^{th} for bypass events.

MEMBER CORRADINI: Minus you mean, right?

MR. PRATO: Yes, minus. I'm sorry. They're in there. I apologize. We did consider external events. We considered all of the mitigative measures that were available to the licensees. We did and we're doing sensitivity analysis to assess the effectiveness of the different safety measures. A State-of-the Art Accident Progression Modeling based on 25 years of research to provide a best estimate of accident progression, containment performance, time of release, and fission product behavior. We are using a more realistic off-site dispersion model, and we are doing site-specific evaluation of public evacuation based on site-specific updated emergency plans.

MEMBER CORRADINI: Can I repeat something that you guys said in the question and answer earlier, just so I put it in this context for the second bullet? So even though your cutoff, as directed, was that, you went down another order of magnitude and surveyed what you saw at the 10 to the minus 7 cutoff.

MR. PRATO: We're going to get into that.

MEMBER CORRADINI: Okay.

MR. PRATO: We'll show you how the sequence selection was --

MEMBER CORRADINI: The answer is yes.

MR. PRATO: But the --

MEMBER CORRADINI: Okay. That's fine. I'll wait.

MR. PRATO: SOARCA insights. Okay. Sequences are dominated by external events, primarily large seismic events that play out similar to a station blackout. We also identified two additional PWR bypass events that were within the scope of SOARCA.

Previously used sequences have significantly lower probability of occurrence, or are not considered feasible, and that includes the alpha mode, the high pressure melt injection, and ATWS. The first two are considered not feasible, and the ATWS is a much lower CDF than was considered in 1982.

Mitigative measures are proven to be effective at preventing core damage or containment failure.

MEMBER SIEBER: Did you examine the effect of large seismic events on the effectiveness of the emergency planning?

MR. PRATO: No, sir, we have not.

MEMBER SIEBER: Bridges knocked down, roads closed, flooding, whatever.

MR. PRATO: We are doing sensitivity analysis to address that, sir.

Sequence screening process. Okay. It's important to mention at this time that SOARCA was never intended to be a risk study. However, the staff wanted its initial focus for SOARCA to include sequences of greatest interest. Therefore, as the initial input into SOARCA, the staff used the enhanced SPAR model to identify the sequences that are most likely to occur. Using SPAR, we applied a screening criteria for the sequences included within the scope of SOARCA to identify those sequence or sequence groupings that have a CDF of greater than or equal to 1.0 E to the minus 6 to identify those sequences which are most likely to occur.

In addition, we wanted to pay more attention to those sequences that are potentially more severe, but that have a little lower likelihood of occurring. For example, interface system LOCAs that bypass the containment. Therefore, we lowered the screening criteria for inter-system LOCAs to a CDF of greater than or equal to 1.0E to the minus 7.

These are the steps that are used to implement the screening criteria. We started with an initial screening. We used enhanced SPAR model to screen out low CDF sequences with an overall CDF of less than or equal to 1.0E to the minus 7, and sequences with a CDF of less than 1.0E to the minus 8 for bypass events. This step we estimated eliminated less than 10 percent of the overall CDF, approximately 5 percent is what it typically ended up being.

CHAIRMAN SHACK: Typically for two cases.

MR. PRATO: For the two plants, correct.

The sequence evaluation, we identified and evaluated dominant cut sets for the remaining sequences, and we determined systems and equipment availability, unavailability, and accident sequence common to those sequences. We grouped the sequences together that had similar times to core damage, and similar equipment availability. And then we selected bounding sequences based on the most limiting mitigative measures available.

For external events, we performed limited reviews of existing external event studies, and data to identify dominant externally initiated event sequences for each plant of interest. And where available, we specifically identified the dominant accident sequences for those plants using the following steps.

First, we identified dominant externally initiated event sequences for external events, such as fire, seismic, flooding, wind. And based upon available probabilistic assessment documentation, like NUREG-1150, the IPEEE submittals, as well as any additional available supporting documentation. We did not use seismic margins assessment because it lacked the risk information necessary.

We identified potential mapping between dominant external events, and internally initiated events identified by the SPAR analysis. Where mapping between external and internal events are not possible or appropriate, a unique external initiating event or sensitivity study was recommended, and the resulting limit --

CHAIRMAN SHACK: You need to click your slide.

MR. PRATO: Oh, I'm sorry. The resulting limited set of scenarios obtained for each SOARCA plant was used for subsequent accident progression and consequence analysis.

Containment system states. The objective of this process is to identify the availability of engineering systems that can impact post-core damage containment accident progression, containment failure, and radionuclide release using the following steps. We determined the anticipated availability of containment and containment support systems not considered in the Level 1 core damage analysis. We did this by determining the availability of front line systems using cut set information. If all support systems were considered in the Level 1 analysis, availability was determined based on the cut set information alone. If they were not, we constructed a system dependency table showing the support systems for performance of the targeted front line system.

We then determined the availability of the front line system using engineering judgment. For example, if the necessary support systems were determined to be available or unavailable based on engineering judgment, then the availability or unavailability of the front line system was determined.

MEMBER APOSTOLAKIS: This may be a little bit misunderstood. When you say determine the availability, I believe what the study did was assume that the system was working or not. Right?

MR. PRATO: That's correct, sir.

MEMBER APOSTOLAKIS: Depending on what has been lost.

MR. PRATO: That's correct, sir.

MEMBER APOSTOLAKIS: In PRA space, the availability will be a probability.

MR. PRATO: That's correct.

MEMBER APOSTOLAKIS: And you didn't do that.

MR. PRATO: That's correct, because we weren't doing that.

MEMBER APOSTOLAKIS: Okay. So that's another thing that's different from traditional PRAs. The containment systems are either there or they are not, and you have some logical criteria to decide that.

