Official Transcript of Proceedings ACRST-3300

NUCLEAR REGULATORY COMMISSION

ORIGINAL

Title: Advisory Committee on Reactor Safeguards
518th Meeting

Docket Number: (not applicable)

PROCESS USING ADAMS
TEMPLATE: ACRS/ACNW-005

Location: Rockville, Maryland

Date: Thursday, December 2, 2004

Work Order No.: NRC-127

Pages 1-284

NEAL R. GROSS AND CO., INC.
Court Reporters and Transcribers
1323 Rhode Island Avenue, N.W.
Washington, D.C. 20005
(202) 234-4433

SISP
Review Complete

TROY

DISCLAIMER

UNITED STATES NUCLEAR REGULATORY COMMISSION'S

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

December 2, 2004

The contents of this transcript of the proceeding of the United States Nuclear Regulatory Commission Advisory Committee on Reactor Safeguards, taken on December 2, 2004, as reported herein, is a record of the discussions recorded at the meeting held on the above date.

This transcript has not been reviewed, corrected and edited and it may contain inaccuracies.

UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

+ + + + +

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS (ACRS)

518TH MEETING

+ + + + +

THURSDAY,

DECEMBER 2, 2004

+ + + + +

The meeting was convened in Room T-2B3 of Two White Flint North, 11545 Rockville Pike, Rockville, Maryland, at 8:30 a.m., Dr. Graham B. Wallis, Chairman, presiding.

MEMBERS PRESENT:

MARIO V. BONACA	Chairman
GRAHAM B. WALLIS	Vice-Chairman
GEORGE E. APOSTOLAKIS	ACRS Member
F. PETER FORD	ACRS Member
THOMAS S. KRESS	ACRS Member
RICHARD S. DENNING	ACRS Member
DANA A. POWERS	ACRS Member
VICTOR H. RANSOM	ACRS Member
STEPHEN L. ROSEN	ACRS Member-at-Large
WILLIAM J. SHACK	ACRS Member
JOHN D. SIEBER	ACRS Member

ACRS STAFF PRESENT:

SAM DURAISWAMY Technical Assistant, ACRS/ACNW,

JOHN T. LARKINS Designated Federal Official

I-N-D-E-X

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

8:31 a.m.

CHAIRMAN BONACA: Good morning. The meeting will now come to order. This is the first day of the 518th meeting of the Advisory Committee on Reactor Safeguards. During today's meeting, the Committee will consider the following: Expert Elicitation on Large Break LOCA Frequencies, Proposed Rule for Risk-Informing 10 CFR 50.46, Technical Basis for Potential Revision of the Pressurized Thermal Shock Screening Criteria in the PTS Rule, Preparation of the CRS Reports and Safeguards and Security Matters.

A portion of the meeting will be closed to discuss safeguards and security matters. This meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act. Dr. John Larkins is the Designated Federal Official for the initial portion of the meeting.

We have received no written comments or requests for time to make oral statements from members of the public regarding today's sessions. A transcript of a portion of the meeting is being kept, and it is requested that the speakers use one of the microphones, identify themselves and speak in sufficient clarity and volume so that they can be readily heard.

Marvin Sykes will be leaving the ACRS staff on December 17, 2004 to join the Region 1 staff as the Branch Chief, Reactor Program, Division of Reactor Safety. As a Senior Staff Engineer, he as provided outstanding technical support to the ACRS in reviewing several important matters, including license renewal applications, digital instrumentation and control systems, fire protection issues and electrical group reliability. We would like to thank him for his contribution to the Committee and wish him good luck in his new position. Thank you.

(Applause.)

When is your last day?

MR. SYKES: The 17th.

CHAIRMAN BONACA: The 17th. So we'll see you once again for the MOx fuel meeting. All right.

We will begin with some items of current interest. You have in front of you a package. You may be interested. Inside there are articles to new commissioners. There's an article on that, Pages 12 to 16. You may also note, Pages 19 to 22, that the final 50.69 rule was released. There were some changes made at the last minute after we reviewed it.

You may be interested in looking at those changes. And I believe that Mike Snodderly put together a brief memo that we'll distribute later on highlighting those changes that we have not reviewed.

With that, I think we'll move to the items on the agenda. We have --

MR. RANSOM: You left out the most important.

CHAIRMAN BONACA: Oh.

MR. RANSOM: Pages 27 to 28.

CHAIRMAN BONACA: Pages 27 to 28, let's see. Oh. There is an article on "New Project Flawed," published by the Cape Times. That's a very interesting article. Did you write it?

MR. RANSOM: No, no.

(Laughter.)

CHAIRMAN BONACA: All right. We'll be looking at it. Okay. We have the whole morning dedicated to 50.46, first of all to the elicitation work that has been done and then to the rule. So we will move right away to that item on the agenda, and Dr. Shack is going to lead us through that presentation.

MR. SHACK: Let me turn it over to Rob Tegoning.

MR. TEGONING: Thanks for the introduction Dr. Shack.

This is a little bit of a change with what's in the agenda. The agenda item is to talk about or discuss the expert elicitation on large break LOCA frequencies. When we presented at the Subcommittee meeting about two weeks ago on regulatory policies and practices, it was clear indication from the Committee that they really wanted to see how these elicitation results were used to select the transition break size. So we've modified this talk a little bit and what you're going to see here is a focus on the elicitation results but only on how the elicitation results set the table for the actual TBS selection.

So I will be giving the first half of this talk, again, focusing on those portions of the elicitation that are most relevant for the transition break size selection, and then I'm going to be turning it over to Gary Hammer at NRR who's going to say quite eloquently how they took our information as a starting point and then finally arrived at the transition break size. And he's going to lay out the logic and some of the thinking and the rationale that went into that selection.

So the presentation objectives, I'm going to be leading the first part, providing an overview of the elicitation scope. The pertinent results for TBS selection and some of the uncertainty that arises when you analyze and process the raw input that we got from the experts in a variety of different ways. Then as I mentioned, Gary is going to launch into a description on the approach for selecting the transition break size that's being proposed in the 50.46 risk informed alternative. And that approach, as he's going to describe, used the elicitation results as a starting point. It made sure it incorporated uncertainty and variability within these results, and then it also considered adjustments to account for LOCA frequency contributions which were explicitly considered within the expert elicitation process.

So I think it's important -- I've stated this several times to the Committees, probably three or four different times, but I think it's important again to stress this first slide, which is why it's really up here, to discuss what we did, what were the specific objectives and scope of the elicitation. So which piece of the LOCA frequencies were we really trying to get at with the elicitation?

Again, the primary goal was to develop

generic BWR and PWR piping and non-piping passive system LOCA frequency distributions as a function of the pipe break size or the break size or the opening break size and the operating time.

MR. APOSTOLAKIS: Rob?

MR. TEGONING: Yes.

MR. APOSTOLAKIS: What is a generic distribution?

MR. TEGONING: Generic distribution, we meant to -- essentially, fleet average is another way to consider that.

MR. APOSTOLAKIS: What does that mean? I mean if it's a fleet average, is it a number, a single number? I mean if you have a distribution -- in the reactor safety study when they talked about generic distributions for failure rates, they emphasized that it was the plant-to-plant variability that was a major contributor to those distributions. But I think you had told us that plant-to-plant variability was not a major factor in your case. In fact, if you look at the discussion on safety culture, you say, well, maybe in some plants we may have a higher frequency but we don't really care about -- or we're not concerned with plant-to-plant variability. So I'm wondering how you define and whether actually the experts understood what you meant by generic.

MR. TEGONING: Yes, and we've discussed this previously. I mean by generic we were looking at broad industry averages. We did instruct the experts to consider broad differences in plants, differences due to different design types, but not to delve into differences that might exist at one particular plant.

MR. APOSTOLAKIS: Why not?

MR. TEGONING: Because the way we've developed and used LOCA frequencies in the past has always been on a generic basis. And when we were setting the regulation for 50.46 it made most sense to develop a basis for that based on a generic average, not -- we didn't want this regulation to be driven by frequencies that might be representative of only one plant.

MR. APOSTOLAKIS: So the plant-to-plant variability then will be covered by the selection of the TBS, which presumably will be higher than your estimate. Because somebody has to worry about it, it seems to me.

MR. TEGONING: Well, there's some aspect in the selection of the TBS that covers that, but, again, there's other -- and I think somebody from NRR may want to speak about this, but there's other procedures and practices that we use to try to minimize plant variability, especially in the area of LOCA frequency. And the understanding is that those procedures and practices are going to continue to be in place and continue to be enforced. So I don't know if Rich or --

MR. BARRETT: This is Richard Barrett. I'm with the NRR staff. The selection of the TBS at this point is also a generic consideration. I think the one place where plant-specific LOCA frequencies might come into play is in the risk-informed aspect of this, which you heard about in some detail in the last ACRS meeting. At the point when licensees want to apply this rule, they will have to bring their PRAs in and apply them to plant-specific licensing actions, for instance. At that point, PRA practice, as you know, as you well know, can sometimes use generic or, as appropriate, use more plant-specific information regarding LOCA frequencies. And a licensee may be able to make the case that they deviate from the generic results based on specific operational experience with regard to inspections of the reactor coolant pressure boundary, and I would be speculating at this point about that. But so far everything we've done, up to the point of choosing the transition break size, has been based on generic BWR, generic PWR considerations.

MR. APOSTOLAKIS: Thank you.

MR. TEGONING: Okay.

MR. POWERS: Rich, let me just follow up on that. Suppose a guy comes in with his PRA and he has a peculiarly susceptible piping system. How do you detect that?

MR. BARRETT: Well, I think the correct answer to that question is right now I don't know. You know, that would -- we are in the process over the past three or four years of gaining a great deal more experience with our knowledge of the degradation mechanisms and operational experience with inspections, visual inspections, non-destructive examination of various parts of the reactor coolant pressure boundary, more than we've ever had before, I think.

And so at the time when this rule is implemented, if a licensee comes in and we know of some very adverse operational experience, I think it would be incumbent upon us, our PRA staff working with our materials engineering staff, to challenge a licensee about that operational experience.

MR. POWERS: I guess what I'm fishing for is how do you know? I mean is there some activity that says, "Okay, yes, we did not consider plant-to-plant variability in developing these frequencies, but we know that if a plant has such and such a condition, that it might deviate outside of this or up/down/sideways. These things are consequential." I mean is there such a base of information someplace, is there somebody I can go ask about that? Or do I have to reconstitute this panel of experts in order to -- and then ask them that question, how does plant-to-plant variability affect these?

MR. BARRETT: I think what's more likely to happen is that licensees will take actions to bring themselves into the norm; that is to say I think we would -- rather than challenging a licensee to use a higher frequency number because they've had unfavorable inspection results or unfavorable operational history, I think we would challenge the licensee to take more corrective action to bring themselves more into the norm. And that would be in compliance with bulletins that we have out there, orders that we have out, technical specifications in some cases, voluntary industry inspection programs in other cases, and as time goes along, we are going to be evolving into a more -- into a different regime as to how we inspect and manage the flaws in the reactor coolant pressure boundary. So I think it's going to be more in terms of trying to -- seeing that licensees are more in conformance rather than trying to figure out probablistically how --

MR. POWERS: I think I agree with your comment there. I guess when we look at the materials science, either research program or the capabilities in the line organizations, we need to look specifically in these areas is what you're saying, because -- I mean you in your position are reliant on them of telling you look specifically at this part of the application.

MR. BARRETT: Right. And you've been briefed on the pert process that the Office of Research is going through, and it's a very systematic process. The industry is doing something similar, and we're on a pretty steep learning curve right now, but I think we're heading very much in the right direction.

MR. SHACK: I mean you do some of that in the risk-informed inspection where you actually look at the degradation mechanisms on a piping system-by-piping system basis. You're looking at the number of welds in piping systems. And so you do end up with a variability. I mean not all plants will have the same results, even though you're using sort of generic results on a per weld basis.

MR. TEGONING: And one of the necessary baseline things that you do for risk-informed ISIs, you do what's called a baseline study of your plant to evaluate precursor events and identify those that may be different than industry average and trends. And, again, I think what we're envisioning whenever we see an issue that pops up the first question in our mind is is this a plant-specific or a generic issue? I think if you look at CRDM cracking, that's sort of a classic example where we have been working to identify cause as root causes and differences and identifying those plants which may have bigger problems than others. So I would anticipate that that sort of model would be what we would apply and utilize in this case as well.

MR. BARRETT: Exactly.

MR. SHERON: Dana, if I could -- this is Brian Sheron from the staff. Just in terms of putting a perspective on this, keep in mind that, number one, when we're talking about a transition break size for a plant we've considered the question of other plant-to-plant variability, and we just don't have enough information on all 103 operating plants to be able to sit there and say we can go and pick what the right number is for each one of those plants.

When we went through the process we did put margin in our thinking. In other words, when you see how we arrived at a transition break size, which is basically the largest attached pipe to the primary system, the thinking was is that the most likely pipes, in other words the pipes that are going to have the higher probability of breaking, it's not the main coolant pipe, you know, the big 30-inch or 25-inch pipes, whatever and the like, it's probably the attached piping. And we think we've covered that. In other words, the highest probability piping, if there's going to be a failure, is going to be something that's attached. And so that's why we picked those pipes, the largest attached pipe. Because we think that covers plant-to-plant variability to some extent. A plant that has a 14-inch surge line will have a bigger break than one that has, say, a 12-inch and the like.

The only other piece I would point out is that really what -- you've got to remember these licensees are still required to mitigate up through the double-ending guillotine rupture. The only thing we're arguing about is what kind of conservatisms they put in their analysis when they do the calculation. So I feel -- I mean I personally feel comfortable that we've got enough margin to account for any plant-to-plant variability.

The one place where we did raise the issue has to do, for example, with the power uprate, okay, where they may now be operating the plant at conditions that were greater than what the expert elicitation panel considered, in which case a licensee, I believe, would have to come in and tell us what that effect is.

MR. APOSTOLAKIS: Well, that brings up another issue, though. I mean are we reviewing this work in the context of 50.46, in risk-informed, in 50.46, or are we reviewing it as a piece of work on its own? At the Subcommittee meeting, we were told that these results may be used in other applications, so we have to make sure then that they're reasonable results, but also, you know, it's a NUREG so we have to review it. If we review it only in the context of determining the TBS, then a lot of the details that one can worry about disappear, because if we go with your choice of the TBS, you have such a margin that you add, in fact it's significantly higher than the

95th percentile of the most conservative expert. So what do you want? I mean they just increase it by four inches above the ten-inch estimate of the expert. So then you might even wonder why spend all this money to do this. You could have called up the experts and say the guy who was most conservative was this, we're going to go up and that takes care of that.

So I have been thinking about it. I mean it seems to me at least this committee should review this work in its own right. Does it produce reasonable results independently of how risk-informing 50.46 will take those results and use them. Okay? So in the context of 50.46 and what you guys are doing, maybe everything is okay.

MR. SHERON: Yes. The only thing I would -- I won't say I disagree with you but I don't think there's -- when you say there's so much more margin in the TBS that we selected from the most conservative expert's opinion, we recognize that the expert elicitation didn't consider a lot of -- or not all of the various failure modes. There were some other uncertainties. I think even the Committee raised the question of safety culture and how does that impact --

MR. APOSTOLAKIS: No, I agree. I mean you did a good job listing those.

MR. SHERON: Yes. So I mean there's margin there, but I can't tell you that it's that far above. It's just accounting for things we don't know how to quantify.

MR. APOSTOLAKIS: But my main point, though, is still valid, that since they didn't consider other things, say, four inches or something, then a lot of the details that went into this analysis are not very relevant any more unless this analysis is used somewhere else.

MR. BARRETT: Doctor, I think I'd say that a little differently. I think that the details and the technical analysis and having a systematic elicitation available as a starting point was very, very useful for us at NRR in choosing this TBS because it gave us a place -- you know, we know that we're not at the ten to the minus five mean 50 percent confidence level; we know that. We know that we've placed -- we've gone to a more conservative position. But by having this systematic analysis available and having it available at this point in time, that's very, very useful for us to know where we are. So this is one of those happy cases where a very good research product has come along at exactly the right time.

But I would agree with you that there is a separate question as to making sure that the ACRS fully understands this because it is a piece of work that may be applied in many, many cases in the future.

MR. BISHOP: Dr. Apostolakis, Bruce Bishop from Westinghouse. On the first agenda item this afternoon, we're going to be talking about pressurized thermal shock, and in fact the limiting transients do turn out to be the LOCAs, and we did use -- the staff did use preliminary estimates that came out from the panel, not the final ones. But one of the action items that came out of the joint subcommittee meetings the last couple days was to reverify that the frequencies are consistent for the small and large and medium break LOCAs. So it is being used in different places.

MR. APOSTOLAKIS: Thank you.

CHAIRMAN BONACA: The only thing I wanted to mention, I totally agree that they're different things, and we discussed during the Subcommittee this issue of the bridge from the elicitation to the actual choice of the break and I expressed my interest particularly in those factors such as the bottom bullet here, no significant changes will occur in plant operating profiles.

There was another statement that says that the assumption was that mitigating strategies on piping will be as good as the one used in the past. Now, the question I have at that point is, well, the rule would in fact cause possibly power uprates, which are significant changes in plant operating profiles. The rule may also cause mitigation strategies which are lesser than we have in the past for design basis of transition breaks. And I have an expectation that the bridge going from elicitation process to the choice of a break size will address those issues. Did you talk about those?

MR. APOSTOLAKIS: Yes. Okay.

MR. TEGONING: Okay. Let me continue quickly then with this since I think we've already discussed most of this slide. So, again, we're dealing with unisolable LOCAs, LOCAs related to passive component aging. We looked at a variety of different break sizes, from the classical small, medium and large break up to a double-ended guillotine type of LOCA, which is much bigger than the historical definition for a large break LOCA, and we looked at three different timeframes. Again, the primary focus, the last two bullets, we were looking at frequencies associated with normal operating loads and transients that are expected over the extended lifetime of the plant. So we weren't dealing with rare event loading like you can get for a more significant seismic event. And Dr. Bonaca just talked about the last bullet.

So I wanted to list here next some --

MR. POWERS: Let me ask you on the last bullet it's remarkable and it's like a head-in-the-sand approach. Do you have any evidence that power uprate changes the frequency substantially?

MR. TEGONING: No. The only thing we have is preliminary information. I mean we've seen it in BWRs that --

MR. POWERS: That preliminary information is information.

MR. TEGONING: Yes. Well, we've seen in some instances with boiling water reactors when they have gone through uprates we have seen increased frequency of damage due to internal steam dryer components. So that is evidence that we certainly do have -- there's no other evidence that I'm aware of beyond that. And that's an important cautionary note, and that's one of the reasons that that note was struck so heavily in the NUREG. The experts were provided with the operating experience. The operating experience is valid over the conditions, parameters that represent the way plants have been run over the last 25, 30 years. So that precursor information is important, and it's just an understanding that if we do things that significantly alter I'll say the appearance or the information that's provided in that precursor database, then that would potentially result in a change in LOCA frequencies.

And this caveat is in there just to make sure that we maintain vigilance. When we do things like this, when we make changes, we need to continually monitor precursor events to see if those changes have any end result. I think the steam dryer is an excellent case because we did some power uprates, we were evaluating what happened to the plant after we made those uprates and we realized that, okay, there were some unintended consequences that occurred because of those uprates. And now we've got a fairly -- and I can't speak about this but there's others in the room that can -- but now we have a fairly extensive strategy to go in and modify and fix those issues so that it brings us back down to precursor events which are consistent with our historical operating experience.

CHAIRMAN BONACA: Well, the words in the elicitation document specifically indicate they could be significant increases, and that's what is troublesome about -- one is left with a judgment without information. Significant may be in the eye of the beholder, I mean what does it mean? So, anyway.

MR. TEGONING: Yes.

MR. POWERS: Yes. That's kind of the situation you're stuck in, isn't it, that -- I mean most of these assumptions -- what you'd like the assumptions to know is there's a continuous evolution of things here and now you're left with this may be a clip here and what not and there's no evidence offered and apparently none exists.

CHAIRMAN BONACA: But, again, since the whole process is an elicitation, so it's an engineering judgment being provided by experts. You have to take it in that context as well as the same way you believe in certain estimations of numbers you believe in the word, "significance," and you know how to place it in the context of a estimation of transition break of an Appendix A criteria. I'm talking about this bridge going from one to the other. I mean it's a difficulty I'm having when I read that report.

MR. TEGONING: Well, again, I think that caveat's necessary because you couldn't go into the elicitation and postulate all possible changes that could occur. We know what we know, we know what our history tells. We had to make certain assumptions to try to project that history forward.

CHAIRMAN BONACA: I'm just troubled by those which are circular in nature, which is once applied to a rule, the rule may cause certain changes in the plant which may result in undermining the estimations that we have. And there were two that I saw. One was a potential for less capable mitigating strategies tied to the fact that there will be less focus on beyond transition break components, and this other one was this, but I think there may be additional ones when I read the report.

MR. TEGONING: Well, again, that's why those caveats are in there. And it's not -- we're not developing these results through elicitation and throwing them on the table and walking away from them. Part of the plan is to continually evaluate these things, and if we see changes, that's when the action's necessary.

CHAIRMAN BONACA: Yes.

MR. TEGONING: So there are a number of other LOCA risk contributors that we didn't explicitly consider within the elicitation. And I've listed a few of the more -- I think more prominently discussed contributors. We didn't specifically consider active system LOCAs, stuck open valves, pump seal LOCAs, those types of scenarios. We did not explicitly consider seismically induced LOCAs.

MR. APOSTOLAKIS: Excuse me, regarding your second bullet, if you were to define an equivalent diameter for a stuck open valve or a pump seal LOCA, what would that be?

MR. TEGONING: These are usually small LOCAs at best.

MR. APOSTOLAKIS: Small LOCAs.

MR. TEGONING: At best.

MR. APOSTOLAKIS: So in terms of the choice of the TBS, the fact that you left those out probably doesn't matter that much.

MR. TEGONING: That would be what I would argue, certainly, yes.

MR. BISHOP: Dr. Apostolakis, this is Bruce Bishop again from Westinghouse. We specifically looked at that question. The biggest valve in the Westinghouse plants would be the safety relief valves, and their flow rate would correspond to a break of between a two- and four-inch pipe.

MR. APOSTOLAKIS: Two and four inches.

Thank you.

MR. BISHOP: Yes.

MR. TEGONING: Then as I mentioned, seismically induced LOCAs and other LOCAs associated with what we're calling or terming rare event loading, this would include a rare water hammer, rare major water hammer and a heavy load drop from some causal factor like an overhead crane releasing its load. And Gary is going to discuss these points later in the talk, so he's going to expound on these much more fully. I'm just setting the table right here.

So the elicitation results -- so that's the objective and scope. Now, I want to go right into the elicitation results, and, again, this is a summary of information that I think you're well familiar with at this point. The way the NUREG is laid out we developed baseline results, and those baseline results were developed having measures of both individual uncertainty, so uncertainty that each panelist had, and then also measures of group variability.

With these baseline results, we conducted sensitivity analyses in a number of areas and they were specifically five broad areas because we wanted to look at the effect of assumptions that we made in processing the baseline results, how changing those assumptions might affect the results that you could glean from the elicitation. So there were five broad areas that we looked at. We looked at the effect of distribution shape, looked at overconfidence adjustment, we looked at the effect of assuming different correlation structures, different methods of aggregating expert opinion and then also different ways of capturing panel diversity.

And I've bolded two here, the overconfidence adjustment and the aggregation of expert opinion. These are the two that we thought were most applicable to the TBS selection. So this was information that very early on the results of these sensitivity studies were communicated to NRR. And the baseline results, as modified by the overconfidence adjustment, is what they were using and what they were basing their TBS selection on. And then we gave them various results with various aggregation schemes so they could take into account or understand the uncertainty that arises when you process the results in different ways. So these were the two components to the baseline results that were added and included in the NRR selection.

MR. APOSTOLAKIS: Well, did the experts see any of this? Did the experts see the sensitivity analysis, did they see your final results, did they express any views?

MR. TEGONING: The experts had reviewed the NUREG at two different phases. We had a teleconference in July with the first draft of the NUREG and we had all the sensitivity analyses conducted in four out of five of these areas. The only thing that we hadn't finished at the time was the effect of distribution shape on the mean. And the other thing that we didn't show them at that point is we didn't have the mixture distributions developed. But they did see the difference between arithmetic mean aggregated and geometric mean aggregated. Now, since we've completed these additional sensitivity analyses, we've sent the NUREG back out for final review. So they've certainly seen all of these. We haven't had another video teleconference or another gathering of the experts, again, to comment again, but we did have relatively rather extensive comments at the July meeting.

I think just to summarize some of the most -- there was generally good agreement on most areas of the NUREG. I will say there was some probably some violent disagreement when we got into the different ways of aggregating. And there were --

MR. APOSTOLAKIS: Which we will probably have here as well.

MR. TEGONING: Yes. Yes. This is a common theme here. When we get into aggregation there's violent disagreement amongst individuals. And I would think, and this is probably not surprising, most of the disagreement was against using an aggregation scheme like an arithmetic mean type approach or a mixture distribution approach because, again, I think the thinking was it didn't accurately represent the group as a whole.

MR. APOSTOLAKIS: But, you see, that's my question, really. Were the experts as a group ever given an opportunity to say, "Yes, what you guys are putting in the executive summary represents our group and maybe by extension the state of the art." Or a guy's sitting in his office in California, he gets the NUREG, reviews it, reads its, now, again, it depends on the point of view he takes, says, "They represented me well? Yes. Then they did all these analyses. They sound reasonable to me. It's okay." But he never really asks himself do I agree that this final distribution of the staff report reflects my views as well. So the whole thing is you should give a chance to the experts after you do the sensitivity analysis and everything to revise their views and maybe try to come up with a consensus curve. And I'm asking whether they actually had that opportunity or they just individually reviewed the NUREG to make sure there was nothing unreasonable?

MR. TEGONING: Yes. And you get into different strategies. That would have been one strategy that we could have taken with the elicitation. We specifically did not want to develop consensus curves because we did want to have a measure of what the differences in opinion would be.

MR. APOSTOLAKIS: Right. I mean remember now you're --

MR. TEGONING: And, again, the sensitivity analysis and getting input from the panelists were important; however, it's recognized that while these are experts in materials and fracture predictions and the technical subject matter of the elicitation, they're not experts in aggregating group opinion or applying these results to a 50.46 rule. So there's only certain -- their comments are very valuable and they formed a necessary basis for this entire document; however, there are aspects of the document that quite frankly I don't feel that the experts -- they're certainly welcome to comment on them, but I don't think they're necessarily qualified to.

MR. APOSTOLAKIS: But you're not asking them -- well, they're certainly more qualified than talking about safety culture, okay? And you have them talk about safety culture and speculating that safety culture will improve in the future. I mean they're absolutely not qualified to say things like that.

MR. TEGONING: They are with respect to LOCAs; sure, they are.

MR. APOSTOLAKIS: I don't think so. I think anybody can say things about safety culture. I mean here you have experts on probablistic fracture mechanics passing judgment on safety culture. I mean --

MR. TEGONING: Only as it relates to passive system failure. That's a very small aspect of safety culture.

MR. APOSTOLAKIS: They can say something useful as to the impact of a given culture on the failure of a passive system but they cannot say anything useful to me regarding what safety culture we'll have in the future. That's an entirely different ball game, whether people will do things like Davis-Besse and so on. But, anyway, that's a separate issue.

The point is, though, that the experts would probably have benefitted a lot by seeing the sensitivity analysis. Because, you know, once you pass judgment and seeing them and having an opportunity to change their judgments possibly --

MR. TEGONING: But they did see them. They did see them.

MR. BISHOP: Dr. Apostolakis, Bruce Bishop again. I was a member of the --

MR. APOSTOLAKIS: Bruce Bishop, did you have a role in this?

MR. BISHOP: Yes. I was a member of the Expert Panel, and I did make some comments about --

MR. APOSTOLAKIS: So you are one of the experts.

MR. BISHOP: Right. And I did make some comments about --

MR. APOSTOLAKIS: So you think the safety culture --

MR. BISHOP: -- the safety culture, but I don't want to talk about that. What I want to talk about is that at the next to last meeting when we were provided a draft of the NUREG there was some violent disagreement on the overconfidence adjustment among the experts, and Rob and Lee provoked -- proposed --

MR. APOSTOLAKIS: Provoked too.

(Laughter.)

MR. BISHOP: -- some resolution of those comments. And those were discussed. And the basic agreement was of the Panel that that appeared acceptable to all of us. So there were opportunities to do that. At the second meeting where we were presented preliminary results I do know that Panel Members did make adjustments to their individual contributions because the results after that changed, in particular the small, like the four-inch diameter PWRs were increased significantly for the PWSCC concerns that the Panel -- most of the Panel did not believe that we had that under control yet, and I mean at the time for the 25-year elicitation results. So there was that feedback.

But the latest results we've been shown, and what Rob did is sort of he gave, "Well, here's the ratio of the numbers you had at the last meeting, and here's the ratio of the new numbers." So we could see very clearly what was changing. And I would characterize most of the changes that have been made recently have been relatively small. I mean we have not seen big orders of magnitude changes or things like that. We've seen adjustment factors, typically a factor of two or less or something in the last adjustments. And so I think most of the Expert Panelists would agree that that's probably within the scope of our estimates. So I just wanted to set that straight.

MR. DENNING: I'd like to make a comment because my concern is exact opposite of George's and that is I think that there is danger in driving to approve consensus, and it goes along with some of the things that you just responded back to George. Because I think there is substantial uncertainty here, and I think that the value of the group getting together is to understand what the other people are saying and sometimes they get additional insights. But their danger is that you'll drive them towards minimizing what's a real uncertainty. So my concern in the aggregations and those group elements of this is that we're making the uncertainty appear much narrower than the reality is.

MR. APOSTOLAKIS: Well, again, it depends on what the experts are doing. The experts, in my view, should see the sensitivity analysis, because experience has shown that the results of this analysis provide very useful insights to them, and they may want to change the thing. But we'll talk about the form of the results later.

One question that I have, because that really confused me when I looked at the whole thing, shouldn't your final results be in the executive summary?

MR. TEGONING: The final baseline results are in the executive summary.

MR. APOSTOLAKIS: I'm sorry, not baseline. Your final word should be in the executive summary. And I'm confused now. At the Subcommittee meeting, you told us, I think, that the results with the overconfidence adjustment are your results, period. Is that correct?

MR. TEGONING: The statement that we make in the executive summary is that we provide baseline results and then we have a statement in there that says, "The particular results that you use for a given application will be dependent on the intents and purposes of those applications."

MR. APOSTOLAKIS: I understand that, but in --

MR. TEGONING: And that's why here I'm highlighting what results and what adjustments due to the sensitivity analysis are most appropriate for the 50.46 transition break size selection.

MR. APOSTOLAKIS: In Chapter H of your report, there is a series of results. If I read the executive summary, it seems to me that's the purpose of the executive summary, I should be able to see what your final conclusion, your final results are, and you may have a -- you know, "We also did a lot of sensitivity analysis, go to H."

So at the Subcommittee meeting, I got the impression that your results included overconfidence adjustment, but the executive summary has only the baseline results without the overconfidence adjustment. So now I'm confused. Which one's would you advocate, the ones with overconfidence adjustment or not?

