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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS (ACRS)

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RELIABILITY AND PRA SUBCOMMITTEE

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MEETING

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TUESDAY,

NOVEMBER 27, 2007

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ROCKVILLE, MARYLAND

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The meeting was convened at the Nuclear Regulatory Commission, Two White Flint North, Room T-2B3, 11545 Rockville Pike, at 9:00 a.m., George E. Apostolakis, Chairman, presiding.

SUBCOMMITTEE MEMBERS PRESENT:

GEORGE E. APOSTOLAKIS	Chairman
DENNIS C. BLEY	Member
MARIO V. BONACA	Member
OTTO L. MAYNARD	Member

WILLIAM J. SHACK Member

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P-R-O-C-E-E-D-I-N-G-S

(9:04 a.m.)

CHAIRMAN APOSTOLAKIS: The meeting will now come to order.

This is a meeting of the Reliability and PRA Subcommittee. ACRS Members in attendance are Mario Bonaca, Otto Maynard, and Dennis Bley. Girija Shukla of the ACRS Staff is the Designated Federal Official for this meeting.

The purpose of this meeting is to discuss the NUREG-1829 on estimating LOCA frequencies through the elicitation process, and a NUREG report on seismic considerations for the transition break size. We will hear presentations from the NRC staff.

The Subcommittee will gather information, analyze relevant issues and facts, and formulate proposed positions and actions, as appropriate, for deliberation by the full Committee. The rules for participation in today's meeting have been announced as part of the notice of this meeting, previously published in the Federal Register. We have received no written comments or requests for time to make oral statements from members of the public regarding today's meeting.

A transcript of the meeting is being kept and will be made available as stated in the Federal Register Notice. Therefore, we request that participants in this meeting use the microphones located throughout the meeting room when addressing the Subcommittee. The participants should first identify themselves, and speak with sufficient clarity and volume, so that they may be readily heard.

We were just joined by Dr. Shack, and we will now proceed with the meeting. I call upon Mr. Richard Dudley of the Nuclear Reactor Regulation staff to begin.

MR. DUDLEY: Good morning. I'm Dick Dudley. I'm the Rulemaking Project Manager for the 50.46a rule to risk-inform the large break LOCA ECCS requirements.

The 50.46a rule specifically is not part of today's presentation. What you're here to hear about today are two studies, though, that were done in support of that rule and are very important parts of that rule, so we thought it would be appropriate to give you a summary status of where the rule stands as of today.

The last communication that the staff had with the Committee on 50.46a was the ACRS' November 16th letter to us in which you recommended that we not issue the final rule in the form that it was in, and you recommended numerous and significant changes be made to that draft final rule.

Because of the significance of those recommendations, as we reviewed them we saw that they would require significant time and resources to address those recommendations, so we requested Commission guidance before we proceeded in that area. Specifically, also, because a number of the recommendations we received were different from Commission guidance that we had previously received on how to do this rule.

So we wrote SECY-07-082, which went to the Commission on May 16, 2007, to get -- to make sure the Commission was aware of the significance of the ACRS concerns and to reaffirm or get new Commission guidance for how we should proceed with this rulemaking.

The Commission responded to our SECY paper with an SRM in August of 2007, and basically the SRM did three things. First, the Commission agreed with the staff that the priority of the rule should be reduced. They had agreed that it was not a high priority rule. The staff had recommended a medium priority rule, and the Commission agreed with that.

The Commission also --

CHAIRMAN APOSTOLAKIS: I don't understand what that means. What does it mean? Does it mean that we have a smaller number of people working on it? Is that the meaning of it?

MR. DUDLEY: Well, we have a rulemaking prioritization system. We have a lot of rules sitting waiting for resources to be applied, and so we use this prioritization system to determine how we apply resources to rulemaking and other activities. And by when -- I guess we agreed with the ACRS recommendation So we thought that that reduced the priority of the rule from a high priority rule.

CHAIRMAN APOSTOLAKIS: So that means fewer people are working on it?

MR. DUDLEY: It means that people would be assigned at different times, later times. They might be working on other stuff. The Commission -- and, in fact, we haven't made a huge amount of progress on the rule itself in fiscal 2008. The Commission, in their SECY paper, made it clear that they did not want this rule to languish. They agreed that it was medium priority, but they told us we had to make progress on the rule in fiscal 2008.

They gave us some specific guidance on the relative priority between this rule and a couple of other rules we're also working on. And they told us that we needed to provide them with a schedule for the rulemaking on how we're going to finish this rule by March 31, 2008. So that's what we're working on.

And this rule -- these two issues that you will hear about today are some of the technical issues that we have to resolve before we issue the final rule. And depending on how these issues --

MEMBER SHACK: But you had selected a break size already.

MR. DUDLEY: I'm sorry?

MEMBER SHACK: What issues do you have to resolve today?

MR. DUDLEY: Well, the Commission's SRM also, you know, it addressed the priority of the rule. It also agreed with the ACRS's recommendation that we should increase defense-in-depth provided by the draft final rule. The Commission, however, did not specify to the staff how we should increase defense in depth.

So increasing defense in depth is a very large part of what we have still to do on the rule, along with closing these technical -- these issues with some technical uncertainty, which would be the seismic report and the expert elicitation.

So we have a number of things we still have to do, and right now we are trying to address these two particular issues. And once we get that under control, we'll -- we will put together a final schedule and we'll proceed with this rulemaking in accordance with that schedule.

MEMBER SHACK: Just coming back to this priority question, how does this stack up against the PTS rule?

MR. DUDLEY: Well, the Commission specifically said that this -- that the PTS -- let me just see here. I think they said that the PTS rule was -- let me just see.

MR. COLLINS: I have that, Dick. I have the SRM right in front of me. My name is Tim Collins from the NRR staff. The SRM says that the 50.46a and the 50.46b rulemakings should be given a higher priority than the pressurized thermal shock rulemaking, and that the LOOP LOCA rulemaking priority should be lower than the one for the pressurized thermal shock. So 50.46a and b are higher than both the pressurized thermal shock and the LOOP LOCA.

MR. DUDLEY: And part of the issue is that we also have limited rulemaking resources also, and we were also expecting the 50.46b rule to come to us about the same time. And so we were trying to make sure that we had staff available to work on that rule as it went into the rulemaking process also, because we knew that that was a very significant rule, and we wanted to make sure we could not delay it by not being able to apply rulemaking resources.

CHAIRMAN APOSTOLAKIS: I'm sorry. What were the three rules that you mentioned? I --

MR. COLLINS: The three rules -- 50.46a, 50.46.b. 50.46b is the cladding -- changes to the cladding criteria.

CHAIRMAN APOSTOLAKIS: And a? A is --

MR. COLLINS: A is this one. A is this one.

CHAIRMAN APOSTOLAKIS: This is it.

MR. COLLINS: Right. Be is the cladding

criteria.

CHAIRMAN APOSTOLAKIS: Okay.

MR. COLLINS: Okay? And then, the other two were the pressurized thermal shock, right, and the last one was the LOOP LOCAL rulemaking.

CHAIRMAN APOSTOLAKIS: Loss of offsite power.

MR. COLLINS: Loss of offsite power, right.

MR. DUDLEY: Simultaneous.

CHAIRMAN APOSTOLAKIS: So this has a higher priority then the PTS rule.

MR. COLLINS: Yes, that's correct.

MR. DUDLEY: Yes. And, again, I'm just providing a general overview of where the rule stands today. Are there any further questions on what I've given you so far?

(No response.)

Okay.

MR. COLLINS: Dick, could I just make a clarification of something that you said? This is Tim Collins again. We have to provide a schedule to the Commission by March 31st, not a revised rule to the Commission by March 31st. Okay?

CHAIRMAN APOSTOLAKIS: A schedule --

MR. COLLINS: A schedule to the Commission for completing this rulemaking. The schedule has to be to the Commission by March 31st, not a schedule to complete the rule by March 31st. Okay?

MR. DUDLEY: Thank you. I --

CHAIRMAN APOSTOLAKIS: Is the Committee going to look at that schedule, or it's none of our business?

MR. DUDLEY: We hadn't intended to come to you with that.

CHAIRMAN APOSTOLAKIS: You had not.

MR. DUDLEY: We had not intended to do that.

CHAIRMAN APOSTOLAKIS: But if we ask you to, it would be nice to show up, right?

MR. DUDLEY: We'll certainly figure out a way to work that in there.

CHAIRMAN APOSTOLAKIS: Okay.

MR. COLLINS: Since we'll be blamed for it.

(Laughter.)

MR. DUDLEY: Well, I don't know.

Okay. Next, Rob Tregoning and Lee

Abramson are going to talk about the --

CHAIRMAN APOSTOLAKIS: I understand there was a differing opinion on the ACRS recommendations. Has that been resolved?

MR. DUDLEY: It was. In the SECY paper, if you look at SECY-07-082, Gary Holahan's differing view was addressed in that paper. It was appended to the back. It was made available to the Commission, and the Commission, when it made its decision on 07-082, factored in that differing view.

CHAIRMAN APOSTOLAKIS: Okay. Thank you.

MR. DUDLEY: Okay? Thank you.

MR. TREGONING: Thanks, Dick.

Okay. I'm Rob Tregoning, and this is Lee Abramson. And we're here to present information supporting the developing of NUREG-1829. The subject is the development of passive system LOCA frequencies to support the risk-informed revision of 10 CFR 50.46.

I need to apologize for all these slides up front. I've got the wrong Subcommittee label on them, so please forgive me for that. So I'll correct those before we enter them into the final record.

CHAIRMAN APOSTOLAKIS: You have quite a lot of history here.

MR. TREGONING: A lot of history.

CHAIRMAN APOSTOLAKIS: Can you just go over it quickly?

MR. TREGONING: Yes. We can -- this first -- there's two --

CHAIRMAN APOSTOLAKIS: Go through the panel selection as quickly as you can.

MR. TREGONING: Okay. There are two presentations here, and let me go through the objectives at least with you. The first presentation, the idea behind that was to outline the LOCA elicitation that's chronicled in draft 1829 and used as part of the tech basis.

Now, we certainly recognize that we've presented this information to this -- to the ACRS numerous times. I think I counted about 12 times we've been in front of the ACRS on this subject from 2001 to 2005. Even -- we were here with our plans for conducting this exercise through the completion of the draft NUREG.

The only reason for providing this overview is the last time we were here was 2005, and there are several new members since then. So we at least thought it would be appropriate to provide some overview for those new members, realizing that Professor Apostolakis and Dr. Shack had heard this information many, many times. So we can go as quickly as you'd like through that.

The second talk, which is probably going to be of much more interest, is the new information, and that's really to discuss the activities on the NUREG since the last time we were here. And that primarily consists with the public comments that we received during the public comment period and the responses that we've put together to address those public comments.

We have also done additional quality assurance analysis, so a quick update on the results of that. And then, we've made some -- some changes to the NUREG, largely as a result of the public comments that we got. So the second talk will really be the more interesting one. That's the new information.

So you said you want to skip through as quickly as possible?

CHAIRMAN APOSTOLAKIS: Let's keep it just -- you know, just as quickly as you can.

MR. TREGONING: Okay. Let me go through the executive summary, and then we'll try to skip through the panel selection, if that's okay. So these are the main messages up front, and I like to give them up front, so you can see how they're supported as we go through the presentation.

But just to give you an indication of how this was done, we used a formal elicitation process to develop estimates for generic BWR and PWR passive system LOCA frequencies associated with material degradation and aging. We used things -- if you read the report, we developed these piping and non-piping base cases.

What they were, they were -- they were essentially scenarios or conditions that were analyzed and used to anchor subsequent elicitation responses. They're not the responses themselves, but they were important to help the panelists come up with their final estimates. We'll talk a little bit about those as we move forward.

The elicitation panelists themselves, they provided us quantitative estimates, but they supported those estimates by qualitative rationale. And the report itself summarizes both the estimates and the rationale used to support those.

The thing that you see is there was

generally good agreement among the panel members on the qualitative LOCA-contributing factors. The interesting thing comes when you ask people to quantify what that rationale means, and when we saw the quantification from the panelists, of course, we weren't surprised by this, but you do see at that point large individual uncertainty and panel variability in quantitative estimates.

So by large individual uncertainty, I mean by that the confidence that any individual panelists had in their best estimate responses. And by panel variability I mean differences among the panel members.

So, and then one of the principal things that we did in the analysis, we developed individual estimates for each individual panelist, but then we aggregated those estimates to develop a set of group results. And, of course, this is probably the most interesting and one of the most controversial things that we've done here, and I know that we're going to have a lot of discussion about this today.

But we looked at several different aggregation schemes. The one that -- the one that is -- I'll call the principal scheme is geometric mean aggregation, and we do believe that that aggregation scheme is consistent with the elicitation objectives. And the results that you get from that aggregation are generally comparable with NUREG/CR-5750 estimates.

NUREG/CR-5750 was the last comprehensive look on initiating event frequencies, and they did a small evaluation of LOCA-initiating event frequencies as part of that study. However, the results are very sensitive to the way that you aggregate group opinion, and we -- we investigated in the NUREG several alternative aggregation schemes, and these alternative schemes can lead to quite different estimates, and typically they're higher LOCA frequency estimates.

And so we thought it was important to provide in NUREG-1829 the sensitivity of the results to these different schemes. And when NRR -- we're not going to talk about this per se today, but when NRR has taken this information and used it to select the transition break size, they factored in all of this variability that you could get through aggregation, so that they appropriately selected a TBS that they thought was reasonably conservative.

MEMBER BLEY: Excuse me. Rob?

MR. TREGONING: Yes.

MEMBER BLEY: Can you tell me what you mean by that the geometric mean aggregated results are consistent with the elicitation objectives?

MR. TREGONING: They are consistent with the objectives in the sense that they give you estimates that are about the middle of group opinion, sort of the median of where the group falls. The geometric mean is a better -- a better estimate of the group median than other aggregation schemes.

And the median -- when we set up the elicitation, one of the objectives was to provide best estimate LOCA frequencies, and we thought the best estimate frequencies were best represented by sort of the median of the group opinion. And that's consistent with a lot of elicitation practice.

CHAIRMAN APOSTOLAKIS: What you just said is really a tautology. You said the geometric mean is closer to the median estimate. I mean, the geometric mean --

MR. TREGONING: In this study.

CHAIRMAN APOSTOLAKIS: -- is the median.

MR. TREGONING: In this study.

CHAIRMAN APOSTOLAKIS: So in that sense, yes, it better be consistent. I don't know. It's okay. It's one of the schemes.

MR. TREGONING: Yes.

MEMBER SHACK: Well, the way I look at it is that you were actually looking in a sense for a consensus of the technical opinion which is best represented by the median.

MR. TREGONING: We don't call it a consensus --

MEMBER SHACK: You don't call it a consensus.

MR. TREGONING: -- for very good reason, because we didn't ask -- we didn't --

MEMBER SHACK: Right.

MR. TREGONING: The goal was never to develop a consensus, but you're right, it has the effect of being a consensus.

MR. ABRAMSON: I should add that we were very cognizant of the fact that we're getting this through an expert elicitation, and there is a lot of work and experience people have had with elicitations, expert or otherwise, and the empirical evidence is that something in the middle of the group is the best kind of way to get closer to the truth of whatever it is you're trying to get at than something outside of it. That's the essential rationale I think for the aggregation.

If you're going to use, say, elicitation techniques, the evidence is -- the empirical evidence is you should do something in the middle of the group rather than an extreme, more away from the center of the group. So I'd say that's the main rationale, in my mind, as to why you want to go to the middle of the group. It's because you're dealing with an elicitation.

CHAIRMAN APOSTOLAKIS: Which is inconsistent with what NUREG-1150 did, though. NUREG-1150 worked with the arithmetic mean.

MR. ABRAMSON: Yes. Well, we --

CHAIRMAN APOSTOLAKIS: I think it's a matter of aesthetics. People look at this number of points, and they say, you know, something in the middle is probably better than something on the extreme. But NRR took care of it, right?

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: Okay. So it's --

MR. TREGONING: NRR, for their application, took care of it.

MEMBER BLEY: I'm sorry. Since I'm new to the Subcommittee --

CHAIRMAN APOSTOLAKIS: Go ahead.

MEMBER BLEY: -- what does that mean, George?

(Laughter.)

CHAIRMAN APOSTOLAKIS: They added conservative margins beyond whatever, the most conservative estimate.

MEMBER BLEY: I wanted to ask you one more question about one of your bullets.

MR. TREGONING: Sure.

MEMBER BLEY: You had generally good agreement -- oops. That isn't what I wanted to ask. Large individual uncertainty and panel variability, when you say that, are you talking about in their best estimate values? Or once they've added their uncertainty, were they still widely variable?

MR. TREGONING: These are two components of -- two components -- you know, a component of uncertainty and a component of panel variability. We asked for best estimate results, but we also asked for essentially the bounds of that, so we asked for -- essentially for all of the different answers that we asked them in the elicitation, we said, "For this one answer, give us your best guess," which we interpreted to be like the 50th percentile.

And then, we asked -- we didn't ask for upper and lower bounds, but we -- we essentially asked for a high and low estimate, which we interpreted as being the 5th and the 95th percentile of that estimate. So when I say large, individual uncertainty, I mean quite a bit of spread between the 5th and the 95th percentile estimates for any single panel estimate.

And then, when I talk about group variability, I'm specifically referring to the differences between panelists A and B, let's say.

MEMBER BLEY: On their middle value or on their whole distribution?

MR. TREGONING: On their whole -- well, either. I mean, they tend to be --

MEMBER BLEY: Both.

MR. TREGONING: Yes.

MEMBER BLEY: Okay.

MR. TREGONING: I'll use it maybe synonymously, but quite often I'll be talking about their median estimates. But it's equally applicable to their whole distribution method.

MEMBER SHACK: There are three points.

MR. TREGONING: There's three points of the distribution, right.

MR. ABRAMSON: And just to clarify, we were very explicit about telling the panelists. We didn't use the term "best estimate." We didn't say we were getting a best estimate.

MR. TREGONING: Right.

MR. ABRAMSON: We told them, "Think about your subjective distribution with the numbers we're asking you to." There's the mid-value, which is like the median, and then there's a high value, upper -- a high value and a low value. The high value is like the 95th percentile, the 5th.

So we gave them those numbers, but obviously it was up to each one to decide how they -- to try to extract from what it is that they knew about this or guessed or felt about this, something in this range. So we were very explicit about this. We didn't make a big point about it, but we needed -- we felt we gave them some guidance as to what to do, and we did, you know, some training exercises, too, along these lines.

MR. TREGONING: Okay. And the last bullet is, again -- this is certainly the author's opinion, and hopefully it will be ACRS's opinion, but we do believe that 1829 provides at least a sufficient technical basis to support risk-informing 10 CFR 50.46, which is the ECCS rule.

Again, when we're back in front of you to talk about the rule again, this wasn't the only information that was used to develop that rule, but it was one piece. And I think --

CHAIRMAN APOSTOLAKIS: Rob, maybe it's worthwhile here to say a few words about what the experts left out for the benefit of the new members. The experts did not consider everything.

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: Unless you have a special -- oh, you have a special slide?

MR. TREGONING: Yes, I do.

CHAIRMAN APOSTOLAKIS: Okay. Let's go there. I think it's important.

MEMBER BLEY: So they systematically excluded some things.

CHAIRMAN APOSTOLAKIS: Yes.

MR. TREGONING: Some things we excluded. Again, we tailored the elicitation to look at -- and let me go quickly back to this one, because the motivation was not just to support 10 CFR 50.46, but we also wanted to develop LOCA frequency distributions that could be used in plant PRA modeling.

So we wanted to be consistent with how those LOCA frequency distributions were developed and what sequences they have been modeling historically. So we didn't look at every single thing that could cause a LOCA. So if I go to the scope and objectives, it's really defined here.

Again, the main thing we were focusing on was piping and non-piping passive system LOCA frequencies. So we weren't looking at active system LOCAs that you could get from stuck open valves, IS LOCAs, things like that. We were looking for these things as a function of leak rate. Of course, leak rate is -- and I know there's some -- flow rate is probably more accurate, because flow rate really means a function of the LOCA size, and operating time up to the end of the license extension period.

We were focusing on LOCAs, which of course initiate in the unisolable portions of the RCS. And the LOCAs were principally related to passive component aging, looking at the effects of tempering by mitigation measures.

We relied quite heavily on the operating experience. So while we considered plant transients, we didn't consider extreme plant transients that you would get from a very rate seismic event, let's say, 10^{-5} to 10^{-6} frequency of a current seismic event. What you're going to hear this afternoon talks about those additional risks associated with that type of an event.

We didn't consider the very rare water hammer. You know, water hammers, frequencies of, you know, 10^{-2} or -- I'll say 10^{-3} or less. We looked at the more typical water hammers that you would get in BWR/PWR plants.

And, really, that scope was a function of the fact that we were relying on operating experience, the amount of pipe failures that we had historically. So we wanted to make sure when we were evaluating that information that we had it in the proper context, realizing that that information had been developed based on the same sort of transients and operating history.

And that's why the LOCA frequency distributions themselves you see in this -- in this middle bullet really developed for typical plant operating cycles and histories. And a major assumption in the elicitation was that there would not be any significant changes in future plant operating profiles that would have a profound effect on passive system aging or failure.

So there was an assumption that what we've done historically, and how the plants have been operated, will essentially continue in the future up until the plants are, you know, decommissioned or the end of the license extension period was as far as we went there.

Skip through this, George.

Just let me briefly touch on the approach. I mean, this is -- I don't want to spend a lot of time on this. This sort of runs through the recipe of how we did this.

CHAIRMAN APOSTOLAKIS: I'm sure most people are familiar. Rob, people are familiar with this.

MR. TREGONING: People are familiar? Okay.

CHAIRMAN APOSTOLAKIS: Otto? Yes, let's skip it.

MR. TREGONING: Skip it? Okay. Let's talk about the panel selection itself. This obviously, when you look at any elicitation, is one of the most important aspects of the elicitation itself. So we spent a long time just developing the panelists. We developed criteria of technical specialties that we wanted in the panel initially.

Then, we sought recommendations from a variety of sources -- industry academia, national laboratories, contracting contractors, other government agencies, and international agencies. We solicited from a lot of people, and we were looking for people to represent a wide range of organizations as well as a relevant range of technical specialties.

We were looking for people that had probabilistic fracture mechanics, piping design, piping fabrication, operating experience, materials, expertise in degradation mechanisms, at least knowledge of thermal hydraulics and typical operating transients, mitigation practices and procedures, stress analysis, non-destructive evaluation. Those are just some of the technical specialties we were looking to represent on the panel.

You see I've listed the panelists there.

We had 12 panelists, eight of which -- we asked them to self-select, even though we developed BWR and PWR estimates. We didn't want people to provide estimates if they didn't feel like they had expertise.

One person on the panel actually gave us no quantitative estimates at all, so we had 11 that gave us some answer, and I think of those 11 eight of them supplied estimates for BWRs and nine for PWRs. So we had a fairly large sample of estimates to draw from.

Now, the ones that are bolded here in this list, they are ones that made up our base case team. So these are the people that provided quantitative estimates of these special base cases that we're going to talk about here shortly. And they were chosen as well. Two of them conducted their analysis primarily through evaluating service history records and experience and developing estimates based on that. The other two were probabilistic fracture mechanics experts, so they developed their estimates based primarily on modeling.

The other important aspect to panel selection is we had the experts themselves, of course, but we also had a facilitation team that was put together to help guide the process and the experts themselves.

And the facilitation team was comprised of both normative -- or people like Lee who are the experts in the elicitation process and the analysis of results, and then the substantive experts, the people like myself and others who knew something about the subject that could help guide the experts and help develop questions and support the extraction of testimony from those experts.

The facilitation team -- the other thing that the facilitation team was used for is we wanted to make sure that we minimized both motivational and cognitive biases. We were -- the substantive experts, if we got an answer from an expert, we usually just didn't leave it at that. We tried to probe more deeply to find out why they were giving us this answer. So I think it was important to get that feedback, so that they made sure that their answers had at least some basis that they could defend.

And the other thing that the facilitation panel was used for is we wanted to ensure that the results at least were comparable, so that expert A was answering the same question as expert B. It's important when you try to combine group opinion that people are answering the same question. And when you see our base case analysis later, that becomes -- that becomes very obvious.

MEMBER MAYNARD: Did you do any review of the results to see if there were any biases based on background or any trends there? I mean, you have industry, you have labs. Was there any -- going back, any information to see if there was --

MR. TREGONING: You know, it's interesting. I always get asked that question. I think people have some deep-seated skepticism that one group is going to be substantially different than another.

Surprisingly, no, there was -- this is really no apparent correlation between organization and where their results fell. What was interesting, though, we did see -- if we saw any correlation in anything, it was in their uncertainty. And some groups tended to be much more certain about their estimates than others, so that was the only correlation that was really even remotely apparent.

MEMBER BLEY: Can I ask you a question about that?

MR. TREGONING: Sure.

MEMBER BLEY: I didn't sit through your training, so I'm not sure exactly how you carried it out. But did your training include that aspect that lets the people understand where there are -- and your training was with these kind of things everything knows a little bit about, but not everything about -- where their answers fell and thinking about how they should account for their uncertainty, for their high and low ends, to account for the fact that they're missing the true answer on things. Do you think it did that well?

MR. ABRAMSON: Yes. Well, we did emphasize in the training the fact that people are very often under -- this has been shown time and time again -- underestimate their degree of uncertainty. And we do this with so-called almanac-type questions where, you know, we know the answers, obscure facts or something like that.

MEMBER BLEY: Did you have enough time to let them experiment --

MR. ABRAMSON: Yes.

MEMBER BLEY: -- at trying to get --

MR. ABRAMSON: Yes.

MEMBER BLEY: -- their answers to fall into all four --

MR. ABRAMSON: Yes, we did.

MEMBER BLEY: -- quartiles, or that sort of thing?

MR. ABRAMSON: Yes. Yes, exactly. Actually, with the training exercise we asked them four questions, which we presented to them. They happened to do with health statistics about men over 65, so we felt that there was one woman on the panel. We felt that most of them could identify with this cohort, okay, and they came up with the answers and we analyzed them and asked for them their confidence intervals, and so on and so forth.

MEMBER BLEY: You had them all together for this.

MR. ABRAMSON: And demonstrated that -- once again that there was a nominal -- 90 percent confidence interval was in fact more like 50 percent. In other words, so only about half their confidence -- their 90 percent confidence intervals covered the value. So the idea was, again, to show them that people are overconfident in their results, and the idea is to try to get them to mentally loosen up and to -- and to be less sure than they think they are, so we did emphasize this in the training.

And, of course, the purpose of the training exercise as well, since everybody -- I think most people would be understandably very skeptical about this whole procedure, the elicitation procedure itself, was to demonstrate to them that, yes, there is some value in it in the sense that you can use it when you group the answers to come closer to what the correct answer is. So I hope that this would -- that this would help them accept and buy into this procedure.

MEMBER BLEY: I have one more question. I don't know which of you should take this. It's probably one you've heard a lot. I've read kind of quickly, so I may have missed things, but it -- I like the way I think you began, which was to send the information to everyone, have them do their own analysis, probe them as you did.

I think what you did after that was feed back the information to them from each other and let them revise their estimates. You said you didn't try to get to consensus. The thing I guess I don't like -- and I wonder if you've thought -- how much you've thought about it -- I'm sure you've thought about it -- you had this broad mix of expertise.

And it seems to me the real way to take advantage of that broad range of expertise is to get them all back in one room after they've done their initial estimates and really trade information and probe each other. And that may have brought them toward a real consensus.

Did you think about doing that? Did you do that? Or did you not have --

MR. TREGONING: Oh, yes.

MEMBER BLEY: You did do that?

MR. TREGONING: Yes. We had -- we had what we called a wrap-up meeting. It wasn't truly a wrap-up meeting, but it was more of a results meeting where we came in -- we had completed all of the individual elicitations, right? We had all of the estimates, preliminary analysis done, and we had a two or two and a half day meeting where we sat them in a room and we presented all the estimates to all of them, and, you know, we sort of -- we coded, you know. We gave -- it was anonymous where people fell, but obviously people knew which results were theirs.

And with each one we probed and we looked at, you know, in some cases you had maybe one panelist that was quite a bit different. And then, you know, when you get into those situations everyone wants to know, well, what was your thinking? What was your rationale? And we had a lot of discussions about what the rationale was behind people's -- you know, where people fell on these distributions and what was their justification for that. So --

MEMBER BLEY: Did that process bring them closer to a consensus, or did you not try to --

MR. TREGONING: Well, what we did after that is we had discussions, and we said, you know, anyone is free at this point, if you want to go back and revise any of your estimates that you've given to us based on anything that you've heard today, feel free to do that.

We had some corrections, but by and large people -- people were comfortable with the answers that they gave us, and I think the fact that they were either on the extreme or not, they felt okay with that. So we gave the panel the option of going back and modifying their responses. Some did, but it was relatively limited.

MR. ABRAMSON: I'd like to just -- I think it's very important to distinguish between the kinds of responses we got. We of course got what Rob has been talking about mainly I think of as the rationales, as the qualitative responses. And there we were very open and everything, and in a sense there was a kind of perhaps consensus, which is reflected in our -- you know, we report it.

But I think what you're referring to, or what is certainly part of it, is the quantitative answers. And for that I would -- I would -- my position is, my feeling is that nobody is an expert in this. These people were chosen for their expertise in all of the various disciplines that Rob has done that way, and certainly they are truly expert in that.

But nobody is an expert -- it is impossible -- on the quantities, and the reason is obvious because this goes far, far beyond theory, modeling, experience, and so on and so forth. But we're asking them to make their judgments. And everything, by the way, was relative. We asked them to -- relative to the base case, and so on and so forth. So we tried to -- we tried to frame the questions in a way to -- to make it -- to draw as closely as we could on their actual expertise in the scientific area.

But as far as the quantitative answers were concerned, our position was or our starting point was nobody is an expert on this. That's why we're using the expert elicitation process. And from that perspective, it doesn't really make any sense to try to get a group consensus. What we did is we did a mathematical aggregation as we described, and so on and so forth.

But as far as a group consensus is concerned, I think it's very different from trying to get a consensus of something like this than, say, a consensus on the rationale for things, which is possible. We didn't specifically do that, but I think some developed actually with the open discussion we had.

MR. TREGONING: Yes. As I mentioned earlier, we had pretty good agreement. I don't want to say a consensus, but we did have agreement on the qualitative rationale and issues that arise with LOCA frequency estimates.

MR. ABRAMSON: And that's reported on in the report. You know, we talk about all the rationales.

MR. TREGONING: But like Lee said, the difficulty, then, becomes attaching a number.

CHAIRMAN APOSTOLAKIS: But did the experts, though, see the slides that you are going to show us soon with the uncertainties, the geometric, the mean, and did they see --

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: -- those things?

MR. TREGONING: Actually, they saw much more detailed information where we --

CHAIRMAN APOSTOLAKIS: Okay. So they knew that these kinds of pictures will go to NRC management.

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: Okay.

MR. TREGONING: Now, we showed breakdowns for every question with, you know, box and whisker plots for each individual panelist, and you could see them on like a histogram for where people fell.

MR. ABRAMSON: For all the panelists.

MR. TREGONING: Oh, yes. So we had a lot of detail that we presented in this wrap-up meeting on every question that we had. So believe me, they knew where they fell, and they knew --

CHAIRMAN APOSTOLAKIS: I believe you, Rob. I believe you.

MR. ABRAMSON: You've got a small --

MEMBER SHACK: Just on your mix of disciplines, I mean, I count seven or eight fracture mechanic structural guys, only one materials person. And since degradation here is one of the big things, you might have, you know, had one or two more.