MR. PRATO: That's correct.

MEMBER APOSTOLAKIS: But there is always a possibility of a random failure, or whatever the PRA does to come up with the unavailability number.

MR. PRATO: We did not do an HRA.

MEMBER APOSTOLAKIS: They didn't do that. I mean, that's --

MEMBER SIEBER: That may be more --

MEMBER APOSTOLAKIS: Determine the availability can be interpreted in different ways. Okay. That's just a clarification.

CHAIRMAN SHACK: But, again, their argument is their additional random failures would be lowering the frequency.

MR. PRATO: That's correct.

MEMBER APOSTOLAKIS: Yes. This cutoff is always running our lives here.

MR. PRATO: Okay. In addition --

MEMBER APOSTOLAKIS: Wait a minute now. The cutoff is for core damage frequencies.

CHAIRMAN SHACK: But it's an overall sequence. The cutoff is really on releases, and they -- well, that's the directive of the SECY.

MEMBER APOSTOLAKIS: Wait a minute now.

MR. PRATO: It was release frequency.

CHAIRMAN SHACK: Release frequency.

MR. PRATO: 10 to the minus --

CHAIRMAN SHACK: Right. And the staff took a conservative approach by going to the --

MR. PRATO: Core damage.

CHAIRMAN SHACK: -- core damage, because, again, you're not going to get a release without core damage.

MEMBER APOSTOLAKIS: That's what was done.

CHAIRMAN SHACK: That's what was done.

MR. PRATO: Okay. In addition, the availability of containment systems determine the availability of systems such as the low pressure injection, and that can potentially impact containment accident progression. For example, cooling debris in the reactor cavity, or cooling reactor vessel after the core damage, or prior to vessel failure. Those are the systems we also considered for containment system states, as well.

Mitigative measures analysis. The

mitigative measure analysis that we performed were qualitative sequence-specific system and operational analysis based on licensee identified mitigative measures from EOPs, SAMGs, and other severe accident guidelines that were determined to be applicable to and available during a specific sequence, whose availability, capability, and timing were utilized as inputs into the MELCOR analysis.

CHAIRMAN SHACK: What does it mean by the qualitative part, since you really used these to set the boundary conditions for your MELCOR analysis, as I understand.

MR. PRATO: In other words, we didn't quantify it, and we didn't assign a risk to it. That was the --

CHAIRMAN SHACK: You mean a probability that it would be done.

MR. PRATO: That's correct, sir.

CHAIRMAN SHACK: You assumed if it was in the procedure --

MR. PRATO: Well, we did more than that. We verified that the equipment was available. We verified that there was no reason to believe that it was not accessible. We insured that we took consideration for communications, resources. We did a very extensive --

CHAIRMAN SHACK: Okay. You just didn't assign numbers to those.

MR. PRATO: That's correct. That's correct.

The process that we used to do this, for those dominant sequence or sequence groups within the scope of SOARCA, we determined the potential availability of mitigative measures. We performed a system and operational analysis based on the initial condition, and the anticipated subsequent failures. We determined the anticipated availability, capability, and time to implementation. And we put all of that information into MELCOR to determine the effectiveness of those mitigative measures. We never, as part of the mitigative measures, assessed its effectiveness. We let MELCOR determine that as part of the modeling.

MEMBER BLEY: Let me back you up to that last question, just make sure I'm following. So you, essentially, or as you said earlier, you did no HRA. You, essentially, said if the equipment - this scenario, if the equipment is there that could work, and the procedures would make it work. We'll say it works and put it into the MELCOR analysis. You've essentially assumed people perform perfectly given the equipment's available.

MR. PRATO: In general. However, we assign very conservative times to the implementation. We did try to consider accessibility, availability in not only support systems, but support equipment that was needed. Was it on hand, was it pre-staged? We looked at a lot of the parameters that you would consider in an HRA.

MEMBER BLEY: If there's time to do it, and if the equipment works, it will be used and it will work.

MR. PRATO: That's correct. We looked at availability of capacity. Are the storage tanks available, the bottom of the storage tanks.

MR. DUBE: Don Dube, NRO. I want to just add that for most of these sequences, the MELCOR analysis was done with and without the mitigative measure. Right?

MR. PRATO: That's correct. We are doing sensitivity analysis both with and without --

MR. DUBE: I know probability was assigned, and it failed to --

MEMBER BLEY: But we have both results.

MR. PRATO: Yes, sir.

MEMBER BLEY: Before you got here, I want to understand the statement you made earlier. When you did the basic scenario, did you look at -- you didn't do an HRA there, either. Is that right? Effectively, the same thing, if the equipment is there, you assume the equipment will --

MR. PRATO: CDF you include --

MR. CHEOP: To get to the CDF portion of the analysis, we did HRA as part of the CDF calculation.

MEMBER BLEY: Okay.

MEMBER STETKAR: What there isn't, isn't a conditional dependent HRA for the Level 2, Level 3 mitigative functions. They're -- HRA ends at Level 1, basically.

MR. PRATO: The structural analysis. The objective of the structural analysis was to evaluate the behavior of containment structure under unmitigated severe accident conditions and to predict the following criteria; and that is, functional failure due to pressure, the structural failure due to pressure, and to develop leak rates, and leak areas as a function of internal pressure.

As a result of the structural analysis, the dominant cause for containment failure at Peach

Bottom is the -- we determined to be the head flange bolts strained under gradual increasing internal pressure. And for Surry, it was cracking around the equipment and personnel hatch.

MELCOR analysis. As stated earlier, MELCOR or a similar model wasn't available in 1982. MELCOR, since that time, has been developed, and it has evolved, and we have implemented significant improvements to get it to where it is today, which we believe is the state-of-the-art. More recently, and specific to SOARCA, we improved the MACCS output interface. We implemented fuel collapse model logic. We updated MELCOR defaults, and we added approved model.