MR. TEGONING: For 50.46 TBS selection, we are advocating use of the overconfidence adjustment results. Again, the NUREG's meant to stand alone and deal with other applications. There may be other applications for whatever reason you don't want to use the overconfidence adjusted results.

MR. APOSTOLAKIS: But the overconfidence adjustment has to do really with the experts themselves, so I can't see an operation where the experts cease to be overconfidence.

MR. TEGONING: Do you want to --

MR. ABRAMSON: This is Lee Abramson of the staff. The whole idea of overconfidence adjustment is somewhat controversial. We used it because there's a lot of evidence in the literature that people, experts in particular, people in general, tend to be overconfident in their judgments. Of course, our whole elicitation process was designed to try to minimize this with training of the experts and so on and so forth.

So we don't know, certainly, in this case to what extent they may or may not have been overconfident. However, there's some indication internally from the results that we got, namely the very wide disparity between the experts, that some would seem to be certainly far less uncertain than others and so on. So we felt that we had to explore this and we did this through a sensitivity study.

So I would think it would depend on a combination of to what extent you are concerned about this possibility of their being overconfidence -- that is as somebody who's going to apply these results. If you're particularly concerned that perhaps the experts might have been overconfident, you can't be sure, then you may say we need to use an overconfidence adjustment. In other words, you want to conservative in your results because of the kind of application you're using.

So it's a combination of both your assessment of whether they might have been overconfident or not and the risk you're taking in using the results with or without adjusting for it. It really depends a lot on the application and on your approach to the whole problem.

MR. APOSTOLAKIS: I'm not sure that the application is so significant here, and it's really -- the problem -- well, I have a few comments on this. First of all, having seen the statement of the considerations, I went back to the report and I tried to figure out where they got the range of six to ten inches for PWRs. I thought it was going to be a straightforward thing, and it wasn't. I had to speculate a lot. Maybe they used this figure, maybe they used that figure, maybe they used a mean here and median there and so on, and that question will come up again.

And then I thought that maybe in the executive summary there should be sufficient information for me to figure out very quickly how NRR selected that range. And by reporting only the baseline results and then maybe using something else, that doesn't help. And I think, Less, coming to your point about you have to use your judgment for this and that, I think you are putting and awfully large burden on the user here. You are asking the user to decide on which sensitivity analysis is appropriate, you are asking him to go back and read the literature to understand what overconfidence means.

I mean one would expect that the project of this nature where experts in these things got together and produced a report that these guys would resolve these issues for people like Brian, for example, so he wouldn't have to go back and say, "Gee, what do they mean by this. Should I do this, should I go with the median." No.

As far as I'm concerned, one should read the executive summary and that should say, "This is our final word on this with all the uncertainties, if you will, and so on," and right now all it says is the study does not recommend whether the frequency estimates corresponding to the baseline or in particular sensitivity analysis should be used in applications, which means, "Mr. User, you have to read all this NUREG, hire your own consultants and make your own judgments." I just couldn't figure out this six to ten inches where it came from.

MR. ABRAMSON: Well, you're right, we do put a burden on whoever's going to apply us, and I think that's an appropriate burden to place because you're talking about decision making under uncertainty. That's what's being done here, regulatory decision making for something like that. And we're providing them a tool to do this. But the purpose of this was not to provide them with the criteria that they were going to use; we don't know that. That's why we emphasize the importance of the application, the criteria, the risks they're willing to take of all sorts and so on. So we cannot do that. Now, that's another effort perhaps that we could explore, but that was not the purpose of this NUREG.

MR. APOSTOLAKIS: But if Rob is telling us that the overconfidence adjustment is really something that you guys like, let's put it that way, why aren't you reporting these results in the executive summary and you're reporting only the baseline?

MR. TEGONING: Because I'm a particular advocate or not an advocate of a particular set of results, we wanted to make the NUREG a stand-alone document, essentially, without consideration of any application, although we realize 50.46 was the first application that was going to most extensively utilize these results. And, quite frankly, as Lee mentioned, overconfidence correction is fairly controversial. There is no standardized way to do that. We explored a number of different ways to do that, and just because Lee and I preferred a particular way and we think we have some basis for that, I mean we could make that recommendation, and I understand your point, but we just chose not to at this point in time because, again, there's no standardized way to do this.

MR. APOSTOLAKIS: You know what's going to happen. I mean people are going to go to the executive summary and lift numbers from there, period. Maybe NRR won't because this is a big deal, I mean risk-informing 50.46, and they will call you and ask you and all that. But once this NUREG is released, people are going to be using your baseline results in the executive summary, especially when you say that the sensitivity analysis didn't affect it much except for the application.

MR. TEGONING: Well, it sounds like you're advocating maybe instead of having a particular set of results in the executive summary having no results in the executive summary.

MR. APOSTOLAKIS: Well --

(Laughter.)

MR. APOSTOLAKIS: -- actually, the opposite should be results. No, I just don't think it's fair to the reader to have results in the executive summary but when we talk to you you say, "No, these are not what we're really recommending. It's something in Section H."

MR. TEGONING: No, I totally understand your point.

MR. APOSTOLAKIS: All right.

MR. TEGONING: Okay. So the next slide is going to get at the results that George has been looking for. So this shows the results that were provided to NRR. Of course, they were given preliminary results; these are final. I think we gave a set of earlier results to NRR end of May-June timeframe. We've been tweaking things, as Bruce had mentioned, in the interim, but there hasn't been significant changes in the results since what NRR was provided with in May and June.

So these show the BWR results and we just have -- all of these results are adjusted using our error factor adjusted correction.

MR. APOSTOLAKIS: It's obvious your heart is there, Rob. I mean you really like the adjusted results. All you need -- you really want people to spend hours trying to figure how the six to ten inches were produced. It's obvious to me that you really like this, and I have no objection, actually. I mean this is your professional judgment, I know the issue. Fine.

MR. TEGONING: This is why I don't play poker, obviously, George.

MR. WALLIS: Rob, I have a question here. In reaching these numbers, you're treating these as if they were continuous curves, it seems to me.

MR. TEGONING: No. We say that in the report.

MR. WALLIS: This is appropriate to join them up. Don't you have different classes of piping that certain types of pipe are going to break in certain ways. So there really isn't a continuous curve. When you've changed from one kind of a pipe to another one, it's a different story. Maybe we should have a step function between sizes or something.

MR. TEGONING: That's right. And we indicate --

MR. WALLIS: That makes a big difference when you start to say you've got some place where you've got ten to the minus five.

MR. TEGONING: And that's a valid point. We state in the NUREG that these lines between the points -- we asked the experts for discrete points, and those are the dots you see in the figure. The lines are just there for trending.

MR. WALLIS: These numbers at the bottom, though, they seem to correspond to intersections between the continuous lines in some curve.

MR. TEGONING: The numbers --

MR. WALLIS: That's what I interpolate.

MR. TEGONING: The numbers at the bottom are interpolated numbers based on --

MR. WALLIS: No pipe size at that size at all.

MR. TEGONING: And that's why when you see the rest of this talk that's why these numbers are just a starting point. You bring in those considerations later on when you look at interpreting and applying these numbers in a regulatory sense. So that's an excellent point, and that's the point that I would --

MR. WALLIS: This goes again to George's point. Someone's going to say, "Aha, we've now got this magical number 19, and that's the answer."

MR. SHACK: But you could also have a leak that's not a break.

MR. TEGONING: Yes.

MR. APOSTOLAKIS: True.

MR. SHACK: So if you have a 32-inch pipe, you can have a break size in that 32-inch pipe anywhere from a leak size --

MR. WALLIS: A 20-inch size break in a 30-inch pipe is probably a completely different animal in terms of probability from a 20-inch pipe which itself breaks. It's a different problem.

MR. TEGONING: Maybe not that different.

MR. WALLIS: Not that different?

MR. TEGONING: You're talking about a major -- we would classify that as a major failure of that pipe in any instance.

MR. SHACK: No, but the question is what is the likelihood.

MR. WALLIS: It's quite a different thing.

MR. SHACK: It's quite different.

MR. TEGONING: Yes. And I don't know that I would make that assertion.

MR. WALLIS: But this is another thing that the intelligent interpreter should take into consideration. As they have, I think.

MR. APOSTOLAKIS: This last row, mixture

of distribution, is that in the NUREG?

MR. TEGONING: Yes.

MR. APOSTOLAKIS: Where?

MR. TEGONING: Well, it --

MR. APOSTOLAKIS: I've been looking for it.

MR. TEGONING: Yes. The version of the -- let me be clear, the version of the NUREG -- the NUREG has been in continual preparation. There was a section added after the version we gave you for review.

MR. APOSTOLAKIS: So which NUREG are we reviewing today?

MR. TEGONING: You're essentially reviewing a preliminary version that will be available for public comment. So we've added -- this is the only section that's been added.

MR. APOSTOLAKIS: Can we have that section today?

MR. TEGONING: Yes.

MR. APOSTOLAKIS: How long is it?

MR. TEGONING: Less than a page.

MR. APOSTOLAKIS: Oh.

MR. TEGONING: A page or so.

MR. APOSTOLAKIS: So just for purposes of clarification, geometric mean means that you take, say, the medians of the experts, multiply and take the end root.

MR. TEGONING: Yes.

MR. APOSTOLAKIS: Okay. Arithmetic mean means you take the median of the experts -- or the means of the experts, add them up and divide by N.

MR. TEGONING: Yes. Of the various parameters of the distribution, either the fifth, the median, the 95th or the mean.

MR. APOSTOLAKIS: Yes.

MR. TEGONING: Because we've got estimates for each of those.

MR. APOSTOLAKIS: Some characteristic value.

MR. TEGONING: Right. Right.

MR. APOSTOLAKIS: Yes. And then you find some distribution for the expert value fitting a curve or something. And mixture distribution means that from what the expert gives you, each expert, you produce a distribution and then for each diameter you add up the probabilities and divide by N. This is the NUREG 1150 approach.

MR. TEGONING: Yes. Yes.

MR. APOSTOLAKIS: Very good.

MR. TEGONING: Yes. Yes. And, again --

MR. APOSTOLAKIS: I really want to see that section.

MR. TEGONING: And we'll provide that. Again, I apologize. Due to the schedule and --

MR. APOSTOLAKIS: No, that's fine.

MR. TEGONING: -- due to the scheduling of these meetings, we've been trying to give you the most up to date version.

MR. APOSTOLAKIS: You know, I spent hours trying to figure out why you guys didn't do that when NUREG 1150 did it, when the seismic study did it. The seismic study says in fact that working with the percentile ties is wrong. You didn't want to help me, though. Okay, now I understand, and it's important to see that the mixture distribution is at least a higher percentile than the geometric mean, higher numbers in general, because the mixture distribution contains expert-to-expert variability and uncertainties of the experts. And I remember when we were reviewing 1150 we had long discussions about these things, what it contains and Steve Horac gave us a long spiel there. And then the seismic study confirmed that. So that's very important to bear in mind.

MR. TEGONING: But the interesting thing

and part of the results that we've been looking at from the beginning is not only the mean values but evaluation of the confidence bounds. And what's interesting there is the mixture distributions, if you compare with the geometric mean when you consider the 95 percent confidence bounds, you get a pretty good correlation there.

MR. WALLIS: Well, despite all this, some naive person like me looks at the numbers down at the bottom there and says, "These are pretty big pipes."

MR. TEGONING: Yes.

MR. WALLIS: What rationale you're going to use, those are pretty big pipes you've got down at the bottom.

MR. TEGONING: Yes.

MR. APOSTOLAKIS: Yes. That's why the choice is, what, 20?

MR. WALLIS: Well, they're pretty -- 24. Numbers above 20 look pretty hairy to me.

MR. FORD: Rob, could you just satisfy me on one thing?

MR. TEGONING: Sure.

MR. FORD: Looking through your presentation you don't talk about future performance, the end of six years. Tell me again, for instance, flow assist occurs in the carbon steel piping in BWRs?

MR. TEGONING: Yes.

MR. FORD: May well increase in probability, they go to power uprates. How is that fed into this sort of rationale? Is a plant-specific analysis that is done at a later date?

MR. TEGONING: Well, again, when we did the elicitation, we looked at different time periods, and those sort of longer-term trends that you would get from predicting either probablistic fracture or other types of predictions, were included in those trends. I don't summarize in here just because for the most part there were no strong time dependencies that were predicted by the experts.

MR. FORD: I saw nothing in any detail on FAC in the station report. This is why I bring it up.

MR. TEGONING: Yes. No, FAC was -- and I don't -- maybe you can clarify in terms of what sort of detail you're looking for, because FAC was definitely a prominent mechanism that was discussed for -- again, it's a small subsection of piping. It's really only the feedwater piping and steam piping in BWRs that are really susceptible to FAC in the primary side system. But that was certainly an important consideration. And we discussed as a Panel quite at length the relative merits between IGSCC likelihood of failure versus FAC likelihood of failure.

If you look at Appendix L, I believe, which has the detailed results, there's a lot more system type discussion that's provided in that appendix.

MR. FORD: This definition of the TBS will be very much plant specific.

MR. TEGONING: No.

MR. FORD: Well, this is what puzzles me, because it's got to be, it should. I mean if you've got a plant that's on hydrogen water chemistry -- we're talking about BWRs -- those curves are going to shift dramatically as to whether a specific plant is on -- all the plants are on hydrogen water chemistry now but --

MR. TEGONING: Yes, but we considered the effects.

MR. FORD: And your past performance has been based on normal water chemistry.

MR. TEGONING: But we have performance based on both, and that was another explicit point in the elicitation is we looked at the difference in operating experience as a function of the various mitigation steps that had been done over the years to account for IGSCC cracking and the effectiveness of those mitigation strategies.

So, no, that was a very -- we had very lengthy discussions about that as well. We looked at data, both pre and post sort of early '80s timeframe, looked at different trending, looked at what plants were generally doing out there to mitigate for IGSCC and, again, the explicit instruction that was given to the experts was when you consider IGSCC, you consider IGSCC as it exists now, not as it existed back in 1980. So there was one instance where we had to be very careful because the operating experience is clouded by a lot of data based on pre-mitigation IGSCC.

And that's where the experts really earn their money. Of course, some of them weren't paid, but that's where they were really important because they had to distinguish between what part of the operating experience was most important and what was most relevant to current-day estimates.

MR. WALLIS: Well, I'm wondering -- you've had your time, according to the schedule. You seem to be about a third of the way through it.

CHAIRMAN BONACA: And let me just make a correction to the record before I turn to Dr. Shack --

or it's actually Dr. Apostolakis is the one that is leaving this session here. So now the record is corrected.

MR. APOSTOLAKIS: So now that we're running out of time --

CHAIRMAN BONACA: We're running out of time because --

MR. APOSTOLAKIS: -- you turn it over to me so I will be going.

CHAIRMAN BONACA: We're running out of time because we heard your presentation rather then their presentation.

MR. ROSEN: So that you will lead us out of the problem.

CHAIRMAN BONACA: That's the reason why we're running out of time.

MR. WALLIS: Well, it seems to me these are important conclusions here. Are you giving us conclusions in this page or are we still discussing all about the methods you employed?

MR. TEGONING: I'm giving you results that were used as a starting point by NRR for selecting the transition break size, not conclusions.

MR. APOSTOLAKIS: Maybe I missed it, what do you mean by mean with 95 percent confidence?

MR. TEGONING: Well, when we did the geometric mean aggregation, we also --

MR. APOSTOLAKIS: Can you point to the figure and show us which one would that be?

MR. TEGONING: These bars represent confidence bounds. So this value represents essentially where this bar would intersect, ten to the minus fifth.

MR. APOSTOLAKIS: So you would go to the curve labeled mean?

MR. TEGONING: Yes, with 95 percent confidence.

MR. APOSTOLAKIS: Show me the 16? Where is --

MR. TEGONING: It's between here and here.

MR. WALLIS: It's the red bar there.

MR. SHACK: You go to the mean curve and you go to the arrow bar on the mean curve, go to the top of it.

MR. TEGONING: This represents the upper confidence boundary.

MR. SHACK: And then you can draw interpolations between those points.

MR. APOSTOLAKIS: So at the point where the straight line intersects with the ten to the minus five? Okay.

MR. TEGONING: Yes, essentially. Let me move on. I don't think we need to talk about means. These show essentially the same thing, but they're the PWR results. And, again, I've chosen to show here just the adjusted geometric mean and the adjusted arithmetic mean results. The mixture distribution results, if I plotted them, look very similar to the adjusted arithmetic mean results.

MR. APOSTOLAKIS: Now, the mixture distribution will have a mean value but it will also have a 95th percentile.

MR. TEGONING: Yes.

MR. APOSTOLAKIS: And where is that? Oh, it's over there, ten.

MR. TEGONING: That's it. We didn't develop confidence intervals for the mixture.

MR. APOSTOLAKIS: No. For the mixture, it doesn't make sense to do that.

MR. TEGONING: We could, but we didn't. We could use boot-strapping or something to do that. We just didn't.

MR. APOSTOLAKIS: No, this is good enough. So, essentially, the mixture distribution defines the six to ten range.

MR. ABRAMSON: Just a point of clarification. The mean of the mixture distribution is always equal to the arithmetic mean.

MR. APOSTOLAKIS: Yes.

MR. ABRAMSON: However the 95th percentiles will differ.

MR. WALLIS: Well, I note that one of the numbers is 31, so if you wanted to be ultra-super risk averse, you might pick the biggest pipe in the system.

MR. TEGONING: Yes. And that number is essentially a threshold. If you look at the arithmetic mean, you start to get -- I mean the shape characteristics you get much more of a plateau with the larger break size.

MR. WALLIS: It's interesting that you can come up with a number 31.

MR. APOSTOLAKIS: And these results are adjusted. Now, in my view, if I were writing this, I would report a mixture distribution, and I would say, "These are adjusted because this is our professional opinion. Thank you very much. If you want to see variations, go to H." That's what I would do.

MR. WALLIS: He's telling us this is what was given to NRR. I think that's significant. This is what he gave --

MR. APOSTOLAKIS: No. The NRR has a whole NUREG, I hope, right?

MR. TEGONING: NRR has a preliminary --

MR. BARRETT: NRR had the whole NUREG and Rob was attached to it.

MR. APOSTOLAKIS: He was Appendix A.

(Laughter.)

MR. TEGONING: Appendix R.

MR. KRESS: With respect to Graham's comment, if I wanted a bigger number, I could use 97-97 or 99-99.

MR. APOSTOLAKIS: I know but 95 is sort of the one that's used traditionally.

MR. KRESS: Yes, for no apparent reason.

MR. APOSTOLAKIS: Thirty is kind of curious. Thirty-two is the biggest pipe you considered, is it?

MR. TEGONING: Well, the 31 is in there. It's just meant to essentially be you get no reduction if you use the 95th. With the 95th percentile, you're essentially at a double-ended guillotine break at the largest pipe in the plant. So that number's a bit of a misnomer.

MR. APOSTOLAKIS: It seems to me what makes sense is to report a mixture distribution graph.

Because if we go that way, as I said earlier, you might say, "Well, gee, I want to be conservative. Tell me what the highest number any one of the experts reported and I'll go with that." I mean supposedly we're putting some rational thinking into this, and in my view, that's in the mixture distribution.

MR. ABRAMSON: This is a comment about that. As I said, it's controversial about whether you want to use the mixture distribution or the geometric mean, and we talk about that in detail in the report. What I feel in the report is that it makes sense with the kind of data we have and the kind of situation that the geometric mean makes a lot much more sense than the mixture distribution.

As far as conservatism is concerned, I think what you should do is use the best most appropriate methodology you have for aggregation, and then if you want to be conservative, put the conservative on top of that. And you can do that in several ways. One, for example, you could use the 95th percentile instead of the mean or you could use a confidence bounds or some other measure of variability. I think it's a mistake to use as an argument for the mixture distribution that it gives you larger results, namely more conservative ones.

MR. APOSTOLAKIS: That's not the argument.

MR. ABRAMSON: Well, that sounds like what the argument is that you're making. I think if you're going to use the mixture distribution, you should use it because in your best professional judgment it's the best way to aggregate. But you should not use it -- the fact that it is --

MR. APOSTOLAKIS: It doesn't give me --

MR. ABRAMSON: The fact that it gives you more conservative results is a benefit, but that should not be your main reason for using it.

MR. APOSTOLAKIS: It doesn't even do that. The geometric mean of 95-95 is 14, so that's not my reason.

MR. WALLIS: The message I'm getting, George, is that there's an argument about which number should be used; therefore, I've got to be careful how I use any of the numbers.

MR. APOSTOLAKIS: Okay. The mixture distribution, in my view, reflects uncertainties due to expert variability and individual expert uncertainty. It was used in the NUREG 1150 routinely, it was used in the seismic studies and so on.

MR. POWERS: George, one of the things that you can agonize over is number manipulation, which is not the word I would use if I weren't on the record, but it seems to me that's not what the conclusion that NRR came to. They came to a rather interesting conclusion that says very interesting all this stuff on your pipe break frequency. What I'm going to do is say the largest pipe attached to the main coolant pipe is my break frequency. And interesting conclusion because it does not exactly follow from any of this expert elicitation that I'm aware of. Did the experts in the course of doing this come to the conclusion that it's really this attached piping that's the most vulnerable? I mean did they drive this conclusion or is this creativity -- a creative interpretation, and I mean that in the very best sense, by the way, on the part of NRR?

MR. TEGONING: Well, no. Frankly, I'll just say flatly, no. I think it's well known, and it was documented and stated in the elicitation as well that it's very well agreed upon in the community that the largest pipes, the coolant piping is robust piping. There are a number -- and the fact that it is large, relatively thick-walled, more likely to exhibit leak before break, there's a number of reasons why that piping is robust, as well as it's proven over its lifetime to be robust.

MR. POWERS: So all this agonizing over numbers means nothing. The fundamental physical phenomenon here, the fundamental physical insight is that the main piping is robust and it's the attached piping that's vulnerable.

MR. ROSEN: Is that what you just said?

MR. TEGONING: No. I'm not saying it's vulnerable. I'm just saying compared to the largest piping --

MR. APOSTOLAKIS: Yes, it's less likely. It's more likely.

MR. TEGONING: -- it's more likely.

MR. APOSTOLAKIS: Now, if you look at the argument, though, the NRR gives in their statement of considerations, I think it's an excellent application of defense-in-depth in fact, both rationalists and structures. The stopping point is the result here, six to ten inches. Then they have a list of all the assumptions and what's left out, which Rob also showed us on his second slide, I think. Then they said based on all these things that are missing and based on the fact that the expert elicitation came up with six to ten, we have to do something bigger -- choose something bigger, and then the issue of the largest attached piping came in and they said, "Well, gee, that's great. Fourteen inches for PWRs makes eminent sense to us." So this was their reasoning. It's not that they ignored everything that was done here. I mean they started but these guys did a good job, I mean Rob and Lee and their colleagues, in listing what is missing and various sensitivity studies and so on.

MR. WALLIS: Well, they may have done a good job but it seems to me that that idea that these numbers that you're agonizing over are the starting point is not correct. It is supporting information to the decision that was made that's really based on a very phenomenal logical kind of point of view.

MR. APOSTOLAKIS: I don't know, Dana. If those guys had come up with a range of ten to 18 inches, I don't think those guys would say, "No, we'll go back to 14 because --

MR. WALLIS: Well, I think instead of speculating about why NRR made the decision, why don't we let them tell us why they made the decision.

(Laughter.)

CHAIRMAN BONACA: Well, there's a section on transition break size --

MR. APOSTOLAKIS: But the other point, though, is I think Dana is raising a very important point, which I tried to raise earlier. The other thing is let's not forget that the SRM says pick the mean frequency of the distribution. So if one were to go with the SRM, then the stuff that these guys did acquires tremendous significance because now you go with the curve they have. I don't know how you could do that when they also tell you, "We left a lot of things out." So that creates a problem there.

CHAIRMAN BONACA: The following slides talk about what has been left out. I think they're important. We may have to move --

MR. APOSTOLAKIS: No, but this is the real issue. Are we reviewing this work in the context of 50.46 or in its own right?

MR. TEGONING: And I would argue that you're doing both. You really need to do both.

MR. APOSTOLAKIS: You're probably right.

MR. WALLIS: But it should certainly stand in its own right.

MR. APOSTOLAKIS: It should. It should stand.

MR. WALLIS: It shouldn't be warped by some consideration of 50.46.

MR. APOSTOLAKIS: So what do we do next? So, okay, now --

CHAIRMAN BONACA: We're moving ahead,

George. You're ten minutes past the time and you have to manage this next seven or eight --

MR. WALLIS: Minus so many minutes.

MR. TEGONING: I'm going to turn it over to Gary Hammer now of NRR and he's going to -- although I think George sort of outlined the rationale to get us back on time pretty eloquently.

MR. APOSTOLAKIS: Would you please when you give numbers tell us exactly from which figure or table you got them from?

MR. HAMMER: Yes, I will attempt to do that.

MR. APOSTOLAKIS: Thank you very much.

MR. HAMMER: Yes. Thank you, Rob. And, yes, I'm Gary Hammer with the Office of NRR and the Division of Engineering. As Rob said, we wanted to use the expert elicitation results as a starting point to give us some idea about what it is we're looking at in terms of some of these frequencies, pipe sizes. And what you see, as he said, is that there are a range of pipe sizes which correlate to the frequency that we're trying to target, which is ten to the minus fifth per calendar year.

And as he indicated, there's a lot of uncertainties, both in the process of the elicitation and in variability and some of the things that have been discussed here already. And those are some of the things that we indeed have also been thinking. I heard a lot of things being said that sounded very familiar to some of our own internal discussions.

And we felt like the selection should accommodate some of these various considerations. And there are considerations, as Rob mentioned, which do, ultimately, we think, impact the selection, at least potentially. Because they weren't specifically considered in the elicitation process, and those would be categories of active LOCAs and low-generated LOCAs. And then, finally, we think that we need to consider the actual configuration of the plant, anything we know about specific operating experience that could be brought to bear on the final selection.

Regarding the other considerations not addressed by the Expert Elicitation Panel, Rob hit on these briefly, if I could go into just a little more detail. You have the topic of active LOCAs. As it was mentioned a little earlier, we think those are generally small-break LOCAs from stuck open valves, failure of seals and gaskets. Those valves and seals and gaskets don't end up being that large. I think we had a question just the other day, "Well, what about the loop isolation valves?" Those are the biggest valves we can think of. They're actually in the main coolant lines. If you failed the seal on those, however, that wouldn't even be as big as the TBS that we're looking at because you're looking at a valve that's basically backseated in its normal configuration, and if you had the seal blowout, it wouldn't be that big.

They are a higher frequency than pipe-break LOCAs. It is something that --

MR. WALLIS: Don't those big valves, excuse me, have some bolts in them in the way they're put together? So they could -- if there was some overtorquing of the bolts or something, that would be a cause for --

MR. HAMMER: Yes, but --

MR. TEGONING: We covered those in the elicitation. Those types of failures were considered in the elicitation.

MR. WALLIS: I know, but those valves actually if they popped would give you a break which is comparable with the break of the major pipe.

MR. TEGONING: To the pipe size that it was attached to, right.

MR. WALLIS: Okay. So I was hoping that

your --

CHAIRMAN BONACA: Are we talking about loop installation valves?

MR. TEGONING: If the whole casing failed, you would get that.

CHAIRMAN BONACA: The loop installation valve, some plants have them, most plants don't.

MR. HAMMER: Right.

CHAIRMAN BONACA: Okay. But that's consistent with the thought process that they will be plant-specific, and that may cause the consideration of different break size because you have a certain component there.

MR. ROSEN: Did you cover reactor coolant pump bolting, the bolts that hold the halves of the reactor coolant pump together?

MR. TEGONING: Yes. That was all covered in the elicitation. The only thing that wasn't covered was, again, mechanical operation of the valves.

MR. ROSEN: When you say it's covered, what do you mean? I think I know what you mean but I'm --

MR. TEGONING: It means that we discussed piping and non-piping contributions to the LOCA frequencies. We developed failure scenarios for each of those components. They were discussed -- identified, discussed and then evaluated by the experts. So that's what I mean by considered.

MR. ROSEN: So in that discussion someone talked about, for instance, boric acid corrosion of the bolts that holds the coolant pump together.

MR. TEGONING: Yes.

MR. ROSEN: And that has the operating experience of corrosion of those particular bolts.

MR. TEGONING: And we provided operating experience of especially in primary systems bolts that had failed. And there was a realization that any bolt failure would need to be a common cause type of mechanism, and we discussed various common cause mechanisms, from boric acid corrosion to systematic overtorquing to some maintenance error. And we developed these failure scenarios that we then provided back to the experts and asked them to assess.

MR. ROSEN: I asked you a very specific question about the reactor coolant bolts, you answered it. Can you apply that same answer to the manway bolts?

MR. TEGONING: Yes. Yes.

MR. ROSEN: Because those are very large

breaks if the bolts unzip.

MR. TEGONING: That's right.

MR. ROSEN: In other words, one fails or two fail and then the rest start to fail.

MR. TEGONING: That's right. And when you get up to the Category 5 we call them in the elicitation results Category 5 and 6 type LOCAs, they're large-break LOCAs, manway failures becomes a contributor to those break sizes. It's still not as big a contributor as a piping failure, but, yes, it factors into the final numbers.

MR. SHACK: In a sense, are you saying it's less likely or it's an 18-inch hole?

MR. TEGONING: It's less likely.

MR. SHACK: Oh, you still think it's less likely.

MR. TEGONING: I don't think so. That's what --

(Laughter.)

MR. TEGONING: That's what the elicitation results --

MR. ROSEN: That's what the experts think.

MR. TEGONING: That's what the experts think.

MR. ROSEN: Now, it would be wrong, would it not, for me to say that I can buy what the staff has done by taking a bigger break than the elicitation because I'm worried about the kind of breaks we just discussed? That would be double counting it, wouldn't it, from your point of view? You say you've already taken into account, and if I were then to say, "Well, you need more margin because of those kinds of regs," that's double counting.

MR. TEGONING: Yes. It depends on how you look at it. If this were -- if we were going to do a risk-based rule that was primarily going to be based on development from the elicitation results, you could argue that it is double counting. However, when you factor in other considerations, and, again, like Bill had said, if you don't necessarily believe that the elicitation may have accurately considered those things and you want to allow yourself some increased margin, then it's not necessarily double counting. I don't want to get into how the staff had used these because I don't believe they double counted the results in any way.

MR. APOSTOLAKIS: When in doubt, you should double count.

(Laughter.)

MR. ROSEN: When in doubt, double count.

MR. APOSTOLAKIS: We are regulators. Defense-in-depth. Would you please proceed.

MR. HAMMER: Okay. So, in genera, we found that they're limited in size at least by the size of the associated pipe. And they're certainly not larger, at least we couldn't find anything that would be larger than the largest attached pipe, which we'll discuss a little later on, and that becomes a consideration.

There's another type of load, heavy drop loads that Rob mentioned a little earlier. There has been some work done on that, and I've got there in the first bullet there were a couple of studies done back in the '80s and then more recently with the generic safety issue. Therein you'll find estimates of various type of accidents due to load lifts, frequencies of those types of things that can occur. And from that they estimate the probability of occurrence of damage to various safety equipment that's based on an estimated average number of lifts that are made at the plants.