MR. TREGONING: Well, I would argue a lot of the --

MEMBER SHACK: A lot of the fracture --

MR. TREGONING: A lot of the fracture mechanics people had expertise in a variety of areas, including, you know, the degradation mechanisms associated with the things that they are trying to model. So while I would agree that there's only one, maybe two, you know, "material scientists" I'm still -- I'm pretty comfortable in the makeup of the panel in terms of the people that we got.

CHAIRMAN APOSTOLAKIS: But it --

MEMBER BLEY: I'm sorry.

CHAIRMAN APOSTOLAKIS: Go ahead. Go ahead.

MEMBER BLEY: When you had a guy's -- I'm still going to call it a distribution, you've got three points -- but did you do anything like break it up into quartiles or something and feed back to him the implications of what that distribution was to see if he was comfortable with the implications that came out of the distribution?

MR. TREGONING: Yes.

MEMBER BLEY: Because most of these people aren't the kind who are comfortable --

MR. TREGONING: Right.

MEMBER BLEY: -- playing with these day in and day out.

MR. TREGONING: Well, again, we broke -- we broke the -- we broke what we were looking for, these bottom-line frequencies, into a number of individual questions. I think there were, you know, roughly 100, 200 individual questions. And you add all of these things up essentially to get the bottom line estimates.

When we got -- for each individual, when the analysis was done, we fed that analysis back to them and said, "Look, here's what your testimony, here's what your -- here's what your results, here's the bottom line, right? And this is what this means, not only in terms of the bottom line, but you said, for instance, that this type of LOCA was more -- was more likely than this type of LOCA. Do you mean that?"

You know, this maybe isn't supported by your qualitative rationale. And we were looking for inconsistencies like that, and there was actually -- that part of the feedback loop, there was quite a bit of modification that the panelists did, you know, supporting that. So we initially did feedback individually, and then we brought the group together.

And I think most of the panelists felt like they had done enough iteration initially on their individual responses that they thought they were supportive of -- generally of their qualitative rationale, and I think that's why we didn't get many more modifications later once we brought the group together. So we did feedback in two different loops, both individually and then as a group.

MEMBER BLEY: Okay. Thanks.

MR. TREGONING: Any other questions?

CHAIRMAN APOSTOLAKIS: Oh, there are many questions, but keep going.

(Laughter.)

MR. TREGONING: Let me briefly move through this slide, just to put some context on what we did. We looked at six different LOCA categories, and we categorized these based on flow rate thresholds. Categories 1, 2, and 3 are fairly consistent with what people consider to be small break, medium break, large break LOCAs.

We added three other sizes, because we essentially wanted to go up and probe and evaluate frequencies associated with larger pipe breaks. In LOCA Category 6, you're essentially pretty close to a double-ended guillotine break of the largest pipe in a PWR plant.

CHAIRMAN APOSTOLAKIS: Which one is that, Rob?

MR. TREGONING: LOCA Category 6.

CHAIRMAN APOSTOLAKIS: 6.

MR. TREGONING: So that's -- LOCA Category 6, at least for PWRs, is close to the existing design basis. For BWRs, it's closer to Category 5 existing design basis.

And we looked at three different time periods. We looked at the current day, essentially, what the LOCA frequencies are at this point in time or the point in time that we conducted the elicitation two years ago. We looked at the end of the design life, which is 15 years hence, and then we looked at the end-of-life expansion. So we asked for information for three different time periods.

CHAIRMAN APOSTOLAKIS: And there was a question, I remember, about what the effective break area was, right, which is the double-ended you have provided?

MEMBER BLEY: I'm a little curious here. Did you present the sizes in terms of the flow rate to them, or in terms of hole size in the pipe?

MR. TREGONING: Well, we developed -- as a group we developed these categories, and the category definitions were based on flow rate. But then, we developed correlations to relate the flow rate to break sizes, realizing that, again, most of the panel, their expertise was in thinking about failure sizes as a function of size.

So, no, we related these flow rates to -- to effective hole sizes in the various different systems. And we had three different correlations. We had correlations for PWR primary systems, and then we had a BWR liquid and a BWR steam correlation.

MEMBER BLEY: So three different correlations that they used, and they had those correlations when they did their elicitation.

MR. TREGONING: Oh, yes. We essentially -- I don't show it here, but we had -- we essentially had a table that said, you know, for this flow rate, you know, this is the effective break size in these systems. And that was primarily the information that they used. Then, when we consolidate and bring everything back together again, we show it in terms of flow rate again usually.

CHAIRMAN APOSTOLAKIS: So the important point here is that the experts were involved in just about every step of the way.

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: Understanding the table you have there, what it means in terms of break size, and so on. So it was not just at the very end that you showed them results, and you said, "Give us now quantitative" --

MR. ABRAMSON: And they were instrumental --

CHAIRMAN APOSTOLAKIS: -- which is the way to do it.

MR. ABRAMSON: And they were instrumental in defining the six categories and what the break points were, and so on. Very much so.

CHAIRMAN APOSTOLAKIS: Good. Good.

MR. TREGONING: I should briefly show this, just for clarification on scope again. General issue classification -- again, you can think of LOCAs, you have passive system and active system LOCAs. I realize or I stated earlier that the elicitation only evaluated passive system LOCAs. The idea that the active system LOCAs are pretty well handled by service history, and those rates are -- have been stable, at least relatively stable, over time.

We broke the problem down into various important variable categories, and I just wanted to list what those categories are here. You know, we looked at effects of geometry, loading history, maintenance and mitigation, materials, and aging mechanisms. And we developed for each of these categories a whole host -- essentially through brainstorm, we developed all of the appropriate variables that would fall within each of these boxes.

So for geometry we looked at all of the primary systems and identified the system names, what types of pipes, what the pipe materials are, what the sizes are, what aging mechanisms could be active for those materials. Okay. It doesn't mean they are active, but which ones are plausible. We looked at at least qualitatively describing the type of loading history -- is it primarily primary loading, what's the transient history like, and then we talked about maintenance and mitigation practices.

So a lot of the issue development that we did initially was focused on brainstorming, so that we had a complete set of information and variables that these guys could go back and evaluate.

And the elicitation itself I'll just briefly mention. We actually had two sets of questions as we had -- some of the people were very comfortable -- in fact, the way they thought was more of a bottoms-up approach as I call it, so they -- they wanted to give you the frequency associated with this degradation mechanism in this system due to these transients, where you have other people sort of the service history oriented people, which were more comfortable in looking at failure experiences for systems as a whole and thinking about what that meant in terms of frequency.

So we structured the elicitation so that they could -- they could answer questions in a variety of different ways, because we wanted to give flexibility to the experts. We didn't want them to have to bend their thinking to the questions. We wanted the questions to reflect their expertise.

So let's talk a little bit now about these base cases, because they ended up being an important -- important conditions that were used to anchor the subsequent elicitation responses. And what are these base cases? Well, as I mentioned here, we defined five of them for piping systems. And if I go back to this other slide, you see -- on the lower left-hand corner you see the variable categories that were identified as being important to determining what the LOCA frequency or the LOCA susceptibility of any given system was.

So what this base case did is they specified for each of these variables a unique set of conditions. Okay? So we defined, for instance, for the BWR base case, which we -- BWR-1, which was on the recirculation system, we defined a system that we were going to evaluate, mitigation practices, the active degradation mechanism that we were going to look at, and sort of typical loading histories.

So each of these various base cases were very definitively defined, and we tried to pick a range of different degradation mechanisms and a range of important systems, so that we could get some -- so that we could sort of cover the watershed of many applicable mechanisms and systems.

So for BWRs we had one base case that dealt with the recirculation system and one that dealt with the feedwater system. In the PWR we looked at the hot leg and the surge line, and then we wanted to make sure that we evaluated smaller line, and we -- we picked the high pressure injection makeup line, because that was a line that had had some -- had some problems in the past.

The base cases -- again, they were defined by the group themselves. The group, through brainstorming and collaboration, picked the base cases that they wanted to evaluate. And then, the base case team, the bolded people that I showed earlier, these folks, they were charged with actually -- they were given extra homework than all of the other elicitation panelists, because they were asked to independently provide estimates for the frequencies associated with failure for those base cases.

And the way we did that is we had several group meetings where we tried to define, with just the base case group, where we defined conditions and what was going to be analyzed in as much detail as they needed, and then we sent them off and had them do their analysis independently.

And then, we had another meeting with not just the base case team members but with the entire elicitation panel, and all we did at that one meeting primarily was to present these results and discuss the differences that we got, and what were some of the reasons behind these differences, and which of these differences were significant, which were an artifact of the way the analysis was done. So we had a separate meeting just discussing the results that this base case team developed.

And I mentioned earlier that four panel members were on that base case team, and two of them provided estimates primarily based solely on operating experience, and two used probabilistic fracture mechanics.

I love showing this, because this always engenders a lot of discussion, because it's a very interesting slide. But what it shows here is these individual points represent for each of these base cases the initial estimates that we got from all of the base case team members, so every point that you see here is an estimate from one member.

The dashed lines are just --

MEMBER BLEY: I'm sorry. Say that one again.

MR. TREGONING: Let's look at the plot on the -- and I apologize, I know these are a bit busy, but I've tried to summarize everything in a couple of plots. So let's look at the BWR base case plot.

MEMBER BLEY: Yes.

MR. TREGONING: There's the red plots and the blue plots -- points. The red points are all for the BWR-1 base case, so this was IGSCC cracking in the circ system. And each of those individual points for any -- at any one LOCA category -- remember, each of those LOCA categories represents a different size break. So LOCA Category 1 represents a very small break, where the higher LOCA categories represent the biggest breaks.

And so each of those points for a given LOCA category represents the different estimates that we got from each of the base case team members. And those dotted lines or those dashed lines, all that is merely there to do is to provide some visual evidence as to what the spread is. Okay?

So you see there -- I said we had five base cases, so you can see the two BWR base cases on the left-hand side, and then the three PWR base cases on the right-hand side.

MEMBER BLEY: And these are the results of one of your team members?

MR. TREGONING: These are all four.

MEMBER BLEY: All four.

MR. TREGONING: Now, not all -- not all four always answered every question, so sometimes you'll only see three.

MEMBER MAYNARD: So the two ends are the highest and lowest of the four?

MR. TREGONING: Yes. Two ends are the highest and lowest.

MEMBER MAYNARD: Okay. But do they just provide one number, or did they provide their high, low, and best estimate?

MR. TREGONING: For this, they provided what we treat as their best estimate. Their best guess.

MEMBER MAYNARD: Okay.

MR. TREGONING: One number.

MEMBER MAYNARD: So the range we see here is the range between the four people, not their high and lows of --

MR. TREGONING: That's my point. And if I wasn't clear on that, yes, that's -- that's correct. Thanks.

MEMBER BLEY: And why is there -- I guess certainly number five is curious to me. But why is there no number six for the BWR?

MR. TREGONING: Oh, just because in the piping --

MEMBER BLEY: Oh, it is -- it's a particular pipe, that's right.

MR. TREGONING: Yes. The piping couldn't support a LOCA Category 6 in --

MEMBER BLEY: Okay. And this was, okay, the recirc and feedwater. Okay.

MR. TREGONING: Yes. If you look at BWR-6, it's a 500,000 gpm break. It's a pretty big break, and there was no BWR piping that could support that.

MEMBER BLEY: Okay.

MR. TREGONING: So, you know, as you see this plot, there's a couple of things that obviously strike you. The first one is that there is a lot of variability amongst the various members -- in some cases, even if you look at this one, you know, you've got on the order of, you know, I think about 10 orders of variability. So just a huge difference of opinion, so --

MEMBER BLEY: Except for the biggest break.

MR. TREGONING: So, well, one of the guys did -- well, this guy stopped. He didn't give us five, so that's why -- so this is -- this is --

MEMBER BLEY: Oh, okay.

MR. TREGONING: -- because this guy only went up to four.

MEMBER BLEY: Oh, okay.

CHAIRMAN APOSTOLAKIS: That's a good point.

MR. TREGONING: So this is --

MEMBER BLEY: On the top we've got two guys, and on the bottom we've got two guys.

MR. TREGONING: -- this is a little bit

misleading, yes.

MEMBER BLEY: Okay.

CHAIRMAN APOSTOLAKIS: How can this guy -- it must be probabilistic fracture mechanics. I mean --

MR. TREGONING: Must be.

CHAIRMAN APOSTOLAKIS: -- 10 to the minus --

(Laughter.)

MR. TREGONING: In their right mind.

CHAIRMAN APOSTOLAKIS: 10^{-17}, I mean --

MEMBER BLEY: He's saying it won't happen to me, right?

MEMBER MAYNARD: It just won't happen.

CHAIRMAN APOSTOLAKIS: Probably thinks that, yes. That's an incredible number.

MR. TREGONING: It is an incredible number.

MEMBER BLEY: He's probably the guy who didn't give you an estimate on the five?

MEMBER SHACK: He didn't give us the 10^{-35}. These guys are pikers.

(Laughter.)

MR. TREGONING: So when we started probing this, of course we had a lot of interesting discussions on it.

CHAIRMAN APOSTOLAKIS: Were you -- did you ask for --

MR. TREGONING: What's that?

CHAIRMAN APOSTOLAKIS: This number there, I mean, 10 -- between 10^{-16} and 10^{-18} --

MEMBER BLEY: Well, for six he's got an even bigger one.

CHAIRMAN APOSTOLAKIS: Did he give you any -- or she give you any explanation, I mean, how --

MR. TREGONING: Sure. Oh, sure.

CHAIRMAN APOSTOLAKIS: So that number is the result of a calculation?

MR. TREGONING: That number is the result of a calculation. And the only thing you can really interpret from that number is for the conditions that were analyzed, and the model that was used, failure at that LOCA size is just highly improbable.

CHAIRMAN APOSTOLAKIS: I would say so, yes.

(Laughter.)

MR. TREGONING: Well, you get a number out of a model --

(Laughter.)

-- people are smart enough not to attach, you know, quantitative significance to that number.

MEMBER BLEY: Is the same guy always the low guy on that?

MR. TREGONING: For this particular evaluation, yes, he was always the low guy.

MEMBER BONACA: And the top was -- they were the same, the same guy?

MEMBER BLEY: No, those are two different guys.

MR. TREGONING: Yes, these --

MEMBER BONACA: No. I mean a different guy but the same four estimates.

MR. TREGONING: Normally, what you saw was the service history guys were grouped closer together, and the PFM guys were grouped relatively closer together.

CHAIRMAN APOSTOLAKIS: And lower.

MR. TREGONING: Not always, but more times than not, yes. More times than not, lower.

MEMBER BONACA: No, I was asking about the BWR case where you have estimates for different categories of LOCAs. Always is a value of about 10^{-2}.

MR. TREGONING: So very high ones.

MEMBER BONACA: Very high one, yes.

MR. TREGONING: Well, that's actually -- this actually was a PFM estimate here. So that was one case where the PFM was not lower. But essentially what this person was saying, that the likelihood of a small break was pretty much the same as the likelihood of a big break in that system for that base case.

MEMBER BONACA: Yes. That's why I was asking that.

MEMBER BLEY: And the two guys who are close together, the blue and reds, are the systems guys, the operating experience guys.

MR. TREGONING: They were much more close -- they were -- they were closer together than the PFM guys, and there is good reason for that. And when we look for that, the service history guys, the conditions that they evaluated, and their approaches, were much more similar than the PFM guys. Okay?

Even though we defined the base cases very definitively, right --

MEMBER BLEY: I'm just curious, because I want to drop back to that other thing. I'm not 100 percent in agreement with Lee's position on -- that you can't do anything quantitatively for consensus.

But I would think up at the high probability end on some of these the operating experience guys ought to have something somewhere where they've seen a break of some sort. I'm just curious. Was that true? And if they traded information in the real world that actually made a break, did the guys doing the calculation say, "I don't care, I'm still doing my calculation"?

MR. TREGONING: We did. In fact, one of the things that we did -- there have been some small breaks --

MEMBER BLEY: Yes.

MR. TREGONING: -- that you could characterize as small break LOCAs, which would take us to the cusp of this.

MEMBER BLEY: Yes.

MR. TREGONING: There's been a lot of leaks or relatively -- I don't want to say a lot, there has been a relatively higher number of leaks. But anything beyond here it's extrapolation. Anything. And we actually did -- and we document it in the report -- we did the initial evaluations, and then we came together and we said, "Okay, we want to try to calibrate some of the PFMs," or we looked at one of the PFM models, "We want to calibrate based on service experience."

So we actually did some calibration where the PFM leak rate was matched up to the leak rate for those -- for that system and those conditions based on service experience. And then, the estimates for extrapolating beyond those leak rates were given. And even when we calibrate it in that way -- at the low end -- it was still a tremendous range in what happened later on.

But the -- you talked about what we did for training. This was another thing that we did in training, because we presented all of this prior to the elicitations. And one of the reasons for that was to -- was to show people, hey, we've got four people, told them to give us our best guess. This is the variability that you get. So this was another illustrative example about the dangers of trying to, you know, overestimate your confidence in your elicitation estimates, because they can be very sensitive.

MEMBER BONACA: Would a small break -- they would be dominated by the service history. We don't dominate it by active system fractures probably. Or did you look at it? I mean, I don't know how that would affect, in fact, you know, the --

MR. TREGONING: Well, again, we weren't looking at active system failures here.

MEMBER BONACA: No, I just was wondering how that would affect this curve, I mean, if you throw in -- it would be still on -- on the small break size type contribution, but you were referring to service history, you know, for small breaks.

MR. TREGONING: The service history for passive system failures leading to small breaks.

MEMBER BONACA: I understand that. But, you know, the LOCA rule includes any break.

MR. TREGONING: Right. Right.

MEMBER BONACA: So I just was wondering how that would affect this curve in the lower break range.

MR. TREGONING: Well, you know, I don't want to speak for someone's ECCS analysis, but when they would do an ECCS analysis they have to consider, you know, all of the risk contributors, right? Including from active system breaks. But one of the objectives of this elicitation we thought -- the failure frequencies that we had for active system breaks were robust and continue --

MEMBER SHACK: I think what Mario is asking is: what is the comparative number for active system failures versus these passive system failures?

MR. TREGONING: Ah.

MEMBER BONACA: I would expect that they would dominate this.

MR. TREGONING: Yes. Yes, that's true.

CHAIRMAN APOSTOLAKIS: Tell me again what active system failure is.

MR. TREGONING: Stuck open valve.

MEMBER BONACA: Stuck open PRV or --

CHAIRMAN APOSTOLAKIS: Now, which of these LOCA categories is -- has been observed in the past? Has any one of these been observed?

MR. TREGONING: Well, we've had a few -- certainly, for Ps, we've had -- there's been instances of steam generator tube ruptures, which have -- you know, which met our definition for a small break LOCA.

CHAIRMAN APOSTOLAKIS: Okay.

MR. TREGONING: Okay? And there have been a couple of BWR small pipe failures which are on the cusp of either one or two, depending on how you count them, which are on the cusp of being 100 gpm leaks.

CHAIRMAN APOSTOLAKIS: So the others were a result just of calculations or evaluations?

MR. TREGONING: Extrapolation of that experience.

Okay. So one --

MEMBER SHACK: And just coming back -- I mean, did the fracture mechanics guy really believe that was a best estimate? Or that's simply -- he just presented that as a result of his model?

MR. TREGONING: He presented that as a result of his model. And as part of that discussion he said, "Here's how the calculation was done. Here's what I assumed. Here is the reason that this estimate came out low." So, yes, it was what his model could give us essentially.

So in not every case -- in fact, when we probed deeper, the thing that we found was that what the models were developing -- or what the models were telling us, and in some cases even what the service history estimates were telling us, they weren't actually analyzing the problem that we defined. They were analyzing the problem that they thought was as close to what we defined as they could handle.

So if you look at a lot of the reason for the inconsistency, it was mainly because even though as a group we agreed to how we define these base cases, people just had various abilities to really analyze for those unique set of conditions. And that's what we found. There were differences in what people actually considered versus didn't consider as part of their modeling.

And the service history estimates, I mean, they're models in a sense as well, because you have to figure out which part of the service experience is really applicable. So you have to make assumptions and, you know, decisions when you go through these calculations.

CHAIRMAN APOSTOLAKIS: Why -- I don't know if you discussed this, but if you look at the PWR, why is the lowest frequency assigned to Category 4? Five and six have higher --

MR. TREGONING: With the PWR?

CHAIRMAN APOSTOLAKIS: Yes.

MR. TREGONING: Well, again, this guy for instance didn't give us five and six.

CHAIRMAN APOSTOLAKIS: Oh, he did not. All right.

MR. TREGONING: So if you look for this PWR-1 case, the lower bound is roughly the same. So essentially this person is saying, you know, the likelihood of a four is pretty similar to the likelihood of a five or a six.

So, like I said, we didn't get estimates for every category for every case from every base case team member.

Now, why we did this exercise, the goal was not to get consensus in the base case estimates. The goal was to provide this information to the panelists, so that they could use it in an informed way when they developed their elicitation estimates. So part of the elicitation --

MEMBER BLEY: Can I slip a question in there to Lee?

MR. TREGONING: Sure.

MEMBER BLEY: Since all of the literature you referred to and the experience in doing elicitation I think has shown that anchoring itself is one of the most powerful biases, even when people know it's an artificial anchor, how do you feel about developing an anchor that the -- you then spin off the other results from --

MR. ABRAMSON: Well, we felt in this case that we had no choice whatsoever.

MEMBER BLEY: Just because of time and --

MR. ABRAMSON: No, no, because of the nature of the problem. The anchoring was done so we could get absolute numbers. What came out of here, as Rob said, was their best guesses, and what happened in each individual elicitation was every expert was free to choose which one of these base cases, or some modification or combination that they would use as their anchor. So that started the process. You had a number here, 10^{-2}, something like that, as the base case. Everything else, all of the other questions were all relative to this number here.

MEMBER BLEY: So from the best you could do, this is a reasonable anchor.

MR. ABRAMSON: Right. And all -- the only -- we only asked -- the only numbers that the -- that the experts gave us were relative numbers. That's the only quantitative information they gave. Everything was relative, and ultimately relative to a base case frequency.

MEMBER BLEY: Let me pursue --

MR. ABRAMSON: To this anchoring of a --

MEMBER BLEY: -- this just a little more. Under BWR-2 for LOCA Category 4, the geometric mean of the two you've got there is roughly 10^{-12}. Is that what you used as an anchor?

MR. ABRAMSON: No, no, no. No, no.

MEMBER BLEY: What did you use --

MR. ABRAMSON: No, no.

MEMBER BLEY: -- as the anchor?

MR. ABRAMSON: Again, what happened was all of these results --

MEMBER BLEY: You've used a physical description as an anchor?

MR. TREGONING: We didn't have an absolute anchor. That's not what we did at all. What we did is we presented this information to the panel like --

MEMBER BLEY: So this picture was the anchor.

MR. TREGONING: This picture was the anchor, and then -- in a sense, but what we asked in the elicitation, if you look at that last bullet, we asked individual panel members to critique this evaluation that each of the base case members did. And we asked them to -- a particular evaluation or analysis to use as their anchor, the one that they thought was most appropriate.

MEMBER BLEY: Okay.

MR. TREGONING: Okay? So we didn't try to aggregate this in any way, shape, or form.

MEMBER BLEY: I assume this picture of the base case anchor comes with qualitative descriptions of each of the analyses. That was part of the anchoring?

MR. TREGONING: Like I said, we had an entire meeting where --

MEMBER BLEY: On this.

MR. TREGONING: -- that just discussed how each of the base case team members, what their assumptions were, what their approaches were, what -- assumptions, approaches, results, and implications.

MEMBER BLEY: So, really, all of that is part of the anchor. It's not this --

MR. TREGONING: All of that is part of it.

MEMBER BLEY: -- picture.

MR. TREGONING: Yes, all of that is part of the anchor.

CHAIRMAN APOSTOLAKIS: How long did your whole exercise take?

MR. TREGONING: From?

CHAIRMAN APOSTOLAKIS: From beginning to end.

MR. TREGONING: It hasn't ended yet.

(Laughter.)

CHAIRMAN APOSTOLAKIS: No. I mean, when you wrote the report.

MR. TREGONING: Well, we started -- I think we started -- we started developing the criteria for panel members in, what, fall of -- summer of '02, and then we finished the draft report at the end of '04.

CHAIRMAN APOSTOLAKIS: Two years.

MR. TREGONING: So about two and a half years.

CHAIRMAN APOSTOLAKIS: And how many meetings did you have with the experts?

MR. TREGONING: We had -- we had three -- we had three group meetings, plus we had a teleconference where -- after we completed the preliminary version of the report, we had a teleconference, so that we could get critiques on the report itself. So I'll count that as another group meeting, even though people weren't physically located in the same room.

CHAIRMAN APOSTOLAKIS: Okay. Let's move on.

MR. TREGONING: Okay. I'll talk a little bit about the non-piping base cases. If you understand the piping, the non-piping -- or they're analogous. They're not quite identical. There's a lot more non-piping failure mechanisms that can occur that we talked about. You know, people could not tighten a bolt on a reactor head right that could potentially lead to a LOCA.

So the failure mechanisms weren't -- were dissimilar, so we didn't apply the same piping base case approach. We did something that was analogous.

The other thing with non-piping is for piping we had a very robust precursor database. There has been a lot of work into cataloguing and evaluating and classifying piping precursor failures. There wasn't the same amount of information for non-piping, so we have to do a little bit more legwork for the non-piping.

We actually had to develop an initial precursor database that we provided to the panelists, and we also used -- we used some existing PFM modeling results to develop LOCA frequencies for some targeted degradation mechanisms. And we did things that people had been working on either currently, things like CRDM ejection when we were -- when we did the panel. Of course, Davis-Besse had occurred, the Oconee head-cracking had occurred, VC Summer had occurred, so a lot of people were familiar and working on these various CRDM ejection models. So that was a natural base case to pick.

There had been a lot of work on vessel rupture, either through PTS or through LTOP, so we used a lot of that existing work to provide non-piping base case information. And we really were -- tried to be as flexible as possible in letting people choose their appropriate base case. They could either use the non-piping precursor database, they could use one of the piping precursor database, they could use a piping base case, or a non-piping base case.

So we really -- we really -- what we wanted to do was to get them to pick a set of conditions that were most similar to what they were evaluating. So if they were evaluating CRDM ejection due to PWSCC, it may have been perfectly appropriate for them to use a hot leg cracking due to PWSCC as their base case versus another small pipe rupture due to flow accelerated corrosion for instance, because the failure mechanisms were more consistent.

MEMBER BONACA: You did not address directly Davis-Besse, right? I mean, you mentioned Davis-Besse, but you didn't --

MR. TREGONING: No, we didn't try to analyze Davis-Besse. We didn't analyze Davis-Besse.

MEMBER BONACA: So it was not included as a basis for this.

MR. TREGONING: Right. Right. But there as a lot -- because it was -- you know, Davis-Besse happened around the time we started, or just before we started, so there was a lot of discussion of implications of Davis-Besse and what that meant with respect to the LOCA frequencies that we were developing.

I've talked about most of this. We developed questions to evaluate the base cases. We asked the panelists for quantitative responses. And as Lee mentioned, we asked them to provide --

CHAIRMAN APOSTOLAKIS: I think we have to go a little faster now.

MR. TREGONING: -- mid, low, high values, and then qualitative --

MEMBER BLEY: If you go faster, let me sneak a question in.

CHAIRMAN APOSTOLAKIS: You can ask questions.

MEMBER BLEY: One of your key objectives I think in the report was to identify interfacing system LOCA frequencies. I'm a little surprised you didn't pick one of those as a non-piping base case. Did you think about that?

MR. TREGONING: Well, we didn't cover IS -- we didn't cover IS LOCA per se. We were looking for --

MEMBER BLEY: Okay. Early in the report you had talked --

MR. TREGONING: If that's in there, we've got -- that's a correction.

MEMBER BLEY: Okay. Just do a search on it.

MR. TREGONING: We were looking for LOCAs which initiated unisolable portions of the RCS.

MEMBER BLEY: Okay.

MR. TREGONING: So that specifically precludes a secondary side failure. An IS LOCA, you'd have -- a classical one, you'd have a secondary side failure coupled with a valve failure, of course, that would lead to -- that would lead to a LOCA. So we were focusing on the primary system failures.

MEMBER BLEY: Well, it wouldn't be. Now, in a PWR, it's not secondary side. You break through into the recirc system and you blow open a safety valve. You have the original one from WASH-1400. But go ahead. You didn't look for those. You didn't look for those, so --

MR. TREGONING: But you still need a failure. You'd still need --

MEMBER BLEY: You need a failure of a valve disk.

MR. TREGONING: You need a failure coupled with a valve failure, right.

MEMBER BLEY: Yes.

MR. TREGONING: Right. But no, we didn't look at those.

We've talked a lot about the framework, so maybe I'll skip through this.

CHAIRMAN APOSTOLAKIS: Yes. Insights.

MR. TREGONING: Let me go to insights, and the next couple of slides -- these are qualitative insights that were provided by the panelists. So the first slide talks about insights that we got of BWR and PWR plants. For BWRs, these are the degradation mechanisms that the panel largely agreed were the most important ones -- thermal fatigue, IGSCC, mechanical fatigue, FAC. The operating transients that people talked about with these, there was concern about the increased likelihood of water hammer compared to the BWR plants.

On the good side, many panelists identified the fact that the BWR community has a lot of experience, probably more experience than the PWRs, in identifying and mitigating degradation due to the IGSCC experience.

MEMBER SHACK: That's a good thing, huh?

(Laughter.)

MR. TREGONING: Well, it wasn't always -- it wasn't at the time. It wasn't at the time. But when you're up on the learning curve with anything, it makes you more likely to pick up new things that come down the pike.

Now, it looks like the PWR community is rapidly catching up with that experience as we go here.

The other thing that was -- that was

important, and we spent a lot of time discussing this, is that when you look at the service experience per say for the BWR plants, you really have to be careful about how you evaluate it, because a lot of the events were pre-mitigation, IGSCC precursor events. So you really have to analyze that service history with quite a bit of care, and we talked a lot about that as a group and how to use that service history appropriately.

For PWR plants, PWSCC, of course at the time we were doing this PWSCC was becoming more and more prevalent. So this was really the -- probably the major risk driver in the PWR plants. It was a degradation mechanism that most people were concerned about at the time.

But thermal fatigue and mechanical fatigue as well were identified as important degradation mechanisms. And I mentioned that PWSCC concerns were paramount for many of the panelists. Many of the panelists indicated that near-term frequency increases due to PWSCC were probably likely. And why is that? Well, we were just -- we were on the cusp a couple of years ago of trying to understand how widespread PWSCC is out in the fleet.

And the analogy with IGSCC was quite often given that said, you know, we saw some initial failures, and then when we really started to look we realized how widespread the problem was. And there was -- there was opinion that it's probably the same type of path that's going to be followed from PWSCC. As we go down the next few years, we'll see how prevalent PWSCC is, and it might cause some near-term elevations of frequencies.

Now, it's interesting two years hence to sort of see that play out, because that's exactly what has been happening. But there was an expectation, much like with IGSCC, that once mitigation measures have been developed and implemented, some time after the fact, that the frequencies due to PWSCC would start to decrease again.

So some time in the future -- it's not there yet, because we're in the midst of going through mitigation now -- there was an expectation that frequencies would drop again.

Some more insights related to piping and non-piping -- a couple with piping. Most people identified that the complete failure of a smaller pipe is generally more likely than the partial failure of larger piping. So for any LOCA size, right, you get -- you get contributions due to a complete rupture of the smallest pipe that can give you that flow rate, or a smaller failure in a larger pipe.

By and large, what you tend to see is the systems that can -- that complete failure will give you that LOCA tend to be the ones that dominate risk, at least with respect to the elicitation. The only exception to that was the recirc system in BWR to IGSCC. That was still an important risk driver for a lot of the LOCA categories, except the very smallest ones.