For each of the analysis that have been completed, we developed a site-specific model, and we performed the accident progression for each plant using MELCOR computer code to determine source term, potential containment failure states, and time of release as an input into the MACCS analysis.

Similar with MACCS-2, MACCS was around in 1982. It has evolved significantly. For the purposes of SOARCA, we implemented a significant number of improvements to bring it up-to-date, and to make it state-of-the-art. I'll go over just a couple of these. We increased the number of evacuation cohorts from three cohorts, which was previously, up to 20. We have the capability of dividing up into 20 different cohorts. We increased angular resolution from the typical 16 compass points up to 64 segments. We added more plume segments, and we included KI ingestion model, as well. And these are just a couple.

MEMBER BLEY: What do you mean when you say "more plume segments"? Can you describe that?

MEMBER SIEBER: It is finer.

MR. PRATO: Jocelyn, do you want to --

MEMBER BLEY: Are you letting them move as the weather changes?

MR. PRATO: No.

MS. MITCHELL: We usually break it up. In previous analyses, there were usually one puff release, and then a long tail, so you had two plume segments. And now we take this very long, drawn out release, and break it up into typically one-hour releases, and MACCS will pick up for the release of the second plume. If the weather has changed, the wind speed has changed, then it will pick up a new weather sequence, so it's --

MEMBER BLEY: Opens up then.

MS. MITCHELL: Yes, right. Right. But it is a one-hour, typically one-hour releases.

MR. PRATO: We performed the consequence analysis for each plant and each sequence using the MACCS-2 computer code to determine early fatalities and latent cancer effects.

Some of the MACCS-2 assumptions that we used, we assumed that no contaminated food or water would be consumed. We used the latest federal guideline dose conversion factors in Federal Guidelines 12 and 13 for specific isotopes, to specific organs, given different specific pathways.

We assumed KI ingestion by half of the 10-mile population, and we used sub-optimal timing. That sub-optimal timing results in a fraction efficacy and if you do the KI ingestion at just the right time, it's approximately in the mid-090s range. We used 70 for the efficacization.

We used medium values from the U.S. and European study for uncertainty for non-site-specific parameters. This study used expert elicitation for approximately 140 different points important to off-site calculations, and resulted in the distribution of the response for each of those inputs. And we used the mean as the input into MACCS for these things.

And it includes things, such as the dry deposition velocity, the wet deposition velocity, some of the parameters in the food chain model, a set for those type of parameters are included in those 140 different inputs.

MEMBER BLEY: Who were your experts for that?

MR. PRATO: That was a separate study done for not only our modeling applications, but for the Europeans, and the Asians, as well. It was a combination of U.S. and --

MEMBER APOSTOLAKIS: It was years ago, wasn't it?

MR. PRATO: Yes, sir.

MEMBER BLEY: Oh.

MS. MITCHELL: This is Jocelyn Mitchell. The study was done about 10 years ago, and had six different panels that worked on different disciplines that were necessary, and they had usually eight experts on the panel, four from the U.S., and four from the EC countries.

MEMBER BLEY: George just reminded me. This is the one using Roger Cook's approach.

MEMBER APOSTOLAKIS: Yes.

MEMBER BLEY: Where it calibrates the

experts.

MS. MITCHELL: The expert elicitation itself was conducted by the U.S., and the European part of it was to take the discrepant distributions that come out of it, and evaluate them. We have redone that, because we found some problems with how it was done, and so we have re-sampled it, and have a distribution that encompasses the eight expert views, and we take the median, the 50th percentile from that resulting distribution.

MR. PRATO: Okay?

MEMBER BLEY: Just a quick question, because two or three slides earlier you talked about -- you ran -- I thought I saw uncertainties on the parameters, but I'm not so sure I'm hearing that any more. You looked at the uncertainties, and then you picked mean values or medians, and ran them through the --

MS. MITCHELL: The difference is that the MACCS code itself now has a user-friendly front end that enables a relatively easy parameter uncertainty consideration, so you could put in a range of values, and a degree of belief, and it would sample out of that range and degree of belief, and construct multiple MACCS decks, run them sequentially, and evaluate the grand averages when you get finished. That model is available for what -- and we intend to exercise it, but we have not, as yet. We intend to do it, but for a point estimate for the stuff that we have done to-date, we have taken that range of values and degrees of belief, and have taken the 50th percentile, and put it in as our point estimate.

CHAIRMAN SHACK: But you do the meteorological stuff statistically, and take a mean value.

MS. MITCHELL: The meteorological stuff is sampled in a stratified random sampling method. This would be for other things, like the dry deposition velocity, and a whole bunch of other things.

MR. PRATO: For each site, we use site-specific population meteorological data. We use an assumed projected relocation dose, and time for the area beyond the evacuation zone during the seven-day emergency period of 5 rem and one day for relocation, and for 2 rem, two days for relocation. Return criteria at Peach Bottom we use .5 rem, which is EPA-specific, and for Surry we used 4 rem and five years, which is EPA-specific. In general, releases are divided into one-hour plumes, as Jocelyn explained previously.

Emergency preparedness. We modeled the protective response afforded by current site-specific emergency preparedness programs to improve realism. We used site-specific evacuation time, time estimates for evacuation of the EPZ. We used a new program called OREMs, which is Oak Ridge Evacuation Model, to model evacuation of the 10 to 20 mile area.

We modeled cohort data, such as population, evacuation time, travel speeds, and roadway networks. And the data was used in MACCS-2 to develop consequence estimates. Peer review.

MEMBER APOSTOLAKIS: So there was no uncertainties.