However, when you look into that, though, you find that a lot of those lifts are made during shutdown conditions, so they wouldn't specifically be of interest to us for this so much. Very few lifts are made during power operations, and they tend to be a little lighter. You're not lifting things like their reactor vessel head and these kinds of things. So you're not getting into something that looks like it would be very significant in terms of the contribution to LOCA, at least at the ten to the minus five level.

Then the other thing that we've spent some effort on, and Rob can help me here if we want to go into some great detail about it, because they have been sponsoring a study on seismically induced LOCAs over the past few months, and so it is something that we considered. It wasn't specifically addressed in the expert elicitation. As you're probably aware, a seismic event at the ten to the minus fifth per year frequency is a fairly large magnitude earthquake. It would vary from plant to plant. Some plants have a quite a bit higher than the SSE, some a little less. Less so, I think all of them are probably at least in the SSE area.

And what we found in general is that we would expect that some plants -- and this is based on sort of a generic study with some conservative modeling -- we would expect that some plants might have a higher failure frequency, especially if they havE degraded piping. However, what we found was that, generally, for undegraded piping, we're not expected to have a significant effect in this frequency range. And if you have small flaws, it's essentially equivalent to the undegraded condition. You're just not affecting the response of the system or the failure mode for a small flaw. However, for some larger flaws, and certainly for the worse flaws that are possible, we would see an unacceptable increase in failure probabilities.

MR. POWERS: This sequence of comments --

MR. HAMMER: Now, this is, like I said --

MR. POWERS: This sequence of comments makes -- I mean I'm not -- I'm very confused by it. It says if I have very tiny flaws, it doesn't affect the probability, and if I have very big flaws, it does in a dramatic fashion. There must be then some intermediate flaw that does affect the distribution.

MR. HAMMER: It is actually a multifarious effect, obviously, and there's a continuum. You could have varying load levels and various flaw levels, and it would be, like I said, a multivaried effect. And it was this area where you could have this spectrum that we were actually worried about.

MR. TEGONING: We explicitly looked at

that. I'm going to just say and then you can jump in, Bruce, we did a case study, we looked at -- by flaw sizes here, you specifically mean flaws that you would leave in due to current Section 11 procedures that you wouldn't have to repair. So for those types of flaws, given the nature of the piping, essentially you see no increase in failure probabilities to those level of degradation.

For worst-case flaws here, what we did is we actually looked at the Dwayne Arnold safe and cracking, which is about the worst thing that we've ever seen in service. Now, obviously, if you evaluate that extreme, you are going to see big increases in failure probabilities at that type of -- if that pipe would have been hit by an earthquake at that time, it likely -- much greater increase likelihood of rupture.

We did do a third thing here that Gary hasn't captured is we looked at distributions of damage that are more representative, and where we got that information is there's quite a bit of information for IGSCC cracking about the sizes and flaws, types of flaws that they found when they've gone in and done these augmented inspections and then they reported these and then gone ahead and repaired them. So we looked at those distributions which you would argue, if anything, are slightly conservative, because these are flaws that they found and then repaired. And when you compare undegraded versus degrading for those types of distributions, you can get up to maybe quarter of magnitude increase in failure probability. So still significant but not as significant as you would get if you looked at these worse-case flaws from something like a Dwayne Arnold.

MR. BISHOP: The point I wanted to make -- this is Bruce Bishop again -- and, again, it's on Page 4 of SECY 04-060, that we did in fact, like Rob said, we did in fact discuss the rare events like the seismic events, water hammers and various events like that. It isn't that we didn't discuss them, it's just the conclusion was that based on our experience, even if you have flawed piping, typically, like Rob said, the failure probabilities can increase by as much as a factor of ten but not less than a factor of 100 is what we've seen in all the PFM calculations we've done, even with degraded piping that has flaws and you somehow miss those flaws and they continue to grow.

The consideration you always have to remember, though, is what's the probability of that event occurring during a given year and when you factor that into it, the only event that really appears to be significant is the water hammer event because we do have experience with water hammers occurring, so the frequency is not 1E to the minus fifth. It's maybe like 1E to the minus two, in that range. And when you factor that into it, the net effect is maybe to double the frequency of the undegraded piping due to water hammer.

So there is an effect there. I know in the Risk-Informed ISI Program we have in fact run into situations where that's been the controlling mechanisms for doing an inspection. So we do take that into account, but, again, it's not a factor of ten or 100; it's a factor of two, typically, which we believe is within the scatter of the estimates to begin with.

MR. APOSTOLAKIS: Well, what is your bottom line conclusion?

MR. POWERS: Nothing that's been said has helped me at all on these two bullets. It seems to me that you've got something that must truly have to do with your probability of detection of flaws. Are the two bullets telling me you just didn't worry about that?

MR. SHACK: Well, no. You need a probability of occurrence of flaws too. I mean, you know, the --

MR. APOSTOLAKIS: What is the bottom line?

MR. SHACK: The probability of occurrence of flaws in stainless steel PWR piping is pretty low. So unless the conditional probability of failure is --

MR. POWERS: It's adequately done with a probability of detection, because if it ain't there, I'm not going to detect it.

MR. APOSTOLAKIS: Is your bottom line conclusion that seismically induced LOCAs will not change the frequencies?

MR. HAMMER: I think the bottom line that we've kind of come upon right now is that since it's a flaw sensitivity problem and it becomes an issue of being able to detect and monitor and take adequate corrective action for the flaws, as necessary, so what we're going to do is complete our confirmatory studies and we'll publish the work that research is currently got ongoing, and then we'll ultimately issue guidance on what has to be done for the licensees to ensure --

MR. APOSTOLAKIS: I find the last bullet very strange: Licensees need to ensure inspection. Well, yes, licensees need to comply with the regulations, they need to be good guys. I don't know what it means in the context of revising the rule.

They need to ensure inspection plans are adequate. Is there any possibility that they don't need to ensure that?

MR. HAMMER: This isn't really in the context of existing regulations so much. There might be necessary improvements to the inspection plans in order to ensure that you don't have a break larger than --

MR. APOSTOLAKIS: I don't understand this.

MR. WALLIS: How did this consideration affect your choice of pipe size?

MR. APOSTOLAKIS: Right, exactly. That's the question.

MR. HAMMER: Well, what we would argue is that specifically for the seismic consideration we really wouldn't have a specific consideration on the TBS because of the way this has fallen out. And I think, see, if you have undegraded piping, then you don't have a significant effect. It's really these levels of degradation that we're worried about, and we want to be able to detect them and then that sort of eliminates it as a large consideration.

MR. WALLIS: Well, certainly, the experts when they did their work looked at the probability of flaws in pipes?

MR. APOSTOLAKIS: No, because they didn't consider seismic --

MR. WALLIS: Not with seismic but when they did their other --

MR. APOSTOLAKIS: Oh, the other stuff, yes.

MR. WALLIS: So you should be doing the same sort of thing.

MR. TEGONING: I think the point we're trying to make here is if you have a TBS of like 14 inches or 20 inches, seismic considerations are not expected to be significant with that transition break size. However, if we would have used the elicitation results as they stood, six inch, ten inch, then seismic would have had likely a much more significant -- would have had a more significant risk contribution.

MR. POWERS: And it seems to me they're going on and saying, "We're coming to this conclusion and we're not going to consider degradation of that piping in coming to that conclusion because it's the small sizes that don't affect it and the big ones we're going to detect and fix."

MR. APOSTOLAKIS: That's right.

MR. POWERS: Ergo the bottom line.

MR. APOSTOLAKIS: That's right.

MR. TEGONING: That's right.

MR. POWERS: It seems to me it would have been easier to say on the slide than --

MR. WALLIS: Is that what that says?

MR. POWERS: Yes. That's what that says. We're going to blow off piping degradation and we're going to cover it on our In-Service Inspection Program.

MR. APOSTOLAKIS: We're not going to cover seismic events.

MR. POWERS: I think that's what it says.

MR. APOSTOLAKIS: This confirms again that you can't separate all this stuff from the final decision, and the final decision is define the TBS, what does that do to you, what does it do to the plant and so on? Because I mean in the deterministic world, you assume the biggest pipe breaks, you cover everything, small flaws, large flaws, whatever. Now that you want to be risk-informed, you have to agonize over all these things.

MR. HARRISON: Dr. Apostolakis?

MR. APOSTOLAKIS: Yes.

MR. HARRISON: My name is Wayne Harrison. I'm going to speak for the rest of my group later on today but I want to put on my South Texas project licensee hat and speak to that last bullet. I think from the comment, the licensees would tell you that inspection plans and in-service inspections and so forth are designed such that we expect to find flaws such that we have no breaks. And we don't say no breaks larger than the transition break size. We're looking for any flaws and it's not dependent upon the size.

MR. APOSTOLAKIS: The whole point of the DBAs was to give you -- it was really an implementation of defense-in-depth, right? So, yes, we'll do the best we can not to have flaws and this and that, but in case the largest pipe breaks, here's what you have to do. Now you go to this conformed world and all of a sudden things change.

CHAIRMAN BONACA: What about this summer? This summer had the crack right through, I mean we didn't see it. I mean they didn't see it. I wasn't there.

MR. RANSOM: You'd have seen it.

MR. SHACK: Well, I think it does impact the notion of a risk-informed inspection because in a risk-informed inspection the largest diameter pipe always falls out of the inspection plan because it doesn't contribute to risk. The inspections of the largest pipes are always defense-in-depth inspections.

MR. APOSTOLAKIS: Right.

MR. SHACK: And you're saying that you need those defense-in-depth inspections essentially to maintain your confidence in your assumptions here. So you do want to watch the argument that when you go through your risk-informed inspection plan you put some floor on it that covers your defense-in-depth considerations for this large-diameter pipe.

CHAIRMAN BONACA: That's right. And those are provisions that -- why don't we move to the water hammer? We just heard that that's a much more likely event.

MR. HAMMER: All right. Yes, on water hammer we --

MR. APOSTOLAKIS: Well, Mr. Chairman, what do you want to do? I mean we're behind here.

CHAIRMAN BONACA: Well, I think we have to hear this. I think --

MR. APOSTOLAKIS: No, we'll hear it but do you want to continue until we're done or stop?

CHAIRMAN BONACA: I think so. Let's try to see if we can do it by 10:30, around 20 minutes.

MR. HAMMER: Okay. On water hammer, it's

another consideration that we wanted to take, and we're talking only about the primary system and the breaks that could occur there. So while there have been a lot of water hammers in other systems, you don't expect water hammers to occur during normal operation because the system is filled with water. There aren't voids, things of that nature that can normally occur. However, during a small-break LOCA accident, there is a scenario that we were considering that would involve condensation-induced water hammer involving a situation where during the small-break LOCA the water level drops below the top of the hot coal legs and gives you a squatter steam interface. Then due to the cold injection water, you would form a slug which would then trap a void and a classical condensation induced water hammer scenario.

It's possible that you could get very large pressures from such an event which might rupture a pipe, which could create a bigger LOCA, so this was of particular interest to us in that regard. This was reported in the NUREG CR 3895. Professor Griffith from MIT did some scale model testing and showed that the effect was possible.

In operating plants, we think it's actually in a narrow range of small-break LOCAs. It's plant-specific somewhat in that regard, and, like I said, it requires a level drop amp in combination with having a high enough pressure to drive the slug with some large velocity.

So what we would like to do on this is develop some screening criteria that we would provide to the licensees in a reg guide, and this would allow licensees, hopefully, to demonstrate that they're not susceptible to this type of event.

MR. WALLIS: What I heard from an expert behind me was that this could double the frequency of pipe breaks? It seems to me -- I'm surprised that you're not focusing on that. You seem to be focusing on what licensees need to do. That doesn't sort of affect the choice of transition break size, does it? The water hammer doubles the frequency. Maybe that affects my choice of transition break size.

MR. BISHOP: That information came from work that was done for the pipes. My contribution to the Expert Panel was to take the results that were based on seven plants that had done risk-informed ISI studies and in several plants where there might be a possibility of a water hammer and some degradation going on simultaneously, that increased the frequency by about a factor of two and that would be high enough, okay, that we would go out and do an inspection to make sure that there is either no degradation going on or we would make corrections consistent with what you were saying, okay, to eliminate the water hammer or reduce the probability of having a water hammer.

MR. WALLIS: So what you're saying is if the licensees do certain things, then we don't need to worry about certain increases in this frequency. Is that what we're learning here?

MR. APOSTOLAKIS: I guess now I don't understand what the DBA means anymore. I really don't. We're defining a frequency. We said anything below that, I mean a diameter corresponding to a frequency, will be treated as a DBA, traditional or Appendix K analysis. But since we're not sure about the frequency, we will also establish some programs and so on to make sure that flaws don't exist and all that. What's the idea of a DBA then? I mean what is the idea of doing all this conservative analysis for diameters smaller than that? Now the program becomes an essential part of the regulation, and Regulatory Guide 1.174 says that defense-in-depth means no excessive reliance on programmatic activities, right? That's one of the first bullets.

MR. BARRETT: George, this is Rich Barrett again.

MR. APOSTOLAKIS: Yes.

MR. BARRETT: I think that I don't believe it would be considered excessive reliance on programmatic activities if you were to take into account programs that are part of the licensing basis of these plants and programs that are in fact being implemented every day in the plants. I mean we do, whether tacitly or explicitly, we do rely in all of our regulations on in-service inspection, in-service testing as a way of assuring that the licensing basis is maintained throughout the life of the plant.

I think what we're doing here, and I think this is a very important point that we probably haven't made, and that is that this whole rulemaking is a set of incremental steps. When we enact this rule it's an enabling rule, and that has certain implications. It will take away certain fetters on the licensees in terms of what they can propose to the staff, in terms of changes to the design and operation of the plant, but it will not in and of itself make any changes to the design and operation of the plant. So the question is do we have a sufficient -- and this is a legal question -- do we have a sufficient technical basis to justify the action that we are taking in publishing this as a proposed rule?

This is a first step to propose and enact an enabling rule. The second step is for a licensee to adopt the rule, and then there's a whole set of third steps which are for that licensee to propose specific changes in the operation and design of the plant. And that's what risk-informed licensing process, by and large, which may or may not involve generic involvement on the part of owners' groups and others. So we're talking here about having sufficient basis to enact an enabling rule.

That basis is in the selection of this transition break size, which, in effect, does define the limit of the design basis accident, is, first of all, the elicitation process, which included some phenomena but not others. And then the consideration of other phenomena, some of which are sufficiently well understood but they do not affect transition break size, others of which, seismic and water hammer, will involve some statement on the part of the licensee at the point in which they adopt the rule, some statement as to whether or not they fall within the parameters that would make them acceptable.

Now, what are those parameters? For water hammer and for seismic, those parameters are you have to be -- those screening criteria are yet to be defined and will be in the reg guide. What we're doing here is we're simply stating what are the technical concerns that are still open? Now, the technical concerns are these larger than some threshold flaws which could affect the seismic capability for some plants in high seismic zones. Those concerns may in fact be resolved by the conformity research work that continues, but they may not be.

MR. APOSTOLAKIS: But if I take all the probability of these flaws and fold them into the analysis, would I come up with a break. The frequency of ten to the minus five per year would lead to a larger diameter? Because this is conditional on the flaws existing, right? If the flaws are large, then you get this condition. If I take the probability of the division of the flaw sizes, won't that affect the choice of the diameter? You say no.

MR. BARRETT: Yes.

MR. APOSTOLAKIS: Wouldn't that be a better argument and then say this is our best judgment now, and on top of it we're going to make sure there will a program to make sure that the flaw sizes will remain small rather than say we are relying on the program?

MR. BARRETT: Well, we would --

MR. APOSTOLAKIS: The probability is a risk-informed thing. Because the thing that bothers me a little bit in the whole logic of the thing is that I see the current large-break DBA, LOCA DBA as the ultimate defense-in-depth. If everything else is wrong, we really don't know what we're doing, and you have this big break and you have conservatism all over the place, so now when we become risk-informed, we're going to say, but now this is not the ultimate protection. This is now -- if this program is good and if that program is good, then it's okay. And there is a philosophical question there with what defense-in-depth means anymore.

MR. BARRETT: Well, I would say it differently. In the case of the water hammer, we're asking licensees to describe for us the characteristics of the plant, and if you wanted to go into detail, we have someone here who can talk about the characteristics of a plant such that they would or would not susceptible to this particular water hammer.

And there may be plants that are susceptible and cannot reference this rule, but we believe that, by and large, plants will be able to reference this rule and will be able to pass these screening criteria. In the case of the seismic, I think our feeling is that when Research, when the Office of Research continues this that they're doing and when they begin to do it in a more realistic fashion, and I hope Rob is shaking his head yes, that this issue may in fact not be as big an issue as it appears to be right now. But we don't want to put a proposed rule out based on that assumption. So at the moment, we feel that in order to publish a proposed rule and to be reasonably certain that all of these issues are covered, we're putting this interface requirement in there so that a licensee has to address it.

MR. WALLIS: Well, Rich, what you seem to be saying is if the plant cannot prove that they won't have a water hammer, then they can't use this transition break size. That's extraordinarily difficult because there are all kinds of ways to get water hammer. This is one way. I don't think that should be in the rule at all. Water hammers have already been considered in the elicitation. You can't now put something on top of that. What it said was if they can't show that they don't have a water hammer, they can't use the transition break sizes.

MR. LANDRY: Graham, Ralph Landry from the staff. That's really not what we're saying here. We have to separate two different water hammer effects. One is the water hammer that can induce a break, and what we're talking about here is a water hammer caused because you've had a break. Now, this is a very narrow range that we're talking about here. You already had a small-break LOCA and now you have to have very specific conditions prevail which will allow a condensation-induced water hammer to then occur --

MR. WALLIS: To make a bigger break?

MR. LANDRY: -- to make a bigger break than what you already have.

MR. WALLIS: But then if they can't show this, they can't use a transition break size?

MR. LANDRY: Well, they would have to look at the screening criteria and determine are they susceptible to a condensation-induced LOCA.

MR. WALLIS: When you put cold water into a hot system with steam in it, there are all sorts of ways you might conceivably create a water hammer.

MR. LANDRY: But this is looking at the condensation-induced water hammer in the cold leg. Now, the screening criteria are going to be very specific. You have to have a very particular break size such that you have a water level in the pipe. You can't have a completely voided pipe, and you cannot have a water-solid pipe.

You also have to have the system pressure high enough that when you create the water hammer, the pressure to the water hammer is sufficient to cause damage. And that is typically going to be at least ten to 20 atmospheres and above. You're also going to have to have a very specific velocity range for the fluid in the pipe and you're also going to have to have a pipe L over D ratio high enough --

MR. WALLIS: I understand what you're saying if you can show that. I'm just concerned about this holding up the use of the transition break size for some licensee.

MR. LANDRY: No. We're saying that a licensee that wants to use the transition break size can look at the screening criteria and determine do these screening criteria include their plant or do they exclude their plant? Now, if they include their plant, what is the probability of this precise break size occurring? Now, it's only on a very, very narrow range of break size. It's not over the whole break size range.

MR. WALLIS: Okay. So your anticipation is that they'll be able to show that they meet this criteria.

MR. LANDRY: Yes.

MR. WALLIS: Okay.

MR. APOSTOLAKIS: Okay. So can we move on now to the actual selection?

MR. HAMMER: I'll try to move along as fast as I can.

MR. APOSTOLAKIS: And please don't go line by line.

CHAIRMAN BONACA: We're way beyond time, so let's get to the selection process.

MR. HARRISON: Dr. Apostolakis? I just want to make real brief in one of the key points that we're going to make when the Westinghouse Group speaks -- this is Wayne Harrison again. I just wanted to address your defense-in-depth and we need to keep in front of us that we still have to be able to, from a risk-informed perspective, be able to mitigate the event all the way up for breaks beyond the transition break size up to the double-ended. So we need to keep that in front us too. The defense-in-depth is still there for us.

MR. APOSTOLAKIS: Can you please not go

over these line by line. What is the important message of this slide?

MR. HAMMER: All right.

MR. APOSTOLAKIS: It's 14 for PWRs, 20 for BWRs.

MR. HAMMER: Let me try to condense it down. I think the last time we discussed this with you fellows was back in late October and we had told you then that we had picked some TBS sizes of 14 inches for PWRs, 20 inches for BWRs, and this includes necessary adjustments that we felt like were needed for uncertainties, and it includes the pipes of most concern, which are the attached pipes. And, specifically, we wanted to consider the pressurizer surge lines which have a lot of thermal fatigue and BWR feedwater lines which have more significant flow accelerated corrosion. And so we felt like we captured that by picking those sizes. And the next larger sizes are the --

MR. SHACK: But, again, those were specifically considered in the elicitation.

MR. HAMMER: Yes, they were, Bill, but you could argue that a 14-inch pipe is not a 14-inch pipe is not a 14-inch pipe. Indeed a 14-inch pipe might have more degradation because of some specific environmental consideration, such as fatigue, than a size in that range maybe compared to a ten-inch pipe.

MR. SHACK: Well, I meant but a pressurized surge line, they did a system-by-system analysis, and the surge line didn't come up particularly high, I suspect because although you're going to get thermal fatigue, it's hard to rupture a pipe. You can get cracks, you can get failures, and they're different.

MR. HAMMER: Right. I guess we're attempting to capture some variation from the average based on specific piping that we know about.

MR. APOSTOLAKIS: But thermal fatigue has caused piping failures in the past.

MR. WALLIS: But not surge lines.

MR. APOSTOLAKIS: What kind of failure occurred there. I don't remember whether the whole thing broke or whether there was a --

CHAIRMAN BONACA: They got a big one.

MR. WALLIS: Right.

MR. HAMMER: Right. Okay. The next larger pipe --

MR. WALLIS: I don't understand this, though. This is a preliminary TBS strategy? You're not going to argue forcefully for a certain value today or is that coming up in the next presentation?

MR. HAMMER: It's coming up.

MR. WALLIS: It's coming up in the next presentation?

CHAIRMAN BONACA: We'll see. We'll see.

MR. WALLIS: I'm concerned about this being preliminary.

MR. HAMMER: Next slide. We're attempting to finalize what we're doing. So we're selecting the TBS as the largest size, large pipe attached to the main loop. For PWRs, this is fairly easy to define. It usually comes up as the surge line, and you've got a well defined hot and cold leg, which are very big pipes. For BWRs, you have a maze of piping and not a very well defined loop. The loop essentially goes outside containment through a steam cycle and back through the feedwater. You also have a loop of a very large pipe in the recirculation loop. So it looked like a logical definition for BWRs would be the largest of either the RHR or the feedwater pipes inside containment. And that's around 20 inches.

MR. APOSTOLAKIS: What does the last bullet mean, TBS is actually defined?

MR. HAMMER: Okay. Because the elicitation results that you saw earlier in the curve are in terms of pipe diameter --

MR. APOSTOLAKIS: Yes.

MR. HAMMER: -- and the TBS is more a concept of flow of area as we want to apply it in the rule. And this --

MR. APOSTOLAKIS: Let me follow the logic here. Six to ten was the original, you picked 14. Then what is the rule going to say? It's going to give a flow rate?

CHAIRMAN BONACA: Double-ended break of the 14-inch.

MR. HAMMER: It will essentially give you a flow area based on that size pipe, times two.

MR. APOSTOLAKIS: So you take the 14-inch diameter, you find the equivalent area and then you double it and do what?

MR. SHERON: Now, George, let me -- if I could explain. The way this works is that you pick -- there's an artificiality still about this. We pick the largest pipe, let's say it's 12 inches, 14 inches.

MR. APOSTOLAKIS: Right.

MR. SHERON: When the licensee has to analyze for that break, they have to postulate that break occurring around the loop. And so what they have to assume is they have to find the worst location. So they have to assume a break in the cold leg of that size, which is the equivalent of 12 inches. Well, what you get is an offset. You're assuming that the pipe still has a guillotine rupture but doesn't completely offset. It offsets such that the area for discharge, okay, coming out of both ends of the pipe is the equivalent of 12 inches diameter. So you have twice that area for discharge. Understand?

MR. WALLIS: Seems to be a strange rationale? You've already learned that big pipes don't break and now you're going to assume that they have a 20-inch hole in them? It's a very strange rationale. I would think that you'd consider the small pipes to break and do all that stuff. But then saying that the big pipes that you've proven are not going to break are now going to have a 20-inch hole in them seems a very strange extrapolation.

MR. HAMMER: Well, you can get a little lost in trying to come up with the mechanistic argument like that about why would there be a hole here or there or some other place. Essentially, it comes from breaking in a double-ended guillotine fashion a pipe of this size.

MR. WALLIS: But what the expert said is

a ten to the minus seven chance of the main piping breaking. Didn't they include in that it having a 20-inch hole in it?

MR. TEGONING: Well, again, partial failures of bigger piping is included in the smaller break diameter frequencies.

MR. WALLIS: But it's not a big contributor to that smaller break diameter.

MR. TEGONING: It depends on the plant. It depends on the plant and the expert. For BWRs, actually, if you look, the main partial failures of the recert piping was a significant contribution for smaller pipe failures, yes.

MR. WALLIS: Is it in PWRs?

MR. TEGONING: Not as significant for PWRs, but for BWRs --

MR. WALLIS: Really, a 20-inch hole in the main circulation piping is a contributor?

MR. TEGONING: It can be.

MR. WALLIS: Okay.

MR. TEGONING: Again, when you're looking at characterizing a break size, given that you've got -- I mean these, again, are large ruptures. Again, there's a good bit of uncertainty if that large rupture is going to result in double-ended guillotine break versus a 20-inch hole.

MR. WALLIS: Well, that's what bothers me, this uncertainty. Then it's likely to me.

MR. ROSEN: This is very troubling to me. I don't understand this.

MR. SHERON: There is an amount of defense-in-depth, let me call it non-mechanistic here. One way we could have defined this is we should have said -- we could have said you break the largest pipes -- you break the pipes that are attached to the primary coolant, which means you would only postulate a 12-inch break in the hot leg. The break you would postulate in the cold leg would only encompass maybe an RHR drop line or whatever, smaller size. You would not be postulating -- and the hot leg break is not the limiting break, typically, for a pressurized water reactor as an example. So you would be defining a break that is much, much smaller.

In other words, the Commission told us to pick the break size at the ten to the minus -- as a starting point, ten to the minus fifth, but they didn't say take it to the point where you only postulate a hot leg break of a surge line and a cold leg of a drop line and the like. We still interpreted that to mean that we should still be requiring licensees to look at that break being promulgated around the loop to find the worst location. It's an artificiality, it's a defense-in-depth, if you want to call it that.

Another way you could interpret it, I mean as George said I think at the Subcommittee meeting, you could take this best estimate approach and just apply it through the whole spectrum, okay? Why pick a transition break size?

The only difference really is the degree of conservatism that goes into the analysis model. Again, as Wayne said, regardless of what break size you pick, the system is still required to mitigate it. The only thing that's going to be different is that for the larger or the lower probability breaks, you will not have as much margin in those mitigating systems that you currently have. That's the only difference. But you still will have a system that has been analyzed and capable of mitigating the event. What you're not assuming is that you have 20 percent on decay heat, that you had a single act of failure that occurred. You're still assuming that you have a loss of off-site power, for example. You don't have the highest peaking factor at the same time you have the highest burnup. It's that kind of margin.

MR. APOSTOLAKIS: I think we're going to come back to this the rest of the day. On 14, the next slide, the only bullet that maybe you want to mention is the first one.

MR. HAMMER: Okay. And we talked about that a little earlier about the power uprate condition effects.

MR. APOSTOLAKIS: Okay. So then we covered it. Thank you very much.

CHAIRMAN BONACA: Well, we haven't covered it enough, okay? Because this is dear to my heart. I want to hear about this and that. This rule is going to lead to power uprates and I'm trying to understand how they're going to control this. I would like to listen and hear about this.

MR. HAMMER: Yes. And we think that there will be a need for licensees to explain why their future uprate conditions don't significantly affect break frequencies. That's the key thing.

CHAIRMAN BONACA: That's the whole resolution. Just ask them and they will tell you.

MR. SHERON: No. It's required that they will be required -- as Rick said before, once a licensee decides to use this rule and make a change to their plant including an uprate, obviously they need to get a license amendment to go to a higher power level. That license amendment has to be reviewed by the staff. Part of the staff review, and will probably be incorporated in the reg guide, will be that they need to look and say what conditions in the plant have changed such that they are now outside the bounds, for example, of the parameters in the expert elicitation. If there are things like temperatures, pressures, whatever, that go beyond what was assumed in the expert elicitation, we are requiring the licensee's application to do a detailed analysis of what that means with regard to how that might affect the break frequency.

MR. WALLIS: How can they do that?

CHAIRMAN BONACA: The power uprate rule, though, is not --

MR. SHERON: At higher temperatures, you may be more --

MR. WALLIS: How can they do that? Then they have to convent the same Expert Panel? The Expert Panel doesn't give a formula for calculating these frequencies. How is the licensee supposed to calculate them?

MR. BARRETT: I think that realistically what you're going to find is that if, for instance, PWRs decide to use the latitude provided by this rule to start proposing power uprates, it's probably going to have some sort of an owner's group effort in which they would look at the elicitation, look at the current inspection, inspection requirements, any new kinds of limitations they would like to put on the inspection requirements. This is something that would be looked at generically by the staff, I'm sure, with full participation by the ACRS. This would be a license amendment process that would start with some sort of a generic -- I think that would be the case.

MR. BISHOP: Somebody asked the question about how would you assess -- the way the Expert Panel did this, okay, is the break frequencies are driven by the degradation mechanisms, and you look at the potential degradation mechanisms and certainly if you have stress corrosion cracking which is very temperature dependent and you're changing the temperature, obviously that would factor in. If you're limited by some sort of vibration phenomenon or something like that and you're increasing the flow rate, okay, obviously that's an evaluation you can do to say would that have a chance of taking break frequencies?

But when we were doing this we also -- we took the degradation mechanisms we knew and then somebody pointed out, okay, but we typically get a new degradation mechanism about every seven years, so we tried to put factors into the Expert Panel because I know we discussed this, how do you account for that? So there are factors, typically at least a factor of two, I would say, factored into that already to account for the next unknown degradation mechanism where you don't even know what the effects might be.

So I think a little bit of that is already considered in that allowance, and we know that I think most panel members that were familiar with plant experience knew that, okay, we are going to be making changes in the operating conditions, that we're going to be going to plant uprates, and I think put an allowance in there for some of that to occur. Now, if it was adequate or not is another question, but I think at least it was discussed and considered by the Expert Panel members.

MR. APOSTOLAKIS: When somebody requests some application for a power uprate, can you ask for this kind of thing -- can you bring risk into it?

MR. SHERON: Yes.

MR. APOSTOLAKIS: Frequency?

MR. SHERON: Yes. It's 1.174 and we

issued a risk 2001-02, which I just happened to have read last night. And if you remember, that emanated out of the Calloway situation with Electrosleeving, and what we did is we told -- in the risk, we told the industry that there -- basically, it says there are two conditions for assuring adequate protection. One is presumption that you meet the Commission's rules and regulation. The other is no undo risk. And the staff has the authority and obligation to ask questions about risk. Even if all the rules and regulations are being met, we can still ask questions about risk on that.