And there was also a notion that people thought that the aging -- or material aging and degradation would have the greatest effect on intermediate size piping. There was a belief that the larger size piping, the inspection tends to be good, there is a lot of design margin there, and then the larger piping also has more leak-before-break margin. So the bigger the pipe, the more likely you are to have a leak instead of a break.

Conversely, the smaller pipes, you know, there was -- there was I think a notion that the smallest pipes would, you know, govern best by service experience. And you're always going to have failures due to one reason or another, and that they were -- that service experience did a good job of capturing those failures, so, hence, the thought that aging and any failure increases would have the biggest effect on these intermediate six- to 14-inch pipe sizes.

And it's interesting, when you see the quantitative results -- and we're going to compare them later -- the biggest increases compared to historical estimates that we got from the panel are for these intermediate size LOCAs. I call them intermediate size, but they're on the cusp of being, you know, large break LOCAs, but not double-ended guillotine breaks.

So the estimates that we got are very consistent with this rationale for --

CHAIRMAN APOSTOLAKIS: Is a large break LOCA one that is equivalent to a hole of six inches in diameter?

MEMBER BLEY: In most PWRs anyway, based on the makeup capability.

MR. TREGONING: Yes, that's the cusp. And, again, these are generic size estimates. In the individual --

CHAIRMAN APOSTOLAKIS: For BWRs that's not the case?

MEMBER BLEY: I don't remember.

CHAIRMAN APOSTOLAKIS: I guess I'm getting confused with what --

MEMBER BLEY: I remember the basis on the P, but not on the B.

CHAIRMAN APOSTOLAKIS: -- intermediate size and then a parentheses has sizes associated with large.

MEMBER BLEY: Large breaks.

MR. TREGONING: Well, I say intermediate size piping. They're not the biggest plants, not the hot leg, not the recircs.

CHAIRMAN APOSTOLAKIS: So you're making a distinction between the size of the piping and the LOCA.

MR. TREGONING: Yes. Sorry, I didn't mean to --

MEMBER BLEY: One is a PRA term, one is a piping term.

MR. TREGONING: Non-piping --

CHAIRMAN APOSTOLAKIS: What is the largest piping that -- I mean, if this is intermediate -- yes, in diameter.

MEMBER BLEY: Thirty inches or so?

MEMBER MAYNARD: Yes, 30-some -- about 32 inches, something like that.

MEMBER BLEY: For the Ps. For Ps, yes.

MEMBER MAYNARD: Twenty-eight in the BWR, and --

MEMBER BLEY: Close to 30, then, yes.

MR. TREGONING: Okay. For non-piping, not surprisingly, the panelists agreed that estimating non-piping failure frequencies was more challenging than piping, again, due to the disparity of the different failure mechanisms. The larger non-piping components have bigger design margins, but decreased inspection quantity and quality. So that's something that they had to weigh those tradeoffs off in their mind when they were giving us failure estimates associated with those components.

MEMBER BLEY: Rob, let me correct what I said to you before, because I went back and looked in your report. You don't say that you looked at interfacing system LOCAs. And you say you didn't look at them because they're active system failures. I think that's generally true for BWRs. It's certainly not true for the Ps, and that's an area where this kind of work could have been real helpful.

MR. TREGONING: Well, there has --

MEMBER BLEY: And that's a passive failure of a disc of a large valve that cannot possibly move when the system is pressurized.

MR. TREGONING: And there has been -- you know, there has been quite a lot of work -- there were a couple of -- there was at least one very large study on interfacing systems like that.

MEMBER BLEY: Yes. So you figure that's handled?

MR. TREGONING: Well, I -- given the expertise of the panel, you know, looking at those particular rupture disc failures was sort of outside their expertise.

MEMBER BLEY: Okay.

MR. TREGONING: And this was the area that we thought really needed the most work. So, yes, interfacing system LOCAs -- and I can't speak intelligently about this, but there has been quite a bit of work done historically to try to estimate, you know, the frequencies associated with those. So no, there was no -- there was no desire to revisit that in this study.

And then, the third point -- the final point here -- again, smaller non-piping components, and by that we're talking about steam generator tubes, CRDM nozzles -- the panel expected to most likely benefit for improved inspection methods and mitigation programs. And these are areas that, at least within the community and the industry, there's a lot of focused research on developing those improved inspection methods and mitigation programs.

So let's get to the results, and let me try move quickly here. These show the mean and the 95th percentile results. These are aggregated results, of course, aggregated with the geometric mean for the BWRs and the Ps -- for the Bs, the decreases are more gradual with LOCA size, and, again, that's due to IGSCC concerns.

So -- and for Bs, if you look at the LOCA Category 6, you see a big dropoff here. That's because there's no piping that can give you that. You need a failure of something like the vessel or a large pump or valve casing to get those types of breaks. In fact, I take that back. It's only the -- it's only the vessel that is going to contribute there.

The PWRs, the frequencies of the smallest pipe breaks are higher than Bs, and that's largely due to steam generator tube and CRDM concerns. And, again, for Ps, the large piping becomes more important -- or the large -- the frequencies become higher than the B. So you see like a double crossover point here between the Ps and the Bs.

Now, this first result just shows the mean and the 95th. Now I'm showing the mean, the median, and the 95th, but I also am showing confidence bounds.

So I talked earlier about the difference between individual uncertainty and then panel variability. The difference between the median -- this black line and the green line --

CHAIRMAN APOSTOLAKIS: Let's go to the previous one. I have a --

MR. TREGONING: Sure.

CHAIRMAN APOSTOLAKIS: -- clarification question. I'm looking at Slides 18 and 19, and your comment that only non-piping failures contribute to largest breaks. Right?

MR. TREGONING: For Bs.

CHAIRMAN APOSTOLAKIS: For Bs.

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: And then, you say for Ps they are also a contributor, they maybe not a sole contributor.

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: Now, on 18, you said that non-piping failure -- non-piping components have bigger design margins compared to piping, but decreased inspection quantity and quality.

MR. TREGONING: Right. So you have to weigh those competing factors.

CHAIRMAN APOSTOLAKIS: So the reason, then, they dominate the largest breaks is because of the decreased inspection --

MR. TREGONING: Yes. And they don't -- let me be clear. They don't dominate the largest break. So what happens for Ps -- non-piping dominate the smallest breaks, clearly -- steam generator tube ruptures, CRDM type.

Then, if you go -- as you increase the break size, the contributions for non-piping are very small. Okay? Not that significant. They only become significant again when you get to the largest break. So it's not that they dominate, but they come --

CHAIRMAN APOSTOLAKIS: But why? I mean,

is that consistent with the statement earlier that they have bigger design margins?

MR. TREGONING: Yes, but they're not inspected to the same degree.

CHAIRMAN APOSTOLAKIS: Oh, that's a problem, then, that there is -- the inspection is the problem.

MR. TREGONING: Yes. Yes. So you've got competing factors there. And the other thing, you just have to look at the population, right? The population to give you this, you're essentially looking at RCS piping, and then failure of the vessel, failure of --

CHAIRMAN APOSTOLAKIS: Of the real data.

MR. TREGONING: Yes, failure of the steam generator. You know, you're looking at the big failures to give you this size LOCA. So, I mean, it's -- you've got these competing factors, but you've also got a dwindling population of things that could even contribute to that size LOCA. So I think that probably, as much as anything, is why the contributions start to increase again at that point.

But they don't -- they don't dominate here. You know, I can't remember the number. They might have contributed 50 percent at most. I don't -- wouldn't call them dominate, where clearly the non-piping dominate at the lower.

CHAIRMAN APOSTOLAKIS: And what you're showing in this slide is the geometric mean.

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: The geometric mean of the 95th percentile, the geometric mean of the medians, or whatever.

MR. TREGONING: That's correct. That's correct.

CHAIRMAN APOSTOLAKIS: Later on you'll show actually the --

MR. TREGONING: Later, yes. And these are geometric mean aggregated as well, but we have confidence bounds which depict -- these essentially predict the 90 -- I say 95 percent confidence bounds. They are really 90 percent, so the five percent and the 95 percent capturing the panel variability.

So this single plot, you get an estimate of what the individual uncertainty is as well as the panel variability.

CHAIRMAN APOSTOLAKIS: Now, the Commission, when they set this frequency of 10^{-5} as the determinant for the transition break size, did they say whether it was mean or median or anything? I don't remember.

MR. TREGONING: Lee, do you want to take that one?

CHAIRMAN APOSTOLAKIS: Did they say anything in the --

MR. ABRAMSON: I think they used the mean.

CHAIRMAN APOSTOLAKIS: They said mean I think.

MR. ABRAMSON: Well, I see that --

CHAIRMAN APOSTOLAKIS: Or they implied strongly.

MR. ABRAMSON: The 10^{-5} is a standard, and you -- and so this is a fixed number. There's no uncertainty about this. There's a question of you want to compare -- presumably you want to compare your mean to this, or your whatever it is.

CHAIRMAN APOSTOLAKIS: But is that the presumption, or the Commission actually said it?

MR. ABRAMSON: I'm not sure.

CHAIRMAN APOSTOLAKIS: Yes, don't remember the --

MR. COLLINS: I have the language of the

SRM. It says, "For example, a frequency of occurrence of one in 100,000 reactor-years is an appropriate mean value for the LOCA frequency guideline."

CHAIRMAN APOSTOLAKIS: I remember vaguely it was --

MR. COLLINS: That's the language.

CHAIRMAN APOSTOLAKIS: But they put those two words up front, which is -- are a little bit disturbing.

MR. COLLINS: For example.

CHAIRMAN APOSTOLAKIS: For example.

MR. COLLINS: Yes, right. Right. They were --

MR. TREGONING: There's the flexibility.

MR. COLLINS: There's the flexibility there, right.

MR. TREGONING: Okay. Thanks, Tim. Thanks for clearing that up. Okay.

CHAIRMAN APOSTOLAKIS: Because I believe in some of the debates NRR actually was looking at the 95th percentile or the 95th bar.

MR. TREGONING: Well, NRR has looked at a lot of different --

CHAIRMAN APOSTOLAKIS: I know they did

look at lot of things, but, I mean, if you look at some of the numbers that were cited -- for example, for PWRs, I think the number is something like 10 or so inches, which really is consistent with the uncertainty bar for the 95th percentile.

MR. TREGONING: Yes, they're up at 10 to 12 inches, depending on where the --

CHAIRMAN APOSTOLAKIS: For the BWR, it was about --

MR. TREGONING: 20.

CHAIRMAN APOSTOLAKIS: Yes. Which, again, if you move that bar a little bit, so -- well, it's a good thing you didn't show the 99th percentile.

(Laughter.)

MR. TREGONING: Well, you know --

CHAIRMAN APOSTOLAKIS: I think, though, that's fine. I mean, you know, if you are a real decision-maker, you have to take the totality of this analysis into account. I mean, you don't just take one number.

MEMBER BLEY: Rob, take me back to this figure and tell me again what you said about the individual variability versus the group variability.

MR. TREGONING: Again, what we show here

is the median, the mean, and the 95th. So the individual variability -- or the individual uncertainty is reflected by the difference between, let's say, the median and the 95th, where these confidence bounds really reflect the spread or the differences among the panel members.

MEMBER BLEY: Among the panel members.

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: So each bar is the differences among the panel members.

MR. ABRAMSON: That's right. The confidence interval is what we call diversity -- is the uncertainty or the differences among panelists, and it's measured by confidence -- by confidence interval.

CHAIRMAN APOSTOLAKIS: So the uncertainty --

MR. ABRAMSON: Or the -- I should say the spread -- uncertainty is a bad term here. The spread between the eight or the nine, depending on BWR or PWR, the essence that we got is -- that's what we call diversity, and it's measured by a confidence band.

For example, if we're trying to estimate a mean, so we get the mean aggregation, we use a geometric mean. And then, the question is how much spread there is around this central value over the panel, and that's measured by the confidence band.

MR. TREGONING: And if you look at these plots, not surprisingly, both measures of the differences increase with LOCA size. So if you look at the smallest LOCAs, there's not a lot of difference here, and the confidence bounds are pretty tight. You go up to the highest LOCAs and there's a lot more uncertainty, a lot more variability.

CHAIRMAN APOSTOLAKIS: If you had shown --

MR. TREGONING: That's how it should look, of course.

CHAIRMAN APOSTOLAKIS: If you had shown a brown curve of the 5th percentiles, then the two curves -- the 95th and the 5th -- would tell us something about the individual variability, wouldn't they?

MR. TREGONING: That's right, yes.

CHAIRMAN APOSTOLAKIS: The individual uncertainty. And each bar you're showing now is the expert-to-expert variability.

MR. TREGONING: Yes. We could show the 5th, but the 5th wasn't important for decision-making, and the slide was busy enough, so --

CHAIRMAN APOSTOLAKIS: So is this a good time to take a break?

MR. TREGONING: I think we're going to talk about the aggregation and the sensitivity analysis, so, yes, a quick break would be --

CHAIRMAN APOSTOLAKIS: Okay.

MR. TREGONING: We're nearly finished.

CHAIRMAN APOSTOLAKIS: Okay. And you have a whole other presentation.

MR. TREGONING: Okay.

CHAIRMAN APOSTOLAKIS: Okay. So we'll take a break until quarter of.

(Whereupon, the proceedings in the foregoing matter went off the record at 10:36 a.m. and went back on the record at 10:51 a.m.)

CHAIRMAN APOSTOLAKIS: Back in session.

MR. TREGONING: Okay. So we did the baseline analysis which we indicated among other assumptions. One was the use of the geometric mean aggregation. But we did a large number of sensitivity analyses because we wanted to see what the effect of the various assumptions that we made in our analysis, how that --

CHAIRMAN APOSTOLAKIS: So the results of the previous slide did not include the overconfidence adjustment.

MR. TREGONING: These results --

CHAIRMAN APOSTOLAKIS: Because I remember in the past in your base case results, not base case in the sense you use it, you wanted to have this overconfidence.

MR. TREGONING: These say baseline results and our baseline results do not include overconfidence.

CHAIRMAN APOSTOLAKIS: Okay. This is straight manipulation of the numbers.

MR. TREGONING: Right. So we did sensitivity analyses in five areas to look at the effects of these assumptions and I've listed the five areas. But we're only going to talk about two. We're going to talk about the two that are the most interesting and that's the overconfidence adjustment that Professor Apostolakis just spoke of and then we've already alluded and discussed a little bit about different ways of aggregating expert opinion. We're going to talk about that as well. All five areas are covered in the NUREG, but there are the two that have the most impact. So Lee is going to talk about the sensitivity analysis.

MR. ABRAMSON: Yes. When Rob said that they were the most interesting they are in the sense the most interesting but also the ones that have the greatest sensitivity as well.

The first one we're going to talk about is the overconfidence that starts from the observation that generally elicitation respondents are generally overconfident about their uncertainty and this is not just experts. It's everybody. Whenever elicitation experiments or training exercise are performed, we found that.

CHAIRMAN APOSTOLAKIS: Some of us are humble and we are not overconfident.

MEMBER BLEY: Yes, but it takes awhile to get to that.

CHAIRMAN APOSTOLAKIS: It takes awhile to get there.

(Laughter.)

MR. ABRAMSON: The question is, George, how overconfident are you about your humbleness?

(Laughter.)

MEMBER BLEY: Some of us have things to be humble about.

MR. ABRAMSON: That's true and this has been demonstrated using the almanac type questions which no one answers and the general rule of thumb which I already mentioned before is that the true confidence level is approximately half the nominal coverage level. So 90 percent coverage is really about 50 percent.

I think that this is really a demonstrated phenomenon. So therefore, we felt that we could not not make a correction. Because if we did not make any corrections for overconfidence, then we could be accused of being non-conservative and underestimating the uncertainties. So that's why we felt that we had to make some kind of correction. The question, of course, is what and so what we did is we did a number of different kind of corrections and these are detailed in the report.

What we did settle on for, let's say, our general, our base case, or our base confidence is what we call the error factor adjustment and what we did is as follows. You had, say, eight or nine numbers which came out from the panel either a BWR or a PWR and we took a look at those and what we did is we looked at the error factors involved, the error factor being the ratio of 95th to the median and this is a measure of the spread of each individual one.

For each individual panelist, we did get an error factor. And where those error factors were small, that was a measure of us of overconfidence. In other words, they didn't have much of a spread in their distributions. So what we did is we let the results drive everything. So we looked at the -- We took the geometric mean of all of these eight or nine, excuse me, of their error factors and we took a look and the ones that were above the geometric mean we did not correct because those were a good spread. The ones that were below we set those equal to the geometric mean. And the particular case --

MEMBER BLEY: And you did this regardless of the person. You assumed --

MR. ABRAMSON: That's right. It had nothing to -- Yes, we just took these numbers. Once we have these eight or nine numbers, that's what we were working with.

MR. TREGONING: And depending on where they fell with respect to the other panelists some of their answers may have been corrected. Others would not have been.

MR. ABRAMSON: That's right. We did attempt -- There was no correlation. We did label anyone as being highly overconfident or 100 percent. It often turned out to be that case because people obviously were self-consistent in their degrees of uncertainty that they assigned to their own estimates. But we did this individually for each of what we call the separate, our bottomline, parameters. That is the mean, median, fifth and 95th percentile, and for each of the six LOCA categories. So we did this overconfidence correction separately for each of these cases.

MEMBER BLEY: You calculated this separately for each number they evaluated rather than giving fair correction for median and applying it everywhere.

MR. ABRAMSON: For each estimate what we did is we took all of their answers to their 100 or 200 questions and what we did is we combined these with the various assumptions. You can see the details in the report and we came out with the results for each individual panelists were four numbers, mean, median, fifth and 95th percentile and that's what we worked with.

MEMBER BLEY: Okay.

MR. TREGONING: We worked with their bottom line.

MR. ABRAMSON: We worked with their bottom line.

MR. TREGONING: And again, just to clarify something that Lee said, we didn't adjust anybody's median estimates. Those were never adjusted. The only thing we adjusted were their error factors in these which affects the fifth, the 95th and then the mean. But the median was never. So essentially what we identified as their best estimate we never changed that. We only changed the spread about that best estimate.

MR. ABRAMSON: That's right because the overconfidence clearly is a measure between -- They estimate, say, a median and a 95th percentile where the spread between this is a measure of how certain or uncertain they are about their results and that's what the overconfidence correction is applied to.

MEMBER BLEY: This is just an odd point. I'm sitting here thinking if I had done all these estimates, in some cases, I might be fairly narrow and in other cases, I might be fairly broad.

MR. ABRAMSON: Yes.

MEMBER BLEY: This correction would have kind of made me never show my confidence if I varied --

MR. ABRAMSON: We worked with the group.

MR. ABRAMSON: You see it's compared to your error factors when you compare to other people's error factors.

MEMBER BLEY: Okay.

MR. ABRAMSON: So if everybody felt, say, pretty confidence, in other words, you were pretty sure about this, all their error factors would be relatively small. Then you are always being compared, the overconfidence is relative to other people and there's no absolute measure.

MR. TREGONING: Right, and the tighter the error factors were or the tighter the variability was for the group for a given set of estimates, the less correction would have been applied. So they really only became important for those cases that you had a lot of variability.

MEMBER BLEY: Adjustment is probably a

better word.

MR. ABRAMSON: So in a word, you adjusted to be overconfident if you had a lower spread than other people in your group.

CHAIRMAN APOSTOLAKIS: I think it would be best to present these things as one sensitivity analysis among many rather than trying to really justify that we have to stretch the error factor of the guys who have reported short one compared to the group. In other words, maybe that guy knew that this was justified.

MR. ABRAMSON: Roger Cook did a lot of work on that.

CHAIRMAN APOSTOLAKIS: Yes. So I think as a sensitivity analysis with some rationale behind it, it makes perfect sense to me. But I wouldn't want to defend it as "Oh, no. We have to do it that way." Do you understand the difference?

MR. ABRAMSON: Yes, I understand what you're saying, but I have to disagree, George.

CHAIRMAN APOSTOLAKIS: Go ahead.

MR. ABRAMSON: Because I think you have to keep in mind that this is an elicitation and we know certain things about elicitations and, after all, you can ask how we justify doing this in the first place. Why do we spend all this time and money and effort and everybody spends years doing this. The reason I think a proper answer is because it's been shown to work to give you valuable information in cases where we know about it. But nevertheless it's an elicitation.

So if you accept this premise, I mean, if you accept or go by the logic of this premise which is what my justification for it, you also have to say what else do we know about elicitation. Another thing that we do know and this has been demonstrated over and over again is in general there's an overconfidence.

CHAIRMAN APOSTOLAKIS: I believe that and I agree with that.

MR. ABRAMSON: And that's the rationale for this.

CHAIRMAN APOSTOLAKIS: No. I agree. What I'm saying is that to do defend a particular way of adjusting for this general insight is probably not a good idea. It's a good idea to try to do something about it and present maybe two or three different ways of handling it.

MR. ABRAMSON: Okay.

CHAIRMAN APOSTOLAKIS: In other words, I fully agree with you that it's a fact. But I wouldn't bet my life that "Oh boy, those guys who reported a shorter, smaller error factor were necessarily" --

MEMBER BLEY: And I want to agree with George but go a little further and the work Roger Cook did and calibrating experts I think kind of shows that some people have a tendency to --

CHAIRMAN APOSTOLAKIS: Over do it.

MEMBER BLEY: -- overestimate or underestimate their uncertainty bounds and it's been shown to be reasonably consistent.

CHAIRMAN APOSTOLAKIS: Yes.

MEMBER BLEY: Others -- And you may have had all guys who haven't done a lot of this. But others who have done a lot and have become pretty good normatively when their error bounds are smaller it's for a reason.

CHAIRMAN APOSTOLAKIS: Yes.

MEMBER BLEY: And we're ignoring that. So the only thing I'm agreeing with is, yeah, it's an issue. You've come up with a way to deal with it. If you say that's the right way, you're liable to get hit with contradictory evidence.

MR. ABRAMSON: I would agree. I listened to what you said, George, that you're not arguing against the fact that we need an overconfidence adjustment.

CHAIRMAN APOSTOLAKIS: No, absolutely not.

MR. ABRAMSON: The question of what kind of overconfidence and I agree with you. We hit on this. It seemed to be reasonable to us, but in the report, you'll see we did a lot of other overconfidence adjustments.

CHAIRMAN APOSTOLAKIS: And that's fine.

MR. ABRAMSON: And everything, we felt that this was a reasonable way to do it.

CHAIRMAN APOSTOLAKIS: And I would say over the years that there are people who tend to report larger uncertainties. They tend to be on the side of -- and perhaps of some members of this Committee have been doing this over the years. You know, they tend to exaggerate the uncertainties because that's their job.

MR. TREGONING: We actually saw that here.

CHAIRMAN APOSTOLAKIS: Today?

MR. TREGONING: No.

(Laughter.)

MR. TREGONING: I can't comment on that.

MEMBER BLEY: Probably if you try. Such a great state.

MR. TREGONING: We saw this -- When we first looked at correcting for overconfidence, we used more classic, broad schemes and they didn't work. They didn't work quite frankly because some of the experts were not underpredicting their uncertainty or confidence. So it was clear the fact that these schemes didn't make sense once we had applied them or the results just didn't -- you couldn't adjust them based on reality. It was clear that some of the experts had not underestimated their uncertainty. But there were others who if you looked at the estimates and given what we had asked them to provide us rationale, they clearly had.

So I would agree. We had a bit of a mix here which is one of the reasons why we came up with the scheme and recommend the scheme we do.

CHAIRMAN APOSTOLAKIS: Sure.

MR. TREGONING: But I agree with you, George. It's not to say the scheme --

CHAIRMAN APOSTOLAKIS: Sensitivity study.

If you did many more --

MEMBER SHACK: But, George, you've been the one that's been hammering them all along that they have to come up with a bottom line number when they're done and they're saying their bottom line number is going to include this adjustment. You're not disagreeing with that. It's not just one sensitivity case among others.

CHAIRMAN APOSTOLAKIS: No, because -- Are you going to show your bottom line numbers at some point?

MR. ABRAMSON: Yes.

CHAIRMAN APOSTOLAKIS: I think they are trying to avoid that.

MEMBER SHACK: They are but you've been hammering them since the elicitation began.

CHAIRMAN APOSTOLAKIS: Yes.

MEMBER SHACK: Now you're shoveling back.

CHAIRMAN APOSTOLAKIS: The way I would do it, I would do all these sensitivity analyses these fellows have done for all these issues, not just the adjustment, and then at the very end, I would go back to the facilitators that Rob described in the morning and I would expect the facilitating group to say based on everything we've done, here. That's the way I would do it. Now, Lee, I know objects to that.

MR. ABRAMSON: No. I think in effect we were the facilitators.

CHAIRMAN APOSTOLAKIS: Yes, I know. You and maybe --

MR. ABRAMSON: Rob.

CHAIRMAN APOSTOLAKIS: But essentially you two. But I know that you objected to that kind of thing in the past because you left, Lee --

MR. ABRAMSON: Yes.

CHAIRMAN APOSTOLAKIS: -- that it's the Commission's job to do that. So there was a disagreement there.

MR. ABRAMSON: Yes.

CHAIRMAN APOSTOLAKIS: But I wouldn't rely on any single analysis to say this is the number.

MR. TREGONING: Right, and that's fair. That's a fair point.

MR. ABRAMSON: Yes. Agreed.

MR. TREGONING: We'll talk a little bit about the results, but we want to -- In the interest of moving on, I think Lee is going to try to get on quicker than this morning.

MR. ABRAMSON: Yes.

CHAIRMAN APOSTOLAKIS: That was very polite.

MR. ABRAMSON: I'm going to look at this.

(Laughter.)

MR. TREGONING: The table -- correction.

MR. ABRAMSON: Yes, I can see that. Okay. You can see there. All right. The approach, I just went over that. It says accounting the error factors and this says when we actually made the adjustment. When they were too low, we made the adjustment up to the error factor. No change in the medians as Rob pointed out and we recalculated the means and the percentages and here you see the actual error factor corrections that were made.

For LOCA categories, you can see that these are the error factors after the corrections. Correct?

MR. TREGONING: No. Those are the geometric mean error factors.

MR. ABRAMSON: Yes, but after we had made the corrections.

MR. TREGONING: No. That was if you looked at all the --

MR. ABRAMSON: The original ones. Okay.

MR. TREGONING: Yes. If you looked at the whole panel, that was the geometric mean of all the individual --

MR. ABRAMSON: Error factors.

CHAIRMAN APOSTOLAKIS: So can we take one row, Lee, and explain? Take, say, row number five.

MR. ABRAMSON: Row number five, okay.

CHAIRMAN APOSTOLAKIS: So a LOCA category five.

MR. ABRAMSON: Why don't you do it?

CHAIRMAN APOSTOLAKIS: What does it mean now?

MR. TREGONING: I'll address this.

CHAIRMAN APOSTOLAKIS: The adjustment. What was the impact and so?

MR. TREGONING: Yes. So what you see in this table, it's a function of LOCA category and you see the BWRs on the middle two columns and then the PWRs.

CHAIRMAN APOSTOLAKIS: Right.

MR. TREGONING: So the EF geometric mean, that the geometric mean of the error factor for all the estimates for LOCA category five.

CHAIRMAN APOSTOLAKIS: Okay.

MR. TREGONING: So the average spread in the results --

MR. ABRAMSON: As they made them.

MR. TREGONING: As they made them.

CHAIRMAN APOSTOLAKIS: As they made them, yes.

MR. TREGONING: So the geometric mean of the spread of the different error factors was 14.

CHAIRMAN APOSTOLAKIS: Okay.

MR. TREGONING: So then when we applied the error factor correction scheme that we discussed, that percentage shows how much the mean increased for the geometric aggregated estimates after overconfidence.

CHAIRMAN APOSTOLAKIS: Okay. And the way you did is described on the left.

MR. TREGONING: Yes, which the --

CHAIRMAN APOSTOLAKIS: If the error factor was -- Okay. Good. And then you saw an increase in the mean, the mean of what?

MR. TREGONING: The mean frequency associated with that LOCA category. That's how much the mean increased due to our error factor correction, how much the aggregated mean --

MEMBER SHACK: So it was modest for small LOCAs and big for big LOCAs.

MR. TREGONING: Which is what --

CHAIRMAN APOSTOLAKIS: Essentially it was big for category six. Right?

MR. RODRIGUEZ: But you say big.

MR. ABRAMSON: But it's still a factor of two.

MR. TREGONING: Ninety percent in this game is not big.

MEMBER BLEY: And when you saw those decades of -- yes, that's nothing.

MR. TREGONING: So there are two things to get out of the table. One is how much the error factors varied as a function of LOCA size. You have relatively modest error factors for the small ones. But then when you get up to the big, the error factors are huge and the nice thing, not that I'm recommending this, but the nice thing about this correction in my opinion is that fact that it increases a function of how much that initiation error factor really is. But even across the board, the increases due to the scheme were relatively modest.

CHAIRMAN APOSTOLAKIS: Relatively what?

MR. TREGONING: Relatively modest.

CHAIRMAN APOSTOLAKIS: Yes.

MR. TREGONING: And I show the mean here, but there were similar corrections for the 95th out of a factor of two to two and a half at most and again, the corrections were always biggest for the biggest LOCA size.

MEMBER BLEY: For these spreads, the mean and 95th probably aren't too far apart.

MR. TREGONING: They're still relatively -- In fact, they're farther apart than -- Well, I mean because of the spread you can see how far apart they are.

MEMBER BLEY: Yes, we do.

MR. ABRAMSON: Okay, and now the second part is probably the most -- undoubtedly the most controversial, I would say, and that is aggregating the individual results and as we said, the baseline method used the geometric mean of the individual panel estimates. So it was either eight or nine depending on whether it was a BWR or PWR and we did this separately for our four bottom line parameters.

And the advantages we feel for this

exercise are that, first of all, the group estimates are not significantly influenced by the outliers. That's when you use the geometric mean. Now if we had used the median, then they certainly would not be. If we'd used the median, it would not be effected at all by the outliers.

It turns out though that for the kinds of numbers that we had the same thing was true of the geometric mean. In other words, the outliers were more or less symmetrically, logarithmically symmetrically, alerted about that.

MEMBER BLEY: That's the key.

MR. ABRAMSON: Right. That's the key.

MEMBER BLEY: If you have a single high outlier, an arithmetic average is skewed and if you have a single very low outlier, the geometric mean skews way down toward that way.

MR. ABRAMSON: But it wasn't.

MEMBER BLEY: But when you have outliers on each end, this works pretty good.

MR. TREGONING: Yes. The interesting thing, when we presented the results to the panel we had initially done everything with respect to the media. The panelists were up in arms, many of them, about that because you said -- they essentially said, "What you're telling me then is my estimates really don't matter. It just matters how my estimates fell either above or below that number." So a lot of them took great offense at the fact that we used the median versus some other aggregation scheme. So that was another -- it was interesting to present that to the panelists and hear their response at that point.

MR. ABRAMSON: I would agree that's right that people felt that some of their work was wasted.

MR. TREGONING: Right.

MR. ABRAMSON: And also I think from people in the RSA, the NRR, I think, our friends in NRR who need to use this felt that it made more sense to try to use all of the information and one way to do that is with the geometric mean rather than the median.

CHAIRMAN APOSTOLAKIS: I don't know about that. You use all the information in both places.

MR. ABRAMSON: You do.

CHAIRMAN APOSTOLAKIS: But maybe it is used in a different way.

MR. ABRAMSON: Speaking as an analyst, I'm looking at what seems to work and obviously the median is in the center of the group. In this particular case, it turned out we were able to satisfy, say, both positions. As it turned out, the geometric mean as the second bullet indicates results approximately with the median of the individual estimates. So we were very comfortable and people, the panel, accepted that this was a reasonable way to do the aggregation.