MR. PRATO: Excuse me, sir?

MEMBER APOSTOLAKIS: Were there any uncertainties in these evaluations?

MR. PRATO: That's coming up, sir.

MEMBER APOSTOLAKIS: Coming up.

MR. PRATO: Yes, sir.

MEMBER APOSTOLAKIS: Good.

MR. PRATO: Okay.

MEMBER APOSTOLAKIS: The discussion, or the answer?

MR. PRATO: Just a high level discussion. We plan to do an uncertainty analysis, and a peer review in the next calendar year. Okay?

Peer reviews. Internal, we've done a couple of staff peer reviews, including the PRA aspects that's used within SOARCA. We have recently went to ACNW and given them an overview of SOARCA, as well as discussed the dose threshold issue. And we've been here a number of times.

MEMBER APOSTOLAKIS: Who are the national and international experts? Can you give us a few names?

MR. PRATO: We're working on that right now as we speak.

MEMBER APOSTOLAKIS: Oh, you haven't done this?

MR. PRATO: No, we're working on it. We plan to do it in the next calendar year.

MEMBER SIEBER: We are here.

MR. PRATO: For Peach Bottom, Peach Bottom accident sequences. The PRA model --

MEMBER APOSTOLAKIS: But, you see, when you select the experts, I mean, an expert can come in there. He's an expert say on MACCS. He will look at what you've done. He'd probably say it's very good. Would any of these experts dare question your objectives, the same way this committee is doing? I don't know. I doubt it. It's okay. You don't have to answer.

MR. PRATO: I have no answer.

CHAIRMAN SHACK: Many of George's questions you don't have to answer.

MEMBER SIEBER: We are available.

MEMBER POWERS: But all of George's questions should be appropriately considered.

MEMBER APOSTOLAKIS: It's one of the rare occasions where the fact that I can't hear you is good.

(Laughter.)

CHAIRMAN SHACK: That's the nicest thing you said about him, George.

MEMBER CORRADINI: He's not going to repeat it, either.

MEMBER SIEBER: But George didn't hear any of it.

CHAIRMAN SHACK: Onward.

MR. PRATO: The PRA models indicate a core damage frequency is dominated by seismic events, which functionally work this way out as a long-term station blackout. We did consider fire and flooding, as well, but when we looked at the general damage, the general availability of other systems to be able to mitigate it, the seismic event had much more widespread damage, and we determined that the seismic -- we evaluated the seismic event, we would bound the other two.

MEMBER BLEY: From your statement there, it's functionally a long-term station blackout. It's an earthquake big enough to cause that, but not to damage equipment in the plant?

MR. PRATO: The only thing we considered, we assumed that was undamaged was containment and the RCS, initially. Okay? If we had questions about mitigative measures, we did an additional evaluation to determine if the seismic event would result in that, and damage of that equipment.

MEMBER STETKAR: Let me follow-up on it, because I was in the Subcommittee meeting, and this was one -- this follows up on something Jack brought up earlier, and something you brought up earlier. We don't have the details of this sequence, but it was described in the Subcommittee meeting. First of all, this frequency seismic event corresponds to something, I think I remember something in the 1g acceleration rate, so this is a 7 to 8 magnitude on the Richter scale earthquake.

The analysis of this scenario from the HRA perspective takes full credit for operators manually, locally, mechanically controlling RCIC flow at the RCIC turbine. Think of that. And it does not include possible seismic effects on population evacuation, emergency response. Is that correct, both of those?

MR. PRATO: Correct.

MEMBER STETKAR: Okay. Now on the HRA perspective, it seems somewhat optimistic to say that the operator, under these conditions --

MEMBER BLEY: It does not take advantage of what people have -- unusual behavior we've seen in people under very, very large earthquakes. And for these people, that are very, very large earthquakes.

MR. PRATO: Can I caution us not to get into the details of any of the results from this meeting? It's too preliminary.

MEMBER BLEY: Okay. Not results, but do you model people?

MR. PRATO: Do we model people, sir?

MEMBER BLEY: The thing John was pointing out is, if one did an HRA of human performance after this earthquake, one had better understand the psychological impact of such an earthquake on people. It's not something you can look up in THERP, or SPAR-H, or any of the existing HRA methods. That's what he's saying.

MR. PRATO: Understand.

MEMBER CORRADINI: I think that point was made at the Subcommittee.

MEMBER SIEBER: Not only the operators, but also the people who live around the plant. They go --

MEMBER BLEY: They're not living in houses any more.

MEMBER APOSTOLAKIS: The impact of the earthquake on the evacuation itself.

MEMBER SIEBER: Yes, bridges down, holes.

MEMBER APOSTOLAKIS: Not only just people, but also damage to bridges.

MEMBER BLEY: And I think I saw something about half the people taking --

CHAIRMAN SHACK: I think we better move on.

MR. PRATO: Internal events were all less than 10 to the minus 6, and bypass events were very, very low frequency, much less than 10 to the minus 7.

For Surry, the events included a long-term and a short-term station blackout initiated by a large seismic event. And in addition to that, we had an inter-system LOCA, a LOCA on a low pressure injection system, and we had a steam generator tube rupture.

SBO events are due to seismic, flood, and fire initiators, and are modeled as seismic events, again for the same reason, because there was much more widespread damage, and a lot more equipment that was unavailable.

MEMBER CORRADINI: So in a similar fashion as you said in Peach Bottom, the seismic encompasses what might have been a fire or flood event?

MR. PRATO: That's correct. It bounds it. The IS LOCA and steam generator tube rupture are due to random equipment failure, and then by a number of operator errors.

This is a sample of sequences high level, but hopefully it will give you a feel for how this process was implemented. This sample sequence is a loss of a vital AC bus. This sequence was selected and assessed for demonstration purposes only.