MR. KRESS: Let me ask a philosophical question about that second sub-bullet. It seems to me like you've developed a sort of generic distribution for frequencies of pop rank sizes, and now you're asking a specific plant to alter that generic distribution based on something he's going to do. But how do we know that specific plant has that distribution in the first place? It's a generic one. We're not even asking him to say, "Look at this generic distribution and say now does this generic distribution apply to your plant?" There seems to be a disconnect in going from generic distribution to a plant-specific application of that generic distribution, and I'm not quite sure I understand how that's dealt with.

MR. WALLIS: That's why they have the bottom bullet.

MR. BARRETT: Well, I think that could be a step in the reg guide, couldn't it, where it says in the reg guide that the first thing the plant has to do is come in and say that the generic distribution is valid for their plant for these reasons.

MR. SHERON: And the other thing, by the way, is that we have already had a meeting with the industry a couple weeks ago and I believe they're taking on the initiative of developing an evaluation guide, and I would strongly imagine that they would want to address this in their evaluation guide. In other words, they would propose to develop some method for showing how the licensee would evaluate or propose a method that they would evaluate the effect of an uprate on the pipe frequency. And we would obviously be interacting with them in the development of that.

MR. BARRETT: These are important questions, and this is the reason the rule is written the way it is. The actual rule has in it a prescribed change process and incorporates in it the criteria, very similar to Reg Guide 1.174 in which is a risk-informed process will be used for the review of these proposed changes. This is the first step and do we believe there's an adequate technical basis for this first step, which is the enabling rule? I think the answer is yes.

CHAIRMAN BONACA: Okay. Thank you.

MR. HAMMER: Well, let me summarize real quickly.

MR. APOSTOLAKIS: No, please.

(Laughter.)

MR. APOSTOLAKIS: We understand what's going on.

CHAIRMAN BONACA: Yes. Thank you very much. We'll take a break now until the five of 11 and then start again. We're running about 40 minutes late. We have to try to catch up.

(Whereupon, the foregoing matter went off the record at 10:42 a.m. and went back on the record at 10:57 a.m.)

CHAIRMAN BONACA: Okay, let's get back into session and we have the second part of the presentation which has to do with the proposed rule for risk-informing 10 CFR 50.46. This time Dr. Shack is really the lead and the other time I was wrong. And you have a schedule, if you could try to stay within those times. That will push the meeting to about 12:30 whatever, to that point.

MR. SHACK: We will stay within --

CHAIRMAN BONACA: Thank you.

MR. COLLINS: That was a directive, I take it. I may begin then?

MR. SHACK: Yes.

MR. COLLINS: My name is Tim Collins and I'm with the Office of NRR and I'm here to discuss how the proposed rule conforms with the Commission's SRM. There's no -- you've asked for this presentation, right? I understand that you all have copies of the SRM? Okay.

What I intend to do is walk through it, paragraph by paragraph, and discuss basically what we saw as the key points in each paragraph and how the rule packages addresses each of those key points.

So the first paragraph basically says go do a rule and get it done in six months. Well, we're trying to meet that six month schedule by the end of December to get the package to the Commission.

Second paragraph. The key messages that we saw in this paragraph were that we should use the estimates from the Expert Elicitation Panel in conjunction with other relevant information in determining a transition break size. That we should look for a break size that corresponds to a frequency of about 10^{-5} for reactor year, that we should require the use of a Reg. Guide 1.74 approach with emphasis on the word "require" and that breaks larger than the transition break size should not be treated as design basis exits.

I think it's clear from the previous presentation that we took into account the expert elicitation process and other considerations. We also tried to stay in the range of break sizes that corresponded to 10^{-5}, that will be a point that will be debated forever, I expect.

As regards the use of Reg. Guide 1.174 approach, we've assured its use by including the criteria and the guidance from Reg. Guide 1.174 right in the rule. And sometimes in the rule we use what I call a modified version of Reg. Guide 1.174 criteria. For example, we use efficiently small for the criteria for changes in CDF and LERF when the Reg. Guide actually has plots of CDF versus baseline. We didn't think we wanted to put plots in the rule.

MR. SHACK: Now, why did you feel that was necessary to put that in the rule?

MR. COLLINS: To put the --

MR. SHACK: The Reg. Guide 174 process type as a rule rather than -- you know, it's an enabling rule when they come in and propose a change.

MR. COLLINS: Right.

MR. SHACK: You know.

MR. COLLINS: Well, I point to the language in the SRM. The second sentence in the second paragraph says "the staff should use or require the licensees to use the approach and guidance in Reg. Guide 1.174." So we say require the licensee, well, that means put it in the rule. That's why we did it.

MR. SHERON: The other reason too, I think, and I'm probably practicing law without a license right now, but my understanding is that you know, in a regulation you have to put certain requirements as opposed to we can't sit there and rely on Reg. Guides and then go off and regulate via the Reg. Guides. Okay?

If you remember on 50.55A we ran into that problem where we were endorsing code cases in a Reg. Guide and the attorneys basically said that is de facto, you are implementing -- you're changing a rule without going through the Administrative Procedures Act and a rulemaking process. So we can no longer -- I mean we can endorse code cases to a Reg. Guide, but licensees still have to come in and get individual approval from the Staff. They can't just use them like they can the rest of the ASME code until that Reg. Guide is incorporated in 50.55A in that footnote. And I think it's the same type of logic that we have to put the criteria in the regulation.

MR. COLLINS: There's also some things in the third paragraph, the SRM encouraged us to put the Reg. Guide 1.174 in the rule as well which I'm going to get to now and unless there are other questions on paragraph 2.

MR. POWERS: Well, the paragraph dealing with 10^{-5} probability it seems to cause the most heartburn. You have used the expert elicitation plus other relevant information. And you end up with a qualitative change in the approach in that you're focusing on piping hanging off the main coolant system. And then with a somewhat large break size than I would derive from the expert elicitation, but you can maybe argue that, based on the things that the elicitees did not consider.

Then you toss on that the double flow area on top of that. Doesn't that cause a little heart burn with the spirit of the SRM?

MR. COLLINS: It depends on which heart

you're talking about. Some of the people think that we're too conservative. Others think we're not conservative enough.

I think this is one of the main reasons we need to get this out for public comment, so people can give us their opinions on have we gone too far, have we not gone far enough?

MR. POWERS: Well, I guess I'm under -- I guess what I'm trying to understand is why your selection plus the double flow area, that combination of things you think is consistent with the idea of something like 10^{-5} and the expert elicitation?

MR. COLLINS: We believe that the expert elicitation provides a broad range of values that you could argue are 10^{-5}. Remember, we're looking for -- what we really want is the mean of the LOCA frequency from all contributors. What we have is the mean of the subjective judgment in the elicitation process for some fraction of the contributors.

Now we have to turn that into a value of 10^{-5} as the mean value for all contributors. And the real mean frequency, not just the mean of the experts' judgment. So we're not sure how to do that, okay? The most important considerations that we have in all of this process are got to maintain adequate protection of the health and safety of the public. We have to maintain stability in the regulatory process. Those are the most important things we have to do in this whole thing.

And since we don't know how to do an exact association with this 10^{-5} mean value, we have to do the best we can and that's what we tried to do.

MR. POWERS: I mean what you see from the expert elicitation in broad view is that the probability of rupture kind of is about the same for range of pipe size up until you get somewhere around 6 to 10 inches, somewhere around there and then it starts dropping off fairly sharply.

And that leads you to say okay, well, it's these pipes hanging off the main coolant system and so let's focus our attention there.

And everything is fine up to this point. We got a range. I can always find those pipes, one of them that will fit somebody's -- some expert's range and throw a little uncertainty on it to us, a few epistemics and alliatories in there and you got one of those pipes.

Then you go on and you say yeah, but I'm going to actually specify this as double the flow area which seems to come out of the blue some place.

MR. COLLINS: It's not quite out of the blue. The Commission did say that we should continue to use the existing requirements for design basis breaks. And that is the implementation practice for existing design basis breaks.

MR. SHACK: But you could have used equivalent hole sizes from the elicitation than corresponding to the double flow area. I mean what they gave you was essentially a flow area -- they expressed it in terms of a diameter in the elicitation.

MR. WALLIS: Did they use a single throw area?

MR. SHACK: That is a six-inch diameter flow area.

MR. WALLIS: That was not clear when we were showing these figures this morning or maybe I didn't listen clearly enough, but I assume that when a pipe breaks it breaks into two pieces and that there are two ends to it.

MR. SHACK: They weren't looking at pipe breaks. They were looking at flow size.

MR. WALLIS: But it says here break diameter 10 inches and a pipe breaks, it has two ends.

MR. SHACK: On the other hand, Graham, it could be a big pipe --

MR. POWERS: If I'm going to break a surge line, I'm really not going to care about the flow out of one of the other ends.

MR. WALLIS: So when it says threshold break diameter, what does that mean? Does it mean a pipe which breaks in two or is it an area they're talking about? Or did the experts know?

MR. SHACK: It's a hole size.

MR. WALLIS: It's a whole size, so in a break --

MR. SHACK: Here's one of the experts --

MR. WALLIS: It's pi r^2.

MR. BISHOP: This is Bruce Bishop. We had a lot of discussion on this and one of the things we decided after all the discussion is that all those frequencies and break corresponds because there was a break size and a flow rate because some people felt more comfortable estimating frequencies based on flow rates.

Other people with a PRA background felt more comfortable on the break size. And so the flow rates were always given for double-ended breaks to be consistent all the way down from small -- from the smallest break LOCA to the largest break LOCA.

So the frequencies always corresponded to a double-ended break size.

MR. WALLIS: So it's double ended. There's no argument about double-ended.

MR. BISHOP: The expert panel -- that's what the expert panel considered in their frequency estimates.

MR. WALLIS: It makes a factor of 2 difference. It seems to be important to understand that.

MR. DENNING: The other thing I don't understand is when we look for the worst break location, we recognize that could occur in one of the large pipes and then we artificially give it the double area in one of the large pipes.

MR. WALLIS: If it's in the search line, as my colleague points out, you don't really care about what's coming from the pressurizer.

MR. DENNING: No, no, I agree. But what I was thinking when I heard that they were talking about looking at the largest pipes to, but not including the big main coolant pipes, I was thinking we were excluding those from breaks at all, but we're not. When we look for the worst area, the worst location, we're going to take it and it could be in the main coolant pipe and we're going to give it twice the area.

MR. LANDRY: This is Ralph Landry from the Staff again.

Graham, we do care what's coming from both ends of the pipe if it's a pressurizer surge line because you have primary coolant on both ends of the pipe. The pressurizer holds 2,000 cubic feet of primary coolant, so you do care.

MR. WALLIS: You've lost that. You've lost it all.

MR. LANDRY: When you take it as a double-ended guillotine rupture of a surge line, you're going to lose it. It is was a pipe such as an ECC line which did not have primary coolant from both directions, you would only care what was coming out one end, but when you look at a surge line, you do care what's coming out of both ends because you're losing inventory.

MR. WALLIS: You've lost the inventory from the pressurizer as soon as you've broken that pipe. You only care about how it comes out.

MR. LANDRY: You haven't lost it instantaneously. If it's a small pipe break, you're going to lose it, but that does affect the transient.

MR. WALLIS: Yes, it does, but you've essentially lost all the stuff in the pressurizer when you break the pipe. It's never going to get back into it.

MR. LANDRY: That's correct.

MR. ROSEN: And it has effects on the containment response.

MR. LANDRY: Right.

MR. POWERS: These seem to be a very -- I don't argue with any of the comments, but it seems a peculiar way to -- I mean it seems to me, you're going to have a challenge in doing it this way to claim that you're in compliance with the clear language of the SRM.

MR. WALLIS: Well, the SRM says nothing about double-ended or single-ended, does it?

MR. POWERS: What it says is maintain the standards of the way -- something -- the way we've done it in the past. But it seems that they made a qualitative leap here.

MR. COLLINS: And I admire the leap. I've got no troubles with the leap, but it seems to me that that leads to a different way of evaluating things just to -- in the name of realism.

MR. SHERON: If I could just add a little, in terms of the Staff's thought process, okay? If you remember, I think we told you that one of our ground rules in trying to get a rule out in six months was that we were not going to forge any new ground in terms of methods or create new data or anything and what we decided here was that in picking this break size, this transition break size, okay, and then looking at how the licensees would analyze breaks above this transition break size, we basically said we're going to keep everything the same. In other words, we would expect that they would analyze it the same way they do breaks currently.

All we've done is we've made the breaks, that design basis break size a little smaller, but for going above it, it's just going to be the same way. We will assume a double-ended guillotine. Obviously, if we had more time, we could have thought this through.

We could have said, can I assume, that these breaks will manifest themselves only as splits, you know, in the pipe so it's only a one-ended, it's a one-sided break, you might say.

Do you postulate it on the top of the pipe, on the bottom, you know, on the side? That has big effects, at least on the smaller size breaks when you have separated flow.

MR. SHACK: Let me take a different approach. I mean the reason we're changing this rule at all is we think there's some benefit to changing it and you know, are we going to maximize the benefits from changing the rule by picking the largest transition break size we can possibly justify rather than a somewhat smaller transition break size.

Go back to the leak before LOCA break. We had all sorts of uncertainties in whether we'd have pipe breaks, but everybody agreed -- as John O'Brien used to say we had those evil pipe restraints, you know. But the uncertainties we had in the pipe break frequencies didn't bother us too much. We went ahead and did leak before break and gave them relief from that.

It doesn't seem to be any thought in this thing of kind of maximizing the benefit we're going to get from the rule.

CHAIRMAN BONACA: But what kind of benefit are you talking about?

MR. SHERON: Well, first off, I would point out that we didn't pick the largest break size that we could justify. I mean we didn't go into it with that approach. We went in and we said what is a break size that we feel we can technically defend at this time, based on all the information we have in front of us and the fact that we have a limited period of time in which we can develop this justification and you heard that this morning.

MR. SHACK: I would argue like Dana, it seems to me that you've somehow interpreted this to really come up at the highest possible end of the break size as you could get out of the elicitation.

MR. SHERON: I think we would have a hard time. I mean we could be in here talking about an eight-inch break and then we'd probably be asking, quite honestly, a lot of questions about what about this, what about that? Why didn't you pick this?

MR. ROSEN: You can't win. You can start with that premise.

(Laughter.)

MR. SHERON: What I would point out is that at the last meeting we had with the industry, I mean one of the challenges we gave them is we said -- because I think you'll hear later this morning that they would believe that there should be a smaller transition break size.

The question we have put to them is what is the safety benefit that is derived from a smaller transition break size. Okay? Instead of analyzing a 12-inch break, you analyze an 8-inch break using a best estimate model, okay? What does that buy you? We don't know. We have no information right now from the industry in terms of what is that safety benefit? How will they use that? All right?

If they use it and they say you know, we're going to show you that the risk is going to go way down or qualitatively we're going to make the plants a lot safer, I think the Staff might be very receptive to say yes, if picking a smaller transition break size makes the plant safer, overall, we're receptive to it. If picking a smaller transition break size just says that they're going to crank out more megawatts and make more money, we're not that receptive. That's put in a nutshell.

CHAIRMAN BONACA: That's why they asked that question about what benefits.

MR. SHERON: We put a number out there, as Tim said, we want to get this rule out there. We want to get comments in from all the stakeholders. We want to hear what the benefits are, what the detriments might be and then you know we'll decide if it needs to be changed.

MR. APOSTOLAKIS: But it's not the largest possible. I mean they still have to worry about the incompleteness of the expert.

CHAIRMAN BONACA: Absolutely.

MR. SHACK: We went through the arguments there. We kind of agreed the active LOCAs weren't a big deal. The seismic LOCA, I've got 10^{-5} for my occurrence. I have to have a crack in the first place. That's another probability. By the time I multiply those probabilities together I'm not sure that I've thrown a whole lot away there. It seemed to me -- you can always argue over just how good -- an elicitation is only an elicitation. If we had the truth, we wouldn't be eliciting.

MR. APOSTOLAKIS: But you have to add some defense-in-depth, right?

MR. SHACK: We have defense-in-depth. We are going to mitigate all pipe breaks.

MR. APOSTOLAKIS: So you are saying defense-in-depth on the frequencies are not necessary?

MR. SHACK: You know, if we were going to say there was going to be nothing beyond the transition break size, I have a transition break size that looked about 48 inches, you know.

(Laughter.)

But since you are going to mitigate

everything --

CHAIRMAN BONACA: We don't know exactly how we're going to mitigate it.

MR. APOSTOLAKIS: It's the perennial question of how much defense-in-depth is enough?

MR. SHACK: I don't want to hold up the discussion here too much.

MR. APOSTOLAKIS: But that's the heart of the matter.

MR. ROSEN: And how many of the things, the classic things we've done for mitigation are we going to do? Are we going to continue to do all of those things as well as we always have done them in the past?

MR. SHACK: I would be willing to mitigate -- I'm always willing to -- if I'm sure that what I'm doing actually adds to my safety. The reason we're doing this rule in the first place, I think, is at least there's a conviction that this doesn't necessarily lead to an optimum safety status for the plant, the current rule with the large break as it stands.

MR. APOSTOLAKIS: I think it reduces the burden of licensees in some areas and in some cases --

MR. SHACK: That's another argument for

it.

That's okay. We don't mind --

CHAIRMAN BONACA: I don't have a problem with that. I am asking you to distinguish on the benefit because our task is one of focusing on the safety issue.

MR. SHACK: I'm only working on decreasing overall risk.

MR. WALLIS: I find this all very, very puzzling because if the whole purpose of this rule is to optimize the size based on what's the most safety benefit, then we need to have arguments which justify that safety benefit and we haven't seen a damn thing about it.

MR. SHACK: It's coming. It's coming. We have a presentation later on.

MR. WALLIS: We haven't seen anything qualitative about --

MR. APOSTOLAKIS: We will, we will.

CHAIRMAN BONACA: Correct.

MR. APOSTOLAKIS: Not quantitative.

CHAIRMAN BONACA: Maybe we will, if we have the time, right?

(Laughter.)

MR. APOSTOLAKIS: By 10 o'clock tonight,

that's very important.

(Laughter.)

MR. COLLINS: So now we can go to the third one.

MR. APOSTOLAKIS: Should we have that?

MR. COLLINS: Those are my notes. The third paragraph. It says number 3.

MR. APOSTOLAKIS: Number 3 is the third?

MR. COLLINS: Yes, number 3 is the third.

(Laughter.)

MR. ROSEN: Are you going to tell us about the security thing?

MR. APOSTOLAKIS: Let the man proceed.

CHAIRMAN BONACA: Let's go.

MR. COLLINS: On paragraph number three. We think the key points in this paragraph, that the rule should not be narrowly focused and the scope of changes allowed should not be limited in any way except as to meet the safety principles of Reg. Guide 1.174 and to maintain security capabilities. We think this paragraph is pretty clear and we didn't intentionally, at any rate, preclude any particular type of change in the rule.

We addressed the requirement to constrain in areas needed to satisfy the safety principles of

1.174 in the most direct way we could think of. We put them right in the rule. This is what I made reference to earlier.

As far as security is concerned, we sent a memo to the Commission in October, October 22nd, I believe it was that where we stated that we thought that security considerations could be better handled on a more global basis, since the need to review security impacts any change you make to the plant, not just those that are associated with this voluntary alternative rule. So we thought that that ought to be handled more globally elsewhere and we haven't included anything in this package to specifically address security.

MR. ROSEN: So there's no language in the rule that addresses security?

MR. COLLINS: That's right.

MR. ROSEN: That's consistent with what we did with the operator manual actions thing in fire protection area.

MR. SHERON: Let me explain that we do address security. We will address it, I should say, as part of any change and that is that we have -- we are putting in place a process right now where we will have a screening criteria developed, so any license amendment that comes in, whether it's related to 5046 or something else, will be screened, first by the project manager.

If it trips any of the screening criteria that are developed, then it will go to a special committee which Ms. Brach chairs, Safety Security Interface Advisory Panel. And that is also staffed with members from MENSR and they will make a determination as to whether a proposed change could affect plant security or whether -- and vice versa, actually, whether a security-related change might affect plant safety.

If they believe it does, then it will get a much more thorough security or safety review from that aspect. In other words, the Staff will make a very considered decision on its acceptability based on security considerations as well as safety.

MR. ROSEN: I think that's a good plant.

MR. SHERON: So we have a process in place to catch all that and 5046 changes will fall under that.

MR. ROSEN: A most unfortunate acronym though.

MR. SHERON: It's an unpronounceable acronym.

MR. ROSEN: ASAP?

MR. SHERON: How do you pronounce it?

(Laughter.)

MR. ROSEN: I thought it was intended to be unpronounceable. Anyway, I think that's a good plan. It puts it all in the right -- all in one place for whatever application, with the people who are involved, who have knowledge of the topic.

MR. SHERON: And we also, as Tim said, we told the Commission that it may be more appropriate if we want to actually codify this in the regulation, it may be better, in for example, either 5059 or 5073 or part 73, I mean. And we said we would take a look at that.

MR. ROSEN: Good thank you.

MR. COLLINS: Next paragraph, paragraph 4, comes after 3. The key points in 4, you see the mitigation capabilities for beyond TBS LOCAs should still be required, but should be relaxed relative to the design basis events.

Mitigation capabilities for beyond TBS and changes to them should be controlled by the NRC, but the level of control should be graded based on safety significance. That's the way we read this paragraph.

What we did in the rule, I think it's

clear that we require mitigation, all the way up to the double-ended guillotine break and I think it's clear that the requirements we have are related relative to the DBA. There's no single failure requirement, no loss of off-site power requirement.

By the way, Brian may have misspoke earlier. He said that it still required loss of off-site power. Beyond TBS, we do not require loss of off-site power.

MR. SHACK: I thought there was a last-minute revision to the rule.

(Laughter.)

MR. APOSTOLAKIS: That the Committee had not seen.

MR. COLLINS: We allow the use of nonsafety-grade equipment. There's no specific modeling or input requirements and the acceptance criteria, the last proscriptive. Just coolable geometry.

MR. APOSTOLAKIS: I wonder whether we should keep using the words design basis events after we do all these things. Now we are beginning to dilute the meaning of DBA, aren't we?

MR. COLLINS: Abandoning the concept of a DBA for regulatory purposes would not be a bad thing, George.

MR. APOSTOLAKIS: As you have argued in the past. But really, this is a first step, is it not? It says you do certain things beyond design basis. We control not by voluntary means and all that, so we're beginning the dilution process, which is -- I'm not saying it's bad. But it makes -- so the whole issue here is whether we want the license --

MR. COLLINS: Dilution has a pejorative --

MR. APOSTOLAKIS: Sorry?

MR. COLLINS: Dilution has a pejorative sound to it, George.

MR. APOSTOLAKIS: And who says I didn't want to have that?

MR. POWERS: The challenge, George, that you face is your PRA technology has to be upgraded very, very substantially.

MR. APOSTOLAKIS: I am very busy these days, but --

MR. POWERS: I'm being generous in my vocabulary today.

MR. APOSTOLAKIS: All right.

MR. WALLIS: While you're on paragraph 4, these capabilities for beyond design basis, the mitigation capabilities, that's all left to a Reg.

Guide?

MR. COLLINS: Yes, pretty much.

MR. WALLIS: Mitigation capabilities are commensurate with safety significance is all going to be spelled out in a Reg. Guide?

MR. COLLINS: Yes.

MR. WALLIS: It's going to be explained commensurate with the safety significance?

MR. COLLINS: Yes, that's correct. We've reflected in the Statement of Considerations how we are relaxing requirements, no single failure requirement, no loss of off-site power requirement, nonsafety grade equipment can be credited in the analysis. We don't specify -- there's not required input models for the analysis as there is in Appendix K. And the acceptance criteria is coolable geometry.

MR. WALLIS: I think you have not yet specified what you can require for mitigation capabilities that something is going to be worked out, it's going to be worked out in a rational way based on requirements commensurate with safety significance. It needs to be explained in some basis.

MR. COLLINS: We have based on what we are going to require. WE're going to require coolable geometry. We're going to implement it through guidance --

MR. WALLIS: That's a very general thing.

MR. COLLINS: Well, that's what we're requiring. Okay, we're going to implement it through guidance given in the Reg. Guide which I think is what you're making reference.

MR. WALLIS: I'm saying that that guidance has not got to be whimsical. It's got to be based on being commensurate with the safety significance.

MR. APOSTOLAKIS: Is that pejorative too?

(Laughter.)

CHAIRMAN BONACA: Okay. The next paragraph defines this actually.

MR. COLLINS: There's one more item here in the SRM that I haven't -- yes, it does. Next paragraph, it's in there.

On paragraph 4, there is a requirement that the NRC controlled changes commensurate with the safety significance of the changes. And the way we intend to do that in the rule is to have consequential changes where licensees may make those without prior staff approval provided they have a process approved by the Staff, like in 5069.

MR. ROSEN: When you talk about that in the subcommittee, one of the suggestions which I made was an annual report of inconsequential changes, has that been incorporated?

MR. COLLINS: I don't think so.

MR. ROSEN: Was there a reason it was not incorporated?

MS. MCKENNAH: This is Eileen McKennah from NRR. At the time -- we have put a section in the Statement of Considerations that we sent to you where we discussed this concept and asked for comment as to the benefit of having the report.

We haven't actually modified the rule to put the language in there, but we're looking -- partly it's a question of who are the users of that information because the reports our Staff have access to the records. Other people may have only access to the report. So we're asking for the benefit of having reports of the inconsequential changes.

MR. ROSEN: Well, I thought that the benefit would be that the Staff would have ability to say no, this change which is inconsequential in this report, as is reported in the annual report, we don't think is inconsequential.

MS. MCKENNAH: As I say, that is a possibility, but as I said, since we require documentation of the changes also, the Staff has access to that information.

MR. ROSEN: You don't require that it be submitted, right?

MS. MCKENNAH: That's correct.

MR. ROSEN: So you have to go out to each plant and ask them for what inconsequential changes are going to be made over the last year and inspect that.

MS. MCKENNAH: If that's what we wanted to do, yes, yes.

MR. COLLINS: We're trying to balance the level of oversight that's associated with the less significant items.

MR. SHERON: That's consistent with what we do with 50.59. The licensees make 50.59 changes and the Staff, but I think the project manager usually goes out about once a year and does an audit of the 50.59.

MR. ROSEN: Does 50.59 no longer require an annual report?

It used to.

MR. COLLINS: Eileen?

MS. MCKENNAH: Eileen McKennah. It requires at least a two-year frequency of the reports.

MR. ROSEN: Of the written report. But

this doesn't.

MS. MCKENNAH: That's correct.

MR. ROSEN: That's very curious.

MS. MCKENNAH: As I said, I think it's something we really hadn't thought of at the time and you know -- but we wanted to at least invite the comment on it and then depending on the comment we may add that at the final rule stage.

MR. ROSEN: Okay.

MR. COLLINS: Okay, so changes other than inconsequential, that is, potentially consequential, would receive a risk-informed review by the Staff. And the rigor of that review increases with the significance of the proposed change, just like 1.174 requires right now.

The fifth paragraph? This paragraph repeats the message of making requirements commensurate with safety significance and it also specifies that for the beyond TBS LOCAs, the rules should include a high level criterion of maintaining coolable geometry and also that the rules should include a requirement for containment integrity. And it also indicates that the capabilities for beyond TBS should be provided in a performance-based manner, consistent with the approach taken at 50.69.

And finally, it suggests, depending on how you read it, that we include a requirement of severe accident mitigation strategies in the rule.

So the rule clearly has the high level criterion to maintain quanti-coolable geometry, again, transition break sizes, and it also has a specific containment integrity requirement.

And we believe that the acceptance criteria of coolable geometry is, in fact, performance-based. We don't prescribe how it's to be met. You can use nonsafety equipment. You can use realistic analysis methods. You can use realistic inputs, best estimate inputs and the licensees can even propose implementation criteria for coolable geometry, if they wish.

As regards the suggestion on severe accident mitigation strategies, in developing the rule, we considered requiring licensees to place guidance on the mitigation of beyond TBS breaks into their SAMGs, but when you look at the SAMGs, they really focus on actions that will be taken by the control room operators based on direction that they receive from the technical support center after core damage has already set in or core damage is imminent. But we really think the focus of this rule ought to be on preventing core damage for the beyond TBS breaks.

And then for such -- large break LOCA events are very fast events. And to keep the core cool you've got to get a lot of water in there really fast. And if we have to interact with -- the operators would have to interact with the TSC because they're using the SAMGs, we just don't think it's feasible from a time perspective.

And so we'd -- there would have to be a fundamental change to the scope, the philosophy and the implementation of the SAMGs if we wanted to rely on them for beyond TBS LOCAs and we just didn't think that made a lot of sense. So we decided not to do that.

MR. SHERON: The other piece of this, by the way is that we still have the EPGs in place and when we look at those, you know and the EPGs, the emergency procedure guidelines are what the operators actually use. They're symptom-based, so they do provide that when we call it, you know, I don't care how I got this loss of coolant, I'm going to deal with the symptoms. I'm going to initiate whatever I need to cool the core.

We believe that basically covers that aspect of accident management, you might say. We look at those, for example, when licensees do an ECCS analysis, if they take credit, for example, for operator actions, we obviously during our review, convince ourselves that these actions are feasible and can be taken in the amount of time that's specified. So they are, in fact, factored into the review, to some extent.

While we didn't exactly, as Tim said, we didn't see a way we could get to the SAMGs because it just didn't apply in this case since this action is still required to be mitigated. We think that we're covered with the EPGs.

MR. SHACK: We'd like to finish in 10 minutes.

MR. COLLINS: Paragraph six. Paragraph six, I think the main message here is just a reinforcement that the oversight should be commensurate with the categorization. I don't think there's anything else new in this paragraph that we haven't already discussed.

So unless you have a specific question on something in that paragraph, I'll just go right by it.

Paragraph seven, I think the key points here were we should use existing processes where possible, but if necessary, include a change process in the rule. Except for inconsequential changes, we use the existing processes of 50.90 and Reg. Guide 1.174. And we've elevated the status of Reg. Guide 1.174 by putting it in the rule, but the process itself is the same.

For inconsequential changes, we couldn't use 50.59 because in a risk-informed world, 50.59 does not include acceptance criteria for the metrics that are used in risk-informed evaluations, things like delta CDF, delta LERF and important considerations like how uncertainties are treated and how defense-in-depth is treated.

So we would have either had to change 50.59 as part of this package or just put a process in this rule and we just put a process in this rule which basically took off in 50.69 and said licensees use a Reg. Guide 1.174 type process on your own. If we approve your process, then you can make your inconsequential changes without our prior approval.

Paragraph eight. This paragraph points out that regulatory stability should be an important consideration in the rulemaking. It also says, however, that if we do need to reverse changes due to new information or analysis, that backfitting should not be required and that we ought to make sure that licensees are aware of that.

Okay, so we've modified the backfit rule as part of this package to preclude any reversibility considerations, to facilitate any reversibility considerations. And in the selection of the break size, I don't think the term stability was mentioned, but that was a major consideration that we had, that we didn't want to pick a break size which two or three years down the road we're going to be changing again because opinions of experts can change.

So we built in, I think we built in a margin to -- with stability on our minds.

As far as keeping licensees aware of the potential for backfitting, I think it's clear in the rule, it's clear in the SOC and we don't plan to make phone calls to everyone.

Ninth paragraph basically says that the rule should encourage the use of best estimate methods, but should not require the use of best estimate methods. I think it's just generally understood that the rule is structure that the more realistic your analysis methods are, the more flexibility you're going to have in the changes you want to make. So I don't think we need to go any further than that.