Now we did consider alternative methods to aggregate and in particular, we had a mixture distribution whereby you have the individual ones and you say that these in effect are observations from a distribution and it would equal each one, give an equal weight. It's either one-eighth or one-ninth and you just form a distribution for this and if you take the mean of that distribution, that's equivalent to just taking the arithmetic mean of the individual estimate. So that was one -- That was the only sensitivity study. That was a major -- That was the main competitor, let's say, to the geometric mean.

CHAIRMAN APOSTOLAKIS: But in the first bullet.

MR. ABRAMSON: Yes.

CHAIRMAN APOSTOLAKIS: When you talk about the arithmetic mean.

MR. ABRAMSON: Yes.

CHAIRMAN APOSTOLAKIS: It seems to me that in the interest of fairness you should dot a subbullet saying that "assumes that individual results -- assumes that the logarithm of individual results are obtained from equally credible models."

MR. ABRAMSON: No because we don't use that model, so to speak, of equally credible models.

CHAIRMAN APOSTOLAKIS: But you take the -- you assume that the experts are equally credible.

MR. ABRAMSON: No.

CHAIRMAN APOSTOLAKIS: Because you take the geometric mean.

MR. ABRAMSON: No. The justification in my mind for that is what I mentioned before that when you have results of an elicitation, it makes sense to take the somewhere in the center of the group. This is empirical observation, an empirical observation based on case we know. There's no theory behind it that I'm aware of.

MEMBER BLEY: The center of the log paper.

MR. ABRAMSON: Pardon me?

MEMBER BLEY: The center on log paper is what --

MR. ABRAMSON: No. It's not the center on log paper. We have these answers spread in two or three orders of magnitude and by the center, I mean the center of the group in some sense, in other words, the median, for example. The median is the center. So if you take the median, then that's the median of the distribution. The only question you would have is if you have eight and the group and the median would be the average between the central ones and then the question is what are you do mean by the average. Is it that arithmetic mean or the geometric mean? So you're right. Then it would be ambiguous. You would have to make some kind of decision.

CHAIRMAN APOSTOLAKIS: I think if we take the 95th percentiles and you have, say, eight experts giving you 95th percentiles. Now you are taking the geometric mean of the 95th to come up with an estimate of the 95th percentile.

MR. ABRAMSON: Correct.

CHAIRMAN APOSTOLAKIS: By taking the geometric mean of the eight experts, aren't you saying essentially that you are giving the same weight to the logarithm of the adjustment?

MR. ABRAMSON: Yes. It's -- George, you

are absolutely correct. It's equivalent to that.

CHAIRMAN APOSTOLAKIS: It's equivalent and it has an implication what you said that it's in the middle there somewhere and you're right.

MR. ABRAMSON: All I'm saying is what you're saying is if you had a model that you wanted to do with equal weights this would be a consequence of that model.

CHAIRMAN APOSTOLAKIS: Right.

MR. ABRAMSON: And all I'm saying is that you need that model. You can do it based on -- Another approach is to use the empirical observation about results of elicitations. But you could do it that way certainly.

CHAIRMAN APOSTOLAKIS: But the reason why I'm raising that is because the first subbullet on the second bullet assumes that individual results sort of sends the message that this particular way makes this assumption, whereas the other one doesn't.

MR. ABRAMSON: That's true I think. Let's put it this way.

MEMBER BLEY: Let me sneak one thing in because this is driving me a little nuts. I agree that the geometric mean does you pretty well most of the time and there is a fair amount of experimental evidence to support that. The idea that it's not significantly influenced by outliers or that it approximates the middle of the group is the predicated on the fact you don't have a single low outlier. If you do, this thing comes well below everybody but one of them.

MR. ABRAMSON: You're absolutely correct and these bullets refer to the results of this study.

MEMBER BLEY: Of this study. Okay. Where you have reasonably spread exercise.

MR. ABRAMSON: I had to make a generic recommendation I would recommend using the median. I was recommend using the median. But as we've discussed before, there was resistance to the idea of using the median. So we used the geometric mean. As it turns out for these numbers, it works out pretty well. If it didn't, then I'm not sure what we would have done.

MEMBER BLEY: Fair enough.

CHAIRMAN APOSTOLAKIS: Okay. So let's look at the results unless there are questions.

MR. ABRAMSON: All right.

CHAIRMAN APOSTOLAKIS: Because it seems to me that the decision made took --

MR. ABRAMSON: There, let's see, the first where you have the BWRs and this and you can see the top line, the red line, is the geometric mean.

MR. TREGONING: No, that's the mixture distribution.

MR. ABRAMSON: Excuse me. The bottom. That's the mixture distribution. Right. So you can see what this shows you is the top line is obviously the mixture distribution being the arithmetic mean would always be larger than the geometric mean. That's just an arithmetical fact.

MEMBER BLEY: Sure, and that mixture distribution, by that language, you mean the arithmetic mean.

MR. ABRAMSON: I mean the arithmetic mean. That's right. So this is the arithmetic mean and you can see what it looks like --

CHAIRMAN APOSTOLAKIS: Let me understand that, Lee.

MR. ABRAMSON: Pardon me?

CHAIRMAN APOSTOLAKIS: Let me understand that a little bit.

MR. ABRAMSON: Yes.

CHAIRMAN APOSTOLAKIS: Mixture distribution means you develop the distribution for each of the experts and then do what?

MR. TREGONING: Combine them just like 11.50.

CHAIRMAN APOSTOLAKIS: Reg. 11.50.

MR. ABRAMSON: It's 11.50.

CHAIRMAN APOSTOLAKIS: It's not the arithmetic mean of individual estimates. That was the distribution from each expert and then for each value you took the arithmetic mean of the probability.

MR. TREGONING: We show the mean here. The mean is --

CHAIRMAN APOSTOLAKIS: I understand that. That's the mean of the --

MR. TREGONING: The whole distribution.

MR. ABRAMSON: It's the mixture distribution.

CHAIRMAN APOSTOLAKIS: That's why you call it the mixture distribution.

MR. ABRAMSON: It's the mixture distribution approach.

CHAIRMAN APOSTOLAKIS: Yes.

MR. ABRAMSON: That's right.

CHAIRMAN APOSTOLAKIS: Okay.

MR. ABRAMSON: And it amounts to 11.50 taking the arithmetic mean.

CHAIRMAN APOSTOLAKIS: Right.

MR. ABRAMSON: We've thought of the mixture distribution because that's the rational for this using the arithmetic mean.

CHAIRMAN APOSTOLAKIS: Right.

MR. ABRAMSON: And so you can see here for categories one and two there is relatively, what is it, about 0.5 an order of magnitude difference. It becomes much larger for three and four.

CHAIRMAN APOSTOLAKIS: How much is half an order of magnitude?

MR. ABRAMSON: Pardon me?

CHAIRMAN APOSTOLAKIS: An factor of three or five?

MR. ABRAMSON: An order of magnitude is ten. Half an order of magnitude is about three, yes, where I come from.

MR. TREGONING: Exactly.

CHAIRMAN APOSTOLAKIS: I thought so, but -
-

MR. ABRAMSON: And for the PWRs, first of all, you have the category one. It's all about 10^{-2}. So there this is much more dominated by the actual experience. So there's relatively little uncertainty about it. But then you have maybe about an order of magnitude or so difference between the two estimates as you get increased LOCA sizes. And the message here is as the bottom bullet says, "that the group estimates can be significantly affected by aggregation method if by significant you mean an order or half an order of magnitude" or something like that. That's our take on that.

MR. TREGONING: The other interesting thing with this plot, if you look at the BWR, the spreads are actually increasing for LOCAs categories two, three and four and then they decrease again with LOCA categories five and six. So that's really the most interesting case.

CHAIRMAN APOSTOLAKIS: There was something about category four.

MR. TREGONING: These really -- If you look at the mixture distributions, the mean were really driven by a single high estimate.

MEMBER BLEY: You had that guy who had a constant number.

MR. TREGONING: And you see that there. I mean, roughly the frequencies for the mixture distribution between LOCA category two and four are essentially constant.

CHAIRMAN APOSTOLAKIS: Right.

MR. ABRAMSON: And now, what this shows is again we're comparing the mixture distribution to the geometric mean aggregation and we're comparing the ratio of the means for the two methods. And so this is a ratio comparison and for the BWRs you can see that for one and two, it's -- Well, it's about half an order of magnitude. It becomes much larger for three and four and so on. And you can also see that the comparison of the two methods, the arithmetic mean or the mixture and the geometric mean, is pretty constant whether you're talking about the ratio of the means or the ratio of the 95th percentiles.

CHAIRMAN APOSTOLAKIS: So this is the ratio between the mixture of distribution and the --

MR. ABRAMSON: Yes, that's right.

MR. TREGONING: Between the mixture, 95th and then the --

MR. ABRAMSON: The aggregate, the two methods of aggregation, that's what we're comparing here.

CHAIRMAN APOSTOLAKIS: Okay. Good.

MR. ABRAMSON: This gives you a feel for how much in terms of ratio. Actually, what this is is this is just the previous curve except now we're just putting it in tabular form. You can actually see what it is. You don't have to try to eyeball it.

MEMBER BLEY: I did better with the curve.

MR. ABRAMSON: This is for people who are like myself more analytically oriented as opposed to visually oriented.

CHAIRMAN APOSTOLAKIS: The next slide is similar.

MR. ABRAMSON: Okay.

MR. DINSMORE: Dr. Apostolakis. My name is Steve Dinsmore. I work for NRR. I'd like to give you just a little different cut from these numbers because I mean these guys did a lot of work and they produced a lot of information and we had to take it and use it. And what happened is if you take a look at 10^{-5}. So you want to select your transition break size and you start with a 10^{-5}. It turns out that at 10^{-5} as you indicated earlier for PWRs, 95 percent confidence limit is about 12 inches. The arithmetic mean is about 10 inches and the geometric mean was about four inches.

So that was kind of saying if we're going to use this baseline as our estimate, we're going to start with a four inch LOCA as the largest LOCA that needs to be mitigated within the design basis. It has a very big effect on the actual endpoints.

CHAIRMAN APOSTOLAKIS: Yes.

MR. DINSMORE: And just for the PWRs, it was I think 95 percent was again 20 inches. The arithmetic mean was 14 inches and the geometric mean was six inches.

CHAIRMAN APOSTOLAKIS: In fact, I believe some owners groups wrote documents where they actually argued that we should go with the lower numbers.

MR. DINSMORE: Yes, that we should use these --

CHAIRMAN APOSTOLAKIS: Because the mean value is --

MR. TREGONING: You can see that in these plots because at 10^{-5} which was the initial starting point that's where quite often the differences are the largest.

CHAIRMAN APOSTOLAKIS: Right.

MR. TREGONING: That's depicted by what Steve said. So the implications in terms of how you start with what your initial PBS size is were quite wide.

CHAIRMAN APOSTOLAKIS: Okay. Shall we go to the reviews, slide 28?

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: I think we've seen enough to sensitivity.

MR. TREGONING: I just wanted to chronicle some of the reviews that have been done on NUREG 1829 both internally and externally. First, we've discussed some of these. The expert panel itself reviewed 1829. First the individual responses which we talked about made sure there was consistency amongst all the different testimonies. They looked at the calculations and analysis to make sure that was consistent with again their testimony and then there was also a review of the general qualitative and quantitative findings and conclusions.

We also conducted an external peer review. We had two external peer reviewers, one a decision analyst and a statistician, where we didn't focus so much on the individual results. But we focused on the structure of the elicitation, but even more importantly on how we analyzed the results and the framework that we used. So we talked about the analysis procedure and have them looked at that and the framework, the aggregation and sensitivity analyses that we did and those reviews are publicly available.

We certainly had ACRS review as well. We've had internal staff review both in Research and NRR and then the next thing in bold which we will discuss here subsequently is we've had public review and comment.

I did want to at least from the external review that we conducted with the decision analysts and the statistician wanted to talk about some of the conclusions. They largely said that the process that we used was adequate and sound for our objectives. There was a lot of concurrence on many specific aspects of the analysis procedure. They liked the fact that use this relative ratio structure. They generally agreed with the overconfidence correction using the error factor scheme that we used.

The reviewers were very helpful. They provided us with some additional sensitivity analyses that we needed to conduct. They caught a couple of errors in the initial analysis that we corrected and we largely implemented all the suggestions that we got from the external reviewers.

The next bullet here, I think, it's interesting in light of the continuing controversies. There was no consensus reached at least with the external reviewers on what the most appropriate aggregation scheme was.

CHAIRMAN APOSTOLAKIS: Now let me ask you, Rob. Was the decision analyst in favor of the mixture?

MR. ABRAMSON: No.

MR. TREGONING: No.

CHAIRMAN APOSTOLAKIS: No?

MR. ABRAMSON: No.

CHAIRMAN APOSTOLAKIS: That's very strange.

MR. ABRAMSON: And I could add that there, and you'll see it in the report, is evidence in the decision analysis, literature, and I quote it there, in favor of the geometric mean or the median approach for this kind of data where you have very wide range of opinion and we also add -- Well, in this particular case.

MR. TREGONING: And then the last bullet, I think it's important while the authors, I think both Lee and I do agree and believe that the geometric mean provides the best single estimates of what the elicitation panelists' results were. It is important to look at all these different aggregation schemes and factor that into the decision making process so that people can understand the variability and the uncertainty that's really behind these estimates.

We presented the arithmetic mean for the panelists and some of them were very vehemently opposed to it. I will say that. I guess not surprisingly because in fact in some cases they thought that the results were just too strongly biased by one or two high people.

CHAIRMAN APOSTOLAKIS: I think we covered this.

MR. TREGONING: Go on?

CHAIRMAN APOSTOLAKIS: Yes. Let's move onto the public comments.

III. PUBLIC COMMENTS

Mr. TREGONING: Okay. Now we're going to talk about what we've done since we published, didn't publish, but we wrote draft 1829 and sent it out for public comments.

There are really three things on slide two that we've been focusing on. One, we conducted a final QA verification of all the results. We've completed responses to public comments and then we've updated the NUREG based largely on the public comments, but also made some modifications based on the QA study. I'm going to talk about the QA first just because that's relatively quick and then we'll delve into some of the more interesting public comments that we got.

The initial results in the draft were developed solely by the staff, largely me. So we had a contractor conduct an independent analysis, found a couple of small errors. Once we got the initial errors, we went back and did a third analysis to make sure that the Battelle analysis was correct and then at that point we settled on the final estimates.

While they did find some errors, the ramifications of those errors were not significant at all. So I think the biggest difference we had in any of the estimates was 15 percent. We completed the QA. We're very confident of the results and the analysis we have and then the latest version of NUREG 1829 reflect those results. If you look at figures, you couldn't see a difference. But all the tabular values have been updated appropriately.

So the rest of the talk is going to focus on the public comment period and I did want to just indicate when we went out for public comment we did solicit some questions because we knew there were -- And we wanted to ask questions in some aspects of this that we knew were particularly contentious. We asked three questions when we went out. We asked if the structure of the elicitation process is appropriate for the problem and also the study. We asked if the assumptions and methodology of the analysis framework if they were appropriate and reasonable and consistent. Then finally we asked if geometric mean aggregation methodology was appropriate or should other aggregation methodologies be considered and what are their advantages and disadvantages. So we really wanted to get information from members of the public to try to provide feedback on some of the more controversial aspects of the study.

I just wanted to give some statistics here with this next slide. We completed the draft in June of 2005. It opened up for public comment, I believe, in September of that year. We had a meeting in the middle of the public comment period to facilitate public comment and then the public comment period closed at the end of November 2005.

We got 29 comments from the public and when I say comments, it doesn't mean got 29 letters. Within one letter, for instances, there may have been multiple comments. What we tried to do was we isolated separate issues associated with any one letter and then treated those as a separate comment. So we got 29 comments from the public, a variety of sources. We actually got some comments from one of the elicitation panelist which was interesting. We got comments from Penn State and we got comments from various industry representatives.

We also got many comments from NRR staff. Now at the time we went out for public comment, we had not received NRR feedback on draft NUREG 1829 and that was interesting putting the NUREG out for public comment and we got the ACRS -- you guys recommended that we go out as well. So in parallel to public comment, we also sent the document over for NRR review and we got a number of comments provided by the NRR staff.

Now in the information that I presented, that we presented, prior to this meeting it lumps all the NRR comments in with all the rest of the public comments. You can see the variety and wealth of comments that we got on the NUREG itself. And I think in total we identified 101 separate grouping of comments from the public comment.

CHAIRMAN APOSTOLAKIS: Why did Galyean submit comments?

MEMBER BLEY: Didn't like the way it turned it out I guess.

MR. TREGONING: He took issue with certain interpretations. He didn't take issue with the bottom line, but he took issue with how we arrived at that bottom line and some of our interpretations of the meaning of what that bottom line was.

CHAIRMAN APOSTOLAKIS: But he didn't have a chance during the workshops to --

MR. TREGONING: Yes.

MEMBER BLEY: This was everybody can see his comments, George.

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: The way he did it.

MR. TREGONING: I believe so, yes.

CHAIRMAN APOSTOLAKIS: All right.

MR. TREGONING: It was one of those -- The first part of his comment was, and I'm paraphrasing of course, generally complimentary as to what was done. But then the buts came and then there was a long line of buts of things that he took issue with and then at the end, he said, "However I don't think any of these issues are that significant that they would affect the bottom line." So it was a very long, passionate public comment and we spent a good bit of time addressing that public comment as well.

So what we've done for the purpose of this is I've tried to characterize the public comments that we got and organize them similar to the question structure. We asked one question about the use of elicitation, the appropriateness of the elicitation for this type of question and the scope and the subbullets here talk about the different types of issues and comments that we got with respect to that.

We got a number of comments about the general approach and let me just flip forward here. We asked about the analysis of the individual results and then the aggregation of individual estimates. So the subbullets indicate where we got comments related to these specific subtopics areas.

Now the things in bold what we've tried to do is go in and pull out again some of the more interesting comments within each of these areas and the ones in bold are what we're going to be talking about today; although if you look at the entire Appendix M you can see all the variety of comments in each of these areas.

I alluded to this. How have we responded to public comments? Again, we isolated comments. Again, if one letter had maybe three different issues we isolated each issue and address those individually. We're planning on incorporating all the comments and responses in the NUREG. It's going to called Appendix M. Appendix M the way it's structured has the general comments which are the ones that are applicable to not any one section of the NUREG and they're listed first. And then other comments are arranged categorically by the NUREG section that they largely refer to.

And we did a lot of modification or some significant modification of 1829 in response to these public comments. In many cases we modified or expanded our exposition to clarify the principal messages. A lot of the comments were associated with what are you guys trying to say here. So we wanted to make sure we were as clear as possible.

In some cases, people requested additional results and there were a large number of comments that wanted to see a comparison of operating experience. We've added these additional results and that comparison in the NUREG and there were also comments that asked how we should use and interpret the results. So we provided some additional guidance of that in the NUREG itself.

Let's delve into some of the public comments and, Lee, I think you're going to take over. Lee and I are going to be trading off here a bit.

MR. ABRAMSON: Yes. Tag team here.

MR. TREGONING: He's going to do some and I'm going to do some.

MR. ABRAMSON: The first one talks about justification of the elicitation process and what I've done here is just have a couple of excerpts from the comments. The first one says, "The elicitation is a series of informed but best guesses from knowledgeable experts with essentially no experienced data and limited physical models." And then the second one says, "Expert elicitation process differed in significant ways from the processes used in the well-regarded NUREG 11.50 elicitation." So that's the thrust of the comment and there's some related ones that you can look at yourself.

And our response is as I've ready indicated, the expert elicitation process itself is a well established technique. You use it when you know there is insufficient operational data or a lack of physical models and the elicitation of assumptions and the approach are documented. It's adapted from a NUREG 11.50 and NUREG/CR-5411. There are what I like to think of as standard approaches in this area. It's based on objective and technical subject matter.

MEMBER BLEY: Remind me what 5411 was. I forget which one that was.

MR. ABRAMSON: Which one is 5411?

MR. TREGONING: That's the flaw distribution study I believe.

MR. ABRAMSON: No, I think --

MR. TREGONING: Or is that seismic?

MR. ABRAMSON: I think that's the seismic one. I think so. I'm not sure.

MR. TREGONING: I'll get back to you on

that after the break to clarify what NUREG that is.

MEMBER BLEY: Okay.

MR. ABRAMSON: And in particular, we felt that what we used was compatible to elicitation framework. In other words, this was adapted to the particular kinds of 00

MEMBER SHACK: 5411 is radioactive waste repositories.

MR. ABRAMSON: That's right. Thank you.

MR. TREGONING: So you were wrong when you said it was a seismic study.

MR. ABRAMSON: I said it after you.

MR. TREGONING: We were both wrong.

MR. ABRAMSON: Experts can be wrong.

MR. TREGONING: The median was --

MR. ABRAMSON: You're right. So we felt that in short what we were using was in this area in a pretty well established technique. It was not something that we had invented. We just adapted it and our framework that was the subject, the way we framed the questions and so on was very sensitive to this. And the final bullet is that we would do a number of sensitivity studies to examine what the effect of different approaches and aggregation, overconfidence adjustment and a number of other areas would have been. And our best judgment, that is of the authors, was that results as we presented them was a reasonable way taking into consideration what we were trying to do, our objectives, and the kind of information that we had. So that was our response to the justification or the using the particular process that we actually had used.

MEMBER BONACA: It seems to me that the first comment was more focused on not necessarily to invalidate the elicitation process, but I understood it was more focused on what do you do with the elicitation results. What I mean is that you don't disagree that there is insufficient operational data and lack of physical models maybe. That's why you're doing it and I'm saying that --

MR. ABRAMSON: Right. You're right.

MEMBER BONACA: Then one thing you can do with the result of it is have an estimation of risk or whatever that you can get from that and then you put it on a shelf. And the other possibilities you're trying to modify the fundamental rule. I thought that that was the thrust from what I saw. Maybe I misunderstood it the first question.

MR. ABRAMSON: You're right. The first comment is to say we did this because we felt we had no choice. We had to get some kind of answer and this was the best way that we knew of. As a matter of fact, it was the only way that we knew of to get really some kind of answers which we could use for regulatory purposes.

MEMBER BONACA: I just meant to say that it doesn't seem to me that the commentator disagreed with your conclusions. It is more like he was concerned about the use you are making of this elicitation process.

MR. TREGONING: We've only -- This is only part of the comment. But I think the general thrust of that comment was essentially the basis for even using elicitation to begin with.

DR. BARTHOLOMEW: Okay. That's okay.

MEMBER SHACK: You should justify why it is appropriate to manipulate these best guesses as if they were drawn from sample spaces.

MR. TREGONING: That's right. We got a number of comments related to safety culture effects. This is something that we've discussed. We talked about safety culture with ACRS in the past. We got several comments related to that.

I'm summarize. Two of the important points here, although below you can see the related comments we got in this area, the first one is the panelist believe that safety culture can significantly affect LOCA frequencies at a specific plant. Therefore, this effect should be factored into the estimates or the uncertainty bounds. And the second is the elicitation focused on developing generic or average values. It's not clear how results are applicable to outlier plants, older plants, plants with safety culture problems, plants that have poor QA/QC or in general any plant that strays from the norm.

So these things in some way are related. The first comment says you need to account for these specific plant difference and your uncertainty estimates and then you have to make sure you have to identify how these are applicable to plants that may stray from your underlying assumptions.

In the response to this, we talked a lot of safety culture effects in the elicitation itself. So I wanted to at least initially here, this first bullet, provide some of the insights that we got from the panelists themselves. I mean, there is certainly recognition that safety culture effects are plant specific. And we asked when we talked about safety culture effects specifically the panelists to look at plant specific issues but then also what would be the effect of the median or the average safety culture of the industry.

So most of the participants expected a small improvement in the future in the median safety culture and that was based primarily on continued experience and technological advancements. There is certainly a recognition that the frequencies at the less safety conscious plants could be much higher than the median. And I mentioned this elicitation was conducted around the time of Davis Besse. There was a lot of discussion about effects of plants that may be less safety conscious or not have as strong a safety culture as sort of the median industry safety culture. There was an expectation though that one of the primary roles of regulatory oversight, at least in the panelist's opinion, process in and of itself is expected to provide some mitigation of the risk associated with plants that have deficient safety cultures.

And the other thing is that accounting for unknown plant deficiencies, it's difficult to estimate that and it didn't support a generic evaluation. Again, as I mentioned earlier, the objective of 1829 was to obtain generic or average values.

The SRM itself we were directed to provide realistically conservative LOCA frequencies, not bounding values associated with one or two plants. We did ask the panelists to consider these broad plant and system differences and materials, geometry, degradation, loading and mitigation. These are the things that they identified at least with respect to the material aging that would drive LOCA frequencies and there was agreement that at least among the panel that adequate commonality and these variables exist to support a generic assessment.

But there was a recognition that individual plants could fall outside of these generic predictions. And one of the things that we have to do to consider this factor is we have been directed as well to provide a reg guide to look at applicability of NUREG 1829 results to individual plants and what plants would have to do to demonstrate that they are applicable. So some of this issue, it will be covered in this reg guide.

One of the things we did in the NUREG as a result of this comment is we did make sure we clarified in a number of different sections how safety culture effects were considered and how these generic elicitation results should be interpreted as a result of again these safety culture differences. So we tried to provide some additional clarification in the NUREG to make it clear what the applicability of these results are.

CHAIRMAN APOSTOLAKIS: Was the Davis Besse violation of any regulations? Did they violate any regulations?

MEMBER MAYNARD: I believe they did. I think that they failed to report. I think they intentionally withheld -- Because their court case is going on and I'm not sure anybody would be able to comment on it, at least it appears as though that they had information available they did not use appropriately and that they --

CHAIRMAN APOSTOLAKIS: But they didn't use it appropriately because of a poor judgment or they knew that there was a regulation that was being violated?

MEMBER MAYNARD: I think some of the court decision will probably determine some of that as to how intentional it was. But they had information that hadn't been reported.

MEMBER SHACK: I'm sure their argument is it's poor judgment.

MEMBER MAYNARD: Had it been reported then it would not have -- they would have not been allowed to continue operating.

CHAIRMAN APOSTOLAKIS: Because it seems to me in this context that when you regulate or when you make a decision regarding the TBS, for example, you should take into account the possibility of poor judgment but not the violation of the regulations. Because if you start saying, "I will select the TBS by considering that they may violate the regulations" then where do you stop? I mean, that doesn't make sense to me. But to cover the possibility of poor judgment, it seems to me that, yes, you have to worry about it.

MEMBER MAYNARD: But I believe that's for the new rule to take into account and I think the NUREG it's right to take a look at this is kind of baseline. This is for the norm. The regulation, what regulations come out of that, our guidance is going to put some additional conservativism on this to account for things that may stray from the norm.

CHAIRMAN APOSTOLAKIS: Yes, but I mean, and I fully agree with that, but it seems to me that we have to make a distinction when we talk about safety culture between issues that are at the discretion of the management of the organization and they may decide to go one way which may not be necessarily our way and an outright violation of the regulations. That's very different. You cannot have a new rule that says now what if these guys violate all the regulations. What do I do? You can't do that. So it's really a very tricky area.

MR. TREGONING: I would agree. That's an important distinction to make.

CHAIRMAN APOSTOLAKIS: It is a distinction in my mind at least. Okay. So essentially what you did is your clarified better.

MR. TREGONING: Clarification. More exposition.

CHAIRMAN APOSTOLAKIS: Yes. Okay.

MR. TREGONING: The next, we've talked a little bit about this, but we got a few comments, in fact, a relatively large number of comments on variability that we saw among the base case estimates and again we described and talked about this already today and there was general concern with the large discrepancies that we saw in some cases between the PFM and the service history base case estimates. Some of the comments, they said the reason for the differences were not readily apparent. People questioned in some cases the six order of magnitude difference between the PFM, service history estimates for the BWR two base case through-wall cracking frequencies. Again, I showed this a little earlier.

And there was also questioning about the rationale for the service history estimates to justify the half order of magnitude frequency decrease with increasing LOCA categories. So there were questions related to that as well.

For the responses, again we talked about some of this earlier today, the differences between the PFM and the service history results often reflect basic differences in the various modeling assumptions and the conditions that were actually modeled. There was a recognition. Many of the panelists said this and I think this was something that I would agree quite strongly is that the PFM models, you have to be careful when you use any PRM models, and the accuracy is going to be suspect if they're not appropriately benchmarked either through service experience or some other way of benchmarking.

And the key here is they're not accurate for determining absolute LOCA frequencies unless they're appropriately benchmarked. This was one of the prime rationale for conducting the elicitation to begin with and another couple of points is PFM wasn't solely used by any single panelist to get their elicitation responses. PFM was typically used to extrapolate service history estimates for a bigger LOCA sizes or LOCA in the future. So quite often you saw people using PFM to understand what could happen in the future, relative differences with respect to the current service history.

MEMBER BLEY: I think your second bullet there is a really important one. Now the space cases though, some of them were pure PFM.

MR. TREGONING: Yes. That's right. And again, that was another reason for doing the base cases in that way to essentially illustrate this point.

The failure probabilities that the service history based experts used were justified. There were two different approaches for each of the two different team members. The first approach was justified because it was consistent with typical practice for dealing with these dating to WASH-1400 and also supported by the work of Beliczey and Schulz.

Approach number two didn't consider this assumption but actually analyzed service history and came up with these conditional failure probabilities as a result of looking at service history. And the way it was done is they looked at service history failure in lower class piping where you've actually had service failures up to larger LOCA sizes. So that analysis is actually documented in Appendix B.

What's interesting while these were different approaches they largely came up with the same final answer.

The resulting NUREG modifications, we really increased the amount of explanation and the discussion of differences in the base cases in this Section 4.2. So if you look at that now compared to the draft, there is a lot more explanation as to why these differences are there.

Accounting for mitigation. We got some comments and I think the ACRS has heard comments stating the fact that the elicitation didn't properly account for mitigation in some cases. We specifically got comments related to the fact that we didn't appropriately present a IGSCC mitigation measure at BWR plants since the early 1980s and these are just some of the various mitigation measures that have been applied in BWR plants and there were a few comments that essentially questioned our consideration of mitigation.

And I think these largely stem from a misunderstanding because the BWR-run base case, this particular base case did look at IGSCC failures, but it assumed that we had normal water chemistry in the plant and I think some of the commentors took issue with the fact that we assumed normal water chemistry when, in fact, there's no BWR plant that's operating with normal water chemistry. We defined the base case in this way because it was for convenience so that we could evaluate the effectiveness of a single mitigation strategy in the base case and the mitigation strategy we wanted to look at in the base case was weld overlays. So we had generic inspection requirements as required by 8801. So this sets the periodicity of the inspection.

This set the environment and we wanted to look at the effects of weld overlays. Of course, it was well recognized amongst the panel as well as the facilitation team that this base case isn't representative of present conditions and we did a large number of other sensitivity analyses to evaluate the effect of other mitigation strategies. For instance, we looked at operating experience to look at the effect of global mitigation. We did some PFM modeling to look at the differences between normal and hydrogenated water chemistry assumptions. So we did try to account for other mitigation and sensitivity analyses with respect to this base case.

We didn't talk about that so much today, but we did the base cases where we gave the single estimate. But then each of the base case team members, there were a variety of sensitivity analyses that they did as well and all that sensitivity analysis information was also supplied to the panelists to inform their subsequent elicitation responses.

However, my opinion would be that we did correctly account and recognize the effect of mitigation strategies. However, there still has to be a degradation mechanism that drives risk. There is still something that comes up to be the most risk significant and the panelists by and large for recirculation piping in BWRs they did identify IGSCC as the greatest LB LOCA risk.