MEMBER BLEY: Can I reflect back on your last two view graphs? If I got it right, in general, we're saying we're using, although you're not doing it here, a cutoff of 10 to the minus 6 per year on sequences, and yet our dominant sequence is about 10 to the minus 6 per year. Is that right?

MR. PRATO: That's correct.

MEMBER BLEY: Have I got the story right?

MR. PRATO: Rich, that's correct, isn't it?

MR. SHERRY: Yes.

MEMBER BLEY: That seems troublesome, but go ahead.

MR. SHERRY: I guess I don't understand your question. In what sense were you referring --

MR. PRATO: I understand his point.

MEMBER BLEY: If the biggest thing there is is about one times 10 to the minus 6, and you don't look at anything else, it makes me uncomfortable that there might be other things that would add up to substantially more than that.

MR. SHERRY: In the external events, or internal events, or across the board?

MEMBER BLEY: Any events.

MR. SHERRY: I think I can say fairly confidently for the internal events, that we probably captured the risk dominant sequences for the PWR for Surry. Okay? And I suspect -- well, I really can't make that statement for Peach Bottom.

For external events, it's harder to make that claim because we, essentially, obtained our sequences by looking at older studies. We really didn't use up-to-date -- we didn't have up-to-date seismic PRA or other external event PRA results for these plants, so we essentially looked at past studies, like NUREG-1150, whatever information we could get from the IPEEEs, and made judgments about what the dominant sequence characteristics would be. And, typically, station blackout was a typical frequency dominant sequence for seismic events.

CHAIRMAN SHACK: There's references to SPAR external events models. Just what are they?

MR. SHERRY: For a limited number of plants, there have been a number of SPAR models which have been upgraded to include, to a limited extent, external events. Seismic --

CHAIRMAN SHACK: Does that include Surry and Peach Bottom?

MR. SHERRY: Yes.

CHAIRMAN SHACK: So you do have a seismic PRA of some sort.

MR. SHERRY: Except that these haven't really been validated in any sense. Okay?

MR. PRATO: Okay?

MEMBER BLEY: Go ahead.

MR. PRATO: As I said, sequence was selected and assessed for demonstration purposes as not within the scope of SOARCA, because the CDF is really less than 10 to the minus 6. The MELCOR analysis showed that this event can be mitigated. Okay? So even though the sequence indicated that core damage can be achieved, we were able to mitigate this event.

MEMBER APOSTOLAKIS: I don't understand this. You're showing us an analysis of a sequence that should have been screened out?

MR. PRATO: Correct, sir.

MEMBER APOSTOLAKIS: And the purpose of this is?

MR. PRATO: It's just for demonstration purposes. When we first started up SOARCA in the process, and working with MELCOR, we selected a sequence, and we performed some analysis on it. And we kept it because we felt that it had demonstration value.

MEMBER APOSTOLAKIS: But this is not going to convince us that --

MR. PRATO: It's not intended to try to convince you of anything, sir. Okay?

MEMBER CORRADINI: Well said.

MEMBER STETKAR: Nor could it ever.

(Laughter.)

MEMBER APOSTOLAKIS: Very well put, sir.

MR. YEROKUN: If I may try to fend that off. For the purpose of this discussion, we said earlier we're not prepared to discuss the preliminary results of this, sorry. So we've gone through the process, and all these technical steps. By the way, my name is Jimi Yerokun. But for the benefit of the Full Committee, we thought it would be appropriate to at least give you some demonstration of how this process works through, some hypothetical sequence, and go through the whole analysis. And this is one example where it's not within the scope of the analysis, but it portrays how we step through using some other sequences.

MEMBER APOSTOLAKIS: But you have produced results of this sequence?

MR. PRATO: Yes, sir.

MEMBER APOSTOLAKIS: And you're not going to show them?

MR. PRATO: Well, we show you generally what the outcome is. I'm going to go through that, but we don't have the graphs, and the slides, and everything that went with it. No, sir.

MEMBER APOSTOLAKIS: Okay.

MR. PRATO: Okay?

MEMBER APOSTOLAKIS: All right.

MR. PRATO: The initiator was a loss of Division IV DC power resulting in a scram, MSIV closure, and containment isolation, a RCIC auto-started, and one CRD pump was active and still available. The initial operator actions, the load shed to maximize duration of the DC power, they maximize the flow of the single CRD pump. The CRD pump at this facility has a range of 110 to 180 gallons per minute depending on RCS pressure, but that 110, there is some throttling involved, so they go down and they open it up, and that 110 basically turns into 140 at normal RCS pressure.

They depressurized the RCS in about an hour and a half, which with the CRD and RCIC flow they had to secure the CRD from four to seven hours to prevent reactor pressure vessel over-fill. The capacity of make-up was sufficient to prevent core damage, even though core damage was predicted by the Level 1 PRA.

MEMBER BLEY: I take it this plant's Level 1 PRA did not take advantage of the CRD pumps?

MR. PRATO: Sir, I'm -- that's right on the next slide.

MEMBER BLEY: Oh, sorry.

MR. PRATO: Sufficient injection

capability, there was sufficient injection capability to prevent core damage. The SPAR didn't credit the CRD for coolant makeup. Reactor pressure vessel depressurization, and maximization of CRD flow are important to operator actions to optimize recovery. One other thing that we didn't consider, that wasn't considered was standby liquid control, was also available for high pressure injection at about 50 gallons per minute. And battery duration was determined to be important for RCIC operation and instrumentation.

MEMBER BLEY: Let me interrupt you again. I'm sorry for so many. You thanked the utilities for cooperating. Did they cooperate in the performance of the analysis, or in allowing their plant and their PRA to be used?