And we have not included any requirements that you'd have to use best estimate methods, small breaks, large breaks, any breaks.

Paragraph 10 says to risk inform the operating plants first and do future plants separately and more closely. Well, we can do anything more slowly and we're definitely going to risk inform. This rule does not address anything but operating plants and if we inadvertently constrain some future plant condition by what we've done in the operating plant rule, we can certainly do it in a future plant rule.

MR. APOSTOLAKIS: I'm sorry. On paragraph seven, it says you should follow the existing regulations and guidelines and mentions Reg. Guide 1.174.

MR. COLLINS: Right.

MR. APOSTOLAKIS: In the presentations we've had and I see we have a whole presentation later about tracking the cumulative change in risk, 1.174 requires you to be tracking the cumulative period or risk due to changes, but you are actually requiring the licensees to track the cumulative changes in the context of this rule, right?

So you are really going beyond what the

Commission is saying, aren't you?

MR. COLLINS: Well, I don't think so. Steve, do you want to?

MR. DINSMORE: Hi, this is Steve Dinsmore from the Staff. One way to read 1.174 is that the cumulative increase from all risk-informed changes whatsoever should not exceed 1 times 10^{-5}. We actually interpret that to mean the cumulative risk increase from any particular set of changes or any set of related changes. So I think our interpretation is a little more flexible.

I think both interpretations could be taken from 1.174. The actual individual Reg. Guides are a little more specific in that they say you should look at the cumulative risk increase from the related application.

MR. KRESS: So we need to be creative in how we choose the types of changes we make? Break them up into smaller and smaller pieces?

MR. DINSMORE: No.

MR. APOSTOLAKIS: That's the bundling issue, that you can't really do that. You can't break it up into many, many pieces.

MR. SHACK: Down to five minutes, George.

MR. APOSTOLAKIS: What?

MR. SHACK: We're down to five minutes.

MR. APOSTOLAKIS: For what?

MR. SHACK: To finish this.

MR. COLLINS: I'm on the last paragraph, I believe.

MR. SHACK: You just may go back again. We're never sure.

MR. COLLINS: This paragraph talks about separating the loss of off-site power from the less than TBS breaks and what it would mean here. The Commission basically says we can do that in a separate action. So this rule leaves the loss of offsite power as part of the LOCAs that are less than the transition break size. Larger than the transition break size, we can move loss of off-site power. This initiative is for the design basis accidents.

That's my spiel. Are we within the time frame?

MR. SHACK: We're on time. Mr. Bishop, do you want to make a comment?

MR. APOSTOLAKIS: You can't make it from there.

I don't understand, is this a presentation?

MR. SIEBER: No, we are expecting a

presentation from Steve Dinsmore and Brian Thomas and then Mr. Harrison.

MR. SHACK: Is this a question or comment on this presentation or is this the thing we had scheduled for somewhat later?

MR. SIEBER: I'd rather do it later when we have it scheduled.

MR. APOSTOLAKIS: There's no later. Is there a later?

MR. SHACK: Later in this presentation, series of presentations.

MR. DINSMORE: Hi, this is Steve Dinsmore from the Staff again. How much time do you --

MR. SHACK: You have 20 minutes.

MR. DINSMORE: Twenty minutes.

MR. APOSTOLAKIS: That's a lot.

MR. SHACK: We'll have no problems if you take a little bit less.

MR. DINSMORE: Okay. These are two issues that the ACRS has expressed interest in before and so we're back to explain it a little more.

The two issues are, the first is that the rule requires the licensee to estimate and track the cumulative impact on risk of all changes related to the redefinition of large break LOCA and the second one which is related, but they're not completely dependent is the rule prohibits combining the risk impact of unrelated changes.

Now when we do a change in risk impact for changes, the way you do that is you run the PRA without the change. You redo the PRA with the change and you subtract the two. So this slide, you can look at it over a five-year period. The licensee makes 100 changes. Twenty of them are due to 50.46 and 80 of them are due to other reasons.

After five years, the first bullet says the change in risk that you're reporting has to include all 20 changes. The second bullet says you can't include directly in that change in risk estimate the other 80 changes. They're indirectly in there so far as if you improve the risk provide at the plant, the risk impact to the first 20 would probably be approved, but you can't directly put them in there.

MR. ROSEN: Now is this consistent with your current practice for people doing 1.174 type applications? Do you make them go through the same process?

MR. DINSMORE: Yes. Sometimes -- we have to have confidence that the total impact of all the related changes are less than 10^{-5}. If we believe that they don't have to do the calculation every time, we might not ask them to do the calculation.

MR. SHACK: But they have to have a tracking process?

MR. DINSMORE: They would have to be able to answer the questions we ask.

MR. APOSTOLAKIS: But wait, the cumulative change doesn't have to be less 10^{-5}, does it in the current applications? Each time you approve, you have to bundle the related changes and then you say okay, this now has to be less than 10^{-5}. But in the period of three years, they request six changes, the sum can be greater than 10^{-5}. But 10^{-5} is the current approval.

MR. DINSMORE: No. The 10^{-5} is --

MR. APOSTOLAKIS: No.

MR. DINSMORE: Yes. Let's skip ahead here.

MR. SHACK: He's telling us the practice, George. You may be telling us the theory.

MR. APOSTOLAKIS: Well, what's written in the guide has to mean something.

MR. DINSMORE: Well, this is the cleanest one. If you look at the one in the middle here, 1.175, in-service testing, the cumulative impact of all the risk-informed IST program changes, initial approval plus later changes should comply with the acceptance guidelines.

That's cleanly said. The others in the other guidelines, it's less clear, but that's what we've been implementing.

MR. APOSTOLAKIS: And the acceptance guideline is 10^{-5}?

MR. DINSMORE: Right, yes.

MR. KRESS: It certainly, George, seems to me like this is an interpretation that the ACRS never made when we said we like 1.174. I think our interpretation was that you could have changes as long as bundled changes didn't exceed the 1.1095 and you can approach your way up to the limits. And then, as you approach it, you got more and more trouble trying to get it in the reg. and that's always been our interpretation and this is a different interpretation. I don't know how we arrived at this.

MR. DINSMORE: If you look at the first one here, Reg. Guide 1.174, it says the cumulative impact of previous changes -- which is what I was kind of indicating earlier that we could interpret this to say if you take all your risk-informed changes and you add them together --

MR. KRESS: This just doesn't make much sense.

MR. APOSTOLAKIS: It says available. It doesn't say that numerically they have to be less than something.

MR. RUBIN: This is Mark Rubin. There may be a misinterpretation here by limiting cumulative changes. These are only tightly related activities. There certainly would be multiple tech spec changes. Each of them would be 10^{-5} at the maximum allowed delta. But if you have a program that is very related --

MR. APOSTOLAKIS: Let's stop there. Each run can be 10^{-5}, so if I have three of them, the total will be 3 times 10^{-5}?

MR. RUBIN: If each number was at the limit, say they came in for each -- in practice, almost never are they at the limit.

MR. APOSTOLAKIS: But if they were.

MR. RUBIN: If they were.

MR. APOSTOLAKIS: A cumulative would be what?

MR. RUBIN: Well, they're not usually independent, but if they were truly independent, then it would be 3 times 10^{-5}, but in fact --

MR. APOSTOLAKIS: So this is the way we're interpreting it?

CHAIRMAN BONACA: And I would disagree with the interpretation. What you're setting here is a standard that says 10^{-4} is really the goal and I could be planning on how expanding all my margins there, if I have 10^{-5} to accomplish a lot of wonderful things, nothing to do with safety, okay, eroding that marginal 10^{-4} I think is inconsistent with --

MR. KRESS: But you could interpret the statement that as you approach that limit and needs more regulatory scrutiny as being a cost benefit scrutiny, lots of the safety scrutiny, lots of others things.

MR. APOSTOLAKIS: The guide basically says that --

MR. KRESS: It said that because of that problem.

MR. RUBIN: This was discussed extensively when 1.174 was first put together and we had the same concern that was just mentioned by the Committee and we indicated we'd be following it closely and in fact we're not seeing changes at those limits, but we do look at the cumulative change of past applications to give us a sense of where the collective risk of the plant may be changing, may be going to. But the point Mr. Dinsmore is trying to make is that within specific very tightly related programs, those programs are often assessed as a collective bundle of changes. If it's an IST program that comes in, that will be at a 10^{-5} limit. An ISI program, the ILRT, the type A tests that -- the test that is done --

MR. APOSTOLAKIS: That's consistent with my view and I think Tom's.

MR. KRESS: That's all right.

MR. WALLIS: Before we have a debate on this again, I'd like to say I like the bundling because we're told that this act is supposed to improve the risk state of a plant and all you're arguing about is ways to in-shop to make the risk to the plant worse. And I thought there ought to be some incentive for these guys to improve the risk state of a plant by bundling these changes in some way.

MR. KRESS: We're not arguing with that.

MR. APOSTOLAKIS: We're not arguing with that.

CHAIRMAN BONACA: It's a license to creep.

MR. WALLIS: License to creep, right.

MR. APOSTOLAKIS: No, it was never intended to be that.

MR. RUBIN: In fact, all the changes will be considered as a bundle within the context of 50.46A. So all the changes will be considered as a group, some may well be safety improvement, some may be small increases and as a group, we'll be looking at the cumulative limit that's described as sufficiently small in the rule.

MR. KRESS: Let me ask about bundling. Does bundling have to be a simultaneous effect? Suppose I come in with a change that drops my CDF, delta CDF by 4 times 10^{-5}, decreases it. And then later on I make a related change, based on the rule because I'm not going to make all the changes at the same time and I said this is related. Now I can make this change and it's going to 4 times 10^{-5} increase because I've already had this previous change.

MR. RUBIN: Well, Mr. Dinsmore actually has a slide on that later in his presentation.

MR. DINSMORE: The rule actually requires you to combine those two; the rule requires you--

MR. KRESS: At the same time.

MR DINSMORE: Cumulative. It requires you to credit the early -- in this case, if you've made a risk improvement earlier on but you could not have made because of 50.46.

MR. KRESS: As long as they're related, they don't have to be simultaneously or even --

MR. RUBIN: That's correct.

MR. KRESS: -- close in time.

MR. RUBIN: That's correct.

MR. DINSMORE: That's right.

MR. APOSTOLAKIS: Why didn't you take the geometric average of all the changes?

(Laughter.)

CHAIRMAN BONACA: All right, let's move along.

MR. DINSMORE: I think the tech specs that the control of the cumulative risk increase in tech specs is that you're not allowed to run them at the same time. Now, I'm not quite sure where I am.

MR. APOSTOLAKIS: Keep going.

MR. DINSMORE: Well, this is just a definition of cumulative change that might answer your question --

MR. KRESS: Let me ask you another question about cumulative changes. Suppose I have two changes whose effect on the mean CDF, they're related changes, and they completely offset each other.

All changes within mean CDF are not equivalent. One of them may have a much bigger effect on the uncertainty. One of them may an effect on defense-in-depth, where the other one doesn't. Are you dealing with all delta CDFs as equivalent to each other if they're related?

MR. DINSMORE: We would deal with the cumulative impacts so that it would be both --

MR. KRESS: You deal with --

MR. DINSMORE: -- the earlier change plus the later one. Normally at this point in time we have not been. The only time we've been kind of concerned about the difference in uncertainty is if you're adding seismic changes to internal event changes.

MR. KRESS: So that I could do programmatic changes to offset the hardware changes?

MR. DINSMORE: We --

MR. RUBIN: That would not be significant programmatic changes that would be controlling risk; that would not be within the context of the guidance of 174 --

MR. KRESS: There not significant if they're one times 10^{-5}.

MR. DINSMORE: Well, that's pretty significant change in CDF for a change in human actions.

MR. RUBIN: That sort of offsetting change would probably not be well received.

MR. SIEBER: Scrutiny, I think.

MR. DINSMORE: Well, the reason you do the delta risk calculation is to compare it to an acceptance criteria which is generally 10^{-5}. So we have to know what we're going to compare to that. And what this second bullet is, or this second set of bullets is, to do that again you have to calculate a CDF then you have to -- a before CDF and then an after CDF.

And the way it's set up right now, your before CDF you would calculate by taking all the changes out, including the benefits, and all the minuses. You'd calculate your CDF, you put them all back in, you'd calculate another CDF, you'd subtract those two and that's what you're comparing to the guidelines.

MR. KRESS: So this process, to me, implies that the object of the ruling is to make sure that the plants don't deviate too far from the current licensing basis.

MR. DINSMORE: Yes, well deviate too far. The reason the delta CDF risk guideline is there is to define how far you could deviate without a great deal of concern. So, it's to track and to monitor the deviation and try to keep it below a level that would give us concern.

MR. KRESS: So that a very good plant from the Fed point has a low risk status if you believe PRA. Is constrained to not make -- it doesn't get any benefit from that low-risk status.

MR. DINSMORE: Well, it does insofar as it's risk profile is real low and so the changes that it makes would probably not lead to as large increases as the plant which was already kind of --

MR. RUBIN: Let me supplement that. They would get full benefit of their lower starting point for just the point that Mr. Dinsmore indicated, the charges, hardware, setpoint changes, core power and thermal limit changes would all be starting from a much lower quantification, individual cut set sequences.

And the changes to the plant would have correspondingly lower impacts on risk so they could do more to the plant to start with than the plant that was pushing the limits in the first place.

MR. KRESS: That's certainly a debatable point. They're saying that the delta depends on the absolute value and I'm not so sure I buy that in the PRA spec.

MR. DINSMORE: Well, I said there's some relation -- but okay, these are the justifications, I guess there's still some discussion about that so I'll skip these unless you want to talk about them.

I'll just try to define, well, what the proposed implementation is right now is that they must estimate and track cumulative changes and risk from all related changes. Changes that cause cumulative risk increase to exceed sufficiently small would not be permitted and if the cumulative increase exceeds the sufficiently small guidelines following PRA updates or other changes to the plant.

In other words, if you're doing other stuff out there, and you impact the risk from these changes, and it excess the sufficiently small guidelines, the licensees must take appropriate action which we haven't completely defined yet.

MR. KRESS: Are you going to have any problems with deciding what related to --

MR. DINSMORE: Yes.

MR. KRESS: I might want to come in and make a change that could or could not be construed to be associated with this. I would say I'm going to leave it -- I'm going to not -- if it's an increase in CDF, I'm going to put it off somewhere else.

MR. RUBIN: At this point, the way we're viewing it as, as a related change is when you could not make, if you weren't incorporating the 50.46A authority.

MR. KRESS: Almost any change you can make as long as you can form the 174 --

MR. RUBIN: Most of them we think will be clear. Some of them will probably be fuzzy.

MR. APOSTOLAKIS: The first bullet there, why not? Will not be allowed. Why not? Isn't the purpose of all the regulations is to make the plant safer?

MR. RUBIN: Why not is partly because we don't want to -- when 174 was written, there were cautions in there. The way the change request was discussed about allowing plants to create new vulnerabilities is significant accident sequences.

By trading off other risked improvements to these old -- you didn't want them to create new vulnerabilities. And because we didn't want them to create new vulnerabilities, you can't infinitely trade off pluses with minus. We wanted to control that. And in 1.174 the control was with every application we would think about it, but if we thought it was too great a trade off, in other words you can't say well, I'm going to increase 10^{-4} in this area because I can do my decrease to 10^{-4} in some other area.

MR. KRESS: Doesn't this go against what I just heard, that the plants with low risk status would benefit greatly because the deltas would be -- they'd make more changes to get the same amount of delta. Now that seems -- this seems to go against that because I would like to make some changes to my plant to get down there so that I can have this benefit. But you're saying no, no, I'm not going to let you do that.

MR. RUBIN: We're not saying they can't do it. We think it's a great idea if they want to improve safety in their plant in a bunch of unrelated areas. We applaud them for it. As far as this criteria in the rule, the intent here as Mr. Dinsmore has indicated was to prevent driving risk up inordinately in the areas related to 50.46A.

We didn't want to create risk outliers. We don't want to significantly increase the risk profile in areas that derive from this rule authority. We think 10^{-5} is a pretty significant delta CDF. It's the maximum allowed in 174. In fact, significant changes to the plant that we've been seeing up at this point usually don't come anywhere near that in many cases in order of magnitude.

So we think there's significant changes to the plant that can be envisioned, that can be incorporated without coming anywhere near this limit. And there will be tradeoffs well within the 50.46 regime that makes sense.

As far as unrelated changes, someone wants to offer substantial -- a new substantial enhancement improvement in the plant that will drive risk down, in the later slide Mr. Dinsmore has in his package, you'll see that we will consider that on a case by case basis. If a licensee wishes to propose an unrelated enhancement and use it to tradeoff or buy some additional 50.46A changes, we'll look at it.

It may make a lot of sense in which case the exemption would be granted. But as a matter of course, we do want to have an upper limit of acceptability for the group of 50.46A changes and that's the E^{-5}.

CHAIRMAN BONACA: I think it makes sense to me because I could propose to improve significantly improve the acceptability of the system at the expenses of my CCA, ECCS system. They are two different things and I still rely on ACCS, in my judgment, even with the change in the rule for defense-in-depth.

And so this way I'll be trading some unmeasured defense-in-depth --

MR. APOSTOLAKIS: All those goes back to a point of reference which was the baseline CDF and LERF at some point, right? And then everything else is considered a change after that. If I improve my auxiliary feedwater system and bring it up to the level of SOC techs --

MR. KRESS: You've got a new baseline.

MR. APOSTOLAKIS: I have a new baseline. They don't let me do that. That's not my baseline. It's a change.

MR. DINSMORE: I'm sorry, sir, but actually you're half right and half not right.

(Laughter.)

MR. WALLIS: It seems to me we have an issue here, 1.174, which we can discuss at a last time and sort it all out.

MR. APOSTOLAKIS: What other place? We have to write our letter.

MR. WALLIS: I think that there are bigger issues than 46A that you've been talking about here.

MR. DINSMORE: I think Mr. Rubin got me to this last slide, but I just want to be clear because it's fairly important when they do the change in risk calculation they use the current PRA. It's not -- we're not comparing to an old PRA. We're comparing to the current PRA. They redo the calculation with the whole set of changes.

MR. APOSTOLAKIS: What does cumulative mean? Cumulative from --

MR. DINSMORE: Cumulative from the -- the cumulative risk increases of all the changes that have been allowed on your current plant.

MR. RUBIN: The calculation is very simple. You take the most current PRA model. You take the 50.46A changes out, calculate the CDFs and LERFs and put them back out and there's your delta. And we'll be using the most current PRA model to make the cumulative termination meet the limit. As far as taking -- getting benefit for unrelated changes as Steve was starting to point out earlier, you do get a significant benefit because as you make unrelated changes, the risk profile of the plant will decrease.

Many of the accident sequences that the systems that are related to 50.46A may also be driven down. Consequently, the deltas may in many cases be smaller because of unrelated changes that were made to the plant. It won't be true in all the cases, but it will be true in some of the cases. So there will be some benefit to unrelated changes that will buy them more authority, more flexibility within the 50.46 arena.

MR. SHACK: Thank you very much. We're going to discuss the regulatory analysis.

MR. SHERON: While Brian is getting set there, I wanted to just give an introduction to this on the reg. analysis.

I wanted to remind the Committee that this rulemaking is part of a much broader activity in the Agency which is the implementation, the PRA implementation plan. If you remember, this is -- there were three options, Option 1, Option 2, Option 3.

Option 1 was we continue to process risk-informed license amendments.

Option 2 was we risk-inform the treatment requirements.

And then Option 3 was -- we actually go in and change part 50 and make the part 50 regulations risk-informed.

And this is actually -- this is that third option that we're doing here. So when you're looking at this from the cost benefit or if you want to call it -- that aspect of it, you need to look at it from that broader context as well that there is a broad benefit from risk-informing our regulatory structure and our regulatory processes that needs to be considered when looking at just the individual benefits of a particular rule.

MR. THOMAS: I'm Brian Thomas. I'm with the Financial and Regulatory Analysis Grump in NRR.

I think it's important to point out at the very outset that traditionally when we do reg analysis we look at all the aspects of the cost and the benefits that's associated with the proposed requirements.

In this reg analysis, we opted not to do that and I'll tell you why. Basically, as was said before, this is an enabling rule, so licensees may voluntarily choose to apply this rule and they may do so on a plant-specific basis.

For that reason, it's obvious that there are various aspects of facility design changes, operational changes that a licensee could get out of from implementing this rule. And for that reason we believe that there's a wide variety of uncertainty that's associated with this rule.

The intent of this rule is really to enable the benefit of giving the licensee operational flexibility and in so doing, the licensee, as I said, could implement a wide variety of design and operational changes. However, we do think that this rule does contribute to safety.

MR. APOSTOLAKIS: As opposed to other rules?

MR. THOMAS: Indirectly contributes to safety.

MR. APOSTOLAKIS: I thought all rules contributed to safety.

MR. THOMAS: The focus of this rule is primarily flexibility in operations. We're not going to try to --

MR. APOSTOLAKIS: I agree with you.

MR. THOMAS: We're not going to try to quantify the safety contributions with regard to this and that's --

MR. WALLIS: But you are taking measures to limit the decrease in safety? That's what we've been talking about for the last half hour.

MR. THOMAS: Right.

MR. APOSTOLAKIS: The rule itself won't do anything for safety.

It may or may not. It's a subsequent request.

MR. THOMAS: From a regulatory analysis standpoint, we'll get into exactly what's addressed in the safety space at this point in time.

MR. APOSTOLAKIS: Good.

MR. THOMAS: So the underpinning of this rule is the flexibility in plant operations. Some of the operational enhances that has been identified by industry, specifically the Westinghouse Owners Group, and keep in mind, we try to build on what's been identified in our interactions with industry. Some of those enhancements are power uprates --

MR. WALLIS: Doesn't the first one dwarf all the others? Maybe -- sure, management helps, but you're not going to make much money out of diesel generator start times. You have a huge amount of power uprate.

MR. THOMAS: Exactly, and that's why the primary focus of the reg. analysis in this case is on the economic benefits that come out of power uprates. The rest of the bullets here, as I said, we'll leave it up to industry to translate for us what that means in terms of the economic gain and in terms of safety improvements.

MR. WALLIS: I wouldn't underestimate the third bullet.

MR. THOMAS: Granted, but again, we -- to look at how you get there, what are the specifics of a plant application on the part of a licensee that would get us certain improvements in that area, we did not get into the details.

MR. WALLIS: Very often three is the key to one.

MR. THOMAS: True, true, even though we felt that the benefits in terms of the economics is so significant, the dollar figures in terms of cost savings is so significant that when you talk about power uprates and extended plant operations to license renewals, it didn't really warrant that we even go into the benefits in some of the other areas.

If you turn to the next slide, I think I already talked about this to some extent, safety benefits will vary on a plant-specific basis.

MR. WALLIS: I see that. I think we should stop saying that this rule is going to improve safety, although we have a general feeling it will because we don't have real evidence for that and you're making it clear that there isn't any.

MR. THOMAS: Right. I agree completely.

Our interactions with industry, you'll find that industry will claim that through --

MR. WALLIS: There might be some --

MR. THOMAS: Deficiencies in operations that would lead to economies in their operation which they think can --

MR. ROSEN: I think it's unfair to characterize the safety benefits when we have a presentation later on that subject. I think it's unfair now to characterize it.

MR. THOMAS: Basically, building on what's the driver of this being that it's the power uprates and EDG tech spec changes, the reg analysis, we pretty much did a simplistic reg analysis, if you will, by just taking a broad brush approach at what are the driving dollars, what is the balance in terms of benefit, in terms of cost savings to the industry?

So we used sort of a bounding approach in our reg analysis due to the uncertainty, due to the different levels of participation that's anticipated by licensees and due to not knowing what are the degrees, the various levels of power uprates to be achieved on plant specific basis.

So basically we assume that all the PWRs would take advantage of this rule and power uprates would be perceived as a good thing to do, a great thing to do, very rewarding.

On this slide, basically, what you have before you is just basically our formula for arriving at the bottom line.

MR. ROSEN: But don't you recognize, Brian, that some plants won't be able to do power uprates because they'll be limited by secondary side?

MR. THOMAS: Yes.

MR. ROSEN: You said all 69 plants will do a power uprate, that's clearly not true.

MR. THOMAS: We're assuming that all of the plants would take advantage of the rule, but we do have in our backup slides some scenarios which we show that you would have a number of plants that would maximize their power uprates application as well an even lesser number that would have a lower power uprate application.

MR. ROSEN: Some plants will have zero capability because they're limited by their steam generators or turbine cycles.

CHAIRMAN BONACA: I think with the next one you're showing that you're evaluating a range, so --

MR. THOMAS: That's right. We have three

scenarios and only one scenario has all the PWRs participating in using the rule.

MR. WALLIS: The numbers are so big that we don't need to quibble about them too much.

MR. THOMAS: That's right. Based on a formula, again, you see the bottom line as being significant economic gain. Again, without including any quantification of safety. We see a --

MR. WALLIS: Why is the ROC interested in economic gains to the industry?

MR. APOSTOLAKIS: Because that's what's required.

MR. WALLIS: Is it in its charter?

MR. THOMAS: That's right.

MR. APOSTOLAKIS: When you do a regulatory analysis, you have to consider that.

The question is why do you have to do a regulatory analysis?

MR. THOMAS: That's right.

MR. APOSTOLAKIS: Because it's in the books.

MR. THOMAS: Because it's policy.

MR. APOSTOLAKIS: Not because it's meaningful.

MR. WALLIS: It's interesting because --

MR. APOSTOLAKIS: This is not meaningful at all.

MR. WALLIS: -- and the cost is to you.

MR. THOMAS: This is a voluntary rule and on the outset we can recognize it's purely economic. We really --

MR. WALLIS: Why don't you do all your regulation based on economics, it would make a lot of sense.

MR. THOMAS: Here, here.

(Laughter.)

MR. APOSTOLAKIS: I don't think this means anything. I'm sorry. I don't think it means anything. If you want to save time, keep going.

MR. SIEBER: Well, you can go to the last slide.

MR. WALLIS: Don't you think $13 billion means anything? You come from a rich university and $13 billion doesn't mean anything.

MR. THOMAS: This is done purely for us to be in concert with policy.

MR. APOSTOLAKIS: That's right.

MR. THOMAS: It merely gives us a data point from which we can judge what's the impact on society.

MR. WALLIS: I think it means a great deal because the public is going to think this is why you're doing it.

MR. APOSTOLAKIS: $700 million into $13 billion is what? Is it an order of magnitude plus something?

MR. THOMAS: $13 billion.

MR. SHERON: We may be doing this because I think -- I think there's a legal requirement we have to do it as part of a rulemaking package.

What answer you get is probably anybody's guess. As Brian said, depending upon what assumptions you make. We don't know right now what assumptions to make,you're right. Probably all the plants won't be able to do a power uprate.

I imagine there are other benefits that they'll get, for example, changes in tech specs where they don't have to meet rigid requirements, for example, on accumulator pressures or levels. If they're out of spec, they don't have to take action immediately. They might be able to take accumulators out of service. They can run with three instead of four, so they don't need to have all four in service or stuff like that.

They may be able to get some relaxation on diesel generator start time. I imagine some licensees may be able to do flux reduction, so they can get more life out of their vessel because they'll be able to get higher peaking factors.

We just don't know yet how -- they're going to have to analyze their own plants and see what the actual limits are. My guess is some utilities are going to go in there and they're going to start jacking up the peaking factor and they're going to run into a DMBR limit. And then they're going to have figure out what to do. Or they're going to realize that they can't get a lot of benefit because they're still going to be limited by a steam line break in the containment.

We're looking at this from the standpoint of we believe there are safety benefits that can be obtained from optimizing a lot of their safety systems.

You shouldn't be picking 600 pounds, for example, set all the accumulators, okay? Maybe if you set them you staggered them. At different pressures, you may wind up that even for small breaks you don't uncover any small breaks. Right now, you do. You're limiting small breaks, uncover the core. They may be able to set accumulators so that for any small break you don't uncover the core. I think that's a benefit. So I think there's a lot to be seen.

MR. APOSTOLAKIS: How sensitive are the results of the choice of transition break size.

MR. THOMAS: I'll turn to my contractor assistant over here.

MR. BAILEY: They're not.

MR. APOSTOLAKIS: Who are you?

MR. BAILEY: I'm Paul Bailey. I'm the contractor supporting NRR.

MR. WALLIS: These are not sensitive to transition break size? They're not sensitive? That would make a tremendous difference.

MR. APOSTOLAKIS: If I read the Westinghouse --

MR. SHACK: Let's let Mr. Harrison make his presentation.

Thank you very much.

MR. KRESS: Let me ask one question about this presentation. For some reason it strikes me as rather strange because I'm used to backfit analyses that look at a rule change to impose requirements on a plant and it's justified on the basis of the person REMs that it's going to save related to the cost.

And this seems a little strange to me

because I don't see that kind of consideration in it at all. It's not what I'm used to as a backfit in a regulatory requirement. So you know, it just seems strange that I see any of this.

MR. SHERON: Because it's not requiring anything.

MR. KRESS: I know, but this is the sort of thing, I think the industry would do to see if they want to make changes, but not a regulatory body to justify a rule. That's what bothers me.

MR. SHERON: As I said, we have to do this, I think, as a legal requirement. We have to do a reg analysis.

MR. KRESS: Yes, but I don't think this is a reg analysis is what I'm saying. It's something else.

MR. THOMAS: Like I said, it's a very simplified approach of the reg analysis backfit being that this is voluntary.

MR. KRESS: Reg analysis, even if it's voluntary, when you make it is supposed to be a reg analysis and this is not. It's a cost benefit to make a change.

MR. THOMAS: 51.09, the backfit does not apply in this case.

MR. KRESS: I know, but you don't have to do a reg analysis, but you do it anyway. But what I'm saying is this is not a reg analysis. Even if you didn't have to make it.

MR. SHACK: Let's move on.

MR. SIEBER: Why don't we just go to the last slide.

We're finished here. We discussed it enough. This is the end of it.

MR. WALLIS: Your analysis says that industry is going to save billions. It's going to cost the Agency tens of millions. That's what it says. That's the bottom line.

MR. THOMAS: Bottom line is, the cost to the Agency is negligible and the benefits --

MR. WALLIS: The cost to the Agency is millions --

MR. THOMAS: -- when compared to the billions of dollars to the industry.

MR. WALLIS: And the cost to the Agency is tens of millions on page 4.

MR. THOMAS: There is a backup slide that shows that.

MR. WALLIS: I don't think that the Agency's budget is so large that that's trivial.

MR. SIEBER: It's all reimbursed.

CHAIRMAN BONACA: All right, let's hear from Mr. Harrison.

MR. SIEBER: Forty-seven slides.

MR. APOSTOLAKIS: Forty-seven?

MR. HARRISON: There's only 11 and I have to only go over about 5 of them.

MR. APOSTOLAKIS: Okay.

MR. HARRISON: All right, thank you. First, I want to tell everyone I appreciate the opportunity to be here today and make these comments. I will be brief because a lot of the information that's in here we've already discussed.

My name is Wayne Harrison. I'm from South Texas Project and I'm representing the Westinghouse Owners Group. I'm the chairman of the Westinghouse Owners Group Large Break LOCA Redefinition Working Group.