Now certainly, there's a recognition that mitigation has greatly reduced the failure likelihood. However, two points to keep in mind, much of that original large recirculation piping has not been replaced and many of the pipes retain pre-existing cracks that initiated and grew before other mitigation measures were adopted. So you still have flawed components that are in place and there is some risk associated with the failure of those components.

In the NUREG again, we added some information to clarify how mitigation was accounted for in the elicitation and specifically how it was accounted for with respect to IGSCC.

Now we had one very significant comment that I wanted to spend a little bit of time on. This comment GC15 actually developed alternative LOCA frequency estimates and based on the evaluation that was done, they evaluated their own pipe and leak data and found that there was a significant difference between their data and the breaks spectrum failure frequencies from NRC study and other conclusions were while there are no large breaks in class one piping for the smaller breaks, the data clearly lies above the established break frequencies established in the NRC study. And then the punchline was that this indicates that we should not be revising 10 CFR 50.46 by introducing a transitional break size and reducing the mitigation capabilities of the plant's ECC system and defense-in-depth for the larger break sizes. So this one commentor took basic issue with the results that we got and felt that they weren't supported by their own analysis.

I wanted to show a little bit more in-depth in terms of what that commentor supplied and how they did their analysis and what I'm showing here, this is the PWR results and these three lines are our results from the draft 1829 and then these dots are the evaluation from the commentor.

And this is essentially how they did the analysis. They looked at all the pipe breaks using the pre-existing database that they had. They considered breaks. At least, they said they considered breaks only class one systems that can initiate a LOCA. They said they used similar break sizes as the NRC study and they said they normalized their failure similarly to us by the number of effective full power days for the complete from the fleet. So this initial analysis just considers pipe breaks as they are in evidence in that pre-existing database.

The second one looks at both break and leak evaluation. So you can see with the first study they stop here because there's no breaks greater than this bend between, I don't know, six and 12 inches. That's why the data stops there. But then when they look at adding in leak events, right, and they combine leaks and breaks together, they get these different curves. So this combines all the break and leak events in the database as a function of pipe size.

Now they agreed that this method may bias the results since there are only leaks for the larger pipe and not breaks. However, the commentor said this grouping could be conservative since pipes should not leak in the first place. So you see with their analysis it's quite a bit different and quite a bit higher than any of the elicitation results and again, these are the elicitation, the baseline results. So these have been geometrically aggregated.

Here is our response. I guess the one nice thing is the --

MEMBER BLEY: Yours is all break though. Right>

MR. TREGONING: Yes, ours are all break. The commentor also quite nicely provided the database that they used for their analysis.

CHAIRMAN APOSTOLAKIS: Can you go back?

MR. TREGONING: Sure.

CHAIRMAN APOSTOLAKIS: Again, clarification.

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: Looking at this figure, figure three, this long segment here of maybe 5 or 6 (10^{-4}) it starts at about 14 inches.

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: All the way to 32.

MR. TREGONING: To the biggest pipe, yes.

CHAIRMAN APOSTOLAKIS: To the biggest pipe and this is not the frequency of seeing a leak on pipes of this size, on this range of these sizes.

MR. TREGONING: Leak or break. But in this case it's leak.

CHAIRMAN APOSTOLAKIS: Break? Leak.

MR. TREGONING: It's leak or break for all the data. But in this case, it's just leak.

CHAIRMAN APOSTOLAKIS: It's just leak.

MEMBER BLEY: No matter how small the leak.

CHAIRMAN APOSTOLAKIS: Yes, independent of the size of the leak.

MR. TREGONING: Right. No matter how small the leak.

MEMBER BLEY: And their point is actual data?

MR. TREGONING: Yes.

MEMBER BLEY: The X or the dot.

MR. TREGONING: The dots are the middle of the range. The X is the actual datapoint from the database.

MEMBER BLEY: Okay.

CHAIRMAN APOSTOLAKIS: The X is the actual data --

MEMBER BLEY: The actual size of the pipe on which they found some size leak.

MR. TREGONING: Yes.

MEMBER BONACA: It must be -- This is summer?

CHAIRMAN APOSTOLAKIS: Wait a minute now. I mean, it runs from 13 roughly to 32.

MR. TREGONING: Yes, and it spans all these pipes.

CHAIRMAN APOSTOLAKIS: Then there is this little X that says actual pipe size. What does that mean?

MR. TREGONING: This X means this is the event that they found that they're binning everything in this, they binned all these pipe sizes into this single frequency.

CHAIRMAN APOSTOLAKIS: And they found one event?

MEMBER BLEY: One 28 inch pipe that had some leakage.

CHAIRMAN APOSTOLAKIS: And that's only leakage they found.

MEMBER BLEY: And that found that once in 1,000.

CHAIRMAN APOSTOLAKIS: In a range of all these. I see. But they did not show anything like that in the other bars.

MR. TREGONING: The other boxes they had more than one. They had more than one event.

CHAIRMAN APOSTOLAKIS: More than one.

MR. TREGONING: In the other boxes. But the other boxes were crafted similarly. They came up with a bin and they said they're going to look at events that fall within this bin and I'm going to treat them as being all the same frequency. So that's the analysis was done.

CHAIRMAN APOSTOLAKIS: I see.

MR. TREGONING: Okay.

CHAIRMAN APOSTOLAKIS: All right.

MR. TREGONING: Move on to the response.

CHAIRMAN APOSTOLAKIS: But then the -- Okay. That little, what is it, diamond means nothing. It just says this is PSU data.

MR. TREGONING: It's the middle of the range.

CHAIRMAN APOSTOLAKIS: But they just put it there to indicate that it's their data. It doesn't have any other meaning.

MR. TREGONING: No.

MEMBER BLEY: And on that last part, it

doesn't even mean that. They said we have pipes as big as 32 inches and we don't have any breaks in pipes bigger than 14.

CHAIRMAN APOSTOLAKIS: Right.

MEMBER BLEY: And that's just the middle of those two points.

CHAIRMAN APOSTOLAKIS: I think it's not indicated in the -- they put it in the middle.

MEMBER BLEY: Yes, that's all it is.

CHAIRMAN APOSTOLAKIS: But it's an indicator that it's a PSU data if you look at the legend on the right.

MEMBER BLEY: Right.

CHAIRMAN APOSTOLAKIS: But this is our data.

MEMBER BLEY: Their data is one point.

CHAIRMAN APOSTOLAKIS: One point, yes.

MEMBER BLEY: And you're applying it to that --

CHAIRMAN APOSTOLAKIS: This guy has objected to the revision of 50.46 many times. Right?

MR. TREGONING: That's right. But regardless of that, we try to deal with the substance of the topic.

CHAIRMAN APOSTOLAKIS: I understand what you have to do.

MR. TREGONING: Okay. So I think the authors of the report, we disagree with the original comment assertions and again the nice thing about it is the commentor supplied the database. That was nice because staff was able to go in and independently evaluate the database and when we saw the database immediately I was concerned about the database itself because it looked like it was this very old database that was put together originally by SKI sponsored work. But some of the earliest pipe data was chronicled in the SKI 96.20 report that was developed by Bush, et. al and it was essentially an LER search of failures in the U.S. nuclear plants up to about 1995.

You can see with the database that there were no events beyond like 1995. And the concern was that there had been independent review of this database that identified a large percentage of what were erroneous records. When the database -- When we got the database, there was concern about its integrity. So we went back and looked at all the events that were identified in the evaluation that could be classified in breaks in that database and there's 19 events. And what I had done was taken those 19 events, go pull the original source documentation for several of these events and then also checked the events using a validated database of this OPDE database. This is an international database that's been put together. It's part of the CSNI sponsored program.

CHAIRMAN APOSTOLAKIS: Validated/ unvalidated, can you explain what that means?

MR. TREGONING: Validated means the database records have been checked, QA'ed, by an independent team. They're all referenced so that all of the references have been validated and checked. So that's what I mean by validated there, a database that's received some level of QA associated with it versus an initial compilation of possible events.

For this database, for instances, when there's a new event it's entered into the database as unvalidated and then people are required to go back and pull all the source documentation to validate all the information that's in the database. And this is a current database that the rev I used was dated March 2004. But it's something that's updated at least once if not twice a year and this database is being developed as part of an ongoing collaborative international effort between the U.S. and about 12 or 13 other countries in Europe and Asia.

But again, I went back and pulled source documentation as well and when I did that found, similar to this review, a lot of inaccuracies in the database.

CHAIRMAN APOSTOLAKIS: But, Rob, okay. There are inaccuracies. But their fundamental question is was there a leak in the pipe of that size, that little X we saw. Now whether the date was wrong and so on, who cares? Was there a leak?

MEMBER BLEY: I think what he's showing here is some of, not counting that one, these ones that were listed as actual breaks may have been valves opening, that sort of thing. Is that what?

MR. TREGONING: There were several events that couldn't be referenced to a verified failure, either through -- This database had references as well. When you went back and pulled the reference, they did not indicate that there was a pipe failure.

MEMBER BLEY: A pipe failure.

MR. TREGONING: This happened in some

cases. A lot of times there was incorrect event dates, references of pipe sizes or break sizes. All of these -- If it's an incorrect break size or pipe size, that affects what bin something gets put in. Right? And the other thing, the failure classification itself, whether something was a leak, a rupture or severance, it was found to be inconsistent with a lot of the source documentation. So there were a lot of questions about the integrity of the database.

MEMBER BLEY: Can I ask one particular question? Maybe you'll get to.

MR. TREGONING: Yes.

MEMBER BLEY: From what you looked at, were you able to extract a subset of the data that clearly were breaks?

MR. TREGONING: Yes.

MEMBER BLEY: And did you would plot that?

MR. TREGONING: There were other issues with the analysis I don't want to talk about here.

CHAIRMAN APOSTOLAKIS: But in the previous slide, I have a minor comment.

MR. TREGONING: You have a comment.

CHAIRMAN APOSTOLAKIS: Yes.

MR. TREGONING: Okay.

CHAIRMAN APOSTOLAKIS: I would say in your first bullet the authors disagree with the regional comment of items one and two on slide 17. Item 3 is a policy issue and you really don't want to disagree with that.

MR. TREGONING: That's a fair point.

CHAIRMAN APOSTOLAKIS: Okay. You are dealing with a technical comment.

MR. TREGONING: Dealing with a technical issue. That's correct.

We did two things. We looked at the database and identified these problems but then we also looked at the events that were identified in the database and then tried to match them up with events that were in this OPDE database and we actually analyzed those. Now of the 19, we couldn't even match four of them. So there was no known failure that showed up in this database. What we tried to do, we looked at for pipe breaks at the listed plant in a similar system that was fairly broad or fairly flexible in terms of matching these events.

Of these 15, none of these break events occurred in unsolable reactor coolant pressure boundary piping. So all the break information, what tended to happen was that it was reported as being in class one system but usually it was in a class two or class three system, a lower grade of piping. And this confirms that the analysis that we had done as part of the elicitation. When we did the elicitation, we did all of this same work where we used actually this database to provide all the precursor information of leaks as a function of system and size. All of this information had been developed previously. So when we saw this analysis that was so different than what we had done, we obviously had questions about why is it so different.

If you look at the leak event side, I've talked about the break events here, but I also did a similar analysis just on the leak events and many of the similar issues from the break data also sort of clouded the leak events. The other point, leaks are clearly not breaks contrary to the contention and the comment and this is an important point of the elicitation that the differences between the leak and the rupture crack sizes increase with pipe size. So the largest pipes are more likely to leak than they are to break. And we have more margin against failure after the leak appears in those bigger pipes.

One of the things we did with that as a result of this is we did make sure we added a section in NUREG 1829 that compared these results and showed how they compared with operating experience where we did our own evaluation of what the operating experience would show.

CHAIRMAN APOSTOLAKIS: I don't understand what the point that these reviewers are trying to make is. Yes, so there was a leak. But it seems to me that's something we expect. Right? And we have a leak before break principle. What is the message there? Yes. Okay.

MR. TREGONING: That's just one in that the elicitation was not representative of service experience. That's the first message.

CHAIRMAN APOSTOLAKIS: If you are looking at actual breaks and they are adding this extra bar with the leaks.

MR. TREGONING: Here, this is breaks only.

CHAIRMAN APOSTOLAKIS: Yes, this is breaks. But then --

MR. TREGONING: The message here is that the elicitation is not consistent with operating experience.

CHAIRMAN APOSTOLAKIS: And you provided a series of arguments why this is part of it.

MR. TREGONING: Why we think it is.

CHAIRMAN APOSTOLAKIS: But then when we go to the leak --

MR. TREGONING: Then when you go to this one --

CHAIRMAN APOSTOLAKIS: We know that there will be a leak. Right? That was the Livermore study of the `80s that convinced everybody that there will be a leak before break. Is that true, Bill?

MEMBER SHACK: We made decisions based on that.

CHAIRMAN APOSTOLAKIS: And we made decisions based on that. So just to show this extra long bar, I don't know what the message is. Yes, there was a leak. Sure.

MR. TREGONING: I think this is the commentor's method. Again, they recognize that they could bias the results. However, in the comments opinion, this is a conservative evaluation and at least the commentor believes the pipes shouldn't even leak in the first place.

CHAIRMAN APOSTOLAKIS: It would be nice for them not to leak.

MR. TREGONING: This is a presentation of what the commentor --

CHAIRMAN APOSTOLAKIS: I understand that the first two or three bars are intended to mean something because they include breaks. But the last one I'm not sure that it's a meaningful bar with the leaks.

MEMBER BLEY: If you're interested in breaks, the previous slide has all this supposed break data.

CHAIRMAN APOSTOLAKIS: Yes, I know. That's what I'm saying. The first ones are probably more meaningful. Now these guys are at the university. They didn't have the resources to do what you did, go back and try to validate the database. So they just took --

MEMBER SHACK: He knew that many of those were in secondary systems from FAC. That's in the description of his document. But he just punched ahead.

MEMBER BLEY: Is this the same guy of the same name who was a Westinghouse thermal hydrologist?

CHAIRMAN APOSTOLAKIS: Yes. And he's also listed here on the slide five.

MEMBER MAYNARD: I think it's good that he provided this data. He provided an opinion. I don't think it really fits here. I think you've done a good job researching the data that he provided, see what was applicable and what wasn't applicable and I agree with you that his -- doesn't really go to mixing leak in here and small leaks and stuff that I agree with the way you're responding to this.

MEMBER BLEY: Yes. Me, too.

MR. TREGONING: Again, any comment, we obviously took every comment seriously and you want to make sure that any comment that you got that it doesn't undermine what you did. So that's why we felt like we had to go back and really look at these things to verify that.

CHAIRMAN APOSTOLAKIS: You have to. Yes. No question about it. I'm just wondering about their argument. I mean I can see exaggerating the number of failures and maybe taking some from another system and putting them in. But the leak is a mystery to me. I mean, I don't know.

MEMBER BLEY: I don't see any difference.

You take systems that you know are inferior and have fluids that attack the material. The other one you say where there's smoke there's fire.

MEMBER BONACA: Do you have information about that leak? What plant was that and the event?

MEMBER MAYNARD: Was that the Surry plant?

MR. TREGONING: No. It's not Surry. I can pull it up. I don't have it off the top of my head. It's not Summer though because again the database he had stopped about `96. I forget. I can't remember.

CHAIRMAN APOSTOLAKIS: Okay. Let's go on.

MEMBER SHACK: Yes, we've tripled the number of leaks in 28 inch pipes.

MR. ABRAMSON: Okay. The next comment deals with the interpretation of extremely low estimates. Many of my numbers are extremely low. There's no question about it and the issue in the commentor's words are "there are many LOCA frequency estimates provided in the report, so low as to be unbelievable. No one should believe frequencies orders of magnitude longer than the existence of the universe." And that's a direct quote.

CHAIRMAN APOSTOLAKIS: And I agree.

That's right.

MEMBER SHACK: Is that your comment, George?

MEMBER MAYNARD: No, it wasn't.

(Laughter.)

CHAIRMAN APOSTOLAKIS: Yes. I used a pseudonym, GC.

MEMBER MAYNARD: Not GA. Right?

MR. ABRAMSON: Okay, and this is an important comment even though we disagree with it and you'll see why in a minute because this is not the first time that I've heard something like this from people, the NRC, I'm sure, elsewhere. And I think the response is -- I think it's important to distinguish between whether the analysis is credible and what the interpretation of the result is.

And our response is as follows. Our general comment is the validity when estimate depends on the assumptions in the modeling approach and I think an example here, an analogy, is useful. Suppose you decide to, say, play the lottery and you're going to buy three tickets in three successive lotteries, one ticket in each lottery. Let's say for the sake of argument that each one has one chance in a million of winning. So you have three tickets, each with one chance in a million of winning.

MEMBER SHACK: Let's hope it's not a fixed lottery. So it's really true.

MR. ABRAMSON: What? I'm sorry.

CHAIRMAN APOSTOLAKIS: That's an issue of --

(Laughter.)

MEMBER SHACK: Let's hope it's not a fixed lottery.

MR. ABRAMSON: Right. We're assuming this is a fair lottery here and so on. But you decide to buy, somebody buys three tickets in three successive lotteries. The probability of winning all three times is 10^{-18}. Okay.

An extremely low number. Now what conclusion do you draw? Well, it's in incredible event. It's not going to happen in other words. However, I would argue that the analysis is absolutely correct. I think everybody would agree with me that the number is correct and the interpretation is that it's not going to win. So the extremely low frequency means that the event will not occur, but not that the analysis is incorrect. In other words, we believe the number, but the question is with the interpretation.

So I think that this is -- that the comment itself betrays a misinterpretation of how you're supposed to interpret these low numbers. And what we did do is we modified the NUREG to put in this example and maybe to put it in a few other words to make this point. You have to distinguish between whether the analysis is credible and whether the event is credible.

CHAIRMAN APOSTOLAKIS: I think your example is correct. But you have to give credit to the commentor here. I don't think that person really would question your example or other examples. You know, if I throw 1,000 dice and I want all of them to be sixes, I'm not going to do better than that. He probably meant that in the real world, the physical world, you always have this possibility that something that you haven't thought of might happen and so on.

So, yes, the 10^{-15}, like we said earlier, or something, that's the result of a particular analysis. Now whether this is the actual number that would apply, we really don't know.

MR. TREGONING: Right and I think that's a good point.

MR. ABRAMSON: But let me respond to that. I would tend to disagree with that. I think the commentor really believes that because these numbers are so low, just because of their magnitude, they are not believable. They should be dismissed as being this way.

Now if what you say is correct, of course, you have a whole issue of completeness.

CHAIRMAN APOSTOLAKIS: Yes.

MR. ABRAMSON: Are there things that you haven't thought of? The commentor did not talk about this.

CHAIRMAN APOSTOLAKIS: I suspect --

MR. ABRAMSON: And as a matter of fact, we didn't have any -- The commentor did not talk about it and say maybe this number is so small it's not incredible. Maybe there are some things we didn't think of that would make the actual frequency larger. He didn't say this. He was -- The way I interpret his comment and I said I've heard this before and that's why I'm particularly sensitive to it about another study I worked on a few years ago that our numbers are so small that therefore the analysis itself is suspect that gave rise to these numbers. So

I want to try to clarify this.

You're absolutely correct. You want to look at things we haven't thought of and you're absolutely right about this. That's another issue and an important issue. But I think that some people in my judgment and as I said I was sensitized by this previous knowledge of this. I think that you can dismiss an analysis strictly because the numbers are small and that's what I'm objecting to.

MEMBER BLEY: Lee, I'd like to offer something in addition. I understand what you're saying.

MR. ABRAMSON: Yes.

MEMBER BLEY: And there are numbers very small and you've shown an example. The other pieces of this, we're looking at a study about pipe breaks and if I see numbers about pipe breaks, numbers that small make me very suspicious.

Now the only numbers that were that incredibly small were some of those calculated numbers, at least, that I recall seeing like the ones you showed. You had a bullet on a slide a little while back that said nobody made their pipe break estimate based solely on the PFM calculations. I think as a second piece of this that kind of needs to be here that those were mechanistic calculations of a particular thing and nobody made their overall estimates based on those. That goes a long way to addressing what George brought up.

MR. TREGONING: I think the first two bullets in the response, I think, the validity of the estimate depends on the assumptions and modeling approach. We would agree that that's essentially getting at what you're saying in that you can model something, right, and within the context of the accuracy of your model if you come up with a very low estimate, the interpretation of that is within the confines of that model the assumptions and the approach, if they're accurate, the implication is that failure due to the modeled conditions will not likely occur. That's really the implication.

It doesn't necessarily mean that you've modeled the right thing.

MEMBER BLEY: Exactly.

CHAIRMAN APOSTOLAKIS: But, Rob, I think the message here is that in your response in addition to including the example even though maybe he's right, the commentor did not seem to address the issue of completeness, you should.

MEMBER SHACK: Their actual response does.

CHAIRMAN APOSTOLAKIS: Okay. Because it says here only modified section to include --

MEMBER SHACK: But you look at the one in Appendix M.

CHAIRMAN APOSTOLAKIS: Okay.

MEMBER MAYNARD: I would contend that basically this is consistent with the ACRS's position, maybe different tone and maybe went a lot further. But we've always taken the position or you have that when you get numbers that are incredibly low that you can't believe, it does say it's very low probability. The position we took with the ACRS was we think for the new rule or propose rule, this is a fine way to go, but we still want to see more defense-in-depth. I think you might want to work something like that into the response that --

I hate to say that basically what we're saying is that it can't happen. There's an incredibly low probability, but I don't think we want to say that it can't happen because we're asking for some additional assurances on defense-in-depth.

MR. TREGONING: Again, it's not that the

failure can't happen. It's just that the analyzed conditions are very unlikely.

CHAIRMAN APOSTOLAKIS: Right.

MR. TREGONING: And I would want to be careful.

MEMBER BONACA: The example is good. I think the example in the text is good because it clarifies. It separates into issues and I think that should be sufficient to put in perspective.

CHAIRMAN APOSTOLAKIS: The truth of the matter is that rare events do exist and this is an example.

MEMBER BONACA: Right. Absolutely.

CHAIRMAN APOSTOLAKIS: It's that when I see 10^{-15} automatically I'm closing my eyes. No.

MEMBER BLEY: I think Bill is right. If you go back, there's a full page response, not two bullets.

CHAIRMAN APOSTOLAKIS: Okay. If there is, there is. So maybe the slide doesn't show it. You should expound a little there.

MEMBER SHACK: That's what they're talking about and Appendix M is going to be there in its full glory.

MR. TREGONING: Yes, Appendix M is going to be there.

MR. ABRAMSON: The slides won't be. Appendix M is. This report, the version we have now is a current draft.

CHAIRMAN APOSTOLAKIS: Yes.

MEMBER BONACA: Probably they are having three LOCAs of the same part at the same time.

CHAIRMAN APOSTOLAKIS: Well, it wouldn't --this slide.

MEMBER BONACA: Yes.

MR. TREGONING: And depending on what we -- We need to figure out and we need to present to the main committee.

CHAIRMAN APOSTOLAKIS: Yes.

MR. TREGONING: So that will be particularly appropriate depending on what --

CHAIRMAN APOSTOLAKIS: The Commission. I mean you are going to make presentations to the Commission. All I'm saying is put on the slide what you did in the appendix. For heaven's sakes, it's not --

MR. TREGONING: It can be shown anywhere.

(Several speaking at once.)

CHAIRMAN APOSTOLAKIS: These are part of the record now. Right?

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: Yes.

MR. ABRAMSON: All right. And this was a comment. I think we've already said a lot of what is in the response. The issue was the geometric mean tends to hide the diversity of opinion or degree of uncertainty in the results. And I think that this commentor misinterpreted what or didn't completely understand or maybe we didn't explain it well enough how we dealt with uncertainty and diversity.

We distinguish between the two of them. Uncertainty is captured by the 5^{th} and 95^{th} percentiles. That is the individual uncertainty in the individual results, the individual experts. And the diversity just refers to the differences between the experts and that's captured by the confidence bounds and the geometric mean is just a way to aggregate these things. The geometric mean is just a way to get a group estimate. But we do capture the uncertainty and diversity in other words.

MEMBER BLEY: And in most places you show them altogether.

MR. ABRAMSON: Yes.

MEMBER BLEY: I'm not sure I see them anywhere you capture them --

MR. ABRAMSON: I said I think the geometric mean is just a way -- The purpose of the geometric mean is not to show uncertainty or diversity basically. It's an aggregation technique. And so therefore we didn't make any modifications in the NUREG. We felt we already adequately explained it.

All right. Then there was a number of comments, of course, about the overconfidence adjustment. The issues, one issue was it didn't appear to be a basis for it. Another one is the opinions of the panel members were modified, increased, by the authors. And furthermore, it introduced a conservative bias.

So our response was, first of all, as I've already discussed there is strong empirical evidence of overconfidence and then as far as the second issue is concerned, the opinions of the panel were modified, in effect one of the reasons that we chose the error factor correction was that we didn't have to make any judgment about whose opinion to modify. We just let the results speak for themselves for those people. We compared them --

CHAIRMAN APOSTOLAKIS: Larger or smaller?

MR. ABRAMSON: The larger. The opinions of the panel members were modified increased. This is a quote from the --

CHAIRMAN APOSTOLAKIS: No. On response number two.

MR. ABRAMSON: Our factors larger than the median. That's correct.

MR. TREGONING: No, that's the other way. Yes, the other way. Smaller.

CHAIRMAN APOSTOLAKIS: Smaller.

MR. ABRAMSON: Smaller. You're right. That's a typo. Thank you. Yes, you're right. The error factor is right. It's only the smaller ones that are. Correct.

CHAIRMAN APOSTOLAKIS: Now coming back to our earlier discussion here, Lee.

MR. ABRAMSON: Yes.

CHAIRMAN APOSTOLAKIS: I would state your number one, strong, empirical evidence of overconfidence, and then number two, I would say that what you have done is one way of trying to deal with the issue rather than -- You know, the implication is that this is their way, that you are proposing their way of dealing with overconfidence and I think it's just a sensitivity analysis.

You did your calculations. You saw there were only -- The maximum was 90 percent change which was really not a big deal with category six. In other words, make sure that the reader understands that you are not saying that this is their way of dealing with overconfidence. This is one of the ways and you did it to gain some insights.

MR. ABRAMSON: Yes, but the comment was on the specific way that we had done it.

CHAIRMAN APOSTOLAKIS: Yes.

MR. ABRAMSON: And the comment said -- And so we tried to address the comment itself as it applies specifically to the error factor correction.

CHAIRMAN APOSTOLAKIS: I understand that, but you can still broaden it a little bit and say we appreciate that there is no unique way of doing this.

MR. ABRAMSON: But we said that extensively in the report.

CHAIRMAN APOSTOLAKIS: Okay.

MR. ABRAMSON: We said that with the sensitivity studies. We're just trying to respond to the specific comments here.

CHAIRMAN APOSTOLAKIS: All right. If you think it's --

MR. ABRAMSON: And actually, the comment number two, he says "the opinions of the panel members were modified by the authors." They were not modified at all. We did the -- We were the ones who did the -- devised the error factor correction and applied it. But yet the specific ones depended upon on the error factor and so on and so forth.

MEMBER BLEY: Did the experts agree with you doing that?

MR. ABRAMSON: I think the experts generally felt that this was a reasonable way to do this, yes.

MR. TREGONING: Let me temper that a bit.

MR. ABRAMSON: Okay. I'm clearly biased in this.

CHAIRMAN APOSTOLAKIS: It was with a shorter error factor disagreement.

MR. TREGONING: At least a couple of the --

MEMBER SHACK: Which experts are we talking about? The expert reviewers? Or the experts on the panel?

MR. TREGONING: The panel experts. At least one, maybe two, of them were greatly offended because they thought that their results shouldn't have been tinkered with at all and quite frankly I wouldn't have expected them to behave any other way.

CHAIRMAN APOSTOLAKIS: That's again an argument for telling the world that we will do a number of sensitivity analyses with your results with your input because we want to gain insights. What happens if we do this? What happens if we do that?

MR. TREGONING: And that's how the NUREG is structured. We provide the baseline estimates which is just the strict analysis and then there's a whole big section about the different sensitivity analyses.

CHAIRMAN APOSTOLAKIS: You can send a private letter saying this is sensitivity. Anyway, I think it would help here to put that. But that's fine.

MR. ABRAMSON: Okay. And finally the last point about conservative, I would say on the contrary not adjusting would be nonconservative because this is strong evidence that -- I said we felt we could not

not -- an adjustment.

MR. TREGONING: I have the next one, comparisons with service experience. A number of comments related to this. Several of them said that the SB LOCA estimates were too high and that they are approximately one order of magnitude higher than NUREG CR 57.50. The implication being that there's one SB LOCA every four years entered with U.S. fleet. And the basic contention of these commentors were using the 1829 estimates and existing PRAs which lead to unwarranted impacts that are not supported by Operation's experience.

So again it's interesting. I always figure you're doing your job right if you equally offend people that your estimates are either too low or too high. So here's a set of comments that said our estimates were too high.

CHAIRMAN APOSTOLAKIS: So these guys go the opposite way.

MR. TREGONING: They said our small break LOCA estimates were too high, especially with BWRs.

MEMBER SHACK: At least one of these is NEI.

MEMBER BONACA: So if you --

CHAIRMAN APOSTOLAKIS: It's the -- go ahead.

MEMBER BONACA: If you draw in active systems LOCA, these numbers will come anyway. They will come closer to even higher than what they have shown.

MR. TREGONING: Yes, that's true. But the active systems LOCAs are modeled separately in PRAs as well.

MEMBER BONACA: I understand that but I'm saying that insofar as comparing to service history experience I mean they should have thrown in active system failure, too.

MR. TREGONING: Yes. I think we wanted to consider the total LOCA risk. But, yes.

CHAIRMAN APOSTOLAKIS: But the PSU comment was the opposite, was it not? I mean, here they are telling you that the smallest --

MR. TREGONING: The smaller estimates are too high.

CHAIRMAN APOSTOLAKIS: Yes, and Pennsylvania State said they were too low compared to their experience, your estimates.

MR. TREGONING: But this isn't the Penn

State comment. This is another comment.

CHAIRMAN APOSTOLAKIS: I know. But it's the opposite.

MR. TREGONING: Right.

CHAIRMAN APOSTOLAKIS: Aren't the two comments opposite?

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: Okay.

MR. TREGONING: It's my comment that we pleased another one.

MEMBER SHACK: It's the geometric mean of the two comments.

MR. TREGONING: We please no one. We were too low in some people's opinions and too high in other people's opinions.

MEMBER BLEY: Your point on this one in Appendix M though and I just wanted to bring this up thinking of how this will be used is that, yes, at least I'm looking at the ones from the industry here. You're pointing out that this includes the steam generator tube ruptures and since they're included, I guess if you're somebody over on NRR you almost have to take them apart again for certain issues and I'm not sure in here it gives you a way to take those apart.

MR. TREGONING: That's a good -- You just set me up beautifully for these slides.

MEMBER BLEY: Okay. Good.

MR. TREGONING: I appreciate that.

MEMBER BLEY: Because I don't remember seeing it.

MR. TREGONING: We went back and looked. If you look at 1829 and 57.50, they are generally consistent.

CHAIRMAN APOSTOLAKIS: Who did 57.50?

MR. TREGONING: This was an INEL study.

CHAIRMAN APOSTOLAKIS: Okay.

MR. TREGONING: Initiated there.

CHAIRMAN APOSTOLAKIS: Bill Galyean was involved?

MR. TREGONING: Yes. He did the studies. Yes. He was our bridge for those studies.

CHAIRMAN APOSTOLAKIS: Okay.

MR. TREGONING: The steam generator tube estimate between these two were virtually identical, very change. The BWR SB LOCA estimates were also similar. The only elevation was in PWR SB LOCA estimates. They're higher than 1829 by approximately a factor of five.

Now again, why is that? Well, the panel elevated those estimates based on concerns with BWSCC and the increased likelihood for small piping failures for BWR. So those increases are actually consistent with the qualitative responses and rationale that we got from the panel.