MR. PRATO: They were involved in the performance analysis. We did make a site visit. They did review our sequence truncation and verified it, and we had some exchanges on that. We added one or two, and they've taken one away.

MEMBER BLEY: This one had a lot of human actions. Were they involved in the quantification of the human actions?

MR. PRATO: They were involved in the

mitigative measures analysis. They provided a lot of input and insights.

MEMBER BLEY: I mean, on things like whatever you came up with on -- or is that as shedding load and that sort of thing on the DC part of what you're talking about?

MR. PRATO: We had a lot of procedures for this facility, and we, basically, assessed -- yes.

MEMBER BLEY: And I know you said this earlier, I just want to confirm. For the Level 1 PRA, you used the SPAR PRA. Is that right? I mean, you used that as the basis for coming up with your scenario.

MR. PRATO: That's correct.

MEMBER BLEY: Okay. Do those now really have a thorough coupling of all the dependencies among systems in those models? I know not too long ago they did not.

MR. CHEOP: We have the full set of fault trees, and event trees to couple all different dependent failures, and human errors.

MEMBER BLEY: So that is pretty thorough and the utilities will have --

MR. CHEOP: That's correct.

MEMBER BLEY: Okay. Thanks.

MR. PRATO: Okay. And that brings us to the conclusion on the sequence sample. The only other thing left is the report on the status of the latent cancer fatality reporting. The history behind this is one of the key objectives of SOARCA is risk communication, and for the non-NRC staff, risk communication in this context does not refer to communicating PRA information. It's more general. It's a methodology of reporting very technical information, and coming up with a unified understanding of the information, coming up with a single outcome of the information. And, initially, we proposed to do a range of doses, and we felt that a range of doses conflicted with the risk communication aspect of SOARCA, which is one of our more important criteria, so what we decided to do is we decided to take a look at other options for reporting latent cancer fatalities. And we're putting together a Commission paper to inform the Commission, and right now what you see here is we have three options. Very recently, we had other considerations thrown at us. The three options we are considering on this slide is range of threshold, linear no-threshold, or an estimated point value based on a Health Physics paper. We are also looking at other things, like just reporting dose. We are looking at potentially truncating distance. There's a number, a variety of variations, and our overall objective is to come up with one single answer for each sequence at each site, instead of coming up with multiple.

MEMBER SIEBER: These are very conservative. Right?

MR. PRATO: Yes. This process is still ongoing. We believe that it's going to come to conclusion relatively in the near future, but right now, we're still in the final development stage of the Commission paper.

MEMBER SIEBER: I was thinking if these are realistic, I'm in trouble.

MR. PRATO: Excuse me, sir?

MEMBER SIEBER: If these are realistic, I'm in trouble after of SCAT scans.

MR. PRATO: Me too. Me too. That completes the presentation. Any other questions?

MEMBER BLEY: As you left that last one, what kind of a point value are you leaning toward?

MR. PRATO: We are not leaning towards anything. The staff is working out the options, and we will probably evaluate the options. And we may make a recommendation, we may let the Commission decide. The final form of this Commission paper has not been decided yet.

MEMBER BLEY: Have you got a recommendation from the other Committee?

MR. PRATO: The ACNW?

MEMBER BLEY: Yes.

MR. PRATO: The ACNW did not give us a formal recommendation. They asked us to consider dose, and they asked us to consider risk. We are looking at them as potential options.

MEMBER BLEY: Thank you.

CHAIRMAN SHACK: Any more questions?

MR. PRATO: Any questions, anybody? Thank you.

CHAIRMAN SHACK: Thank you very much. Mr. Lymen, I think you wanted to make some remarks.

MR. LYMEN: Yes, if I may.

(Off the record comments.)

MR. LYMEN: Can you hear me? I appreciate, as usual, the opportunity to make a few remarks here. And, actually, when I walked in and heard the opening discussion, I was wondering if it was even necessary, because I think many of the comments I heard from the Committee already reflect a lot of our concerns.

I'd say the bottom line is that my organization, the Union of Concerned Scientists, is very supportive of an authoritative and independent study that would approve the technical credibility and accuracy of analyses of consequences of severe accidents. And two issues, in particular, is one, can protective actions be improved based on better information, and can better siting decisions be made in the event that new reactors are actually located around the country, which is going to become increasingly important. But our view, at this point, is that SOARCA is not on track to fulfill this role, and that's because the political goals from the outset threaten to overwhelm the technical part.

What is the real point of SOARCA, it seems to be knocking down a strawman, which was the 1982 CRAC2 study. And as you already discussed, a lot of the public rationale for SOARCA seems to ignore the fact that NRC staff, and contractors, and licensees have been severe accident analyses for more than 20 years in the interim, including NUREG-1150, source term is based on MELCOR, at least its initial formulation, and are commonly used in regulatory applications. So there is a whole body of work since then, and to say we haven't done anything since 1982 really makes -- exaggerates the importance of this project. It's really an incremental improvement over what's already been done.

And looking at some of the statements that have been made about it, the SOARCA project may show that a large early release may not credibly exist, for example, raises the concern that the real point is to rehabilitate severe accident analysis to eliminate the most risk-significant sequences on the basis that they're low probability.

Now one concern we have is the inappropriate focus on risk communication. That has been part of this project, in the forefront from the beginning. Results will be presented documented risk communication techniques to achieve public understanding, which is a little Orwellian in my view. The fact is, a risk communication plan has already been developed for this project years before the study's results are even going to be available, and that raises suspicions if the PR aspects of this project are predominant. And we say really, "Just the facts, ma'am." If you really want to achieve public understanding, the best way to be clear about all your assumptions and arguments in a step-wise fashion, so that the impacts of the various changes to previous studies, like CRAC2 or NUREG-1150, can be readily observed and understood. And we would strongly advise against bundling everything you're doing to a black box and spitting out a best estimate, and forcing the public to unravel, to the extent we can, exactly what's been going on here. And I think the discussion earlier makes clear that there is a lot going to this recipe, and it would be better if we did understand those changes incrementally so we can see what are the significant differences.