One of the things I did want to mention was that this is very important to us, to the industry and WOG has already committed substantial resources, about $2 million, to the project authorizations working on this effort.

Another thing I wanted to say here is I think it's important that we recognize this is our only opportunity to redefine the large break LOCA size. It's the window of opportunity. I think there is an opportunity here to optimize safety and operational benefits and going back to something that was said earlier, I think we would say when in doubt, don't double count.

MR. WALLIS: You're going to optimize safety benefits? Would you give us a measure of them and explain how you optimize in some way?

MR. HARRISON: I'm not going to be able to quantify that. I'll come out at the outset and say that.

One point of perspective I want to make here -- I think that it's been talked about -- is that the safety benefits on this are probably in realistically quantifiably close to risk-neutral, just simply based on the large break LOCA event frequency itself, because you're probably talking that when you go core damage frequency -- CDF -- somewhere in the neighborhood of minus six absolute value one way or the other for a delta CDF.

So, that's just my gut feeling for the order of magnitude we're talking about, something 10^{-6} or something less.

MR. WALLIS: How can you have a

safety benefit which is risk-neutral and is not a benefit --

MR. HARRISON: There are benefits though and that's what I wanted to point out -- even for the fuel utilization. We talked about power uprates, but there's another effect of improved peaking factors that comes from the reduction in the break size that you can have power burn-ups, which can result in longer fuel cycles, which results in less thermal challenges and less thermal cycles on the plant.

You could have fewer fuel assemblies that require storage and transport. Now, I don't know how exactly to quantify that, but that's definitely a benefit to the public safety. It's about four -- our analysts tell me it's about four to eight assemblies per cycle. So, that's less fuel that we have to handle on-site; that's less fuel that we have to store on-site or in a repository.

MR. ROSEN: That's per 18 months at South Texas out of typically 80 or so assemblies --

MR. HARRISON: Yes sir --

MR. ROSEN: So, you're saying 10 percent less per cycle per --

MR. HARRISON: Could be, could be --

MR. ROSEN: Could be. Five to 10.

MR. HARRISON: And again, again, per plant, your milage will vary.

CHAIRMAN BONACA: That's because you have higher enrichments?

MR. HARRISON: You'll be able to improve the enrichment --

CHAIRMAN BONACA: Right.

MR. HARRISON: Improve or increase the peaking factor; burn the fuel longer and so forth --

CHAIRMAN BONACA: Yes, so it's as if you were not --

MR. HARRISON: Right.

CHAIRMAN BONACA: -- do something like that because the cost may be going up, I -- that's okay.

MR. HARRISON: Another benefit that I really don't know how to quantify for the power up-rate is the adverse environmental emissions from non-nuclear generation capacity, and I don't know how you quantify that, but it's a benefit.

So that's really the only thing I wanted to -- I want to talk a little about the equipment safety that we talked about. Conceptually, when we talk about potential safety benefits on this slide I'm talking about peak clad temperature, primarily, and we said what is the differences or difference between the break size -- transition break size -- on peak clad temperature.

It's not pronounced, but it does have an effect, we've determined, on the Westinghouse small 2-loop plants that, if your have -- there's a difference between if you have to postulate two times the break size versus if you have to postulate one times the break size. Whether it moves down on the peak clad temperature versus break size curve -- you're kind of dependent upon that. But it's primarily it looks like it affects the 2-loopers more than it does the other plants.

MR. APOSTOLAKIS: What would it be? I mean, can you give me an idea of what the limit on the peak clad temperature could be if we approve a certain size?

MR. HARRISON: I don't have that at my fingertips. I can probably get you some information on that from George --

MR. APOSTOLAKIS: Roughly, do you remember roughly about? It doesn't have to be accurate.

MR. HARRISON: Probably 100 degrees or so.

MR. DENNING: I missed that. How is this a safety benefit? The plant isn't changed at all.

MR. HARRISON: I was saying for the 2-loop plants, we're talking about the transition break size of why the benefits of having a smaller transition break size is better for the 2-loop plants because they will still be large break limited even with two times the largest pipe and they don't get -- they don't really get any PCT benefit at the new break size.

MR. DENNING: Yes, but I argue this isn't a safety benefit at all.

MR. HARRISON: It's an economic benefit or they would be able to have the same safety benefits that we identified in the previous slide for increased burn-up or the peaking factor will affect the increase burn-up or --

MR. DENNING: If you don't do anything and you just change the small --

MR. HARRISON: Then we get nothing.

MR. DENNING: There's nothing.

MR. HARRISON: Nothing for that.

MR. SIEBER: They would actually get something, that it's already hidden into the calculation, so you can't quantify it.

I mean the margin is there. And it's just because of the way you do the calc.

I have a question. When you move from an appendix K calculation to a realistic calculation, you get a pretty big benefit just by doing that.

MR. HARRISON: Yes sir.

MR. SIEBER: And that's probably bigger than you get out of changing the transition break size?

Is that true or not?

MR. HARRISON: I don't know the answer to that.

MR. SIEBER: If I wanted to get --

MR. HARRISON: I think of the --

MR. SIEBER: If I wanted to get margin, that would be the first thing I would do.

MR. HARRISON: Right, but I think -- I'd have to defer to an analyst, a safety analyst on that one because I can't -- I believe we're looking at like, for changing the transition break size, peak clad temperature for a large break, something 400 to 600 degrees.

MR. SIEBER: You get that out of --

MR. HARRISON: Out of changing the break size, but --

MR. SIEBER: -- by changing the calculation regimen too.

MR. ROSEN: Well, 400 to 600 degree reduction in the peak clad temperature? That's enormous, right?

MR. SIEBER: Yes.

MR. ROSEN: Especially where we're maybe hundreds of degrees away from the limit, the 2700 degree limit.

MR. HARRISON: That happens though primarily in the large break LOCA. What that makes, it makes a small break LOCA your most limiting event for peak clad temperature, so you still have to consider that.

MR. WALLIS: You can get an economic benefit. I don't see any safety benefit.

MR. DENNING: Exactly, that's exactly it. It just gives you an apparent margin that you can take back up by increasing the power. In reality, you've undoubtedly decreased safety when you've done that. It's just that it's within some acceptable regulatory balance.

MR. HARRISON: I'm going to talk a little bit about some of the benefits like Brian was mentioning earlier and these are going to vary from plant to plant. These -- because it depends upon the size of your containment. It depends upon the capacity and the capability of your safety injection. It depends upon whether you cool with containment spray or you cool with reactor containment fan coolers. And all the plants are somewhat different.

We talked about containment spray system may not have to start automatically. Safety advantages of that are you won't have safety injection to compete with containment spray for refueling water storage tank inventory, more water to cool the core. Would it clearly have effect on debris transport to the sump and increases your nominal pump suction head.

MR. WALLIS: All of this should improve your CDF.

MR. HARRISON: It could.

MR. WALLIS: I think that's the measure of safety benefit.

MR. DENNING: I agree, that's real. Now the question is would the utility make those changes?

MR. HARRISON: I think the answer to that is yes, if they could make those changes because my perspective on containment spray and I'm speaking for myself, for many plants containment spray does nothing but evil.

MR. DENNING: Especially if it goes off when the --

MR. ROSEN: That's right, the containment spray inadvertent actuation is a very damaging event to the economics of the plant, so if there was a safety rationale for not having it automatically --

MR. HARRISON: It does bad things too.

MR. APOSTOLAKIS: I'm sorry, I have to understand that. Why do you call it evil?

MR. HARRISON: I'll use my plant for an example. South Texas does not need containment spray to mitigate steam line break or the design basis accident, particularly if we were to credit the alternate source term, we don't need it for dose. So here we have this system that automatically actuates I think at 9 PSI or so in containment.

So that if you do have something that causes containment spray to -- it's -- the only thing you can do is something bad for us because we get adequate cooling from reactor containment fan coolers. It's water that could be going to the core. There's just nothing positive it's going to do for us, George.

MR. ROSEN: It competes the loading time --

MR. HARRISON: This is not true --

MR. APOSTOLAKIS: That's not necessarily evil.

MR. HARRISON: Well, it is because --

MR. ROSEN: You're washing down the whole containment.

MR. HARRISON: You're washing down the whole containment. So something that's -- Murphy's Law is going to apply. Something that was qualified for it isn't going to work.

CHAIRMAN BONACA: But have we ever had industry spurious actuation of this price system?

MR. HARRISON: Have we ever had spurious actuation? Not at South Texas, I don't think.

CHAIRMAN BONACA: Jack says yes.

MR. SIEBER: Yes, there have been. It's a mess.

MR. APOSTOLAKIS: And we forced it on 8600.

MR. HARRISON: And what's true of South Texas can apply to everybody.

MR. POWERS: I want to point out that the spray is the most effective way to eliminate radioactive aerosols in severe accident. It's of overwhelming safety significance there.

MR. APOSTOLAKIS: You mean in 8600

situation or in general?

MR. POWERS: At any plant, the spray is the best thing you've got going for you.

MR. HARRISON: I think though if you have that kind of source term generated in the appropriate place, I'm not advocating that you take spray out of the design, but you make spray a manually initiated, so that if you needed in your severe accident management you can actuate it manually.

MR. ROSEN: That's all your slide says, by the way, right? It says may not have to start off automatically.

MR. HARRISON: Exactly.

MR. ROSEN: It doesn't say anything more than that.

MR. HARRISON: That's correct. More effective use of accumulators, this is something we need to quantify and we talked to the Staff about that and Westinghouse is looking at doing this and we talked -- Brian talked about it earlier on the, just a few minutes ago on staggering the initiating set point of the accumulators.

Diesel generators' start times can be increased beyond 10 seconds. I think this is probably more broadly beneficial than some of the other effects because you're not -- it's not something that depends upon peaking factors. It's something that depends upon time sequencing of the accident and how big the break is and so forth and this is a benefit to the diesel, a safety benefit to the diesels because you're not having to -- they will be more reliable for you, reduces wear on the on-going testing and reduces need for invasive troubleshooting.

Again, I want to stress here that we were doing some quantitative evaluations on this and hopefully, we can come back to you guys at a future meeting and give you some more specific information. I know you want to have -- I want to have it because when I go back and talk to my management committee and my management, I need to tell them here are your safety benefits, how good it is and here are the economic benefits and so forth. It's a complete package. I think they're there.

What we proposed is a transition break size equal to the double-ended break of a Schedule 160 8-inch pipe which is 6.9 inch ID and you can see if you take the double-ended break of that, that's comparable to a single-ended break at the most RCS branch connections.

And that's a factor of five margin on the initiating event frequency over the E^{-5} Commission guidance for nominal event frequency and we've taken that from I think Table 3 in SECY 04-60.

MR. APOSTOLAKIS: What SECY is that? Is that new reg we're reviewing?

MR. ROSEN: No. That's the April version of the elicitation.

MR. APOSTOLAKIS: Where did you go to get this again? Explain to me.

MR. HARRISON: I'll show you a table and a picture.

MR. WALLIS: You got a table from the elicitation?

MR. APOSTOLAKIS: Where was the table?

MR. HARRISON: In the SECY paper. 04-0060.

MR. WALLIS: Where does the pipe size come from?

MR. HARRISON: I picked the pipe size because instead of just looking at the break sizes or what we asked ourselves, well what pipe size, what nominal pipe size that you use in the real world corresponds to this?

MR. WALLIS: How did you determine the margin of 10^{-5}?

MR. HARRISON: Well, we used the initiating event -- if you look here, the 8 inch is a 6.9 inch ID which corresponds to estimated mean break frequency from --

MR. WALLIS: From where?

MR. HARRISON: From the SECY 04-600.

MR. WALLIS: That's not from an expert elicitation.

MR. HARRISON: Yes sir. That was a summary of the expert elicitation.

MR. WALLIS: Is it a draft first or something?

MR. HARRISON: Yes. That's from the mean frequency, yes. So that corresponds to E^{-6} mean break frequency.

MR. APOSTOLAKIS: Has that changed since that time?

It has?

MR. POWERS: Yes. That version was an earlier version, George. It doesn't include the over confidence adjustment and the other sensitivities that we talked about. It would be closer to the -- in the executive summary, the baseline.

MR. APOSTOLAKIS: Yes. I thought we reproduced the calculations. We're using their baseline table. But it came very close to what you guys did.

MR. HARRISON: Let me back up here. The point I want to make with this curve and this is a similar curve to what was shown earlier on the Staff's presentation. It's similar to the arithmetic mean, PWR plot. But what you can see here, the point is that the break frequency of the largest attached pipe which is down here is only a little different from the RCS loop piping break frequency which is basically right here.

MR. WALLIS: I didn't get that impression at all. It seemed like the largest pipe seemed to be way below all the others.

MR. HARRISON: Well, actually, this is $1E^{-7}$ and $2E^{-7}$, so when you're talking about E^{-7} frequencies, there's not much difference there, a factor of 2.

MR. WALLIS: It doesn't look like that at all. Does it?

MR. APOSTOLAKIS: What is the --

MR. HARRISON: I'm sorry?

MR. WALLIS: That figure that they showed us this morning, earlier this morning, really fell off on the right hand end and yours is leveling off. Is this because of a bundling or something?

MR. HARRISON: I think you can look at their arithmetic mean. I think it's leveled off similar to this.

MR. POWERS: All of these tabulated values don't recognize that significant list of things that were not recognized or considered in the expert elicitation, so how do you compensate for that?

MR. HARRISON: Well, I'm glad you asked that. And the point I'm going to make with these two charts. And I'm going to come to your question right after that, is that what we're recommending is that we don't need to postulate two times the largest break size, that one times that and still move it to the worse location within the reactor coolant system, but one time the diameter of the largest connected piping would be, I think, a better application of an expert elicitation.

Now for the uncertainty that you were just asked about, I think the first and key thing, we've mentioned it before and I can't stress it enough is that the requirement to mitigate breaks beyond the transition break size is still there as the backup and that substantially compensates for any uncertainty.

We talked earlier and Mr. Tregoning's

discussion and I'm not going to go through all these because they're the same points I think that he made that much of the items of uncertainty were, in fact, discussed by the expert elicitation and accounted for there.

MR. APOSTOLAKIS: How long did it take them to consider the unknown degradation mechanisms?

MR. HARRISON: I'll let Bruce --

MR. POWERS: I still need an answer to my question, this doesn't answer it at all. This just says I don't need to consider all these things and --

MR. HARRISON: I think we're saying they have been considered.

MR. APOSTOLAKIS: That's what the report says.

MR. HARRISON: It has been factored into the uncertainty already and when in doubt, don't double count is the message we're sending.

MR. POWERS: It's certainly not apparent to me.

MR. BISHOP: In the voluminous report, I agree that it's not completely apparent, okay, of all the discussions that were held by the expert panel.

George asked the question, okay, about how do we take into account and I think -- the question was is in the next mechanisms what's the chance that you think it might not have any precursors, you might not have small leaks or you might not find a crack during -- during your normal ISI or something to give you an indication that there's something going on that you could get in trouble to hurry.

I think what people typically did, okay, is they put a factor of 2 to 10 on the current frequency to account for that. And that's just based on discussions we had when the expert panel was talking about this, how do you deal with something like that? That was what we came up with and I think -- speaking for myself -- I put at least a factor of 2 on it. I don't remember exactly, but it was more than a factor of 2.

Other people may have put in a factor as high as 10 is what I heard in discussions.

MR. APOSTOLAKIS: The eight inch, well, actually 6.9, this is the mean of the distribution of or the 95th percentile of the distribution?

MR. WARD: That was the mean which had a mean initiating event frequency about $2E^{-6}$. I think the 95th percentile was right at $1E^{-5}$.

MR. BISHOP: In the SECY paper, the 95th percentile tended to be about a factor of four greater than the mean value.

And so again, that's one of the reasons we came up with a factor of five on the mean value. We thought that that would cover that.

MR. APOSTOLAKIS: The mean value of the frequency?

MR. POWERS: You put up there a factor of five margin. It looked to me, recollecting the curves, that that my level of uncertainty was a little tainted. And so a factor of five is not -- doesn't strike me as any margin at all.

MR. BISHOP: There was a factor -- like I said, in the SECY 0460 paper, the table 3 showed the difference between, for the PWRs, the difference between the 95 percent and the mean.

Again, the experts were asked to estimate the best estimate value which was the median value and then the uncertainty affects the mean value and again, the uncertainty typically the mean value tended to be about a factor of 2 greater than what we would call the best estimate median value and the 95th percentile was about a factor of four or about almost a factor of 10 higher than the 4 times 2, a factor 10 higher than the median value or the best estimate value.

So again, I think -- now again, there have been some minor adjustments, okay, made in the -- like this is an on-going process of the numbers from the expert elicitation with the adjustments and so forth. But again --

MR. SHACK: The eight inches would correspond to the elicitation with the error factor adjustment at the 95th confidence level.

MR. BISHOP: That's right, that's right.

MR. HARRISON: I think the question is how much margin do you need to -- and like 16 inch is the largest surge line I know of. It's an STP surge line which has a 12.8 inch inside diameter. And that's over two orders of magnitude over the SRM guidance of $1E^{-5}$.

MR. WALLIS: I want some clarification here. Since these inside diameters are very different from the outside, the nominal pipe sizes, when Tregoning was presenting, was he presenting based on nominal pipe sizes or real areas?

MR. HARRISON: I understand areas.

MR. WALLIS: So when he says a 12-inch pipe, he really means a 16-inch pipe in terms of nominal --

MR. APOSTOLAKIS: You mean the inside diameter?

MR. HARRISON: That's inside diameter. That could be related to break area.

MR. APOSTOLAKIS: Okay. So the Staff is proposing 14, right?

MR. WALLIS: Which is really 11 in terms of area.

MR. APOSTOLAKIS: No, but the 14 is the inside.

MR. HARRISON: We're proposing the largest attached pipe. So for South Texas, it's the next to last row --

MR. SHERON: So for South Texas, that would be a 14-inch outside diameter, 11.2 inch inside diameter.

CHAIRMAN BONACA: No, that's not right.

MR. APOSTOLAKIS: No.

MR. SHERON: I'm sorry, 16. I am sorry, 26 inch.

CHAIRMAN BONACA: So when you report the 14 inches like you were saying before, you meant really 11.2 inches?

MR. APOSTOLAKIS: No, he meant 16.

MR. SHERON: We said the largest attached pipe.

I think someone on the Staff said gee,

what's the largest attached pipe and someone said, gee, that's South Texas and we think it's 14 inches and obviously it's a larger --

CHAIRMAN BONACA: Some of the numbers that you gave us -- I'm confused now. I'm trying to understand now. We have an elicitation with break size, nominal. Are those in the elicitation nominal pipe sizes?

MR. SHERON: No, they're inside diameters.

CHAIRMAN BONACA: So I should compare really the inside diameter.

MR. SHERON: Yes.

CHAIRMAN BONACA: And not what Members are continuously quoting.

MR. SHERON: That's right.

CHAIRMAN BONACA: Okay. All right, so when we say for most PWR, the pressurizer line is 14 inches, not South Texas. We really mean 11.2 inches.

MR. APOSTOLAKIS: It seems to me that the heart of the argument is that there is enough defense-in-depth in the fact that we are requiring mitigation of breaks larger than the TBS, so this choice can be almost purely risk based, I mean frequency based. Is that really what you're arguing?

MR. HARRISON: That's part of it. I think that when you take two times -- if you take this and we were saying this is a double-ended break, okay, so that's equivalent basically to the single-ended break of a 12-inch line which is 10 inches ID and if you look at the expert elicitation of what is the initiating event frequency for a 10-inch break from a risk perspective, you're going to see that that has an even greater margin to -- from a risk perspective, pipe doesn't, of course, know what the flow area is.

CHAIRMAN BONACA: Let me ask a question. I need to ask a question because I'm confused about this too.

I've just heard the presentation here from the gentleman from Westinghouse that said all these things were really pretty much considered. But then we had the presentation this morning from Mr. Hammer that listed over four slides all the things that were not considered and those included all the active LOCAs, dropped heavy loads, seismic LOCAs with a hammer and certain piping, etcetera, etcetera.

So what's the story? I'm trying to understand whether it's included or not included.

MR. BISHOP: Since I'm the one that made the comment, let me try to clarify. In the Staff discussions they made the statement that it was not considered in what -- what that implies is that wasn't discussed. In the final numbers, that was not considered because based on the discussions there were a number of panel members that actually provided estimates for these rare events, like seismic events and water hammer events.

We actually were provided input for that and we discussed it and the conclusion was that it was not a significant contributor. So when the final numbers were rolled up, it was not included because it -- the feeling was that of the panel that that would not significantly change the numbers. So yes, it was not considered, but it was discussed and there were -- and again, the point I made, the active component failures were not included, but typically for the largest safety relief valve, that corresponds to a transition break size, would not have any effect on a transition break size greater than four inches.

MR. HARRISON: I think we're elaborating on what Bruce is saying and what is said by the Staff earlier. If you look at the nonpiping components and the active components, I think that we're, from what I heard, we're in agreement here that these things are still within the existing design basis. In other words, they don't exceed the transition break size, so we would still be designing for those failures the same as we currently do within our existing design basis.

MR. BISHOP: I think it was a matter of interpretation.

MR. HARRISON: And for the seismic issue, the -- our point was that the probability of the very high loads is very low and when you -- and that compensates for the increased conditional failure probabilities and I think the implementation process that Rich and Brian were talking about would -- for each plant that adopts this rule, you would look at that and confirm that to the Staff's satisfaction for your plant in your license amendment request.

Thank you.

MR. WALLIS: Well, I'm still concerned about the single and double-ended thing. You go from 7-inch, you get $2E^{-6}$ in your table. You've got a 10-inch, $2E^{-7}$, well, the 10-inch single-ended break is equivalent to a 7-inch double-ended break. Certain pipes which attach to the main system don't really go anywhere. They break -- you only care about single-ended break. There are pipes where you really do care about a double-ended break. It makes a difference. You can't just lump them all together. And it makes a big difference if two 7-inches and one 10-inch -- maybe I have to require you consider the 10-inch because it's only flowing one way essentially in an accident. So I don't quite know how to make that comparison.

MR. HARRISON: It is a difficult comparison to make. I think in looking at this what we were a little troubled by, if you will, is taking the equivalent of two times across a sectional area of a surge line and postulating that anywhere at the worst point in the reactor coolant system --

MR. WALLIS: That's far less important than the flow --

MR. HARRISON: Which didn't seem appropriate. Now I think in discussing the implementation of this with the Staff and we're going to be doing a lot of that and working, as Brian said, on some implementation guidance, I think we would be willing to say let's look at the surge line and take the surge line and take the actual effects of the break of a surge line. That's not a limiting event for us.

MR. WALLIS: It's a single-ended break.

MR. HARRISON: Well, you would take the actual effects of that break, surge line analysis.

MR. WALLIS: Rather than doubling its area.

MR. SHERON: Could I read, excuse me, could I read from the SRM that we got on July 1st? This was paragraph 4. It says "licensees should be required by regulation to retain the capability to successfully mitigate the full spectrum of LOCAs for break sizes between the new maximum break size and the double-ended guillotine break of the largest pipe of the reactor coolant system."

We're interpreting that to say is that wherever you pick your transition break size, if you remember the Commission said for transition break sizes below -- I mean for breaks below the transition break size, you do your analysis the way you always have which is you assume a spectrum of break areas, okay, up to that area, all right? And you postulate them around the loop to occur in the worse location and you do your analysis and you calculate your clad temperature.

What this says is that for transition breaks, for breaks above the transition, we have to consider up through the double-ended guillotine, double-ended, okay? It doesn't matter. We're going to have to take that double-ended guillotine everywhere in the primary loop.

MR. WALLIS: Yes, that's fine. The double-ended primary loop, you've got two holes if you break apart.

MR. SHERON: Yes.

MR. WALLIS: But if you break a surge line, you've essentially got one and a little bit.

MR. SHERON: We're only using that as a surrogate to pick a break size.

Once you've picked the break size and I don't care --

MR. WALLIS: Some other space. I mean the reality space, the surge line break is not the same. The double-ended guillotine break --

MR. SHERON: The licensee can pick a surge line and say fine, it's equivalent of a single-ended break. But once I've picked that size of that surge line, in this case it's 12.8 inches, the Commission says take that --

MR. WALLIS: That's fine. We might recommend that you take a surge line, but only a single-ended break of a surge line to get your approval and area. Move it around a little, but you wouldn't move two surge line areas around the loop.

MR. SHERON: Well, the Commission says we have to mitigate up to the double-ended guillotine, so you're still --

MR. SHACK: Graham is arguing for the design basis accident.

MR. WALLIS: Yes, the design basis, the transition break area.

MR. SHACK: We all agree that above -- you're all still going to have to deal with the double-ended guillotine break.

MR. WALLIS: Yes.

MR. SHERON: But below that, at 12.8 inches or below, the Commission said do it the way you normally have done it, all right? The way we've normally done it is we have postulated that break around the loop --

MR. WALLIS: Brian, the thing is what do you use to calculate this mysterious area which appears on the main loop somehow? Do you use one times the cross sectional area of the surge line or two times.

MR. APOSTOLAKIS: Two times.

MR. WALLIS: That's independent of the way the --

MR. KRESS: You guys have to realize that reality space and design basis space are two different things and there is no rational way to look at design basis space and choose the way it's implemented. It's all a kind of judgmental defense-in-depth thing --

MR. WALLIS: We're talking about an equivalent area of a transition break size and that's a new rule and has nothing to do with the way you've been doing stuff in the past.

MR. KRESS: Yes, it does. We're still dealing in design basis space.

MR. POWERS: Tom, isn't the spirit, at least, of the SRM to say look, our technology is an understanding, are vastly improved. Let us move in the direction of greater realism and even though we may not be able to take a complete step here, let's take a partial step?

MR. KRESS: That's exactly right and these people are making a partial step.

MR. WALLIS: You still have to define this mysterious area, this transition break size is an area. What area is it? Is it twice --

CHAIRMAN BONACA: I think we can have this discussion when we go to letter writing.

MR. KRESS: This is a letter writing discussion.

CHAIRMAN BONACA: We have people waiting

for us for the meting and we have to take lunch in between for the next meeting.

MR. WALLIS: Okay, that's it.

MR. SIEBER: Are we done?

MR. HARRISON: I'm done. I can be done. I think I made my key things. One thing I will say, I think we'll be coming back to talk to you guys with some more specific information on quantification.

CHAIRMAN BONACA: All right, let's take a break for lunch. Since we're running so late, take like 45 minutes. Is that okay? Forty-five minutes. So we get back at quarter of one, quarter of two, quarter of two.

(Whereupon, at 12:58 p.m., the meeting was recessed, to reconvene at 1:45 p.m.)

CHAIRMAN BONACA: We are back into session, and the next item on the agenda is the technical basis for potential revision of the pressurized thermal shock. Dr. Shack will lead us through the presentation.

MR. SHACK: Okay. We had a subcommittee meeting on that this week. I think most of the committee members were in attendance, but we're going to go over some of this material again today for the benefit of those who weren't.

MR. POWERS: Mr. Shack, before we get started, I'll mention that, first of all, Nathan Sui is definitely not part of the Probabilistic Assessment Branch of Sandia National Laboratories. Donnie Whitehead is. I have no idea what Donnie Whitehead actually does on this project, but I do work with him, so anything I have to say on this should be understood with a reasonable amount of doubt, as the Committee usually does.

MR. ERICKSONKIRK: My name is Mark Ericksonkirk. I'll be leading the first part of the briefing, which will be the project overview, as Dr. Shack suggested the contents for that yesterday. And then after that, we'll go over the high points of the final comments we received from our peer review committee just recently.

MR. POWERS: And hopefully it will be more accurate than the author listings, right?

MR. ERICKSONKIRK: Hopefully, so, yes.

MR. SHACK: How are you going to launch your time, Mark?

MR. ERICKSONKIRK: I'm planning on getting through this as quickly as possible, so you can pick on somebody else. No. I have 15 view graphs here on the overview, so half an hour, 45 minutes max.

MR. SHACK: You get 45 minutes.

MR. ERICKSONKIRK: Okay, good enough. Okay. So what I'll be covering in the next 13 view graphs --

MR. APOSTOLAKIS: What is integrated systems?

MR. ERICKSONKIRK: That's our thermal hydraulics contractor. See, my title slide is -- but I did get my new name right, so there.

MR. APOSTOLAKIS: Even your name.

MR. ERICKSONKIRK: No, my name is Greg.

MR. APOSTOLAKIS: Shouldn't there be a space between Erickson and Kirk?

MR. ERICKSONKIRK: No, check my driver's license. You know how hard it is to get the Department of Motor Vehicles to not put a space there? That took a while.

CHAIRMAN BONACA: That's conjoined.

MR. SHACK: Onward.

MR. ERICKSONKIRK: Onward. You're wasting my 45 minutes. Okay. We're going to talk about the scope of the analysis we performed and focus on the factors that contribute, most significantly, to the vessel failure probability, those being material factors and transient classes. We will also highlight factors that we believe suggest that these findings can be applied with confidence to PWRs in general. We will propose a reference temperature-base screening criteria, screening limits. I tried to eliminate the use of the word "criteria," so I don't mess it up, that are consistent with reg guide 1.174 guidance on LERF. We'll compare the state of operating PWRs at end of license with those proposed screening limits and discuss conservatisms and non-conservatisms that remain in the calculations from which we derived those screening limits.

So the scope of our analyses, first we focused on performing three plant-specific analyses using one vessel from each of the three major PWR manufacturers. We then worked on generalizing those results to apply to all PWRs. As I said, we have a frequency limit of 1 times 10 to the minus six. It's consistent with guidance on LERF. And on the basis of that limit and the three plant-specific analyses, we proposed a revision to the PTS screening limits for NRR to consider.

So first off, we're going to look at what material factors control vessel failure. The first statement is perhaps obvious, that in order to have any hope of correlating or predicting the failure frequency of a vessel, we need to know the toughness properties at the flaw locations. And so in order to characterize those toughness properties, we use a reference temperature approach, and that's useful because a single reference temperature tells us what the temperature dependency and the scatter characteristics are of all the fracture toughness measures that we use in our calculations, as illustrated in the figure at the bottom of the slide.

So in order to know what reference temperatures to pick for these metrics, we need to know where the flaws are. And so that's illustrated on the next slide, where the big blocky thing is my attempt to show the interior roll-out of a vessel, at least schematically, so you can see the axial welds and the circ welds. You see that there's a layer of stainless steel cladding overlaid on it, and then the red squiggly lines indicate that the axial and azimuthal variations of fluence.

So some observations on where the flaws are. We've got embedded weld flaws that populate the fusion lines, so the axial flaws contain only axial welds and the circ flaws contain only circumferential - yes, that's it. The circ welds contain only circumferential flaws.

We have a population of surface-breaking flaws associated with the cladding layer because our destructive inspections and our physical understanding showed that if you were to get a particularly severe lack of inter-run fusion between the cladding weld beads, you could have a surface flaw. Because the cladding weld beads are laid down only in a circumferential direction, all of those flaws are oriented only circumferentially. And then, finally, the plates have a flaw population that's distributed throughout their volume with no preferred orientation, and that's what we simulate in FAVOR.

So in order to construct our reference temperature metrics, we use this information to guide us. And so we said okay, well, let's take the axial welds for example. What's going to control or be the worst case for a flaw in an axial weld? Well, it would be, it would certainly happen at the maximum fluence location along the axial weld, and then you'd need to figure out -- so say the maximum fluence location is there on the axial weld, just for example, and then you'd need to figure out what embrittlement properties to associate with it, so you can choose. It's either going to be the plate properties or the axial weld properties, the worst of the two, so that's what we pick.