We also went in and did an evaluation with operating experience that we detail in the NUREG to show that the estimates even though there is rationale for this elevation that even with the elevation they're still consistent with operating experience and I mention that the differences that we do have are supported by this quantitative and qualitative information provided by the panelists.

So what did we do as a result of this? Well, first of all, like you had indicated, initially we had combined the steam generator tube and all others. We've now separated those. So you have separate steam generator tube rupture frequencies as well estimates for all other PWR small break LOCAs. So we show the combined as well the split estimates and there's a whole section that talks about that.

We had more extensive comparison between the estimates and historical results and then we added a whole new section on comparison with operating experience. So actually the most significant modifications that we got that are in the NUREG are really resulting from these types of comments. We thought it was important to go back and do this operating experience comparison.

Lee, do you want to pick up?

MR. ABRAMSON: Okay. Just quickly. The aggregation again, the comment was the geometric mean is used, this is an observation. Aggregation, the arithmetic mean is used in NUREG 1150 and 57.50 and that tends the diversity of opinion of uncertainty in the results which we do not if we're ready and our response was we felt that it was appropriate for the study again because I said the group estimates should be in the middle of the group and also this came to light, I mean, many commentors are outside and inside the NRC and they said why don't we use 11.50 results because it's a precedent and that's, of course, something to consider.

But the draft NUREG was published and out for comment, it was brought to our attention that there were some previous studies, actually NRC sponsored work, dealing with similar situations where you have a very wide, based on expert elicitation, range of results and in that case, they specifically used the median. So we have it and we put this in the current NUREG, it is in the NUREG now, these references to previous work which in this particular case and that's our case where we have a very wide range, several orders of magnitude, where the median is recommended. So we felt that that was a precedent for our approach.

And again, I said the point is geometric mean approximates the median. Even though they recommended the median here, as I discussed before, the geometric mean is for the data we have. It essentially gives you the same results. And as far as the issue with diversity and uncertainty, I've already dealt with that in a previous comment and then the resulting NUREG modification. What we did was we added different discussion and also references in the report to this previous recommendation of using the median for data such as we have.

MR. TREGONING: Okay. So the last couple of slides, this last slide, we wanted to provide some of the more significant to the NUREG and this was really to support a little bit people like Professor Apostolakis and Dr. Shack who had read the draft and I'm sure they were interested in focusing on the areas where the most significant changes occurred in the NUREG. So we tried to identify about ten issues or ten areas that were most significant.

Of all of these, I think the ones that are most significant is this new Appendix M and then this new section where we compared the estimates of the operating experience.

CHAIRMAN APOSTOLAKIS: I think that's excellent, I mean, the comparison with the experience is.

MR. TREGONING: There was a clear hole. I mean, sometimes, we didn't see it at the time, but it was a clear hole that we've gone back and filled.

The one thing I will say is right now Appendix M has all the NRC as well as the public comment.

MR. ABRAMSON: Right.

MR. TREGONING: It's not clear to me in the final NUREG if we are going to strip the staff comments out and deal with them separately. We typically deal with staff comments internally. So the final Appendix M may only have the public comments. That's the only thing that's in flux at this point in terms of the final NUREG. But we wanted to provide you with Appendix M in draft form so you could see everything.

CHAIRMAN APOSTOLAKIS: Good.

MEMBER SHACK: Yes. I mean, those discussions are very interesting. I think it would be a shame to leave them out of the final document.

MEMBER BLEY: I do, too. Some of those are the most interesting ones in there.

CHAIRMAN APOSTOLAKIS: That's very unusual though.

MEMBER SHACK: Yes, it certainly is unusual.

CHAIRMAN APOSTOLAKIS: Rob is right.

MR. TREGONING: It is unusual for us to do that.

CHAIRMAN APOSTOLAKIS: A NUREG report that reflects the staff's views. Right. So to say that in an appendix, but then some members of the staff disagree with the staff.

MEMBER BLEY: Well, I disagree with something that existed two years and --

CHAIRMAN APOSTOLAKIS: That's the staff.

MR. TREGONING: We can do it in a way where we could potentially keep the comments and then make them anonymous essentially. That would be an area that if ACRS felt strongly about something that conceivably you could recommend to us.

CHAIRMAN APOSTOLAKIS: I'm not sure.

MEMBER BLEY: I think something like that would be good because there is some very useful discussion there that's not in the main report.

CHAIRMAN APOSTOLAKIS: If it's very useful, then why don't you move it to the main report, the essence of it?

MR. TREGONING: It has been. The essence of it has been moved to the main report.

CHAIRMAN APOSTOLAKIS: So if it has moved. I just don't know that publishing a NUREG report from the staff to have comments.

MR. TREGONING: It's not --

CHAIRMAN APOSTOLAKIS: This in an internal process.

MR. TREGONING: Right.

CHAIRMAN APOSTOLAKIS: As a result of the internal process, here is the public document. So if the essence of the comments is already in the main report, I would, I mean I don't insist, but I would say --

MR. TREGONING: Normally, what we would do and what we'll do anyway is all the staff comments that we got we would peel those out and say we got your comments. This is our response and this is how they were addressed in the NUREG. Here's the updated NUREG to account for your response.

CHAIRMAN APOSTOLAKIS: Right. And then it goes to the ADO.

MR. TREGONING: That's how we typically do it and then we give the offices or the people that commented one last chance to say are there any other modifications that they see as a result of this.

CHAIRMAN APOSTOLAKIS: Right.

MR. TREGONING: And we'll certainly do that. But it was just a question of what ends up in the final Appendix M.

CHAIRMAN APOSTOLAKIS: Good. So it was good and are there any more comments from or questions from the members? Are you going to stay this afternoon here?

MR. TREGONING: Cool.

CHAIRMAN APOSTOLAKIS: Okay.

MR. TREGONING: I'm sure there might be related questions.

CHAIRMAN APOSTOLAKIS: There might be. I don't see the seismic guys here. Nilesh is not here.

MEMBER SHACK: They bolted.

CHAIRMAN APOSTOLAKIS: But he's coming back.

MR. TREGONING: He's coming back.

CHAIRMAN APOSTOLAKIS: Can we start at 12:15 p.m.? Is 45 minutes okay?

MEMBER BLEY: You mean 1:15 p.m.

CHAIRMAN APOSTOLAKIS: 1:15 p.m., yes. So the answer to my first question is no.

(Laughter.)

CHAIRMAN APOSTOLAKIS: You don't have to -- Just say no.

MEMBER BLEY: We are actually ahead of schedule.

MEMBER MAYNARD: Yes, we are.

MEMBER BLEY: Because we finished --

CHAIRMAN APOSTOLAKIS: We're going to lose at least one member before 4:00 p.m.

MEMBER BLEY: But we were scheduled to get to the point we're at at 2:45 p.m.

CHAIRMAN APOSTOLAKIS: I really want to have the subcommittee discussion before you go.

MEMBER BLEY: It's going to be hard.

CHAIRMAN APOSTOLAKIS: It's going to be hard.

MEMBER SHACK: Take a half an hour for lunch.

CHAIRMAN APOSTOLAKIS: We can do that, too. So we start at 12:10 p.m.

(Laughter.)

MEMBER MAYNARD: No. 1:10 p.m. we might, but 12:10 p.m. you can't.

CHAIRMAN APOSTOLAKIS: Let's start at 12:00 noon.

MEMBER MAYNARD: You can't make up for this --

(Several speaking at once.)

CHAIRMAN APOSTOLAKIS: I'm not sure Nilesh needs all this time for his presentation. I mean if he gets in --

MEMBER SHACK: It depends whether we want to understand what --

CHAIRMAN APOSTOLAKIS: The esoteric -- of his structures -- Unless Dr. Shack -- I don't think the rest of us will. So let's say 1:15 p.m. I think that's reasonable.

MR. TREGONING: Before we break, we're scheduled to come for main committee on the 6th. What would you like us to present? We'll have an hour at main committee.

CHAIRMAN APOSTOLAKIS: An hour only? Including the seismic?

MR. TREGONING: No. We have two hours total. Right? Yes, an hour each.

MR. ABRAMSON: We have 45 minutes for our presentation and 45 for the seismic.

MR. TREGONING: Okay.

CHAIRMAN APOSTOLAKIS: I think you should outline again the main approach without as much detail and you guys can correct me here. This is important.

For the full Committee meeting, I would suggest that you give us the main results, the two or three slides you have with the various results, have some discussion on the various -- I would say all of the sensitivities, the way you handled the geometric mean, arithmetic mean, overconfidence, all that stuff because that the Committee it seems to me is interested in how these results will be used in rulemaking.

MR. TREGONING: Right.

CHAIRMAN APOSTOLAKIS: So the main message at least the way I see it is we did perform a set of sensitivity analyses addressing various issues that people have observed over the years regarding expert opinion elicitation and here are the results. NRR will use them and then spend some time on selected public comments that you feel are important.

MR. TREGONING: Pick out a couple of the ones we discussed today, a further subset of those.

CHAIRMAN APOSTOLAKIS: Yes. A subset of those.

MR. TREGONING: Okay.

CHAIRMAN APOSTOLAKIS: Like this business about very low, rare events that the public has said something that it's incredible or something, I don't think. The Committee knows that.

MR. ABRAMSON: No. I wouldn't put that in.

CHAIRMAN APOSTOLAKIS: Yes, but the other stuff that you had, the comparison with operating experience, for example, is something the Committee would be interested in, I think.

MEMBER MAYNARD: As I recall, Dr. Banerjee had a lot of questions about the elicitation process for this one and so there may be a lot of discussion on that.

CHAIRMAN APOSTOLAKIS: Well, if he raises questions, obviously you will answer them.

MEMBER SHACK: Why weren't there more professors on the panel?

CHAIRMAN APOSTOLAKIS: No, his main comment as I recall was that the lack of external review. Didn't he -- I think that's where he --

MR. TREGONING: That was one comment that he made.

CHAIRMAN APOSTOLAKIS: And you've had several external reviews. But I don't think you should address them in detail. If he asks a question, then you answer.

MR. TREGONING: We'll split it. If we have 45 minutes, we'll plan on roughly 20 minutes of overview.

CHAIRMAN APOSTOLAKIS: Right.

MR. TREGONING: And roughly 20 minutes of public comments and responses. Okay.

CHAIRMAN APOSTOLAKIS: Right. Any other --

MEMBER MAYNARD: If you plan on speaking that long, that's not going to allow for any discussion.

MR. TREGONING: Yes. We have 45 minutes total. But that's what I mean, 20 minutes of including --

MEMBER MAYNARD: Okay. As long as you're including --

MR. TREGONING: So that would be about three minutes a slide.

MEMBER SHACK: Ten minutes of that is yours. Yes.

MR. TREGONING: Ten minutes of that is mine. So it would be about --

MEMBER SHACK: The way you go through slides, that gives you about four slides, yes.

(Laughter.)

MR. TREGONING: You're always critical of my speed at which I move through presentations.

MEMBER SHACK: You're great for subcommittees, Rob, but you're hell on full committees.

(Laughter.)

CHAIRMAN APOSTOLAKIS: And you will not have as understanding a chairman at the full Committee.

(Laughter.)

MR. TREGONING: I recognize that.

CHAIRMAN APOSTOLAKIS: And on that happy comment, we break for lunch. Off the record.

(Whereupon, at 12:43 p.m., the above-entitled matter recessed to reconvene at 1:25 p.m. the same day.)

CHAIRMAN APOSTOLAKIS: Okay, we continue now with Nilesh Chokshi, seismic considerations for the transition break size.

SEISMIC CONSIDERATIONS FOR THE TRANSITION BREAK SIZE

MR. CHOKSHI: Good afternoon.

I think I'm going to start first with introducing the people who are here on the project team, and then we'll start talking about our presentation.

We're going to make a presentation in three parts. I'm going to cover up to the unflawed piping, and I'm going to leave the more difficult and challenging part to Gary, Dr. Wilkowski, to come back and talk about the floor piping. And then I'll come back with the indirect failures, and then wrap up the whole --

MEMBER SHACK: You get to handle all the fractals.

MR. CHOKSHI: That's right. I have a little rough challenge for me. So.

But I think there are three or four of us right here, myself, Dr. Wilkowski, and Khalid Shaukat. This work was done when I was still in research two years back, and you might not see me the next time this subject is being talked about in my current job.

CHAIRMAN APOSTOLAKIS: You will not come to the full committee?

MR. CHOKSHI: Oh, I'll come to the full committee. I'm talking about when you see some more data of this thing.

MEMBER SHACK: Hey, it will all be for new reactors. You'll see.

MR. CHOKSHI: So Mr. Hammer was part of your team, Gary Hammer?

MR. HAMMER: I was prior to a year ago.

MEMBER SHACK: They were just out to spread the blame with all the guys here.

(Laughter)

MR. CHOKSHI: This was a crash study. What you see, this report, and all the results and things, they were done in about less than three months time.

So we've gotten a number of people - they also wanted to make sure that the program offices and research and everybody was connected. And there was an important function. Gary and John and others giving the NRR perspective on the rule. So it was - that's why you see the number on our team both external and internal. So it was done in a very short time.

So what - I'll start with - let me what I will describe, outline my presentation. Now since the committee has not heard at all on this subject from us, I know you have the report, so my basic I think oral objective is to explain the study, the basic assumptions, the resources, and some of the conclusions.

I will also talk a little bit more about the responses we got during the public comment period on specific questions on this.

This issue was one of the issues identified in the draft proposed rules as a potential for a plan-specific assessment, and there were related questions.

And then ultimately I think I'll talk about some of the factors which we may have to consider what to do in the future, or what we may consider, so some of the factors that might affect decisions on where we go from here.

So let me - oh, I'm sorry, that's what I'm not doing. So this was my outline of the presentation I just described. I'll start with one of the biggest objective approach, resource, and then hear questions and public comments.

I think we're going to concentrate more on the conceptual approach on the calculations than on details. I think you will see the report, some of the details can take a lot of time, and I don't think it's germane.

So let me talk about a little bit of diagram. You heard this morning and you know that the stopping point of the defining transition break size was the expert elicitation. And I put up a chart for the PWR, and then we are just at 10^-5 breakpoint.

Now in order to make a similar comparison with the - for the seismic induced frequencies, to make a direct comparison I would first have to estimate a given assumption -

CHAIRMAN APOSTOLAKIS: I'm sorry, maybe it's not part of what you guys are supposed to do, but using just the frequency of the -5 would be fine for the TBS. But regardless of the actual scenario, it strikes me as a bit odd. Because in an earthquake, when you reach those levels, you probably have damaged a lot of other things.

In fact, as you know, the dominant contributors in PRAs to seismic risk are station blackout and loss of power, and then you have the LOCA.

So is that something we should worry about, what else is lost?

MR. CHOKSHI: Yes, we should.

CHAIRMAN APOSTOLAKIS: Or strictly look at the frequency -

MR. CHOKSHI: No, I think at the end - I know, my presentation, you will see that that comes into a picture in a big way.

CHAIRMAN APOSTOLAKIS: And then the SRM itself though doesn't seem to address this issue. The SRM just says, you know, define the TBS using a particular frequency.

MR. CHOKSHI: But I think as we talk, that was a starting point, and then I think we have to look at other factors.

CHAIRMAN APOSTOLAKIS: Okay, so you will worry about it.

MR. CHOKSHI: Absolutely. In fact, what I was trying to - in this letter, ideally one would have to do the same thing with the seismic bumper, the seismic-induced break frequencies. And you will start with probably a similar resource, an estimated conditional property of a certain size of break given a ground motion, then you would have to use hazard information on a plant-specific basis to develop correctly what were the break sizes.

And this was done up here, and I will talk about Livermore study much earlier. But that is extensive proposition. You not only have to address various piping systems, but you have to address all the locations which are potential breakpoints. It's already plant specific. You have to make a number of assumptions. You have to have all the digression models.

And within three months, I don't think we could have even had this.

CHAIRMAN APOSTOLAKIS: Why did you have only three months? This is an important issue.

MR. CHOKSHI: No, one of the reason was, and I think maybe on my next slide, I'll address that, why, why we wanted to do that. But I think even if we had time, that was not I don't think a feasible approach. It was more like a research program.

You would have to address a number of things. And when Livermore did that study in 1980s, and I don't know how much familiarity with it, but in 1980s Livermore undertook a study, they were basically looking at the dynamic effects of the pipe rupture. And that was a major program, three years of program.

CHAIRMAN APOSTOLAKIS: Well, that was the first major program addressing earthquakes.

MR. CHOKSHI: Earthquakes and the pipe breaks, yes.

CHAIRMAN APOSTOLAKIS: And it was originated because of this meeting.

MR. CHOKSHI: And also this was the follow up to the SSMRP, you know, we should remember.

CHAIRMAN APOSTOLAKIS: I remember the SSMRP too.

MR. CHOKSHI: So I think in principle it's feasible, but you know, I think it's impractical. Dr. Wilkoski might allude to the recent more development in the probabilistic factoring score, you know, in due time.

CHAIRMAN APOSTOLAKIS: Oh, you are the 10 ^-15 guy?

MR. CHOKSHI: So we decided that that's not what we are going to do. We are not going to try to produce a seismic index break frequencies.

We are trying to have a different question answered, which I think is more germane to this particular rule.

And so we wanted to know that now the timeframe I am talking about this study was completed was in the middle of December 2005. The draft rule was put on the publically available some time in November, right, Dick, I think, sometime in November?

And in that rule there was a discussion about the seismic that we are still struggling with, and that we will provide additional information to address in the questions.

So given I think that, I thought we thought it more appropriate, the question to answer is the conditions and likelihood of seismically induced breaks which will basically become incompatible with the proposed TBS.

I think in other words under what conditions the seismically induced breaks will be larger than the TBS, and will have a frequency of less than 10^-5 or more.

So I think that was more a manageable question to answer.

And I think that will be directly correlated later what the discussion on the draft, the TBS was proposed, and now people can look at the text on seismic on the proposed TBS. I think to me it was more direct link, and then gives a prospective so they can respond to some of the questions.

In order to do this we basically divided it into six activities. As listed here, unflawed piping, flawed piping, indirect failures, and then review of past experience, past PRAs, and the review of Livermore study.

Now the first three basically deals with different failure mechanisms. The next two I think it's a good calibration point, plus we are seeing what are the insights, or this result comes with that, and also are we finding something which is different than what we have learned in the past.

And the Livermore study was the one study which had really done this at that time in a comprehensive manner, and we used that approach directly for the indirect failures. And I'll discuss that later and give you more detail about the Livermore study also a little later.

Now we did not - and our approach was deterministic and probabilistic. For indirect failures it was more likely calculating the failure probability using the hazard and the fragilities very much like a seismic PRA.

On other ones we used mean seismic hazard results, and then selected some deterministic parameters. At the time we did not do uncertainty. It would have been easy to do some of the parametric type of uncertainties.

But we did some sensitivity studies on some of the key assumptions and key parameters.

CHAIRMAN APOSTOLAKIS: What was the problem again here? Why didn't you do an uncertainty analysis?

MR. CHOKSHI: It was simply a question of time. But also the other question was that we could handle with sensitivity studies. So we did some sensitivity studies, and I will point out. And I think one of the questions about hazard is - so -

MEMBER BLEY: Nilesh, can I ask you for a favor? When you go through those, if you could tell me how you address this problem, and that is, I've only tried it once or twice, tried running a seismic PRA against the mean hazard and you get nothing, of course, because the design is such that -

MR. CHOKSHI: The radial fragilities.

MEMBER BLEY: Yes. Because it ought to be that way. So if you do it on a mean basis you don't see any -

MR. CHOKSHI: In fact you will see one result. I will show it, it basically falls off.

MEMBER BLEY: How you dealt with it.

MR. CHOKSHI: Yes, I want to compute down to the -17, but if I compute something like that.

CHAIRMAN APOSTOLAKIS: Wait a minute, when you say mean causal, do you mean the mean curve?

MR. CHOKSHI: If you run the two mean curves against each other, instead of doing the whole uncertainty, your risk curve is nil.

CHAIRMAN APOSTOLAKIS: But doesn't the mean curve extrapolate all the way to very high accelerations?

MEMBER BLEY: It does, but at very very low frequencies.

MR. CHOKSHI: I think it's relative positions of the fragility, and in some cases, you will get a mean failure probability.

CHAIRMAN APOSTOLAKIS: So your point, Dennis, is that the uncertainty analysis really shows -

MEMBER BLEY: All the risk comes from the mixture from the composition.

CHAIRMAN APOSTOLAKIS: Okay, yes?

MS. UHLE: This is Jennifer Uhle from the staff. I just want to just follow on to what Nilesh just said about the major reason why this approach was used was time.

I think it's also a matter of, this approach was found to be technically appropriate. And obviously we were trying to do it in the most efficient way possible. So time wasn't the only factor.

I mean we wouldn't be relying on this if we found that there were big gaping holes in the technical validity of it.

That's obvious to Nilesh. I just wanted to make sure that that was clear.

MR. TREGONING: And this is Rob Tregoning of the staff. I just want to buttress what Jennifer said. I think given all the work that had been done in Livermore, the major piece that was really missing here was the response and the performance of flawed pipe. That was the thing that we really wanted to look at here.

There was a pretty good basis from the Livermore study for evaluations of unflawed piping, as well as other work that had been done, and then the indirect failures. So really the major piece that this was trying to get at was the evaluation of flawed piping, and how flawed piping as Nilesh said at the TBS side, how that would perform under these very large infrequent earthquakes.

MR. CHOKSHI: So, all right, I think so I'm going to start with the discussion of approach and key assumptions and the scope of the work.

And these are basically, what I'm going to discuss is applicable to the unflawed piping.

One of the most I think difficult problems, and in doing this kind of - initiating this work is have plant specific information in terms of stresses, normal operating stresses, seismic stresses, material properties, and the design information which is very hard to generally get.

And the one source of such results available to us was the leak before break data list. And that only includes PWR plants. So we were limited to that.

But out of the database we selected about 27 PWRs, covering mostly Westinghouse and CE plants; 24 of them were on the rock site; three on the soil sites. And rock sites are of more interest because you have higher seismic stresses at the rock site generally.

Now the other information you need is site specific seismic hazard. And we after some deliberation we decided to go with the 1994 version of the Livermore, which was the revised Livermore. We knew that this was doing this on ESP, what was going on, that there has been some new estimates of the seismic hazard, and we chose some different basis.

That was only available for one of the

sites.

Given that I think we wanted to look at more, there were about 27 sites. So we still decided, we decided to use the Livermore.

CHAIRMAN APOSTOLAKIS: So you didn't consider the EPRI hazard curves?

MR. CHOKSHI: No. We did two aspects. One of the reasons you see a fourth bullet here that determine the seismic stresses, both at 10^-5 and 10^-6. In part idea of 10^-6 was to look at what happens if the hazard changes. Also we wanted to look at it, does it clarify that certain crack sizes you know become critical.

That and our public response comments. EPRI is part of the NEI comments looked at some of the new results. The data had available more EPRI results than we did obviously.

So they did look indirectly at various mixes, more difference. And then so when I discuss public comment I'll discuss those results.

So we had additional results from the EPRI study, EPRI hazard approach.

CHAIRMAN APOSTOLAKIS: Are these, for example, that was something that I didn't understand.

10^-5 or 10^-6, you said?

MR. CHOKSHI: Yes.

CHAIRMAN APOSTOLAKIS: This 10^-6 is intended to cover the possibility of different set of hazard curves?

MR. CHOKSHI: Or higher stresses. In part it addresses what happens if hazard goes to higher hazard same as - and I'll show you, I'll show you results, you'll see.

CHAIRMAN APOSTOLAKIS: Now regarding this first sub bullet, evaluations are linked to PWR. So what is the rule, what does it say about BWR?

MR. CHOKSHI: Well, I think, can I discuss that toward the end? Because I think if you look at the results, I will show that the results and conclusions are to me at least equally valid for BWRs, what we know, seismic and piping. All of them, and I'll give you my first conclusion, that seismically in this pipe here you need a really very large flaws. And this is - before that happens. And I think that confusion is not only the BWR specification -

CHAIRMAN APOSTOLAKIS: So the basis of your conclusions, jumping ahead a little, is that you would need unreasonably large piping flaws at the level of the TBS that has already been defined in order to exceed the frequency -

MR. CHOKSHI: For the large piping. We - and then that's one of the other things I wanted to say, since the PBS - and that's why I think one of the reasons for using this approach was, okay, the TBS was determined. So we wanted to -

CHAIRMAN APOSTOLAKIS: So you start with a TBS that has been determined or proposed?

MR. CHOKSHI: Right.

CHAIRMAN APOSTOLAKIS: So it's 20 inches for BWR.

MR. CHOKSHI: So 14 inches or so for the PWR -

CHAIRMAN APOSTOLAKIS: Twelve to 14. So if I have a pipe of 12 to 14 diameter which already meets the Regoning/Abramson 10^-5 criteria, right, then what would be the conditions, the seismic conditions, that would it fail with a frequency greater than 10^-5?

MR. CHOKSHI: No, 14 is the break size you want to design for, under the normal design basis rule. I want to look at the next pipe up. So whatever is bigger than 14, what is the failure frequency, seismically induced failure frequencies?

And that's why we looked at piping systems larger than the TBS diameter.

CHAIRMAN APOSTOLAKIS: Okay. If you look at them then, and you - if you're asking what should be the flow size to make that pipe fail -

MEMBER SHACK: With the 10^-5 to 10^-6 seismic load.

CHAIRMAN APOSTOLAKIS: Okay.

MR. CHOKSHI: Actually we came up with the flaw, what the flaw size, should become critical.

MEMBER BONACA: I had a question regarding the applicability on the west side of the Rockies. Why cannot you apply directly your results? Is it because you did not look at specific sites?

MR. CHOKSHI: Oh, you can use this approach at any site. There is nothing - the same approach can be applied. It shows the availability of data.

CHAIRMAN APOSTOLAKIS: And the hazard curves are more difficult.

MR. CHOKSHI: Well, yes, and easier to get the plant specific hazards. But yes, in fact we say in the report that this is applicable to the, you know -

MEMBER BONACA: Because there is really the higher seismic challenge is west of the Rockies, so you want to have some understanding of if you want to have any relaxation of 50.46, there is some need there for those plans to be part of this finding here.

MR. CHOKSHI: But they also have a higher design basis, so you'll have to look at that and see how -

MEMBER SHACK: Well, I think that comes back to the reg guide that Rob was talking about, that somehow you're going to have to demonstrate your plant falls under these things, or you're going to have to do additional calculation in order to use 50.46a.

MR. TREGONING: Yes, that's certainly a consideration.

MR. CHOKSHI: Now the other thing, I think an important thing, and this is the scale factors; that in order to do the calculations at the highest traces you've got to do a realistic calculation or the stress is not real, looking at - you know, not the design pipe. So in order to estimate the earthquake stresses at 10^-5 or 10^-6, we applied seismic pressure linear methods, basically. And in the report there is an extensive discussion of how do you take a design value and then apply correction factors to come up with a million capacity, as well as the uncertainty on the capacity, the fragility curve.

MEMBER BONACA: You go through that, right? Because I mean that's one place where I have some questions. You are reducing conservatism there and I want to see how you get there.

MR. CHOKSHI: All right. I will do that. Let me - I'll do that in the next slide, okay?

So these are the basic assumptions or the approach for the floor and unflawed piping.

CHAIRMAN APOSTOLAKIS: Why did you feel you had to remove the conservative?

MR. CHOKSHI: Oh, because you are estimating now stresses at the higher level. If you use - in the design, there's a lot of - you overestimate because of the conservatisms, so you know, in order to really assess what are the break - what is the likelihood of the flaw size, you want to look at it as a more realistic stress picture as possible rather than an arbitrarily really conservative value.

MR. WILKOWSKI: The other thing is, when you do the flog pipe evaluation, you are using elastic plastic, not linear fracture mechanics analysis methods. But the input elastically calculated stresses, your driving force is just way too high. So you need to bring those in line with each other.

CHAIRMAN APOSTOLAKIS: Well, this is because also the SSE are supposed to be designed stress.

MR. CHOKSHI: Right, SSE is design stresses, and I'll talk about some of the factors in a minute. In fact, let me -

MEMBER SHACK: He's looking for a realistically conservative answer.

MR. CHOKSHI: Right.

So let me start off to describe the process we used for the unflawed piping. This first three boxes - the normal stresses, and seismic stresses, and normal cross-section stresses, they come right out of a LBB database. We went into the LBB database for those three lines, selected - got the results. One more thing we got from the LBB database was the S sub m, the ASME allowable code value used in the design.

So this parameter comes directly from the LBB database.

Now the scale factor. Now let me -

unfortunately I don't have a slide on this, but if you imagine in your seismic exploration, big ground exploration design basis, this is .15 G, okay, then you do - you have a standard design spec. You do this while structuring correction analysis, your building analysis, then you do piping analysis. You use the core specify or the reg guide or SRP specified damping values. You conservatively combine dynamic modes.

And so there are a number of steps in between where you use very conservative properties.

In the seismic group PRAs and in the seismic margin, what you do is that instead of looking at this generic design basis spec, which is like reg guide 160, you look at the site specific sector, which tends to be lower than the design sector. So you got a big margin from that.

You look at the Q damping values, median damping values, from the stress data. You look at the more realistic failure modes. So when you couple all these factors - now this is a very standard methodological seismic PRA, and that right approach was used.

So what you do is then you correct your basically design stresses to account for those conservative ones. And then you go into the - calculate the stresses for different factors of SSE, one time, two time, this alpha factor.

Now but these are the more realistic factors. So for example -

CHAIRMAN APOSTOLAKIS: Excuse me, in box five, the word, scale, is not the same as in box four.

MR. CHOKSHI: Yes, these scale factors is basically a factor that reduces - this scale is so simple - suppose your design was .15G. At 10^-5 my down motion level is about .45 G. I multiply stresses by three.

CHAIRMAN APOSTOLAKIS: Which is the factor of safety?

MR. CHOKSHI: The factor?

CHAIRMAN APOSTOLAKIS: You have a factor of safety, don't you?

MR. CHOKSHI: That's the scale factor.

CHAIRMAN APOSTOLAKIS: The scale factor is the factor of safety?

MR. CHOKSHI: It's inverse.

CHAIRMAN APOSTOLAKIS: The inverse of the scale factor.

MR. CHOKSHI: Unfortunately we were writing so fast that some of the terminology, we had to use both interchangeably.

So as soon as you got the stresses associated with different level of earthquakes, okay. And then I compute the stress ratio, which is the normal stresses plus the earthquake stresses at different earthquakes divided by S sub m. And I'll explain why we do this in terms of stress ratios, because our failure criterion is directly linked to S sub m, how many times S sub m.

And now because alpha SSE, now you can associate frequency of occurrence directly with the hazard. So now you have a probability of exceeding this stress ratio, okay. This is now unflawed piping, and then you can compare with the failure criteria.

So what I'm going to show here on this plot is the reasons of 27 systems, this were the most highly stressed system from the 27 PWR.

CHAIRMAN APOSTOLAKIS: What's the definition of unflawed pipe?

MR. CHOKSHI: Okay, that's a good question. In the report I'm going to - let me show you. I'm going to put this up, because this is something -

okay.

CHAIRMAN APOSTOLAKIS: Do we have that slide?

MR. CHOKSHI: No, but this comes from the report.

CHAIRMAN APOSTOLAKIS: In the report, yes.

MR. CHOKSHI: Right, it's a footnote when you first talk about unflawed piping. I think it's basically the piping which is in the code considerations. You are treating the entire cross-section as resisting the loads. It's nothing more than what mentioned pipe, something which code would accept as an unflawed piping. But it's a pretty inward definition.

CHAIRMAN APOSTOLAKIS: Okay, so the failure modes are different?