Now with regard to risk communication, going into a little history, there's a reason why CRAC2 made the front page of the "Washington Post" in 1982, was not because it was necessarily so frightening, it was because NRC was originally only planning to release the mean values across the meteorological distribution that is generated by these codes, and someone leaked the files that showed the maximum, or peak consequences for in worst case, where the scenarios were evaluated, and it was that fact, I think, that led to the reason why it got as much press as it did, which should be a lesson, that if you're open about what you're doing, you don't try to parse the results for public perception that you're going to be better off in the long run.

This excessive secrecy marking this project, which is a concern. The original SECY and SRM are still being withheld from the public in their entirety, despite the objection of one of the Commissioners. And many of the meetings discussing SOARCA have been closed, in some cases with, apparently, inappropriate rationales. And one example was the ACNW meeting a few weeks ago where I was closed on the basis that it would be pre-decisional, meaning it would have a severe impact on a regulatory decision, except that SOARCA, by definition, has no regulatory application, and is a project plan, so it looked pretty ridiculous to say that that was pre-decisional with regard to any regulatory decision, and the meeting was opened after that was challenged.

MEMBER APOSTOLAKIS: The SRM is not public?

MR. LYMEN: No. And I asked for it. I sent a letter and was told it's sensitive.

Now with regard to SOARCA, there are good things. And to the extent MELCOR, accident progression and source term development can be updated using reactor-specific data, input decks and experimental insights that have been achieved through Phebus and other severe accident studies, that's a good thing. And understanding, if there is more data to better understand containment performance, and I'm not sure there actually is, but to the extent there is, and that can be fed in, that seems to be a big uncertainty in my review of NUREG-1150, and the way expert elicitation was used to gloss over things that weren't known about containment performance. That would be very beneficial.

And, again, if you can model protective actions better and with more accuracy, and you can come up with better results in what you do, those are good. But one of the bad things is what appears to be a totally inconsistent way of truncating these low CDF sequences, and I think if you're screening out five, or even 10 percent of a CDF, that is not an insignificant chunk. In fact, if you look at what the LERF would be for those plants, you can see that it would be less than 10 percent, usually, so you would be clearly taking a big bite out of a LERF. And it seems like external events are being treated inconsistently, low powering shutdown risks are being treated at all, which is absurd, because it's the shutdown risk that may dominate early releases if the containment is open. And it just seems like the way the truncation is being done, you're introducing round-off error that will round-off and eliminate exactly the things that you should be looking at, and it's absurd, circular reasoning. If you take out the events that would lead to most severe consequences and you find the consequences are less than you thought, that's circular reasoning.

Also, credit for unregulated measures, like SAMGs, seems to be inappropriate. I think that what I heard earlier is that these have been demonstrated, accidents have been demonstrated to be preventable where containment failure or whatever through SAMGs. I don't think SAMGs have ever demonstrated anything. Those are voluntary measures, and they're not regulated, not tested in any verifiable way, and they should not be credited, at least, to the extent they are. If, again, as I heard earlier, both the results with and without SAMGs have been evaluated, and present both of those to the public, and explain why you believe the SAMG one is more credible.

Finally, the "ugly" is the use of dose thresholds in direct contradiction to recommendations of the BIER VII Committee, among others, and I won't belabor this point, but NRC is going to have to do a lot of work to justify why this recommendation should be ignored in the study, if it goes in that direction. And I think that would severely undermine its credibility.

Now what was so bad about CRAC2? Well, I went back and looked at CRAC2, and it seems like it, obviously, isn't applicable today, but the reasons are different from what we've heard. For instance, CRAC2 uses 1970 census data. It actually assumed the entire EPZ would be completely evacuated within six hours after the warning was issued, when current evacuation time estimates are generally much longer than that, so it was actually more conservative than appropriate in some cases. It assumed medical treatment for all victims of acute radiation exposure would be aggressive. It used a BIER III correlation for cancer fatalities, which is out of date, and underestimates by a factor of four, compared to ICRP-60. And it only sampled a handful of weather sequences, because it at that time, it took a long time to run a weather sequence. Today, you can run an entire year's worth of weather times 16 wind directions in a matter of minutes, so there's no reason to use sampling any more. You can use the entire year's worth of weather data, and you get a much larger number of results for your consequence distribution. I found comparing that sampling only 100 weather sequences compared to the full year underestimates peak consequences by about 30 percent.

So we've been using MACCS-2 for a long time, and find, in particular for Indian Point, the CRAC2 was -- actually gives fairly good results compared to what you can do with NUREG-1465. And we used NUREG-1465 as a starting point for source terms, because it is recommended, has been vetted, expert panel reviewed it for applicability to high burn-up fuel only a few years ago, and basically confirmed it was appropriate. It's being used for design-basis applications by a number of licensees, but it's not being used for any severe accident applications, even though it does actually cover severe accidents. And the question is why? That's generally because if you use it for design-basis applications, it gives the licensees a benefit, while actually make things worse if you look at the full severe accident term, so just showing the source term that I used based on NUREG-1465 truncated after about two hours, so there's a tail that we need to consider, comparing that to, let's say, what Entergy used derived from MAAP for Indian Point in their license renewal application, a source term which has a lot of odd things about it, 22.9 hour duration, and release fractions which are generally lower in most categories than for NUREG-1465. And the consequences are dramatic in comparison, and so the results within 50 miles, the mean consequences for Indian Point using the 2034 population density are 860 early fatalities, mean latent cancer is 38,500, the peak early fatalities 70,800, peak latent cancer is almost 700,000. So since NUREG-1465 corresponds to a low pressure event like large break LOCA, you can see why staff from these results may not want to consider large break LOCAs, if they can avoid it.