And as you can notice from the schematic, the placement of the axial welds can have a significant influence on their fluence loading. Sometimes they're near the cross, sometimes they're near the peaks, and that needs to be accounted for.

Conversely, for circumferential welds and for plates, because both of those effectively go all the way around the vessel, you can be very sure that the circumferential welds and the plates will always see the maximum fluence in the vessel. So in calculating, say, the reference temperature for the circ weld, you just need to know the maximum fluence in the vessel. And then you figure out, then you calculate the embrittlement, I'm sorry, the RTNDT irradiated at the maximum fluence using the plate properties and the circumferential weld properties and again, pick the highest, because it's going to be controlled by the least material. And then for the plates, again, just calculate the RTNDT at the max fluence because the plate is always going to hit that and pick the maximum value. And that's really all this slide says, so I'll bypass the math, but the math is a representation of how we do what I just said.

And in doing that, we can now look at the results from the probabilistic fraction mechanics calculations. On the vertical axis over on the left-hand side, we've got through-wall cracking frequency associated, a through-wall cracking frequency caused by the axial weld flaw population plotted versus the reference temperature for the axial weld. In the middle graph, we've got the through-wall cracking frequency for the plate generated by the flaws in the plates plotted versus the reference temperature for the plates. And on the third graph, the through-wall cracking frequency associated with the flaws in the circumferential welds plotted versus the reference temperature for the circumferential welds.

And one thing I would note in passing is that all of these reference temperatures can be computed based on information that's available and docketed by each of the plants. So we're not requiring anything new of the licensees here, other than perhaps more elaborate calculation.

The take-away point from this graph is that the through-wall cracking frequency from each of these weld populations is reasonably consistent from plant to plant for reasons that I'll go into in just a minute. The other thing to take way is that the axial weld flaws are dominating the through-wall cracking frequency, simply because they tend to be the largest and they're axial-oriented. Then the plate flaws contribute the next portion, but that's at an equivalent level of embrittlement. That's 100 times lower, and that's because the plate flaws are smaller than the weld flaws.

And then third and almost negligible contribution to the through-wall cracking frequency is made by the circumferential weld flaws, not because they're small but simply because they're circumferentially-oriented. So we'll get back to these graphs in a little bit because it's on the basis of these lines fit through our results that we derive a through-wall, I'm sorry, that we derive reference temperature limits that are consistent with the through-wall cracking frequency limit of 1 times 10 to the minus 6 events per year.

But before I go there, just a couple of slides on why the through-wall cracking frequencies for, you know, three, what I think people would regard as being different plants made by different manufacturers with different materials in them and so on and so on and so on show such remarkably consistent behavior in the frequency of through-wall cracking. And that's in large part due to the fact that the transients and the transient classes that are controlling, that are contributing the most to the through-wall cracking frequency are pretty consistent from plant to plant, and that point is made on this slide and on the next slide.

First, we observe that it's primary side failures that dominate risk. Seventy-five percent of the risks of the through-wall cracking frequency or more in all of these plants comes from either the medium to large diameter pipe breaks on the primary side or stuck open valves on the primary side, and then they reclose later. You can notice from looking at the graphs that there's a crossover in these two that, at lower levels of embrittlement, it's the primary side. It's the stuck-open valves on the primary side that may later reclose the control because, at the lower levels of embrittlement you need that re-pressurization in order to derive the crack through the wall, whereas when you crank up the level of embrittlement and get out the very high levels of embrittlement approaching the 1 times 10 to the minus 6 limit, then you find that the pipe breaks are dominating because, in that situation, I'm sorry, at those high levels of embrittlement, pretty much getting a crack going is all you need and it will go through the wall.

The other thing to note from this is, unlike in the previous analyses of pressurized thermal shock, the main steamline break and, indeed, all secondary side events are making a much smaller contribution relative to the primary side breaks. And the reason for that is simply that, in a secondary side break, the minimum temperature in the primary can't go below the boiling point of water at the pressure of the break location. And that keeps the toughness of the material high enough to, in large part, resist frequent crack initiations and through-wall cracking.

The other thing I'd note, which is indicated by the parenthetical comment under the main steamline break, is that there, and I won't go into details here unless asked, but there are various conservatisms in our model of main steamline breaks, the most prominent of which is the most severe steamline breaks are breaks which occur inside containment. However, even for a break inside containment, we've modeled the minimum temperature as being 212 degrees Fahrenheit, which is to say we don't account for the beneficial effect of the break inside containment, pressurizing containment and delaying the boiling point of water to something like 450 or 460 degrees Fahrenheit.

So that's a 30 or 40-degree conservatism that, if we were to include it, would drive the through-wall cracking frequency contribution to main steamline breaks --

MR. POWERS: You mean 240 or 260?

MR. ERICKSONKIRK: Yes, I'm sorry. I'm sorry, yes. Would drive the main steamline break contribution even lower than we're showing.

MR. POWERS: And the motivation for not including this?

MR. ERICKSONKIRK: I'm not sure I could speak to why we excluded it originally. Maybe Dave can?

MR. POWERS: Well, we would have had to have added some type of containment model, coupled with RELAP. Basically, that would simplify the analysis.

CHAIRMAN BONACA: That raises a number of issues then regarding the steamline break during the subcommittee meeting that I think they're going to look at. The first one is the generalization. I know, for one, the behavior of the steamline break in a B&W plant is critically different from the one, just simply because of the huge inventory of water in the steam generators of those plants, and B&W not having any steam generator inventory. So, therefore, you have a much more rapid cool-down. I was told that this cool-down that they assumed anyway bounced that valve, so I think the cool-down rate for the B&W plant is much more severe.

The other issue that I just brought up was the concern that it was a steamline break is because you have cool-down and then you have the re-pressurization of the plant. And it was assumed, after TMI, no credit for the operator to shut off a high-pressure injection. And the reason is that they have no symptom-rated procedures. It was 1980. We were very concerned about the operator simply not taking action. But I was told during the presentation that re-pressurization doesn't count, doesn't matter anymore. So those issues I just brought up, and I think it's good we have it in the record.

MR. ERICKSONKIRK: Yes. I think we certainly intend to respond to your comments with better explanation and documentation. Since you brought it up again, I did want to throw this up. These are, on the top, temperature transients, and the bottom, pressure transients for main steamline breaks at hot-zero power, and the numbers are just the transient numbers in Oconee, Beaver, and Palisades.

And the thing I wanted to point is that the smaller inventory in Oconee relative to Beaver and Palisades indeed does lead to a very rapid initial cooling rate, which you almost can't see here because it's right at the tip of my cursor. So, indeed, Oconee does cool off very fast initially, but because the steam generator boils dry so very, very fast, it can't cool down the primary anymore. So, in fact, the cool-down rate in Oconee is much more gradual over the long haul than in Beaver and Palisades, and that's consistent --

CHAIRMAN BONACA: Actually, because you assume isolation of the water.

MR. ERICKSONKIRK: That's right.

CHAIRMAN BONACA: Well, okay. The original calculation didn't have that.

MR. ERICKSONKIRK: Yes.

CHAIRMAN BONACA: Okay. And what is the second, the pressure of what, primary system pressure --

MR. ERICKSONKIRK: Primary system pressure.

MR. SHACK: It might also be useful to

plot that graph as a conditional probability of failure, as well as a through-wall, so you'd know how much was due to the fracture mechanics and how much was due to the frequency of the events.

MR. ERICKSONKIRK: Yes. In order, the graph, though, I think the graph you're referring to is this one. You can only do -- and I've gotten myself caught in this before, so I'm not going there again; bad experience. You need to find -- I think we can do that, and that would be a useful comparison. But you need to do head-to-head comparisons of individual transients to compare conditional probabilities. You can't add up all the conditional probabilities from various transients without waiting by the frequencies, or the PRA people start to throw things at me, and I don't like that.

So, anyway, you're absolutely right. There are differences between the two plants that we need to --

CHAIRMAN BONACA: Most of all, I mean, I think, as I suggested the day before yesterday, it's good in the report to have historical perspective.

MR. ERICKSONKIRK: Yes.

CHAIRMAN BONACA: You know, isolation of water was unassumed. So you had a cool-down and as rapid as the beginning to the end.

MR. ERICKSONKIRK: All the way to the end.

CHAIRMAN BONACA: And so that's the questions that Tom Burley has asked.

MR. ERICKSONKIRK: Yes.

CHAIRMAN BONACA: He was thinking out of memory as I did.

MR. ERICKSONKIRK: And that would certainly be more severe, and that illustrates the point very well, so we'll include that.

Okay. We already made, or I already made in the last slide the first point here regarding the transients controlling failure to, generally, the secondary side breaks are much less damaging than primary side simply because you can't drive the temperature in the primary anywhere near as cold as you can when you have a primary side break. The other point to make is that, while we've made what we feel to be reasonable and appropriate credits for operator action, in the end, when you look at the transients that are dominating, that are making the largest contribution to the through-wall cracking frequency, operator action credits have relatively small influence on those results.

For example, a pipe breaks on the primary and no operator action credits whatsoever. And for the stuck-open valves on the primary, while we did make operator action credits, the operator has to act very rapidly and then can only prevent re-pressurization under hot-zero power conditions. So the net effect on the transients that contribute to the through-wall cracking frequency is, again, small. And as we said, operator actions influence main steamline break, but they're just not severe enough to count.

So this is a really short summary of why we believe our findings can be applied without too many reservations to PWRs in general, the first being a point I made that transients that contribute most of through-wall cracking frequency have approximate equal occurrence rate and approximately equal severity across plants. Operator actions don't count for much.

Other factors that contribute are the PWRs that we're regulating have very similar designs, similar operating pressures, similar vessel thicknesses, and so on. Our sensitivity studies have shown that the calculational models we use are robust to credible changes in the sub-models and parameter inputs subject to some reservations, which may be discussed later, hopefully when I'm not up here.

We have many conservatisms left in the model, and I'll provide a list of those. We do have two equivocations that we put forward regarding the general applicability of these results, and one is with regard to forgings that are prone to sub-clad cracking. If those were taken to very high embrittlement levels, we would suggest that the licensee or interested parties would be well advised to do a more detailed analyses than we've done here.

And, also, our analyses have been performed on vessels that are in the range of eight to nine inches thick. And that's as thick or thicker than all but three vessels in the PWR fleet. As you go up to thicker vessels, you get a systematic increase in through-wall cracking frequency. Fortunately, the three very thick vessels are the Paulo Verde vessels, and they all have fairly low levels of the radiation embrittlement. So in principle, there's a limitation there, but, because the Paulo Verde vessels aren't very embrittled, I don't think it's a practical one.

Again, more formulas. The formulas shown on this view-graph are simply the equations that were fit to the through-wall cracking frequency results that I showed before. And so we're proposing an estimation formula, if you will, that says the total through-wall cracking frequency is equal to the sum of the through-wall cracking frequency produced by the axial welds, by the plates, and by the circumferential welds.

And then we can use this formula together with the 1 times 10 to the minus 6 limit on through-wall cracking frequency to derive combinations of these various reference temperatures that are either above or below the 1 times 10 to the minus 6 limit. So, for example, if we want to derive the acceptable limits on reference temperatures for a plate-welded plant, we already said that the circ weld contribution is very small, so, for purposes of illustration, you can just set that to zero.

And that leaves us with two variables in the equation: reference temperature for the axial weld and reference temperature for the plate. Set the total through-wall cracking frequency to your proposed limit of 1 times 10 to the minus 6, and then just simply set it up in a spreadsheet and plug in values for reference temperature axial weld and calculate what the value is for reference temperature plate to get you to 1 times 10 to the minus 6 total. And if you do that again and again and again, you trace out failure low sides that look like this, which, as one of my colleagues in NRR pointed out, is effectively a box with the corner cut off.

Be that as it may, these are now low side of constant through-wall cracking frequency. And we've highlighted the low side associated with the 1 times 10 to the minus 6 limit in red, so that would, effectively, become the proposal for your new screening limit. So that's to say that for plate-welded plants, reference temperature of the axial welds has to be below 210 degrees, reference temperature for plates needs to be below, I think that's like 475 degrees.

For forging plants, since they don't have axial welds, you don't need to worry about that. Reference temperature for the circ weld is 460, which is too high to matter to anyone. And, again, reference temperature for the forging is the same as reference temperature for the plate, and that's about 375.

So then the question, of course, comes up, well, where are the plants that are operating today relative to that limit? So we use the information that's available in ARVID to calculate these various reference temperatures for all the PWRs that are currently in operation. This shows the results of that assessment at end of license or 40 calendar years, and, as you can see, the plate-welded plants are, generally, a bit worse off than the forging plants but none of them have a failure frequency above 1 times 10 to the minus 7 events per year, and none of them are within even, I think, 60 degrees Fahrenheit of the screening criteria, screening limit. I'm trying not to use the word criteria.

If you go up, if you crank up fluence to EOLE and, of course, in doing that, you have to assume constant fuel loading, the plants all move 10 to 20 degrees Fahrenheit closer to the screening limit. The histogram here shows the estimated through-wall cracking frequency for the population of all the PWRs made out of rolled plates and all the PWRs made out of forgings out to 32 effective full-power years or EOL. And you see that, by and large, certainly, the mean of the distribution is very far from 1 times 10 to the minus 6 limit. And even when you go the upper bounds, the plate vessels are more than an order of magnitude away, and the forge vessels are like four orders of magnitude away.

So now the question arises should someone, could someone take the limits that are proposed and just use them straight out, or should you add some sort of a margin term to it? And that's obviously a judgment that doesn't get made by me, although, like everybody else, I'm entitled to my opinion. But I would propose and, indeed, one of our reviewers, Dr. Murley, proposed that an appropriate way to make that judgment would be to sit and get a piece of paper out and write down all of the residual conservatisms and all the residual non-conservatisms that have been left in the analysis.

And we've taken a cut at doing that. We tried to make it comprehensive. There are probably things that we missed here, but I think these are the type of factors that people need to keep in mind when saying, you know, do I need to apply a margin to this. Now, some of these are unequivocally conservative or unequivocally non-conservative. For example, we clearly have over-represented the contribution of main steamline break because we've modeled the minimum temperature as being too cold. Having said that, main steamline break doesn't matter too much anyway.

We've unquestionably overestimated the plant-specific variability in copper, nickel, phosphorous, and fracture toughness relative to any plant-specific analysis. Other things are more subject to judgment, but I think this is an appropriate approach to look at this.

MR. SHACK: Would the neutron attenuation function be a big player? Somehow, it seemed to me it would.

MR. ERICKSONKIRK: I think so, but, and I've got to say the but, is that neutron attenuation is going to be a much bigger player in the heat-up and cool-down limits than it is here because, if you remember the plot that I had yesterday, it showed that everything that's contributing to the through-wall cracking frequency is within one inch of the inner-diameter of the vessel wall. So, I mean, the further, the deeper you go into the vessel wall, the more the attenuation function you use counts, whereas you're just not attenuating all that much in the first inch.

MR. SHACK: I was hoping it would give you more crack arrest.

MR. ERICKSONKIRK: Well, no, no. I'm sorry. You're absolutely right. You're absolutely right.

MR. SHACK: It wouldn't do anything for initiation.

MR. ERICKSONKIRK: It wouldn't do anything for initiation. It would do something for crack arrest. You're absolutely --

MR. ROSEN: How about if a plant started using a lot of MOX fuel? What would that do to this? Would that have an effect?

MR. ERICKSONKIRK: And that would increase the -- yes.

MR. ROSEN: -- towards a harder spectrum or --

MR. ERICKSONKIRK: I think what would, I don't think it would change the limits in particular. What it would change is the rate at which you're approaching the limits. It would change how -- I mean, if a plant decided to change to MOX fuel and it had a failure point that was moving out, you know, sort of on that slope, it might change to a higher slope and approach the limits faster.

MR. POWERS: Let me point out that most of the plants that use MOX don't have it out on the outer perimeter, so it's basically shielded, so it really doesn't see the harder spectrum at all.

MR. ROSEN: So the wall wouldn't see it?

MR. POWERS: It doesn't really see it.

MR. ROSEN: Pardon me?

MR. POWERS: It doesn't really see it. I mean, essentially, when we looked at the LTAs, it was just --

MR. RANSOM: How about power-up?

MR. ERICKSONKIRK: Well, again, I think anything you do to change the, anything you do to change the fluence, change the rate of which you're damaging the vessel is going to manifest itself not in a change in these failure loci, but it's going to change how fast a particular plant is getting there.

MR. RANSOM: I thought the general conclusion was that this stuff didn't seem to be a problem, and I think we've heard that as far as power-up is concerned, too. And I'm just wondering is that a problem?

MR. ERICKSONKIRK: I think yes, it would depend upon the plant. And that's something you're going to find out because you're doing surveillance. I guess the other point I want to -

MR. POWERS: Do these plants have coupons for doing surveillance on a regular basis?

MR. ERICKSONKIRK: Bruce can probably answer that better than I, but when somebody goes for a license extension, in a lot of cases don't they put in extended surveillance?

MR. BISHOP: Yes, yes, you have to.

MR. ERICKSONKIRK: You have to.

MR. BISHOP: You have to. The comment about the power uprights, if you did power uprights very early in life, it could have a big effect. But because the fluence effects tend to saturate once you get above a certain level, power uprights later in life have much less effect.

MR. ERICKSONKIRK: I guess the other point that I wanted to bring out, just in terms of people thinking about margins because I'm sure we're going to be thinking about what to do with this for some time, is that, you know, effectively, what you're doing when you're putting a margin on these curves is you're saying that my acceptable limit is not 1 times 10 to the minus 6, it's something lower. So there is a rough equivalence there, and if we've spent all this time on establishing what an appropriate limit is, is it then appropriate to apply a margin to that, or perhaps we should just think that we need a more restrictive limit.

Because that is, indeed, what you're doing. I mean, I counted it out. Every 80 degrees Fahrenheit of margin or whatever you want to call it that you move, you're knocking off two orders of magnitude. No, I'm sorry, one order of magnitude is 80 degrees of Fahrenheit on RTAW. Go ahead, I'm sorry.

MR. DENNING: I think I look at it differently, which is that instead of coming up with the best estimate value and putting uncertainties on it, you've built in conservatisms into your model.

MR. ERICKSONKIRK: Yes.

MR. DENNING: And so I think the question of margins is a question of do we really believe that the conservatisms that you've built in adequately account for the uncertainties. The margins that you put in, do they adequately account for that? So I don't think anybody is going to argue, particularly 1 times 10 to the minus 6. I think it's a question of have you really built in the conservatisms.

MR. ERICKSONKIRK: That was the end of my prepared remarks. If there aren't any further questions on this, we can move to the final comments from the --

MR. RANSOM: A quick question. On your, I think, third slide that material factor is controlling vessel failure. You have three curves on that.

MR. ERICKSONKIRK: Yes.

MR. RANSOM: With this comment to reference temperature characterizes all of the toughness properties of interest, I don't recall you talking about that the other day.

MR. ERICKSONKIRK: Well, what that says is, and I guess I might have given that the short shrift, is that the reference temperature, once you establish the reference temperature for the cleavage crack toughness initiation curve, all of the other reference temperatures, if you will, where the arrest fracture toughness curve is, where you hook on the ductal upper shelf fracture toughness curve, they're all linked. They all can be calculated from that as a unique function.

MR. RANSOM: Do they play any role?

MR. ERICKSONKIRK: To those other curves? Oh, absolutely, because, well, the arrest toughness curve is what we use as we propagate. Once the crack initiates, then we need to decide has it arrested, has it stopped. So that plays a very big role in whether the crack gets all the way through or not. And then --

MR. RANSOM: I thought this reference temperature was used to just as a parameter for through-wall cracking frequency.

MR. ERICKSONKIRK: It's both. It's something that characterizes the position. For example, if you were doing experiments in the laboratory, you could figure out by conducting cleavage crack initiation test where your reference temperature is to characterize that. And then if you were to do subsequently arrest fracture toughness test and a ductal upper shelf fracture toughness test, you could estimate where those test data would lie based on the knowledge of this. But then you also use it on, it's a convenient parameter to use on the back-end simply because it does characterize all of the fracture toughness values that are what's stopping the fractures.

MR. RANSOM: Well, do these other toughness factors come into play in those plots that you make up for the three different kinds of transients?

MR. ERICKSONKIRK: Yes, yes. It's all in there.

MR. RANSOM: They play a role?

MR. ERICKSONKIRK: Yes. Because the through-wall cracking, remember the through-wall cracking frequency is the integration of how all these toughness values are acting to resist the applied loading. Any other questions? Okay, then we'll go through the peer review comments and the usual and customary PRA thermal hydraulics PFM order, if that's okay.

So we'll start with PRA. And Donnie Whitehead will make that presentation.

MR. WHITEHEAD: My name is Donnie Whitehead, and I will talk about the one additional new comment that we received from the peer review group. Dr. Murley provided --

MR. APOSTOLAKIS: I'm sorry, who were the peer reviewers?

MR. WHITEHEAD: The peer reviewers? I know Dr. Murley was one. There's a list. I can't recall all of their names.

MR. HISER: This is Alan Hiser from research. Dr. Catton from GRS Germany, Eric VonWalle from SEKC in Belgium.

MR. APOSTOLAKIS: Who was the PRA expert reviewer?

MR. HISER: David Johnson from ABS Consulting.

MR. APOSTOLAKIS: ABS.

MR. HISER: Thermohydraulics was Ivan Catton and Cumard Brohotki.

MR. WHITEHEAD: Dr. Murley had a comment dealing with the assumption that was made for the SRV opening size being uniformly distributed, and he said that he believed that that was intuitively incorrect. Our initial response is to agree with the comment that he's made and that we probably shouldn't have made this assumption.

What we're going to do is we're looking in and investigating to see what kind of effect that would have on the overall results. It's not expected to have, you know, too big of an impact on the results, but, at this point in time, we've got the comments like the day before yesterday, and we just simply haven't had enough time to determine its, you know, total impact on the results but --

MR. POWERS: Donnie, is it the wings that are the ends of the distribution that will have an effect, or is it the mean?

MR. WHITEHEAD: The value that we used for this SRV opening was just simply a fraction, so there's no real, we did not sample any uncertainty associated with this. So it would be a matter of just simply taking out that particular basic event from all of the cut sets that we calculated.

MR. POWERS: I mean, typically, any high entropy distribution, if it's the wings that are doing it for you, it didn't matter which one you pick.

MR. WHITEHEAD: And we don't really expect a substantial change from this, but we just simply haven't had the opportunity to --

MR. POWERS: What's holding you up?

MR. WHITEHEAD: I guess I'm just slow.

MR. POWERS: You're just slow, I guess.

MR. WHITEHEAD: And that's actually the only additional comment that we had.

MR. APOSTOLAKIS: That's it? That's all Dave Johnson said? He didn't say anything.

MR. ERICKSONKIRK: That was the only negative comment. Dr. Johnson commented that he thought that our modeling of stuck-open valves in the primary that later re-closes is grossly conservative.

MR. APOSTOLAKIS: So he disagreed with Murley.

MR. ERICKSONKIRK: Yes. Dr. Johnson commented that he felt that if a valve was going to reclose, it was likely to reclose very early in the transient rather than later, which would lead to a much lower through-wall cracking frequency than is incorporated in our models. So yes, he disagreed with Murley on that point.

MR. BESSETTE: I'm going to try to briefly review the main issues that were discussed with respect to the thermohydraulic analysis. These come under the general heading of the three boundary condition parameters that we provide to the fracture mechanics analysis, namely pressure, temperature, and heat-transfer coefficient.

One of the comments that most important parameters that were evaluated in the uncertainty analysis relate to system boundary conditions rather than physical phenomena modeled by RELAP itself. And examples of boundary conditions of break size, break location, ETCS flow, ETCS temperature, and those sort of things.

MR. RANSOM: Is that a negative comment?

MR. BESSETTE: It wasn't negative. It was like is this really factually correct or, you know, how can it be that boundary conditions play such an important role? It's like an observation or something. It's, in a sense, can you show me why this is so.

We did all the analyses, but, basically, the all the analysis for RELAP5, which is one-dimensional thermohydraulic code. And our questions with respect to fluid temperature or thermal stratification and mixing in the cold leg and downcomer and also questions with respect to the treatment of convective heat transfer in a downcomer in the fluid to the wall.

And this shows an example of, tries to show an example of why the boundary conditions tend to dominate the analysis. These are the five transients that represent the small break LOCA for Palisades, and you can see that, basically, within the regime of small-break LOCA, you get a very wide range of behavior. This is temperature on the left and pressure on the right. You can see variations of 100 degrees K or more for a class of transient called small break LOCA.

And the reason for that, of course, is that the system in-flow and out-flow, the break flow, and ECCS flow dominate the parameters of temperature and pressure to the system and overwhelm other effects. These are the issues, main issues with respect to the treatment of the adequacy of RELAP for PTS analysis. This prediction of downcomer temperature and pressure. The question of are there substantial non-uniformities in downcomer temperature that are not captured by RELAP? Like I said, the wall heat transfer.

So to address these, we performed substantial PTS-specific assessment, and we used the best available integral system test data that was available to us from past programs. This included UPTF, LOFT, ROSA, APEX, and MIST. And MIST was a facility that was configured according to B&W design. The other facilities were all scaled according to CE Westinghouse-type reactors.

From these five facilities, we selected 12 experiments that represented the same types of sequences that show up as risk-dominant PTS transients. And using these 12 experiments, we assessed the code for downcomer temperature and system pressure and performed statistical comparison between RELAP and experimental data, and we found that, overall, RELAP predictions were within four degrees K of the total body of experimental data.

MR. DENNING: Excuse me. I really have to object to that 4 K and what its meaning is. The way you've taken differences between downcomer temperatures for these, some of them much greater than 4 K positive, a lot of them much less than 4 K negative, average them together, that just doesn't make sense. That's not a characteristic of the accuracy. Your second one, the 11 K, is certainly much closer to a true characterization, but the way the 4 K is done, it's just nonsensical to think that that represents a measure of the accuracy with which

RELAP has predicted the behavior of those facilities.

MR. BESSETTE: Well, certainly, standard deviation captures what you're talking about. If you prefer using absolute value, 4 K becomes 8 K, 7 or 8 K. I personally think that 4 K, using arithmetic average is appropriate because you're interested in what boundary condition is being fed to FAVOR. And, it's true that, sometimes, during any given transient, RELAP may be sometimes over-predicting temperature and under-predicting temperature. That total behavior is captured by standard deviation, but you're also interested in absolute accuracy.

MR. DENNING: That didn't help my comment, I don't think.

MR. BESSETTE: I do. I certainly listened to your comment yesterday. Like I say, if for anyone who prefers absolute value, it comes out to about 7 K instead of 4 K.

And it's the same thing, the comment applies to system pressure. And we come up with an average. You might call this the average deviation is like a bias, an average bias between RELAP and the data. And that's about 9 psi for pressure for the standard deviation of about 60 psi.

MR. RANSOM: Well, is that comment

relative to the transient behavior with time?

MR. BESSETTE: Yes, this is over the duration of the entire transient, entire experiment.

MR. RANSOM: And as I understand it, probably the more important thing is whether or not the rate of change of temperature is correct.

MR. BESSETTE: Well, you want to know that your rate of change is correct, certainly, yes. You're interested to know if your -- see, the vessel time constant as a whole is on the order of a thousand seconds or more. So if your fluid temperature is roughly accurate over that kind of a time constant, then that's one figure of merit.

You also have to be concerned about shorter times, like in the order of ten or tens of seconds because the short thermal variations do effect the near-surface temperature of the vessel, which is where the flaws are that cause the vessel failure.

MR. RANSOM: Well, did you provide these two papers, the one by Ivan Catton on the impact of heat-transfer coefficient and the other one on the, more or less, the mixing in the downcomer? They seem to support what you were saying yesterday that these results are somewhat insensitive to heat-transfer coefficient and also the things are relatively well mixed, I guess.

MR. DENNING: I didn't think that the Catton paper really provided much evidence that his heat-transfer correlation would be applicable here because he certainly was dealing with an established plume. And from everything we've been hearing, it's really well mixed and not an established plume.

MR. BESSETTE: Well, Catton, you know, Catton wasn't really dealing with plumes. He was dealing with average behavior. I mean, he wanted to know how much mixed convection would enhance heat transfer.

MR. DENNING: But if you look at his theoretical development, he's got a plume that moves down, well defined, and he looks at what happens at the interfaces of that plume.

MR. RANSOM: Well, one point was that he was arguing, you know, modeling the natural convection that occurs with the cold water/warm water, and that that resulted in a well-mixed situation apparently or enhanced mixing.

MR. SHACK: Yes. What I took away was he got a heat-transfer coefficient that's about three and a half times Dittus Boelter.

MR. BESSETTE: Yes.

MR. SHACK: And that's a little bit larger than yours.

MR. BESSETTE: No, well, you know, he compared his to Dittus Boelter, had low-flow conditions, Churchill-Chu was invoked was in RELAP, and Churchill-Chu is, the reason it's invoked is because the higher value in Dittus Boelter. So if you compare Catton to Churchill-Chu, you don't get this three and a half times.

MR. SHACK: What do I get?

MR. BESSETTE: Twenty percent. Well, I should say, overall, it's 20 percent. It can be more than that or less. It can be up to twice as much as Churchill-Chu.

MR. WALLIS: Okay. But it looks more like the sensitivity results you were showing us yesterday.

MR. BESSETTE: Yes.

MR. WALLIS: So there is a basis for those sensitivity results?

MR. BESSETTE: I didn't make them up. But at any rate, so you're dealing, basically, during these transients, you're dealing with overall temperature changes of about 200 or 250 K during the course of the transient. So RELAP has to track an experiment which starts off at 550 F and ends up at about 100 to 150 F. And so you can see that it's tracking this behavior pretty well.

And then the final point is we looked at the available experimental data with respect to temperature stratification, non-uniformities, you know, particular plumes in a downcomer. And we looked at the integral system test data and went back and looked at the separate effects tests that were done during the 1980s, and we find plumes to be either very weak or essentially non-existent. I mean, the definition of weak or non-existent is about 5 to 20 K.

All the experiments show substantial stratification in the cold leg due to ECC injection. I'm just going to show one example, and that's from APEX. Typically, you see stratification in the cold leg of about, oh, in this case, anywhere from 50 to 100 degrees K, and you can see for this experiment the stratification is very nearly the difference between the initial system temperature and the ECC temperature. So you get most of the mixing that occurs is not occurring in the cold leg. You get some cold leg mixing, but that's, you do get substantial stratification in the cold leg. And, of course, RELAP cannot account for this kind of behavior.

But then we turn to the data from the

downcomer. This is the same experiment from APEX. And you can see, like, taking this at random. We ran about 20 different experiments in APEX in support of the PTS program, where we tried to run as a variety of PTS-type transients, and this is one of the experiments.

The other experiments saw the same behavior. On the left is the measurements at, this is axial variation in downcomer temperature from just underneath the cold leg, at one point three diameters down, to eight diameters, it's around the top or mid-plane of the core. So there's no evidence of axial variation. And the RELAP calculation for this experiment is on the right.