MR. CHOKSHI: Right, exactly right. It will - and going back to the - I'll discuss in a moment.

CHAIRMAN APOSTOLAKIS: Now does this have anything to do with our ability to detect flaws?

MR. TREGONING: Not so much. I mean again it's more about how the pipe responds. If the pipe knows that there is a flaw there or not. And that's essentially what this definition was intended to capture.

CHAIRMAN APOSTOLAKIS: So are most pipes unflawed or flawed?

MR. CHOKSHI: Initially I think most of them unflawed.

CHAIRMAN APOSTOLAKIS: Okay, and unflawed pipe then years down the line can become flawed?

MR. CHOKSHI: Under certain conditions.

CHAIRMAN APOSTOLAKIS: Some flaws just grow? Okay.

(Off-mike comment)

VOICE: And vice versa my colleague here says.

(Laughter)

MR. TREGONING: That's right, and vice versa happens if a flaw is detected and then repaired. That's the -

CHAIRMAN APOSTOLAKIS: It's not self healing.

MR. CHOKSHI: So let me start with what's on this block. So this is the stress ratio, which is the normal plus at seismic at different levels, divided by S sub m, okay. And this is the probability of accident or frequency per year.

CHAIRMAN APOSTOLAKIS: You think that was what Sm means?

MR. CHOKSHI: Sm is ASME allowable. And if you look at the previous criterion, the one percent probability of failure for one particular weld of cracking is 4.5 times S sub m. That was the reason to normalize this, so you can make a direct comparison.

CHAIRMAN APOSTOLAKIS: Right.

MR. CHOKSHI: Now what you are seeing here, for the stress ratio of two, okay, the range of the probability of accidents is roughly 4 X 10^-5 to less than 1 X 10 ^-7.

CHAIRMAN APOSTOLAKIS: I'm sorry.

MR. CHOKSHI: If you look at how the different range of results, on stress ratio two, okay?

CHAIRMAN APOSTOLAKIS: Yes, okay.

MR. CHOKSHI: The probability of accidents ranges from about 4 X 10^-5, to less than 1 X 10^-7. At 1 percent probability of failure, which goes from the 4.5 S sub m, you know, you are already looking at 10^-7. And now remember, this is a point, in order to come up with a mean probability of failure, I would actually have convert with this distribution, there's a 50 percent.

MEMBER BLEY: Since we're back to that, I should say I misspoke earlier. When you take the medians against each other you get no risk.

MR. CHOKSHI: Oh, medians, yes. I was going to say that. If you rewrite, then you should capture some.

MEMBER BLEY: Yes, of course.

CHAIRMAN APOSTOLAKIS: So at 4.5 of this normalized quantity there is a 1 percent probability that the pipe will fail according to the failure mode you showed us earlier.

MR. CHOKSHI: That is right for that graduating mode. And this - let me tell you a little bit more about the failure mode. This criterion comes from it, dynamic tests which are done by EPRI and NRC also was in it for Gombi Dam. And these results from the - there were 37 components, straight pipes, elbows. And results of this program were used to propose the modification to the ASME Section 3 design code. And NRC did some independent review, and to all of the established eloquent design criteria with sufficient margin, we evalutated and developed this failure probabilities.

And so this comes right from the NRC study of the 37, which I think we came and talked to you several years back, when there a big controversy over the seismic rules.

MEMBER BLEY: But each one of these -

MR. CHOKSHI: From the 27 plants, this is done in one of the PWRs.

MEMBER BLEY: For one of the PWRs.

MR. CHOKSHI: Each curve is one PWR. And we picked the highest location from the data as we have.

MEMBER BONACA: So from PWR when a specific component -

MR. CHOKSHI: Yes, this would be like a hot log - hot leg, cold leg and one location.

MEMBER BONACA: Be the same component for all these plants?

MR. CHOKSHI: No, this is the highest stress location.

CHAIRMAN APOSTOLAKIS: And if you consider now a full uncertainty analysis, can you speculate what would happen there?

MR. CHOKSHI: I think the probability of failure is still very low. Because this one we basically have the probability of failure criterion you have that covered and then what you will do is, the hazard you will have to basically stress, seismic stresses is really controlled by the hazard.

And I think you want - if I were to take the highest curve, okay, and convolve with this, the mean probability of failure will be something like 10^-10.

CHAIRMAN APOSTOLAKIS: So these curves then use what, the median hazard curve?

MR. CHOKSHI: This is mean, mean hazard.

CHAIRMAN APOSTOLAKIS: Mean hazard.

MR. CHOKSHI: Yes, we purposely wanted to keep the conservatism in the seismic stress side, and then in the material properties, and when Gary talks about it, we used more realistic for those. So most of the conservatisms is kept in the hazard type.

Now one other thing I wanted to point out was the sensitivity to the hazard. If you look at the 10^-5 to 10^-6, and if I look at this curve, which is the extents, at the 10^-5, this stress ratio is about 1.8. At 10^-6, it's about 3.2. So there is a substantial increase.

Plus this underestimates this type of

hazard, because these are normal plus seismic stresses. If I were to look at these ratios in hazard space, the hazard corresponding to the 10^-6 will be even higher than that ratio. So it's almost about 50 percent higher almost.

So in a sense it addresses what happens with the higher hazard. And if I look at - in fact I looked at what EPRI had done and the new hazard curve they used, they would be roughly exhibit that kind of increase.

MEMBER BLEY: Nilesh, can you take me back to the origins of the 1 percent probability of failure at 4.5 times SM, where does that come from?

MR. CHOKSHI: Okay, there was an EPRI program a certain number of years back. They did the 37 tests, dynamic tests. And of the piping, the straight pipes, elbows, tees, and was to basically characterize how the pipes fail. So they prepared the report, and the documented and distributed analysis. And then the proposed changes to the ASME seismic design criteria, that we can relax certain of those traces, we can relax some of these.

As a part of our evaluation we looked at this space resource and did a lot of independent studies. And we did all of this - we did basically like a PRA type analysis. So that EPR, my goal is certain - goal benefits. My piping systems are basically distributed systems. How much failure I can tolerate in a piping system, what probability of failures I can tolerate.

And then if you - it's Bob Kennedy's - I think, performance-based design. So we start back -

MEMBER BLEY: EPRI tested 37 pieces to failure?

MR. CHOKSHI: To failure. You know, some of them - yes.

MEMBER SHACK: Now are these elastically calculated stressed I'm dividing by S sub m?

MR. CHOKSHI: Yes, these are elastic.

MEMBER SHACK: These are elastically calculated.

MR. CHOKSHI: So these relate to the design. That's why it was all converted back to the - and I think if you - Bill, you might remember, it was a Ken Jaquey's report, and in fact we had a number of questions. We did look at the M ultimate and the historic behavior.

But this was looking at the failure data, and then imposing margin, what type of margin you want in your design. So these are the values.

MR. WILKOWSKI: But failure might only be a leak in most of these cases, not really a complete break. So there is some additional margin there.

MEMBER BLEY: They tested them until they at least put a crack in them?

MR. CHOKSHI: Or the test becomes unfeasible, they can't sustain it.

CHAIRMAN APOSTOLAKIS: So I want to know then the frequency of a leak or whatever failure is defined, and I would look at the uppermost curve, that tells me that there is a frequency of say 2X10^-7, but I would have a ratio of 4-1/2, right? Now the actual frequency of the leak is that number, 2X10^-7 times .01? Because that is the condition of probability of failure?

MR. CHOKSHI: Yes.

CHAIRMAN APOSTOLAKIS: So we're going down now to 10^-9.

MR. CHOKSHI: See, that's what I was saying.

CHAIRMAN APOSTOLAKIS: That number is comparable to what the previous values.

MR. CHOKSHI: Exactly, so when you are - if convert, if I wanted a mean probability of failure, I would convert over the entire spectrum of conditional probabilities, densities.

CHAIRMAN APOSTOLAKIS: But even if you don't convert, I mean, that's exactly what it says.

MR. CHOKSHI: Yes, I mean you can see it right there.

CHAIRMAN APOSTOLAKIS: This is the frequency of going to a conditional probability of 1 percent of leak.

MR. CHOKSHI: It's only one - 10^-9.

CHAIRMAN APOSTOLAKIS: Yes.

MR. CHOKSHI: That's why I didn't compute it. Because you know then I'll be answering different questions. And that way you can see that this - and it - so let me go to the next slide.

MEMBER SHACK: Your factors of safety, you know, when I get a number like a median factor of .86. Now is that median factor, you went to a bunch of seismic PRAs where they had actually done the calculation and then took off a number?

MR. CHOKSHI: I think that median factor of safety, if I remember right, you are referring to the spectral shapes.

MEMBER SHACK: Spectral shape, right.

MR. CHOKSHI: What that is is the design spectral when it was just something like Reg Guide 160, so because I'm doing a calculation -

MEMBER SHACK: Okay, so that's the relationship between the site spectrum and the 160 spectrum.

MR. CHOKSHI: In fact what .86 means that the site spectra is higher than the design spectra, that's considerably of interest.

MEMBER SHACK: But when you say median factor, is that - these are changed for each of these - you did this for each of these 27 plants?

MR. CHOKSHI: Yes.

MEMBER SHACK: Okay.

MEMBER BONACA: The only question I have is, you do the sensitivity study to the scale factor? Or you just didn't do it?

MR. CHOKSHI: We did - not in this particular case, because this were obviously coming out. But in the indirect failure, what we did was, we changed the beta, the uncertainty to capture - median capacity factors are fairly well known, and then you have uncertainty about them, about each factor. That's why these are a million factors. But in still applying to every - each factor, we varied the final total uncertainty, the indirect failure. Because this was more closer to 10^-5, so we wanted to see.

Now we know. In the Livermore study - I'm jumping ahead, but I'll describe when I come to that study.

But I think from here, I think the point is that this is clearly unflawed piping, so this conclusion, I don't think it's, at least from this study, is much - now I think maybe this is a good time to talk about the experience.

We looked at - in this study we looked at a sample of reports. In particular we looked at two reports which were more recent, and sponsored under NRC. John Stevenson had looked at the power plants and industry in California. And then we looked at four recent events.

Ground motion acceleration, I would say the highest value, around .5 G, and from these and every other studies we have looked at, welded design engineered piping does phenomenally well in the earthquake, because, you know - and this is a good ductile and we see that in structures also, that if you have enough ductility, energy absorption capacity, they perform very well.

Cases of failure we see are primarily associated with a single degradation. Support failures, which is also mostly associated with a degradation of things falling. You know something falls on the piping. And it's an invalid failure.

And the one you see most frequently, or more frequently, but you know, is the related motion, anchor motion, infecting the - this is a Japanese earthquake. And this was not piping, but there was a duct work. And this duct work I think out of seven units, five units had the same detail with the part of the duct was supported outside the building on a separate foundation and then connected to another part; all of them failure similar. So that - this big anchor motion, you know, when you get a very large lateral motion and piping is not flexible to accommodate this motion, you see failures.

Now when I talk failure, again I want to select - it's mostly leaks, and those kind of things. It's not a catastrophic as severance.

So it's not surprising I think what we are seeing here.

From the PRA standpoint, and I'm going to come back more and talk about that, but traditionally in seismic PRA based on a lot of these kind of studies, and looking at the - we don't assume for the undegraded piping you basically say that piping failure probability is very low, and you seldom look at from direct causes. In fact, never, I would say that, particularly something like RCS piping of - routinely in PRA we look at this indirectly. And that has been looked at a number of times.

But I think as I think George you mentioned for the core damage type of sequences, it's generally the seal LOCA or small LOCAs from the loss of power and support systems, or something like that.

If you remember 1150 study, there was a failure mode where the steam generator supports, and at that point you are talking about large movement and things, it has an impact on the early release, because it was in the containment also. But that - but generally they don't show up at 10^-5. There were a lot of breaks. But I will talk more about that.

So I think from the PRA perspective, and generally, the RCS piping, and the thing - no PRS considered the degraded condition. And that was I think the reasons it was a tougher question to answer. And we know how to look for it.

CHAIRMAN APOSTOLAKIS: But your analysis for the unflawed pipe case followed the standard PRA approach. You just didn't do an uncertainty analysis?

MR. CHOKSHI: Yes.

CHAIRMAN APOSTOLAKIS: Because in the second one with the flawed, then you changed your approach?

MR. CHOKSHI: No, the flawed approach is different also. I'll describe it. But the first one, I think to me, the conclusion to me I think the conclusion is very clear.

CHAIRMAN APOSTOLAKIS: If we reach that level of earthquakes where we have damage to the pipes, we have already been in a special blackout -

MR. CHOKSHI: Or many other things.

CHAIRMAN APOSTOLAKIS: - or many other things.

MR. CHOKSHI: And that's why I think my second bullet, we don't ask people to analyze unflawed piping just because I think it's very hard for me to see it adds anything.

I will turn it over to Gary.

CHAIRMAN APOSTOLAKIS: What is this EMC squared?

MR. WILKOWSKI: Engineered mechanics Corporation of Columbus.

CHAIRMAN APOSTOLAKIS: I thought you were doing relativity or something.

MR. WILKOWSKI: I was at Bechtel, Columbus for 23 years before that. So we're about 10 miles relative to the -

So I'll talk about the flawed piping analysis work that was done, and this was really the harder part I think, the core of the work that we were trying to do.

And we stumbled along with, how do we account for seismic stresses when we are trying to do the elicitation efforts, because I was also on the elicitation panel. And so I had - I got the tap on the shoulder that says, well, how should we do this?

And so the best ways that I could think to do this in a relatively short time are presented here. And the first aspect is to determine what types of flaws would be critical flaws at a 10^-5 or a 10^-6 seismic type of earthquake, relative to surface flaws that the ASME code would be able to evaluate and detect and say this is an acceptable or not acceptable flaw.

So you have the inherent protection in the ASME code with all its safety factors relative to these very large postulated seismic events with lower safety factors and more realistic material property evaluations. So that was one way of doing this evaluation.

The second way of doing the evaluation was to determine if- will leak before break analysis that had been previously done for the plants provide you inherent protection against a through-wall flaw that might exist?

So those are - and surface wall evaluations are code allowable flaws. A through-wall crack and a pipe by leak before a break, that's a flaw tolerance approach. We're not saying how these flaws got here at all. What we're going to do is determine if these evaluation criteria - either leak before a break, or the ASME code - have inherent protection at these very high failure stresses.

Now you still have the probability of, will that crack exist at that time to get the full failure probability. So we're only using this; that's why we called it a hybrid type of approach.

So we've got the seismic hazard curve in there to give us the stress levels, but the rest of it is really deterministic in determining the critical flaw sizes for either a surface cracking and code procedures; a surface crack using actual properties; or a leak before break analysis, as was done in the original plant submittals versus doing our best leak before break evaluation.

So those are the two different criteria that we used. And if you passed all these, then you might say, well, I still have a higher probability of failure, because I don't know what the probability of that flaw existing yet is, so you have that additional margin.

Let me first talk about the surface flaw evaluation. And out of the 27 different plants that we had that were all PWRs, we selected 52 different piping systems, hot legs, cold legs, crossover legs, with different piping materials, and took the high stress locations at these different locations and used those to determine what was the surface flaw allowable stresses, either using the ASME allowable flaw size properties, with actual strength or with code strength properties.

And then we'd want to compare them to, let's make our best estimate of what the critical flaw size might be, at a 10^-5 seismic event or a 10^-6 seismic event using the seismic hazard curve with all the scale factors that were developed for the unflawed piping evaluations.

Now flawed piping analysis is a nonlinear analysis, when we do things - a net section collapse analysis, elastic plastic fracture mechanics. Whereas the stresses that are typically calculated are elastically calculated stresses.

So we came up with a first order approximation to try to correct for that. So that if any of these stresses that we calculated at, say, 10^-5, if they were below yield strength, okay, then there is no correction factor. If it's above yield strength, then we did some correction factor from that point up to where we would expect buckling to occur, and studied that equal to - such that the flow stress of the material was equal to 6.3 S sub m, or what they determined as the nominal buckling from elastically calculated stress analysis.

It was a crude approximation. You could do a lot better. But if we got most of the effect from doing that, then that worked out good.

As it turns out, when we applied that correction to the 10^-5 seismic event, there was like only a 4 percent correction; it wasn't a big deal. A 10^-6 seismic event, well, then it was about a 30 percent correction factor. It became more important then.

We used all the stresses that were in the LBB submittals for the Pwr plants, including pressure stresses, dead weights, seismic inertial, SAM for more expansion stresses. We did a more realistic accounting for material strengths and toughness values, if we were looking at an ASME evaluation with actual properties, or using our critical flaw assessment. For instance we had a database on fractured toughness for stainless steel welds; that was our most critical case to look at, was, what was the flaw tolerance for a crack in a stainless steel weld, because some of them have lower toughness values there.

And so in that case we used the mean value minus one standard deviation for the material toughness. We didn't do a full evaluation of all the probabilistic variations with material toughness; could do that, just didn't have enough time.

CHAIRMAN APOSTOLAKIS: Why didn't you look at the ASME code with the actual strength?

MR. WILKOWSKI: Because the code allows you to do that in places.

MEMBER SHACK: It does?

MR. WILKOWSKI: Yes. There are options in the code that says, you can either start off with code properties, or there are some options in the code that says, if you actual properties you can use those.

So we just wanted to cover that base.

I am going to show you a series of three figures here of where we did some of the calculations. These are just examples.

In this first figure, I think in the report we called it a category A type of behavior. And the example here is for a hot leg. It's at a seismic stress of 10^-5 occurrence, and at the 10^-5 event, 48 out of the 52 cases that we looked like behaved like this.

And what you see there is a plot of the flaw depth, A/T, versus the flaw length, surface flaw length, theta over pi.

CHAIRMAN APOSTOLAKIS: Remind us what A over T means?

MR. WILKOWSKI: Surface flaw depth, the depth of the surface flaw relative to the pipe thickness.

And the ASME code has certain limits. For one thing it says, we're not going to allow you to have flaws that are greater than 75 percent of the wall thickness, regardless of how low your stresses are in the pipe system. You have to take that pipe out of service.

The other lower limit is essentially the workmanship flaw standard, which is about 10 percent of the wall thickness, if the flaw is less than that then you don't have to do an evaluation; it's just an acceptable flaw by the code.

MEMBER SHACK: It's unflawed piping?

MR. WILKOWSKI: It's unflawed piping; that's right.

CHAIRMAN APOSTOLAKIS: A quantitative definition?

MR. WILKOWSKI: That would be another way of defining that.

Each of these curves then represents cases where there are different stress levels or safety factors or material toughness considerations, in calculating what that flaw shape looks like.

So you see if you use the ASME code, that's the yellow bottom curve, use the code properties for this particular case, you get a very conservative estimate as to what the critical, or allowable, flaw sizes would be, and that has safety factors and conservative evaluations within the code procedures.

If you used the actual strength properties for this particular case, oh, you could allow flaws that are much larger than just using the ASME code properties, and that's why they have that option in the code.

And then those were all at - those ASME stress values are at normal plus SSE, or Service level D, operating conditions.

If we do our best estimate evaluation at 10^-5 stress with no safety factor, and accounting for the material properties a little more accurately, then you get that red curve that says, oh, even the critical flaw size with a safety factor of one is greater than what the ASME code allows.

So the ASME code procedures have this inherent protection against that flaw ever becoming a critical flaw size. So that was a good result there.

The next case is the case, we called it category B. And this is a case where now the best estimate flaw shape kind of falls in between the ASME actual strength curve, and the ASME code strength curve.

And you'll notice in this case the ASME code strength curve rose as quite a big higher. This is just a particular example for our crossover laid pipe, again at 10^-5 for the best estimate seismic stress evaluation.

And again the ASME analysis is for normal plus SSE stresses. So here you see that the ASME code strength provided the protection - code strength analysis provided the protection against even a 10^-5 type of seismic behavior.

MEMBER BLEY: And the difference is, we go from one to the other, is the size of the pipe?

MR. WILKOWSKI: Yes, plant specific cases, where we accounted for the actual seismic hazard curve, the actual material properties, the actual toughness of the materials, et cetera, and the highest stress locations within that particular hot leg, crossover leg, et cetera.

We did that for each one of these 52 cases. I'm just going to show you three plots here as typical.

So the last case here was category C that we called it, and this was the case where the best estimate critical flaw size of 10^-5 seismic event occurrence for stresses was below that for the ASME curves when the ASME curves uses a normal plus SSE stresses again.

So in this particular case, and this occurred in three out of the 52 times that we looked at - three out of 52 cases - the ASME code did not have the inherent natural protection against those flaws ever naturally being protected against the 10^-5. However, what you see is that those flaw depths are really big. These are huge flaws, and I think that is really the important key thing to show here, is, we are seeing flaws now that if you go to the far side of the curve where it's fairly flat, and you've got these very long flaws where theta over pi, the crack is more than 60 percent around the circumference, it still has to be maybe 40 percent of the wall thickness; that's a humongous flaw to exist.

MEMBER SHACK: You accounted for the fatigue growth here by essentially dropping that fracture toughness by a half; is that what I -

MR. WILKOWSKI: I did not account for any fatigue crack growth that way.

MEMBER SHACK: This is the end of life flaw evaluation flaw size that you would have.

MR. WILKOWSKI: Okay, but the end of life after my seismic event might be very different from the crack size I have at the beginning of the event.

MEMBER SHACK: Yes.

MR. WILKOWSKI: I did account for, on the material toughness I accounted for dynamic loading rates and cyclic effects.

MEMBER SHACK: Right, that's what I meant.

MR. WILKOWSKI: Right, I did do that.

MEMBER SHACK: But that's what you did, you dropped it by a half?

MR. WILKOWSKI: Not always. It depended on the material and the sensitivity of the materials. Some materials were sensitive to that and some were not. Just like the dynamic loading rates. For instance the ferritic steels may be more sensitive to dynamic strain aging and may get a knock down in the fracture toughness, whereas the austenetic materials -

MEMBER SHACK: I'm really thinking of crack growth during the event. I mean these are relatively large cycles -

MR. WILKOWSKI: Cyclic ductile tearing is what you have here.

And we benchmarked all of these analysis procedures against the fullscale seismic pipe tests that we did during the IPERG program. So we got some confidence in that.

MEMBER SHACK: Okay, now how did you run the cyclic load tests in the IPERG? Those are very slow cycling? No?

MR. WILKOWSKI: The dynamic loading of the pipe system at 80 percent of its first natural frequency. If it was a single frequency test. But we also did some tests with random seismic loading where we would take a seismic signature analysis, apply that to the pipe system; if it didn't break, then we would bump the whole system up, or the whole load amplitude up until we had failure.

But we did a lot of detailed analysis

before that, so generally we could predict that fairly well.

MR. CHOKSHI: Yes, I think in that selection I think we tried to be more earthquake characteristic, so the phasing and, you know -

MEMBER BONACA: You call this flaw very severe, and I agree. How does that compare with the Wolf Creek flaws? Some of them were severe, not as severe as this, but -

MR. WILKOWSKI: Right, the Wolf Creek flaws were about, say, 30 percent of the thickness, and 20 to 40 percent of the circumference maximum in length. They were a bit down there. They were more in that kind of range, right there, around there. So there'd be a lot more margin with those particular flaws.

MEMBER BONACA: They were already in the category of what we're addressing here.

MR. WILKOWSKI: And sine you brought up Wolf Creek, the guys in my company also helped NRC with the analysis there.

And when you did the analysis of, for instance, the pressurizer cracks that were in Wolf Creek, the relief lines were such that you could grow very long flaws around the circumference.

However we did some sensitivity studies for the surge line as well as for the hot leg to see how would the flaws generate under PWSEC, what is the flaw shape that would occur.

And the interesting thing is, when you go to the much larger diameter pipe like we are interested in here, the cracks don't grow as fast in the length direction as we saw in the small diameter pipes because of the residual stress fields, et cetera, and the normal operating stresses.

So we tend to get flaws from PWSEC and I have a backup figure that I could always give you at some other time, that tend to say that the flaw lengths, even with the stress corrosion crack in the large diameter line, will be a relatively small percent of the circumference. They are not going to go to these 60 percent, 80 percent of circumference lengths.

You'd have to have a lot of multiple initiations in order for that to occur.

What I'd next like to do is just show you a comparison of all your different -

MS. UHLE: Gery, can I just - this is Jennifer from the staff, and I just wanted to point out what Gery said is not the official NRC position with regard to I would say PWSEC crack behavior and everything. So this is anecdotal and provides some perspective here, but I don't want anybody to walk away from this saying, oh, okay, this is how NRC perceives PWSEC to go around big pipes. Is that safe to say, Gery? I mean this is your professional opinion with regard to your analyses that you have done with with Wolf Creek. But the Wolf Creek question was not specifically asked to address that, I would say, you know, how PWSEC flaws are growing around large diameter pipes.

MR. WILKOWSKI: Right, right. Again, I tried to qualify that by saying if there was only one initiation site; if you had multiple initiation sites you'd get a larger flaw.

MEMBER BONACA: And I wasn't specific about Wolf Creek, except it provides us with a very recent event that is really applicable to this study.

MEMBER SHACK: But what is the schedule for the mitigation of the hot leg welds? Just as a matter of curiosity, even though it's not an official -

MR. SULLIVAN: The mitigation plan was coming from MRP-139, which was an industry voluntary initiative.

My name is Tim Sullivan by the way.

And it comes in kind of two categories. The first category has to - they both have to do a size. I think the break point is about 14 inches. So the piping that - and I'm not sure exactly where the cut is, but for purposes of illustration, I think it's 14 inches and below have to be mitigated by the end of 2008, and then the hot leg piping, larger than that, is 2009. And then the cold leg piping irrespective of size has to be mitigated by the end of 2010.

MS. UHLE: Jennifer Uhle again, and I just want to point out that certainly the TBS, when we put this in perspective of 50.46a, the risk-informed large break LOCA rule, certainly the TBS, or the surge line, is typically less than - I mean the TBS is set less than or equal to, typically, on a PWR, the surge line here, which is the area that you are talking about with regard to Wolf Creek, and really where the deepest cracks were on the relief nozzle, even of a smaller diameter pipe.

MR. WILKOWSKI: So the next plot I'm going to show you is just a comparison of all the different analyses that we did for the very long cracks, when you are out here, with cracks that are say 80 percent of the circumference.

And if you look at a plot of that, here you can see - here's the best estimate, critical flaw, A over T value, that is the depth of the surface flaw to the thickness of the surface flaw. That's at least 80 percent around the circumference where that curve was pretty flat.

Compared to the plan-specific normal plus 10^-5 seismic stresses with all the adjustment factors that we put in there, you see a graph that occurs like that. And you get a line to the lower bound, and the lower bound points 10 to the surface cracks that are about 40 percent of the circumference, or 40 percent of the wall thickness. So they are very deep surface cracks. These are very large cracks that would have to occur for the 10^-5 type event.

This lower line, I will show you material specific results on the next figure, is really for the stainless steel submerged arc welds. Our carbon steel welds tended to be up on the higher side, but we did not consider any cask stainless steels that could be very sensitive to thermal aging in this study.

This next figure is the same type of

result, but for the 10^-6 seismic stress being used. And for that case, what happens then is this lower bound, A/T value drops from .4 to about 30 percent of the circumference.

MEMBER SHACK: Again, these plots confused me a little bit when I first looked at them. But these are just different piping systems, different plants. And if I look at one piping system, I actually get up to 35 KSI in it, and that piping system I could be down to .3, and in another piping system I only get to 10 KSI, and I can -

MR. WILKOWSKI: Yep. Yep, 27 different plants, and 52 pipe systems within those 27 plants, and plot all the results up and this is what you get.

MR. CHOKSHI: And I think the plot I showed that unnormalized, you can see how the slopes varied on site to site, that's showing up there.

MR. WILKOWSKI: So before we started this, we wanted to make sure we weren't down to flaw depths that were in the workmanship size flaw, you know, 10 percent of the wall thickness, because maybe inspection capabilities are limited.

So these are showing us that we have to have really big flaws even at these high stresses, surface flaws. So that was good news.

The other approach was rather than using the ASME surface flaw evaluation procedure was to use the leak before break procedure that the NRC had and had been approved for these particular plants.

And the standard LBB analysis versus the SSE stresses with the applicable safety factors of like 10 on leak rate, and a safety factor of two on crack length. So there are really two safety factors in there.

What we did then is, we also did an analysis for 10^-5 and 10^-6 seismic loading to consider the cases with different safety factors, lower safety factors for those high stress conditions, to see if the normal leak before break analysis that had been done still provides the leakage protection against the critical flaw sizes that could occur at these very high seismic stresses.

This is a plot of one of the sensitivity studies. Somebody asked about sensitivity studies and uncertainty analysis. And in this particular case, let me do a leak before break analysis, the leakage size flaw is very sensitive to the analysis that you use in the leak rate calculations.

And in the leak rate calculations you have to assume that you have a certain type of crack with a certain number of turns, roughness or crack morphology parameters occurring there.

And we had some results to say how we could characterize different types of cracks based on what cracks looked like when they were removed from surface.

So we had those for a PWSEC crack, a corrosion fatigue crack, and an air fatigue crack. And the reason I put the air fatigue crack up there is, in the original LBB analysis, many times it was assumed that if a crack existed in the plant for the LBB analysis, it would be a crack that had the same morphology characteristics as an air fatigue crack, that is, a very smooth crack with no turns to it.

And so I wanted to just point out the differences between what was used in the original LBB analysis, versus PWSEC and corrosion fatigue. This type of plot shows, here's the leakage flaw size relative to the critical flaw size. And in this case this is a 10^-5 seismic loading with no safety factors on the crack length.

So what it really shows is that all of

these occur for different plant cases, plant S, a cold leg, another cold leg, a crossover leg, a hot leg, another hot leg; I just took a number of examples here, that when you plot them up you see that the values are always less than one, which is good. That means you have leak before break behavior naturally occurring without any safety factors applied to the crack length.

However there is the safety factor on the leak rate here, because usually you have all - one GPM is a tech spec leak rate versus a factor of 10 on that to get you to the 10 GMP leakage size crack that we use in the leak before break analysis.

You could normalize the way I chose to normalize these plots is to take the normal stress and divide it by the normal plus the seismic stress, so it was just my way of putting the data from many plants on the same plot, and you tend to get a trend curve like that, and as you would expect, as the normal stresses become a smaller percent of the normal plus seismic stresses, you are tending more to go to not having leak before break behavior.

MEMBER SHACK: Since all these plants had to meet the LBB criterion with an SSE loading, that means the SSE loadings are a lot less than the 10^-5 seismic loading?

MR. WILKOWSKI: Yes, quite a bit less. Quite a bit less. And the details of that are in the report, as to how we determined - we had the accelerations for the SSE, for each of the plants, and we had the seismic hazard curves for each of the plants. And we had - I said the SSE stresses.

MR. CHOKSHI: You know, when you are doing the revision of the siting of the probabilistic hazard, the rough estimate for the recent newer plants would be 10^-4 design if you were to use the newer one, or less; and when an order of magnitude in the frequency has a significant impact on the increase in G.

MR. WILKOWSKI: Oh, yes, significant changes.

MEMBER SHACK: That's a rough estimate, then, that the typical SSE is a little bit more like the 10^-4 hazard?

MR. CHOKSHI: Roughly. But when we were looking at finding some reference probability type thing, the 10^-4 was -

MR. WILKOWSKI: Okay, so this next figure here shows, if you take the 10^-5 seismic stresses with all the correction factors, and we put a safety factor of 1-1/2 on the crack length rather than two that we used for SSE, most of the plants still had leak before break behavior, because they were below this alignment point of one.

There was an occasional plant that might have been above it slightly, but I'd like to also note that was using a safety factor of 10 on 1 GPM leakage detection capability.