So my conclusions are if the main impact of SOARCA is to reduce severe accident consequences by eliminating consideration of large early releases, that's circular reasoning. The more reasonable approach would be to group sequences, not to truncate at the CDF level, to truncate at the Level 2 so that you have the frequencies of the various release classes, and then do your screen at that point, which was an approach that was rejected from the beginning. The inclusion of thresholds without authoritative technical justification is going to undermine the credibility of the results.

And finally, to get an apples-to-apples

comparison, the way to do that is when changes are made, that people understand the impact of each particular change, and not simply the entire package handed to you without a clear understanding of what went into it. So without that, we can't understand what's due to better data, improvement of the codes, better technical understanding, what's due to simply changing the ground rules by what accidents or sequence are and aren't considered. So my recommendation, I'm glad to hear there's going to be an external peer review, that is one of my recommendations, and I believe the best way to -- for the credibility of the study to be insured is to publish the results in a peer review journal. So with that, I thank you.

CHAIRMAN SHACK: Any questions for Mr. Lymen?

MEMBER STETKAR: Can we get a copy of the slides?

MR. LYMEN: Absolutely.

MEMBER POWERS: Ed, let me ask a question. The Health Physics Society seems - and I'm being cautious in what I say - try to reproduce what they've said, but, in effect, they've said look, you get doses below about 100 millirem per year, don't try to do anything with it, because it's difficult to calculate down there, it's hard to measure things and whatnot. So for the purposes of assessing risk, cut it off at 100 millirem for something like that. I mean, what's your take on all that?

MR. LYMEN: Well, my take is that if you're going to make a radiation protection recommendation, that it has to be based really on the most authoritative sources. And the Health Physics statement is not really supported by peer review references, as was BIER VII. BIER VII had the opportunity to consider, and they did at length, the literature supporting the notion for thresholds, and like you said, it wasn't. But the other thing to consider is that well, if you're talking about uncertainties and low dose, that actually does coincide with the uncertainties in doing atmospheric modeling beyond a certain radius, so to avoid having to say you're using a dose threshold, a better approach would simply be to say that we don't think the MACCS-2, the validity of the code is really -- is that useful beyond 50 miles radius, correspondence between lower doses and longer distances, so that would be a way to dance around what the actual --

MEMBER POWERS: Yes, but that's another

take. By the time you get out there, but I was just interested in the view on the Health Physics.

The other thing they emphasize in BIER VII in their view, is there is a wide perception that somehow linear no-threshold comes from people not knowing how you draw a straight line, they know it's based on a biochemical model. And you really can't contest it unless you have a contesting biochemical model. And you struggle heroically to come up with a biochemical model that for low LET radiation could yield a threshold, because it involves double breaks to the DNA strands, which are not easily repaired. And the analogy drawn to chemical effects is inappropriate, because chemical effects are all single breaks to DNA strands. And so, yes, BIER VII is a fairly authoritative, but now you get down to the practicality of doing calculations. And like you say, I mean, it seems to me the argument to make, one argument certainly you could appeal to Health Physics, but the other one is, there's a point where MACCS just breaks down as a useful computation --

MR. LYMEN: Right. And that may not coincide with the dose threshold you're talking about. That's the only thing. But right, I mean the conclusion of BIER VII was that a single DNA lesion can lead to cancer, and on that basis, there is no model that would justify --

MEMBER ARMIJO: Well, that kind of flies in the face of a lot of research at the cell level that directly refutes that assumption, that one single lesion instantly leads, or without doubt will lead to cancer. And I've been trying to read up on this work by --

MEMBER POWERS: BIER VII didn't say that.

MEMBER ARMIJO: Well, that's what I heard from the --

MEMBER POWERS: It's a probabilistic argument.

MEMBER ARMIJO: Well, I think the -- I guess I'm more impressed by experimental work. I've been reading some of the work of Dr. Mitchell of AECL and others, and I think there's just a preponderance of information that tells us that a threshold does exist. And I think it's -- to say we mustn't think about it, we mustn't talk about it, because that's perhaps politically incorrect, just bothers me.

MR. LYMEN: No, that's not what I'm saying. Think about it, talk about it, but support it with argument. And in the context of radiation protection, it really has to be weight of the evidence, and if you don't -- to those who believe the data reads that radiation protection standards and models should be changed, there simply isn't enough of a coherent body of evidence to support that change. And BIER VII, had the opportunity to say that, and they rejected it, so at this point, it does not make sense to include a threshold in these models, and it is going to undermine the credibility of the result, a three-year study and all the effort that's going into all the various aspects of it should not be undermined on that point.

CHAIRMAN SHACK: Further questions or comments?

MEMBER POWERS: It's also fair to say that BIER VII did a comprehensive examination of the biochemical evidence. And, interestingly, they came back saying that there's pretty good evidence that there's no threshold in the case of neutron damage. And it's only the LET, Low Energy Transfer, radiation where this uncertainty exists.

MR. LYMEN: Right, because high LET radiation, high LET particle can cause multiply damaged sites.

CHAIRMAN SHACK: Okay. Well, thank you very much.

MR. LYMEN: Thank you. I appreciate it.

CHAIRMAN SHACK: We're going to take a break now. Thank the staff again for their presentation. Good discussion. Take a break until 3:15. We're off the record.

(Whereupon, the proceedings went off the record at 3:01:19 p.m.)