Now the later plans had submitted LBB analysis had gone ahead and demonstrated, and it was acceptable to the NRC, to use a half GPM instead of one GPM for their leak before break analysis.

And so you can see what happens if you had the five GPM instead of the 10 GPM type of cracks, and that one particular case that was above the line now falls slightly below the line.

I think there is also some industry studies, more recent, within the past few years, that are trying to show that they could detect leakage at much lower than even a half GPM.

MEMBER SHACK: Okay, and that's at least for a corrosion fatigue crack rather than an air fatigue crack?

MR. WILKOWSKI: Yes, so I added in something in there saying that, well, if you had a PWSEC crack you probably ought to mitigate that thing anyway. So let's do something better, a little bit more conservative than just the air fatigue crack, but something not quite as bad as a PWSEC crack, because you got to get rid of those guys.

MR. SULLIVAN: Gery, could I make an addition?

The staff analyses that Gery was talking about, we still maintain a safety factor of 10. So when Gery is talking about 5 GPM, sensitivity was at least as good as detecting a .5 GPM leak.

MEMBER SHACK: Right, I was just sort of thinking how much of that gets eaten up by the fact that if I have a PWSEC some of my 10 goes off to another bin. But have something here.

MR. SULLIVAN: Right, well the other thing is that these plants have been able to show that they can detect changes as small as like .15 GPM from data dump. Ted Sullivan.

MR. CHOKSHI: You know the purpose of this study was to put all the relevant information on the table so people can comment, so we are not trying to draw conclusions. We have the capability to do anything, but here is what happens if you do different things. And that's all that was presented.

MR. WILKOWSKI: So the prior figure that I showed you had the PWSEC versus corrosion fatigue crack. So Bill, you can see that is the difference that you have there between PWSEC and the corrosion fatigue crack.

And of course when we did this study this was when there were only a very few PWSEC cracks to even look at to determine the crack morphology parameters for doing a leak rate study.

There's some ongoing work to try to do some improvements to that.

MEMBER SHACK: One ligament in the crack will throw all this off anyway, so.

MR. WILKOWSKI: So the key findings from this piping analysis was that in most cases the ASME maximum allowable surface flaw evaluation - or surface flaw sizes normal plus SSE or surface level D condition, was smaller than the critical flaw sizes at 10^-5 or 10^-6 seismic event loading, so that was very comforting.

The critical flaw depths are larger than 40 percent of the wall thickness for the 10^-5 type of seismic stresses, and they are extremely long flaws, even at 40 percent deep. Similarly, large flaws that the critical flaw depths would have to be 30 percent of the wall thickness at 10^-6 seismic event. And again that will be almost all the way around the circumference.

So that shows that there is a lot of flaw tolerance for the surface flaws. Even if the cases would be below what the ASME natural protection would provide, the NDE techniques still should be able to pick up those very large flaws. I'm not an NDE expert; just my professional opinion.

Leak before break flaw size is associated with the SEE loading are much smaller than the critical mean flaw size at 10^-5 and 10^-6 seismic events, for most cases. When we applied a safety factor of 1-1/2 of 10^-5 stresses, or I'm sorry, a safety factor of 1-1/2 on the crack length for the 10^-5 stresses, and then we use a safety factor of one for the 10^-6 stresses in doing that leak before break comparison.

There are a few cases that don't pass with these safety factors, but they could do it with lower leakage detection capabilities if they wanted to demonstrate that.

The other last thing that I should say is, all of these findings here are relative to most of the materials we looked at, except for each cast stainless steels, that could be very susceptible to thermal aging. Those would have to be evaluated in a case specific study.

CHAIRMAN APOSTOLAKIS: You gentlemen would like a break before we go to indirect? Okay, so we'll reconvene at 3:00. You need what, about 15 or 20 minutes?

MR. CHOKSHI: Yes, about 15 or 20 minutes.

CHAIRMAN APOSTOLAKIS: There was a question here from John Stetkar, let me ask you before we break.

The same medical state factor is applied over the entire range of evaluated PGAs. For example for plant A the scale factor is .64. Is it reasonable to assume that the same numerical safety factor for piping design and for location applies at seismic accelerations up to 10 times higher than the SSE?

MR. CHOKSHI: Which safe scale factor is he talking about, .64?

CHAIRMAN APOSTOLAKIS: Yes.

MR. CHOKSHI: Oh, that was an example. That varies case to case.

CHAIRMAN APOSTOLAKIS: Yes, but -

(Simultaneous voices)

CHAIRMAN APOSTOLAKIS: that's the question, the constant scale.

MR. CHOKSHI: It's just linear elastic scaling. It's a linear stress, it's linear elastic behavior.

CHAIRMAN APOSTOLAKIS: So it's a constant?

MR. CHOKSHI: Constant scale.

MEMBER BONACA: The evaluation of this factor, I mean is it a standard procedure? Is it accepted?

MR. CHOKSHI: The scale factor I talked about in the PRAs? Yes, for the seismic PRAs that's the standard approach, and has been in use for about 25 years. There has been refinement, but that basically - it's called separation of variable approach, where you break up the responses and capabilities of the independent variables.

MEMBER SHACK: But somehow that must be

affected by the amount of plasticity that I'm getting.

MR. CHOKSHI: Oh, yes. Yes.

MEMBER SHACK: That wouldn't seem like it ought to be constant over that whole range of accelerations.

MR. CHOKSHI: No, if you were to - the reason why because the failure criterion was also formulated with that behavior in mind. So it's consistent with what the failure criterion -

MEMBER SHACK: Oh, I see, the failure criterion sort of includes that effect.

MR. CHOKSHI: Yes, if I had a different -

MEMBER SHACK: If you had a different way of calculating that, you'd get a different failure -

MR. WILKOWSKI: That's for unflawed - the unflawed piping failure criteria.

MR. CHOKSHI: That's why we are to apply correction when we went to the nonlinear correction, which changed that constant factor.

MR. WILKOWSKI: So I had an additional scaling factor that I put on for -

(Simultaneous voices)

MR. TREGONING: The plasticity, right.

CHAIRMAN APOSTOLAKIS: Okay, so we will

reconvene at 3:00 o'clock.

(Whereupon at 2:49 p.m. the proceeding in the above-entitled matter went off the record to reconvene at 3:12 p.m.)

CHAIRMAN APOSTOLAKIS: We are back. And the last presentation is on indirect failures.

INDIRECT FAILURES

MR. CHOKSHI: It doesn't make a different, the type of things we are talking about here. Okay, so I'm going to talk about another type of failure mechanism, which we have to consider in terms of the coming of the break sizes larger than transition break size.

There are two typical I think failure modes are looked at in this, something falling like heavy crane or some real measuring equipment falling on the CS piping system, or the loss of support of a major component.

And the most likely scenarios stated here is the failure of supports, and then when support of a heavy component like steam generator.

In order to come up with estimates of the indirect failure frequency, we use the results from the earlier Lawrence Livermore study I talked about which was done in the mid-`80s. The Livermore study was conducted as I mentioned a couple of - two answers basically, should doubled ended guillotine break be a design basis for the dynamic crack effects of a postulated pipe break? It was like pipe be restrained.

And second question was, should LOCA be combined with the SSE?

The - what the Livermore study did, they grouped plants according to the vendors. There are three PWR groups, and they also looked at one BWR.

For indirect failure, they basically looked at the sample plants, looked at the configuration on the plant specific basis of the component supports, identified critical component supports, and then estimated their fragilities.

And in part of the fragility approach was very similar to what was used to develop the seismic stresses for unflawed piping.

And in the Lawrence Livermore study, they did a one generic curve for east of the Rockies, and they used that seismic hazard curve to come up with the failure probability.

Now most of the methodology is still valid in terms of particularly the approach. We had to make some adjustment. We had to correct for the new hazard information. We also had to change the estimates of fragility to account for the site specific spectra shape.

So out of the Livermore study results, and I'll show you the result in a minute, we picked two plants.

CHAIRMAN APOSTOLAKIS: This last assumption there?

MR. CHOKSHI: I will come and talk about that in a minute.

So we took two - we basically selected two plants, two supports from the Livermore study, because one was characterized in the Livermore study as the bounding Westinghouse, and then we chose on the rock side, and then we looked at one other plant on the soil slide. And then made the adjustment.

Now on the last bullet, I think this goes to some of the risk argument, you know, what happens to the seismic risk. In the last component about risk, you know, I feel as I mentioned, 1150 study there was like a distinct jar in support on Millstone 3 there was a scenario where the crane was falling, then I think there were a couple of other plants where there was a large - and at that point it becomes impossible to do any kind of progression analysis of accident. You basically assume that you are going to have breaks that are beyond your mitigation capability, and that you know you basically go to core damage.

So that's the inherent, you know, assumption made into all of the studies. And I think - but what happens with that, that's why, when you look at those large earthquakes, and what happens with the rest of the plant in terms of the entire risk, this kind of failure, a lot of other things are happening also. And typically on the PRAs these sequences don't contribute to the core damage, but they show up because you also breach the containment slightly, because like steam generator moving, it's going to move that much, it's going to yank out a penetration somewhere.

So that goes to I think the last bullet, that here is - that is a typical assumption. But one other thing I want to point out from the PRA, you know the PRA basically has looked at this failure more closer than anything else, and I think when we did the seismic margin approach, we basically ruled out that 1.5 G level earthquake, the heavy component supports are a capacity higher then we don't need to look at that. The only exception was the PWR pressurizer support at .5 G, you would look at it, and the PWR vessel and the stack support.

So it's been well recognized that these components have very high fragility, and most of the time, which is not surprising, the way the loading combinations and things are designed.

So the failure probability of this indirect failure is low it's not surprising. But what I want to show next is two things. One is the resource from the original Livermore study. And this shows the combustion engineering plans they looked at. If I look at the values, the 50 percent values, you know, they are ranging from 10^-7 to 8, you know, that range, and we made a modification to that calculation using the Livermore hazard and adjusting the fragility, we get about 1.72 - two times 10^-6 mean frequency.

The Westinghouse, in the bottom of this

table, that was the lowest capacity plant, and they were getting about three times 10^-6 at the median level, and when we did that study, the old mean value was about 2.7 X 10^-6, and I think Dr. Bonaca, you asked questions about the uncertainties on all those median values. What we did here was, we used two different total uncertainty values. One we used a beta composite of .42 and .62. .62 is very high, it's log normal distribution. And the only reason we used it, because that's what Livermore had used originally. In the recent information, if you were to use a generic beta C value, you probably would use .44 or .45.

So but that was the way to assess what happens if uncertainties are not larger. We didn't really do the separate calculations.

Now I mentioned EPRI, and the EPRI is a part of the response to public comment, looked at the impact of new hazard. And they did three cases. They selected, also looked at one BWR plant. And they looked at rock sites. And their calculations ranged from about 6 X 10^-6 to 5X10^-8, which again, this Westinghouse plant - now, they applied some other correction factors which are used inside the new reactor licensing, and we didn't use that, so I'm giving you the results, but as you'll see in my last slide -

MEMBER SHACK: And those were mean values again?

MR. CHOKSHI: These are mean values. Now on the fragility they applied, for example, some correction factor for incoherency, which we did not at the time this thing was developed. But we haven't evaluated specific details. They have done some other assumptions. So I'm just giving you results we made after we look at what there is.

But you still get results that are less than 10^-5. I think that there is still some conservatism built into this, so I think it seems that at least if you - if 10^-5 is your threshold, this is definitely below that.

So now I think overall there should be a fourth bullet here, but it's not. But looking at all of these aspects, basically for unflawed piping I think it's clear that the frequency is considerably less than 10^-5.

I think that one of the major - at least the finding may put to informed people so they can make informed comments was the flaw sizes associated with these earthquakes, and also how the leak behavior compared to these faces.

And then finally for the indirect piping failure, at least some of the cases we had, that extended less than 10^-6.

So this was the, as you will see in the report, these are the key findings.

CHAIRMAN APOSTOLAKIS: I thought you were going to say something about the scenarios too. Remember the question earlier about -

MR. CHOKSHI: Yes.

CHAIRMAN APOSTOLAKIS: - the earthquakes shaking the whole plant.

MR. CHOKSHI: Right. Typical scenario, was the PWRs, you basically lose off site power. Either you are going to lost onsite power or lose a component filling or something. Eventually you wind up in the reactor pumps LOCA, or at certain high levels of earthquake that the tubing and other things, small break LOCA, you know, would happen, because it's impossible to walk down some of the lines in the containment. At certain levels you basically go to the small LOCA.

But the wall movement of those LOCA is still small, and that's why when we went to the seismic margin, we only looked at success files for transients and small LOCAs, and decided that the seismic index of large LOCA is much lower frequency.

CHAIRMAN APOSTOLAKIS: I guess we should have raised that question years ago, when the change in 50.46 was first proposed. But -

MR. CHOKSHI: It was raised in the context of seismic margins and work downs, and what happens with that tubing instrumentation.

CHAIRMAN APOSTOLAKIS: It seems to me there is a difference between what Nureg 1819 does, where they look at the frequency of a large break, they decide at 10^-5 you have a certain size. There most likely the rest of the plant is okay, so the actual risk is lower, much lower.

In your case, the rest of the plant is not okay. So -

MR. CHOKSHI: I was going to -

MEMBER SHACK: 50.46 isn't going to help you.

CHAIRMAN APOSTOLAKIS: Is it reasonable to base a decision just on the initiating?

MR. CHOKSHI: No.

CHAIRMAN APOSTOLAKIS: That's my question.

MR. CHOKSHI: I'll go to my last slide. The risk is one of the most important properties -

CHAIRMAN APOSTOLAKIS: Okay, so what do you say in your last slide.

MR. CHOKSHI: So what I want to do - in fact you're going to hear about that also - but we issued the draft rule with an extensive discussion of whether with the seismic issue that we are still studying, and there is an open question whether a plant-specific assessment will be required or not.

And then we said, do we want you to address - there are basically three aspects. The one was NRC requested specific public comments on the effects of pipe degradation on seismically induced LOCA frequencies, okay, and then potential for affecting the TBS.

The second was the NRC also requested public comments on the results of the NRC evaluation.

And the third item was that the NRC requested specific public comments on these and any other potential approaches, to address this issue.

And that was one of the reasons we wanted to put a lot of comprehensive calculations on this. So these three questions were asked.

And we got an industry response. The post to them basically said that staff conclusions of the study results support that TBS is not affected by seismic.

On the second point, your studies, and that's where - and we had also talked about this argument, but here is - I'm going to read that for you, the NEI response.

The median seismic capacities for both the primary piping system and the primary system components are higher than most other safety measure power plant components within the nuclear power plant.

At the very high accelerations associated with the point at which the primary piping or the primary system components will fail, many other similar structural systems and components with work capacities fail.

Now we - I mean that's - and I think that seems to be intuitive that some of this is now - we have to look at other things. But I think we eventually have to look at what's happening in other things.

MEMBER SHACK: I mean that's really delta risk from LOCAs to seismic. Delta risk due to seismic -

MR. CHOKSHI: Right. So I think - so in my last slide that's one of the things going forward, what are the factors we are to consider, and that to me is the key factor.

After we understand what are all the changes in the rule are, and how we are dealing with some of the questions that come up.

I already mentioned the EPRI cases, that they analyzed to substantiate that even with the higher hazard. And the bottom line assessment that you don't need plan specific assessment.

MEMBER MAYNARD: Did it not get any comment from the general public?

MR. CHOKSHI: No.

MEMBER MAYNARD: Did your questions go out separate from what we talked about earlier?

MR. CHOKSHI: No, what we did went out, and when we published our report, we issued another Federal Register notice, and it was posted on the web; everybody was notified.

MEMBER MAYNARD: But your questions were

separate from the 1829 that went out?

MR. CHOKSHI: Yes.

MEMBER MAYNARD: I was just wondering why some of the other people didn't comment on some of these.

MR. CHOKSHI: No, these questions went out with the rule.

MEMBER MAYNARD: Oh, okay. I understand.

CHAIRMAN APOSTOLAKIS: The Union of Concerned Scientists or Green Peace were not -

MR. CHOKSHI: In fact we had a meeting, and I think Dick talked about that earlier this morning, the public comment. I don't believe anybody from outside raised any question on this.

CHAIRMAN APOSTOLAKIS: Were they obtained?

MR. CHOKSHI: I don't know. But I think since this study was done, as I think along with the rest of this rulemaking process, we basically haven't really done much.

But it seems to me that given what the issues that the CRS has raised, what SRM has inquired, we need to wait and see. In particular, I think the things we need to really evaluate is look at the response to the questions, basically some of the calculations and things. The other thing is very qualitative.

But I think it will be important to understand how did the rule that the Commission has sought, regarding the defensing that and mitigation. This will have a direct effect on the delta risk, and then look at the impact on the risk I think. And I think it will be - it's very hard for me to come up with the conditions under which the risk will be affected. There might be, there might be some power plant parameters or pressure parameters, and if I can come up with a scenario which not only includes seismic failures but random failures, non-seismic failures, then - but I can't think of that.

But you have to look at the whole total picture. And then I want - we have to wait and see now that SRM has said that we have developed guidance on how the 18.29 plant has to come, and that show how the 18.29 applies, and to me that may also equally apply to this area, so I think we have to wait and see.

And then we look at whether plant specific assessment is needed or not needed. So that is where we are.

MR. DINSMORE: This is Steve Dinsmore from NRR. There might have been two questions there, the one question about how seismic affected TBS, and the other is how is the change in risk due to implementation of 50.46 going to be affected by seismic?

MR. CHOKSHI: Right.

MR. DINSMORE: To the second question they'd have to do a change in risk with a PRA analysis. So that would all be caught up in this.

MR. CHOKSHI: I think the important factor would be that whether you include degraded piping in that PRA or not. Because I don't think you can do a full blown PRA, so you have to at least have a scheme that where you - you have to get help with the seismic risk, but when you divorce that other legal issue.

CHAIRMAN APOSTOLAKIS: Perhaps these questions should be raised again when we actually talk about the rules. Because you guys are just providing input to the rule-making.

But you know, since we are on the record we might as well raise some questions. But I myself don't see a problem actually. But it's just that this idea of making a decision based on the initiating event frequency alone, I want to understand that a little better. But the numbers you guys are showing us is so low that -

It'll probably come up again at the full committee meeting by the way.

MS. UHLE: Yes, I was just going to point out that the question about basing a fair decision to go forward, or what a plant could do adopting this rule on just the initiating the event frequency.

It's not in the sense that what Steve just indicated is that whenever a licensee would have to - would say hey, I want to reduce my flow rate to this pump, or I want to uprate power, they would then have to do the submittal and there is a risk criteria.

So that's where you are getting - and part of that will be looking at defense in depth and the matters that are similar to the 1174 type approach.

CHAIRMAN APOSTOLAKIS: The decision I was referring to was that not that, it was the decision of what the PBS is.

MR. CHOKSHI: The initial selection.

MEMBER SHACK: But that's not - that's a definition of a design basis. It's nothing to do with risk. The risk is counted for separately.

CHAIRMAN APOSTOLAKIS: We are risk-informing the ACCS rule. I mean how can we -

MEMBER SHACK: You are permitting risk-informed changes. You are not doing anything to the rule.

CHAIRMAN APOSTOLAKIS: I know it's an enabling rule. I know that.

MS. UHLE: And that's what I'm trying to get -

CHAIRMAN APOSTOLAKIS: I understand.

MS. UHLE: Just don't agree.

(Laughter)

CHAIRMAN APOSTOLAKIS: You said something bad about me?

MS. UHLE: Oh, no, I said you just don't agree.

CHAIRMAN APOSTOLAKIS: No, I agree with you.

MS. UHLE: Oh, okay.

CHAIRMAN APOSTOLAKIS: But the decision you are talking about is not the decision I was referring to. The decision I was referring to was the choice of the TBS by us, which is according to the SRM is based on the frequency of the large LOCA, without consideration of what happens -

MR. DINSMORE: It's based on - well, it's also got in there that they can continue to mitigate up until the double-ended guillotine break without as much assurance as they currently have.

It's also one of the reasons we didn't use the geometric mean just to pluck out the 10^-5. So, but there - yes it is kind of based on the frequency that we are willing to live with.

MR. TREGONING: Well, again, that was the starting point for the TBS selection. There were other considerations.

And my own opinion, I don't know if it's anyone else on the staff here opinion, you could pick any TBS you want. There is nothing magical about the TBS selection. It's the TBS coupled with your defense in depth and the additional mitigation -

(Simultaneous voices)

MR. TREGONING: - that really determines what risk you have associated with beyond TBS event. So really you have to look at everything as a whole I think, and not just look at the TBS, devoid of any other consideration.

CHAIRMAN APOSTOLAKIS: Do you have anything else to say?

MR. CHOKSHI: No.

CHAIRMAN APOSTOLAKIS: Good.

(Laughter)

MR. CHOKSHI: What is coming to full committee, submissions and what we should talk about.

CHAIRMAN APOSTOLAKIS: Your presentation was actually fairly short. But you have to make it shorter.

MR. CHOKSHI: Okay.

CHAIRMAN APOSTOLAKIS: But you're used to it. You did the whole study in three months.

MR. CHOKSHI: I can talk longer than that.

CHAIRMAN APOSTOLAKIS: I'm sure you can.

The only place where maybe you can eliminate some slides is the results of the flawed piping. Maybe just show a representative one rather than showing five or six. But the rest really is just right to the point. This is what we did; this is the result. So I don't know.

Did you guys see any other -

CHAIRMAN APOSTOLAKIS: Good luck.

MR. CHOKSHI: I look at the time, it was 45 minutes total.

CHAIRMAN APOSTOLAKIS: You have 20 minutes of presentation.

MEMBER MAYNARD: But if you go after them you are probably not going to have your 45 minutes.

CHAIRMAN APOSTOLAKIS: So that's all I can recommend. I mean I don't know. Everything else seemed to me to be right to the point.

MR. CHOKSHI: I got some of the discussion down.

MEMBER SHACK: I wouldn't go to justifying your approach. I would just tell you, this is how we did it. You spent some time motivating us here today. At the full committee I'd just say, this is what -

CHAIRMAN APOSTOLAKIS: But you may get questions on the subject. Especially from Mr. Stetkar.

MEMBER BONACA: And I think you'll get questions on that factor.

MR. CHOKSHI: Maybe I'll add one slide or something, add some explanation.

MEMBER BONACA: My suggestion you have to think, for PRA the question that comes next is, what do you use the PRA for? And if it is to do a PRA as we did 15 - 20 years ago and therefore you have to make an estimation of that and apply a factor when you get there, that's plenty acceptable. Is it still acceptable when you want to base a rule change on that?

So if you had the minimal sensitivity, you could show that you had so much margin or whatever. But you didn't say that. In the beginning you said it should now leave without applying the factor. So when you are saying that, I am left with the question in my mind, what is the margin of these sensitivities. How much would these results be affected by that.

And so it's another question. But if you have any means of addressing that, that would be helpful.

I like the approach that you used of this flaw - how do you call it, flaw avoidance approach?

MR. CHOKSHI: Flaw tolerance or exclusions.

CHAIRMAN APOSTOLAKIS: How bad should it be, that's good, smart thing to do.

So -

MEMBER SHACK: Well, it's more believable than any probabilities you'd develop from a full fractal mechanics probabilistic analysis.

CHAIRMAN APOSTOLAKIS: It just occurred to me that the earlier speakers, Rob and Lee, said that they did not exercise to help the rule-making, but also the help the PRA people in the sense that they would have a distribution. Where is the distribution? I want to do a PRA. What is your distribution of the frequency of large LOCA? You didn't show it to us.

MR. TREGONING: We showed -

CHAIRMAN APOSTOLAKIS: Oh you showed me a hell of a lot of insights.

MR. TREGONING: We showed parameters from a distribution, medians, means, 95ths.

CHAIRMAN APOSTOLAKIS: Can you give me the distribution, Rob? I want you to tell me, is it log normal, or 50 or 90^{th} percentile? Can you do that? Or would you have to do some work?

MR. TREGONING: We can give you the numbers to use for the various percentiles.

CHAIRMAN APOSTOLAKIS: Log normals, right?

MR. TREGONING: We don't make assumptions about the final - we made split log normal assumptions for the inputs but not the final -

CHAIRMAN APOSTOLAKIS: Then you at your presentation next week have a slide that says, and this is the distribution that you PRA guys should be using?

MR. CHOKSHI: You can show the comparison between -

CHAIRMAN APOSTOLAKIS: No, no comparisons, I want a distribution.

MR. CHOKSHI: The way people are using the PRA.

CHAIRMAN APOSTOLAKIS: Oh, you can talk about it. But it would be nice to see the actual distribution, because, without me having to derive it from other information, here it is. Is it log normal by the way?

MR. TREGONING: It's pretty close. It's closer to log normal than anything else.

CHAIRMAN APOSTOLAKIS: That's very good. Then we can use log normals to approximate by log normals. Not so precise that if I approximate by log normal I would distort anything, right? But it would be nice to show that as a definitive result of this study.

So any other comments to the staff? Thank you very much. This was really a good subcommittee meeting, both earlier today and this afternoon.

Now I need some advice from my colleagues. Shall we start with you? How about we start with Bill this time?

MEMBER MAYNARD: Take your pick.

CHAIRMAN APOSTOLAKIS: I'll take Bill.

MR. CHOKSHI: So we are excused to go?

CHAIRMAN APOSTOLAKIS: Yes, thank you very much.

MEMBER SHACK: I think the exercise has been very well done. You know we've supported it in the past. I think they've made a good case I think for using the geometric mean as a proxy for the median, which strikes me as the right way to go.

CHAIRMAN APOSTOLAKIS: Although it doesn't really matter. From the rule-making point of view, it rule doesn't matter.

MEMBER MAYNARD: Well, in this case it didn't matter.

CHAIRMAN APOSTOLAKIS: Or you mean from the seismic?

MEMBER MAYNARD: Well, for the first part too.

CHAIRMAN APOSTOLAKIS: For the first part? I don't know.

MEMBER SHACK: In a large context, of course, our problem with 50.46 has never been the choice of the TDS, really. I think they - I still think the NRR choices are quite conservative for the TDS based on these results. But whether they had a conservative choice or a non-conservative choice, I'd still feel the same way about the defense in depth requirements.

But I do not think this does provide a good technical basis for choosing a TDS, the seismic stuff supports -

CHAIRMAN APOSTOLAKIS: Very good.

MEMBER SHACK: - what they need to address, I think, with the seismic questions. And again the results aren't terribly surprising, but I think they give you the results you need in order to use it.

CHAIRMAN APOSTOLAKIS: Okay, Mario?

MEMBER BONACA: I echo Bill. I must say I was surprised a little bit by the margin we found for flawed piping, but it was more like, it was rewarding to see that it was a margin. I already made a comment regarding that scale factor. And I think that the results are credible and I think this supports the rule.

CHAIRMAN APOSTOLAKIS: Thank you.

Otto?

MEMBER MAYNARD: I don't really have any concerns or issues with 18.29. I think overall for what the task was I think it's meeting the objective.

I think it is a defendable approach considering everything together. It is far from a bullet proof approach. I don't think there is any methodology, any set of data, anything that is going to come up with a definitive answer on anything. So I think that the approach that was used is good for what we're having to deal with here.

I look forward to the year 102000. By that time we will probably start gathering data to know. So we're dealing with -

CHAIRMAN APOSTOLAKIS: You will not be on this committee at that time?

MEMBER MAYNARD: I won't? I was hoping I would last that long, but I guess that'd be more than the four terms.

We talked about it a little bit. I think it's important to always keep it in perspective. This is never going to come out with a definitive number, and the number, whether we're talking transition break size, or even what the probabilities are, there is never going to be a real definitive number. We are really looking for relative importance of things, and then what do we do with that data, with that information?

We're looking at how we bend things into high, medium, low or incredible probability or occurrence, and then it's up to the rule and the reg guide to deal with, now considering all this, what do we do to really make sure that we do provide protection to the health and safety of the public in a reasonable way. And I think we have to be careful that we never try to defend or imply that these are definitive numbers, either break size or probabilities.

But I think for what the task is I think we should support this.

CHAIRMAN APOSTOLAKIS: What is the question that we are answering in our letter? To issue this or what? Jennifer, what is the request or the decision?

MS. UHLE: From the full committee that's what we're looking for is whether or not the Nureg 18.29, the seismic analysis, complies with the report technically so that we can publish it and move on.

Then another - a secondary question will then be as part of the 50.46a rule-making will be the regulatory guide. And that's later.

CHAIRMAN APOSTOLAKIS: But next week it's just should be published or not.

Now why doesn't the seismic report have a number? Is it an appendix to something?

(Off-mike comment)

CHAIRMAN APOSTOLAKIS: So it's XXXX?

MEMBER SHACK: But it is going to be republished as a new reg or a new reg CR.

MEMBER MAYNARD: We're still on the record, so you need to be at a microphone so you she can catch it.

MS. UHLE: I'm just speaking for Nilesh here, but it is going to be a separate new reg, other than Nureg 18.29, and we don't know the number yet.

CHAIRMAN APOSTOLAKIS: Well, I agree with you guys, this was interesting. I think that - I especially agree with Otto that as I said earlier today, it would be a mistake to try to defend one of these approaches, the geometric mean or whatever, as the approach. This is a good input to rule-making, to decision making. It looks at risk evaluations in the generic sense from different perspectives; recognizes that there is no unique way of doing a particular thing like handling overconfidence and so on; and it provides a number of insights into the decision making.

And I think if you literally, from that perspective, it's really a great piece of work. So -

MEMBER SHACK: Should these estimates now be used for PRAs?

CHAIRMAN APOSTOLAKIS: I think - I want to see the final distribution that Rob is going to show us, and I hope it will not be just a - where is Rob?

MS. UHLE: Can I just ask that question about it's use for PRAs, whenever anybody uses something, submits it for license application review, it's up to NRR to evaluate the data and say, okay, is it adequate to support the action that the -

MEMBER SHACK: No, that was more a question for George as to whether we should say something about it in our letter.

MS. UHLE: I just want to say at the full committee meeting we're not - research is not going to be the one to say this should be used for PRA and we support it. Because that's NRR's decision.

CHAIRMAN APOSTOLAKIS: But there was a statement at the beginning of the day that this project was supposed to support the rule-making plus help the PRA people.

I understand that you cannot -

MS. UHLE: To support, and can be used, but still has to be justified by the licensee. And NRR is the call on whether or not it can be used in the way the licensee wants it used.

CHAIRMAN APOSTOLAKIS: But can the authors of 18.29 say based on all the stuff we have done here is our state of knowledge regarding the frequency of large breaks?

MS. UHLE: Yes.

CHAIRMAN APOSTOLAKIS: That's all I want.

MEMBER SHACK: Well, they've done that for large breaks, for small breaks, and for medium-sized breaks. And the numbers are different than what people frequently use these days.

CHAIRMAN APOSTOLAKIS: Yes.

MEMBER MAYNARD: But they still may not be the numbers that NRR uses to find acceptable.

CHAIRMAN APOSTOLAKIS: No, no, that's a

Nureg reports. Nureg reports are not regulations, okay, you know that.

So Rob, breaks of various sizes, not just large breaks. Distributions.

MR. TREGONING: That's what you want to see at the main committee?

CHAIRMAN APOSTOLAKIS: Yes. All right, anything else?

MR. TREGONING: Do you want numbers or curves?

CHAIRMAN APOSTOLAKIS: Curves, with a little legend on the side that says 93 percent or 3 percent. And a log normal approximation would be nice. I mean if it's close to log normal, why not?

MEMBER SHACK: How close is close enough?

CHAIRMAN APOSTOLAKIS: Well, this has been a very good meeting. Anybody else has a comment? From the members? From the staff?

I guess the public is not here. So thank you very much. Thank you all. This was very informative, and this concludes the meeting.

(Whereupon at 3:49 p.m. the proceeding in the above-entitled matter was adjourned.)