MR. WALLIS: Why does it get that zero? When it comes in with this tremendous stratification? It all disappears at zero? What's that mean? The bottom of the cold leg? What does zero mean there? In the caption down below, it says fluid temperatures at zero.

MR. BESSETTE: Oh, okay. I think the one at zero is in between cold legs.

MR. WALLIS: It says, I think what it means is the below each cold leg centerline, isn't it?

MR. BESSETTE: I thought it was measuring distance down --

MR. WALLIS: Below cold leg centerline, isn't it?

MR. BESSETTE: Bill, you want to say something?

MR. ARCIERI: Yes, this is Bill Arcieri from ISI. I think zero is just basically at the cold leg, and then you go 1.3 diameters down, and then 8 cold leg diameters down. I believe that's the case.

MR. WALLIS: -- cold water is coming out of the cold leg.

MR. BESSETTE: I think this is in between cold legs, though, not right in front, but I can't remember for sure.

MR. WALLIS: Because it can't instantly change its temperature.

MR. BESSETTE: No, I agree with that. We'll check on that. From my recollection, it's not in front of that cold leg, but it's alongside of it. It's just cold water coming in, it won't see that cold water. And this is azimuthal variation looking down the downcomer for that same experiment.

MR. WALLIS: On the outside wall.

MR. BESSETTE: Kind of in the mid-plane of the gap.

MR. WALLIS: Well, then they're in the gap.

MR. BESSETTE: They're in the gap. And so on the left is the overall --

MR. WALLIS: But if the plume were on the wall and were not very thick, you wouldn't see it at all?

MR. BESSETTE: I can't imagine a plume running down the wall all the way down.

MR. WALLIS: I don't know what you can imagine. It's dangerous to imagine.

MR. BESSETTE: Yes. It's only a two-inch gap.

MR. POWERS: I am shocked.

MR. BESSETTE: As you can see, we start out just at the initial condition, and we end up down, this experiment runs from initial condition down to the final injection temperature. And on the right-hand side is a blow-up of this from 800 seconds to 1600 seconds.

I'm going to turn to heat-transfer coefficient. In the PTS transients, the downcomer heat-transfer mode is predominantly what I would call a buoyancy-opposed mixed convection, which means you have a heated wall - in this case, you have heated walls on both sides with a colder fluid running down the middle.

Now, RELAP in this situation applies the maximum of Dittus Boelter, which is a turbulent force convection, and Churchill-Chu, which is pre-convection. And like I say, for low-flow conditions, low-velocity conditions, Churchill-Chu gives higher values of heat transfer than Dittus Boelter.

Now, going back to the original IPTS study, Catton has been very interested in the subject, and he had a program supported by EPRI back in the mid- to late-80s, where he ran experiments on this type of geometry.

MR. SHACK: It's Krillov now. It was somebody else yesterday.

MR. WALLIS: It was a Pole yesterday, now it's a Russian.

MR. BESSETTE: Yes, that reflects kind of what happened. See, when Catton did his work, he compared it to Petukhov Krillov, and when we implemented this into RELAP, we used Petukhov Yulinsky because it's very similar, except that Yulinsky extends the correlation down to lower values of Reynolds Number.

MR. SIEBER: You're making this all up,

aren't you?

MR. WALLIS: I'm very puzzled. There's a friction factor, which you have to calculate somehow in this Petukhov correlation. Where does that come from?

MR. BESSETTE: The calculation of friction by RELAP? It's done by RELAP. You've exhausted my knowledge at that point as to how it's implemented.

MR. WALLIS: Well, I think what you have to do is show us some data from real downcomers, show that these correlations have some relationship to reality.

MR. BESSETTE: Well, certainly. I mean, Ivan compared this to his own data. You know, he - based on this correlation, he applies a correction factor for low-flow conditions.

Now, this is a comparison of the base-case RELAP modeling with what I'll call Petukhov-Catton, and that red being the base-case RELAP and the green being Petukhov-Catton. So you can see I picked four dominant transients from the 12 such cases from Palisades. I'm showing here, for hot leg break, 16-inch hot leg break, our main steamline break. And you can see that Petukhov-Catton does consistently flow above RELAP.

MR. WALLIS: Theory versus theory.

MR. BESSETTE: Which is, you know, what you'd expect. And this is four-inch surge-line break and a stuck-open pressurized SRV.

MR. WALLIS: Are you serious that APEX didn't measure heat-transfer coefficient?

MR. BESSETTE: They measured, they had thermocouples in the wall, but they couldn't get real precise numbers.

MR. WALLIS: But they got something.

MR. BESSETTE: They got something, and it looked --

MR. WALLIS: Did it compare with these, or was it off-scale somewhere?

MR. BESSETTE: As I recall, they compared it against Dittus Boelter, and they got reasonable results.

MR. WALLIS: I think that would be useful evidence.

MR. BESSETTE: Yes.

MR. HISER: It might even turn us into believers.

MR. BESSETTE: If the meeting was tomorrow, I could have dug that out.

MR. SIEBER: Well, we're here tomorrow.

MR. WALLIS: Maybe your final report can compare these theories with data?

MR. BESSETTE: Yes.

MR. WALLIS: Then we might be believers.

MR. BESSETTE: Yes. So you can see the results are similar, generally speaking. Petukhov-Catton is a little bit higher and, overall, for the 12 Palisades transients, it's about 20 percent higher.

MR. WALLIS: So what does this do to the through-wall cracking?

MR. BESSETTE: It's down here. We ran all the 12 cases, both Palisades cases, through FAVOR, and we came up with a factor of three increase in --

MR. WALLIS: That's between the green and the red curve, your factor of three?

MR. BESSETTE: Yes.

MR. WALLIS: But they look fairly close. Well, how can that change the heat-transfer coefficient by 20 percent and create a factor of three increase in the CPF?

MR. BESSETTE: Well, I'll try to show you that.

MR. WALLIS: It looks as if it's important to know the heat-transfer coefficient pretty well.

MR. BESSETTE: Yes. Well, it's not

negligible, the effects are not negligible. Now, I have to remind you, when I say it's a factor of three in CPF, that's what I mean. It's not a factor of three in risk. We didn't go as far as to carry this through the --

MR. WALLIS: Does that mean that if these curves were wrong by a much bigger amount you might get a factor of ten, say, in CPF?

MR. BESSETTE: It's probably more likely they could be less. And we didn't multiply these by frequency of the transient, so we didn't carry this as far as to actually know how much the risk number changed.

MR. SIEBER: The heat-transfer coefficient, I would hope, is not a function of what causes the transient.

MR. BESSETTE: Could you say that again?

MR. SIEBER: Well, the thing you want to multiply should have nothing to do with heat-transfer coefficient. You want to multiply it by the frequency of the transient.

MR. BESSETTE: That's right, yes.

MR. SIEBER: I can't imagine the frequency of the transient effecting the heat transfer.

MR. BESSETTE: No, but the idea is that

not all these transients have equal frequency. And this factor of three may come from a low frequency or a low -- we're only comparing CPFs, and we went pretty far down, so some of these CPF --

MR. SIEBER: Well, for Palisades, you say you multiplied it by the risk-dominant transients.

MR. BESSETTE: Yes.

MR. SIEBER: Okay. And if that gave you a factor of three, your other transients, I don't care what they do, they're not going to effect that.

MR. BESSETTE: No, what I mean is that you could have a transient with a CPF of 10 to the minus 9 that increased a 10 to the minus 8. It's still a miniscule number, but it's now changed by a factor of ten. So in order to get the risk, you have to sum up the things that are down here with things that are up here, you multiply it by the frequency --

MR. WALLIS: But you say transients, plural, so I'm assuming that they change by a factor of --

MR. BESSETTE: No, this is a total number.

MR. WALLIS: The risk-dominant one.

MR. BESSETTE: Yes, but not all risk-dominant ones are equal.

MR. WALLIS: Well --

MR. POWERS: Now, that's a concept I want to explore a little bit.

MR. BESSETTE: Well, if you look at transients, for example, one might contribute 60 percent of the total, one might be 20 percent, one might be one percent. We went down to about a tenth of one percent here.

MR. POWERS: Well, 20 percent is about the same number in PRA space. One percent, I'll agree, is different than 20 percent.

MR. BESSETTE: Yes. At any rate, so this is an indicator but not exactly a bottom line.

MR. POWERS: It's not a 20 percent for heat-transfer coefficient, it's a factor of five between what they calculate and what reality is.

MR. BESSETTE: It might be; I don't know.

MR. POWERS: Well, you don't know.

MR. WALLIS: So the 20 percent change in heat-transfer coefficient is quite easy to get because you have uncertainty in which correlation to use. You have this leverage of a factor of three on the wall.

MR. BESSETTE: Yes.

MR. WALLIS: And it could quite easily be a factor of 50 percent change or 50 percent error.

MR. BESSETTE: This shows you the effect

of varying the heat-transfer coefficient, but we --

MR. POWERS: It doesn't show it on the bottom line.

MR. BESSETTE: No, I know. But to give you an idea how bad can things be, we --

MR. WALLIS: Heat-transfer coefficient.

MR. BESSETTE: This, roughly, corresponds to something like about a one and a half-inch break to about a two and a half-inch break, so it's in the small-break region. On the right, we take the transient, we take this transient corresponding to this one here, where the decay constant is 30 minutes. And we varied the heat-transfer coefficient, and you can see here its effect on the delta T between the wall and the fluid.

MR. WALLIS: You have no idea how much leverage that has on --

MR. BESSETTE: Yes. And we varied it from 800 up to 10,000, and I recall that RELAP is predicting heat-transfer coefficients in the region of 1700, thereabouts, between 1700 and 3400.

So how far off can I be in terms of heat transfer? Well, if you go from the heat transfer of 1700 to infinite, you would vary this delta T by about 25 degrees Fahrenheit.

MR. WALLIS: Well, what effect does that have on the backing of the wall?

MR. BESSETTE: It would have the same effect as a 25-degree change in fluid temperature. My point is that fluid temperatures during these transients is changing by about 300 to 400 F, and so to give you an order of magnitude comparison between the importance of fluid temperature --

MR. WALLIS: I don't have the whole perspective. It may well be that whether you get 300 degrees temperature difference or 325 makes a big difference to thermal shock. I don't know. I think thermal shock is kind of a cliff-like phenomenon where all of a sudden you've shocked it too much and it's gone. It may be that that changed, that little bit of temperature change makes a big difference.

MR. BESSETTE: Yes, well, I agree. This is why, and I think the point I'm trying to make is that you have to consider the total fluid temperature change, which gets back to the RELAP calculation of downcomer fluid temperature --

MR. WALLIS: Surely you have some sort of influence on the bottom line? I don't think you're telling us very much.

MR. BESSETTE: You only get that influence through a full FAVOR calculations.

MR. ERICKSONKIRK: If I could interject, and I think the point is well taken that we need to do what both of you gentlemen just proposed, but just for point of information, when you look at the ten transients that Davis is talking about that are dominant for Palisades, the most dominant transient is the stuck-open pressurizer SRV that re-closes after a hundred minutes. That contributes, of any of the transients, the largest two - the through-wall cracking frequency, and the base-case CPF for that is 6.5 times 10 to the minus 5. When you go with the modified heat transfer coefficient, the CPF actually goes down to 4.2 times 10 to the minus 5. Now, in the interest of providing a --

MR. WALLIS: Would you increase the heat transfer --

MR. ERICKSONKIRK: Yes. On that particular transient, yes. Now, in the interest of providing a balanced perspective, the next most important transient for Palisades is the 16-inch hot-leg break. The base-case CPF for that was 4.3 times 10 to the minus 5. When you go to the Catton heat-transfer coefficient, you go up to 5 times 10 to the minus 4, a factor of ten increase.

MR. WALLIS: A big change.

MR. ERICKSONKIRK: Yes. So I think the point is very well taken that to see the effect of this change of heat-transfer coefficient, things need to be weighted by the initiating event frequencies and --

MR. WALLIS: -- a factor of ten, I wasn't really wrong.

MR. ERICKSONKIRK: For that particular one. I'm still betting that the factor of three is right, but we'll do that and get back with you.

MR. BESSETTE: But you see these kind of sensitivities, for example in a stuck-open pressurizer SRV, it's not, this is kind of a long drawn-out transient. It's not particularly sensitive to age. In this case, the CPF went down. What it's most sensitive to is the re-pressurization.

MR. WALLIS: Well, I think what we need to do is we need to do exactly what Mark was saying. We also need to see what the APEX data looks like. I mean, if you point a data point on it, you know, where is it? And you have data from APEX. I don't think it's good enough to say you didn't think it was very good and it was only compared somewhere with Dittus Boelter or something.

MR. BESSETTE: But you have the whole report. You should have the whole report.

MR. WALLIS: I'm not going to go --

MR. BESSETTE: I know what you mean. It's a 100-page report; I know what you're getting at. So when you look at the effect of this range of heat-transfer coefficient has on the -- now we're getting closer to what you want to see here. This is the predictions of K 1C and K 1 from FAVOR on the left-hand side. On the right-hand side is the K ratio. So you can see a factor of ten change in heat-transfer coefficient gives you, roughly, this kind of change in the K ratio.

MR. RANSOM: Is that the same by group, Palisades or --

MR. BESSETTE: Actually, well, this is this, it's a simple exponential --

MR. RANSOM: Oh, okay.

MR. POWERS: I guess I still don't understand. Earlier, you said it made a 20 percent change in the heat-transfer coefficient and it caused a three percent change in the conditional failure probability. And here you show factors of ten, and these parameters, they change a little bit. Either it's very, very sensitive to those parameters, or those aren't the appropriate parameters.

MR. DENNING: Well, explain what the value of one means.

MR. BESSETTE: Yes. So, for example, this transient looks like it just barely gets to one. This transient, let's say this is at .99. This generates a zero CPF; whereas this one up on top, it may generate a CPF of 10 to the minus 7 or whatever. So you've got the difference to zero and --

MR. DENNING: Well, the crack will start to run, right? And then it's a question of whether it arrests or not. So at one, if we reach one, a crack will start to run.

MR. BESSETTE: A crack could start to run.

MR. DENNING: Well, isn't it that at one for the K 1C, it will start to run, and then the question is will it arrest or not? Or am I wrong?

MR. BESSETTE: No, it cannot, a crack cannot possibly start below one. There's some probability that a crack could start greater than one; but it's a probability, it's not a definite.

MR. WALLIS: Does it depend on the flow size and things like that?

MR. BESSETTE: And so on, yes, all the distributions. And as you go up, certainly as you go up in this ratio, the probability increases. So we seek for the dominant transients, K ratios reach two or three.

MR. ERICKSONKIRK: I think this is one of those odd cases where Dr. Wallis and Mr. Bessette are both right because there is a cliff, and you're going from zero failure probability in the K ratios David showed as below one to a very small failure probability, although that's zillions percent above zero when you go above one. So you're talking about changes in small numbers, but there is, I mean, there's a bifurcation. You can't have fracture toughness values below the minimum value, and so there is something of a cliff there, albeit for small numbers once you start falling.

MR. WALLIS: But a factor of ten could be a factor of ten on something miniscule?

MR. ERICKSONKIRK: That's right.

MR. WALLIS: That's also in the example you gave.

MR. ERICKSONKIRK: No, we need to do the weighted analysis to give you the right perspective.

MR. WALLIS: So you're right, too?

MR. BESSETTE: Oddly enough, yes. So, basically, you just can't take a factor of three out of context.

MR. WALLIS: That's why it's so misleading. I mean, presenting all these curves and say, "Well, look, it looks as if it has a big effect or a little effect." Until you put it in the context of what it does to the fracture of the vessel, you give completely the wrong message.

MR. ERICKSONKIRK: Or else compare it to the real world. I mean, you know, if the heat-transfer coefficient is the heat transfer, you know, we take whatever it gives us.

MR. WALLIS: No, that's the other message.

MR. BESSETTE: So I think the basic conclusions are, is RELAP predicts pressure and temperature adequately for the PTS analysis.

MR. WALLIS: We haven't compared it with any reality here, so how do we know that?

MR. BESSETTE: Well, we've compared it, I've shown you reality in the sense of comparisons with --

MR. WALLIS: But, you see, the analysis is a new geometry. The only one that you've really tried to model in any way seems to have been Catton.

MR. BESSETTE: Yes. But what I've shown, at least for pressure and temperature, the relevant experiments are integral system tests, and we've looked at a large body of integral system tests, and we get what I would say is --

MR. WALLIS: Well, this is all temperature. This isn't heat-transfer coefficient.

MR. BESSETTE: No, I said I was talking about temperature and pressure.

MR. WALLIS: Oh, okay. So you're talking about not the wall temperature, you're talking about the fluid temperature.

MR. BESSETTE: Fluid temperature, yes. Fluid temperature. Experimental data show large thermal stratification in coalesce but nearly uniform downcomer temperature distribution.

MR. WALLIS: It's mysterious how it suddenly mixes so quickly at the cold leg.

MR. BESSETTE: Well, I wouldn't go as far as to say mysterious; it's interesting.

MR. WALLIS: I'll have to look back at the APEX reports, because in some of the early APEX reports, they seem to be plumes that were significant. In a later report, I couldn't see anything like the old plumes.

MR. BESSETTE: Well, if you notice, he doesn't talk about plumes, but then you look at these numbers and then he's talking about 5 degrees K or less.

MR. WALLIS: Well, maybe it was a question of the scale on the pictures he was showing.

MR. BESSETTE: And in the scheme of things, the sensitivity of CPF to heat-transfer coefficient is generally small compared to such things as a boundary conditions with the PRA. We're dealing with ranges of 100 degrees K of boundary conditions compared to -- from this kind of result, you would say the uncertainty for the heat transfer is something like, perhaps, 10 degrees F, 15 degrees F, or thereabouts.

MR. SHACK: Now, I'm confused. I thought a bin had a thermohydraulic history, so it's between bins? Are we on uber-bins and --

MR. BESSETTE: Well, this, for example, is my uber-bin.

MR. SHACK: Okay, the uber-bin.

MR. BESSETTE: This is the uber-bin that represents what is a small-break LOCA.

MR. SHACK: Okay, so it's within a PRA uber-bin?

MR. BESSETTE: Yes.

MR. WALLIS: You're satisfied?

MR. BESSETTE: I'm satisfied we discretized, for example, the LOCA bins, as well as could be justified.

MR. SHACK: If there are no more questions for Dave, we can move on to the probabilistic fraction.

MR. POWERS: An exact science.

MR. SHACK: It has one constant, 10 to the minus 45th per year.

MR. POWERS: Let me ask you a question, Dave, since I don't know, especially on main steamline breaks, but I suppose also on any kind of LOCA, you get substantial vibrations and shocks to the system. Do those have an impact on your fracture mechanics at all, or is it just too weak of a phenomenon?

MR. ERICKSONKIRK: That's not something that's been considered, no. Vibrations causing then what?

MR. POWERS: Affecting the probability of cracking and things like that.

MR. WALLIS: It's not so much the vibration, but, when you have a large-break LOCA, there's a big bump to the vessel, in some cases.

MR. ERICKSONKIRK: That wouldn't have been considered in the analyses we're talking about. That would have been considered in the work that Nathan and his colleagues did in establishing the through-wall cracking frequency limit because that gets to what happens after the vessel fails.

MR. WALLIS: After the vessel fails?

MR. ERICKSONKIRK: Yes, what happens. Does vessel failure lead to core damage? Does vessel failure lead to --

MR. SHACK: No, but the pressure comes early. The pressure thump comes, you know, with all these little cracks in here, that vessel just --

MR. POWERS: I guess I'm not following --

MR. SHACK: This vessel is very robust until you put a big crack in it. You don't put a big crack into it until very late in this transient, all things considered.

MR. POWERS: Plus, the vessel is hot when that occurs.

MR. SHACK: It's hot, it's cracked, you know, it's very robust at that point until you get through it.

MR. SIEBER: Is it that time again?

MR. POWERS: This I understand. This is true.

MR. ERICKSONKIRK: Okay. This is the

review of the final comments we got with regards to probabilistic fracture mechanics. I would, however, before I get into that, just point out as I did yesterday that there were a few comments that our reviewers made that we felt were so significant that we had to take account of them by modifying our model.

One was that Dr. Schultz pointed out that we had ignored the effect of pressure-loading on the crack face in calculating our driving forces, and we realized that he was, in fact, right and, therefore, put it in. And, also, based on comments from Dr. VanWalle, we modified some of the details of our upper-shelf model.

But in terms of final comments, and I apologize, I thought I took the animation away, here I'm summarizing comments made on probabilistic fracture mechanics by Dr. Schultz, Dr. VanWalle, and then Dr. Murley also commented. I put a summary at the top, and I will spare you my recitation of it, but pretty much all these gentlemen said that, generally, things looked pretty good, but they had some niggling details that they wished to go on record as saying that they thought could either be done better or should be changed.

The two, the points that Dr. Schultz made was that he didn't feel that we had adequately demonstrated that the fall distribution that we used applies to all plants; and, therefore, he recommended that, in order to use any rule drawn out of these results, the licensees should be required to somehow demonstrate the appropriateness of the assumed flaw distribution to their vessels.

And Dr. Schultz also commented that he believed we could do a better job at demonstrating our ability to accurately predict crack initiation, run arrest, and re-initiation events. And the interchange on that particular topic between Dr. Schultz and Richard Bass and Clark at Oakridge is preserved in an appendix in NUREG 1680, so that the committee may read it and reach their own conclusions. From Dr. VanWalle, again, generally nice words regarding the overall strategy. His remaining issues regarded the fact that we do not sample on correlation uncertainties for the embrittlement relationships and Sharpie-to-toughness conversions. We discussed that yesterday in the subcommittee. And while Dr. VanWalle accepted that there aren't any procedures currently for mathematically representing mixed uncertainties, he found that somehow unsatisfying.

His closing recommendations were that

continued in-service inspection should be used to substantiate the applicability of the flaw distribution that we used in the PWR of interest, that over time we should be continuing to require surveillance, in particular surveillance involving actual fracture toughness tests, not simply Sharpie tests, so that, over time, we can move from correlative approaches based on Sharpies and RTNDTs toward direct approaches using fracture toughness. And also, he recommended continued and further validation of, indeed, both the crack-arrest models and the upper-shelf toughness models.

MR. WALLIS: I think we like that second bullet there.

MR. ERICKSONKIRK: So do I. It should keep me in business for a while. That's why I put it up. From Dr. Murley --

MR. SHACK: Just out of curiosity, if we ever built a new reactor, would we take that into account when we started a new surveillance program?

MR. ERICKSONKIRK: I'm not sure if we would. I think it would be a good idea, and I know that, indeed, some of the licensees, as they've put capsules into their vessels looking at license extension, they've intentionally put in pre-crack samples. So at least the licensees are looking forward to the future.

Again, generally, good comments from Dr. Murley. He did, however, point out that he had some residual issues and concerns, but he didn't think that they'd seriously challenge the general validity of what we'd done. Those remaining issues -- and, again, I'm focusing here just on the PFM.

In his write-up, it was clear that there were some things that we hadn't explained well enough or clearly enough for him to understand, so we're iterating with him on that to try to make sure that doesn't happen again. And when does that not happen? And then, also, he made a comment regarding the need for more thorough discussion of what he called, and I think appropriately so, the residual uncertainties, both conservative and non-conservative in our analysis. And that was my closing slide at the beginning of this presentation, so we thought that was a very good suggestion that we took on board.

He again, and this is a consistent theme from all three of the fracture or fracture-oriented reviewers, questioned the applicability of the flaw distribution, however admitted that we're kind of in a bind because we're using all and the best information that we have. So, again, his comment had to do with some sort of administrative procedure using continued in-service inspection to continue to check the situation in the same vein that we use surveillance to continue to check the validity of our embrittlement correlations.

And that was it. Any questions?

MR. RANSOM: Aren't thermal sleeves used in some of the nozzles on the vessels to reduce thermal --

MR. ERICKSONKIRK: I don't know.

MR. HISER: I don't think the inlet nozzles upper-head on CRDMs, things like that, they're used in those cases.

MR. WALLIS: There are shields in these downcomers, aren't there, in some reactors? Thermal shields, cylindrical. Does that make a difference to anything here, or do we have to start from scratch when we're dealing with them? The effect of hydraulic diameters change the mixing in the downcomer.

MR. BESSETTE: Well, some plants, I believe, had thermal shields, Palisades for example, but they took theirs out.

MR. WALLIS: They took them out?

MR. BESSETTE: Yes, Palisades took it out, I know.

MR. WALLIS: There aren't anymore there?

MR. BESSETTE: I can't say that there are no plants with thermal shields left.

CHAIRMAN BONACA: There are some still. I think so.

MR. WALLIS: I thought they all came out.

CHAIRMAN BONACA: You may be right.

MR. ERICKSONKIRK: If there are no further questions, back to you, Mr. Chairman.

CHAIRMAN BONACA: You should be commended for having controlled --

MR. WALLIS: I have other questions.

CHAIRMAN BONACA: All right, go ahead.

MR. WALLIS: Way out in this transient is a large break. The downcomer is full of water all the time, is it?

MR. BESSETTE: Yes. The downcomer refills, well, within about 40 seconds or so.

MR. WALLIS: Well, is there some part of your transient where the downcomer is not full of water?

MR. BESSETTE: Yes, during a blow-down.

MR. WALLIS: At the very beginning.

MR. BESSETTE: Yes. The first, well,

within the first 30 seconds. Yes, the blow-down takes about 30 seconds. During that time, the downcomer is mostly empty.

MR. WALLIS: Right.

MR. BESSETTE: And then it refills very quickly, within --

MR. WALLIS: But it's refilling with really cold water.

MR. BESSETTE: Yes.

MR. WALLIS: It has nothing to mix with.

MR. BESSETTE: Well, yes, there is. Actually, it mixes with the steam that's still coming out through the --

MR. WALLIS: You have to get your condensation model right.

MR. BESSETTE: That's correct.

MR. WALLIS: Do you do that?

MR. BESSETTE: That's one of the assessment cases we ran, those UPTF test six to look at condensation, and we got pretty good results.

MR. WALLIS: Okay. So that's all been taken care of?

MR. BESSETTE: Yes.

MR. DENNING: Well, we know in that regime that things are just terribly chaotic and just grossly chaotic.

MR. SIEBER: Get a big water healer.

MR. BESSETTE: Yes, that's very right. It empties within the first 30 seconds, and then it refills within another 10 seconds or so, 20 seconds.

MR. WALLIS: And the grossly chaotic would make it, more or less, equilibrium, thermodynamic, so it's a saturation temperature.

MR. BESSETTE: Well, you're injection, you're pretty much --

MR. WALLIS: Chaotic would give you an enormous heat-transfer coefficient. The wall must be pretty well above the water temperature during this chaotic period. I just don't know if that matters. I mean, if you've only shocked the wall during that very early part of the transient, is this something which is being missed by all this analysis?

MR. BESSETTE: Well, you might initiate some cracks, but you're not going to propagate them because most of the wall is hot.

MR. DENNING: Well, just thinking large LOCA here, where there's no pressure? I mean, that's, you know, that's my experience, large LOCA, no pressure.

MR. WALLIS: So what matters is the

thermal shock, just the thermal effects. You're talking about plumes and all this stuff, this is late in the transient, when everything is full of water. I just want to make sure that you covered the effects during the large LOCA and this is not full of water.

MR. BESSETTE: I thought about that, and I think we're okay.

MR. WALLIS: If RELAP has done it all --

MR. BESSETTE: Well, I'm not saying it's perfect, but we thought about condensation during these EC injections.

MR. WALLIS: This goes into the analysis then properly?

MR. BESSETTE: Sure.

MR. WALLIS: FAVOR does all this stuff at the right time?

MR. BESSETTE: Well, you know, we only carried our break spectrum up to 22 inches, which is pretty large but not all the way. But I don't think we get any further change beyond 22 inches.

MR. WALLIS: So your answer is that your analysis and the FAVOR code properly models the part of the transient where the analysis is full of some kind of chaotic mixture, which might be quite cold? At the beginning of the transient, before it's full of water.

MR. BESSETTE: Well, we've looked at --

MR. WALLIS: In a large break situation.

MR. BESSETTE: Yes. In other respects, we've looked at downcomer temperatures during large break LOCAs, and you get a tremendous amount of condensation during the ECC injection.

MR. WALLIS: What's the period of your - from the slides your heat-transfer coefficient is off scale pretty well.

MR. BESSETTE: In fact, what you tend to end up with, rather than a sub-cooled downcomer, is a saturated downcomer that has boiling from the vessel --

MR. WALLIS: I just want to make sure it's properly taken care of in the whole analysis and the PTS part of it.

MR. BESSETTE: Yes. But at any rate, you tend to end up with boiling in the downcome rather --

MR. WALLIS: I just want to make sure it was taken care of because you're not giving me great assurance. I'm not quite sure. Anyway, bear that in mind.

CHAIRMAN BONACA: When will we have a final report?

MR. BESSETTE: You mean on this supplemental thermohydraulics report?

CHAIRMAN BONACA: Well, I was talking about everything.

MR. SHACK: Yes, the final reports. I think we now have copies, at least drafts, of everything except the thermohydraulics; is that correct?

MR. ERICKSONKIRK: Yes, that's correct. The drafts that you have will be changed only insofar as tech editing, you know, response to comments that have been made here. I mean, we're not anticipating major technical changes to those.

MR. WALLIS: In what sense do we have them? Because I think before we came here we didn't have them all.

MR. SHACK: Well, they're waiting for you in your mail.

MR. WALLIS: What are they? Something like this in my mail, or is it --

MR. SHACK: They're PDF files, so, you know, they're only that big.

MR. WALLIS: So they're waiting for me in my mail.

MR. ERICKSONKIRK: But did you get, were

you inquiring as to when the missing report is going to be available?

CHAIRMAN BONACA: Well, I think we would not write a letter because we don't have the report. So I was trying to understand when you would come up again for us to be able to comment in writing.

MR. SHACK: I think that's the plan is that we would like to have the final reports before we write a letter.

MR. ERICKSONKIRK: Yes.

MR. HISER: Just for the big picture, our expectation is that we will publish the reports that you had been provided with, including the two that got lost in the ether somewhere sometime in the January - February timeframe. So the one report will be the one missing link, if you will, in that chain that provides the basis.

MR. ERICKSONKIRK: I should have that to you by the end of this month.

CHAIRMAN BONACA: Make a decision whether or not we need another update or not.

MR. SHACK: I think we may want to have a presentation on the thermohydraulics again after we've had a chance to review the report. I don't think we want to go through everything else.

CHAIRMAN BONACA: I agree with that. All right. Okay. I think we have an idea. Thank you. Any further comments on this? If not, I think we, first of all, we can get off the record now for the rest of the day.

(Whereupon, the foregoing matter was concluded at 3:29 p.m.)

C E R T I F I C A T E

This is to certify that the attached proceedings before the United States Nuclear Regulatory Commission in the matter of:

Name of Proceeding: 518th Meeting of the Advisory Committee on Reactor Safeguards

Docket Number: not applicable

Place of Proceeding: Rockville, Maryland

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission taken by me and, thereafter reduced to typewriting by me or under the direction of the court reporting company, and that the transcript is a true and accurate record of the foregoing proceedings.

Eric Hendrixson
Official Reporter
Neal R. Gross and Co., Inc.

Technical Basis to Support Revision of the PTS Rule (10CFR50.61)

→ *Project Overview*

Mark EricksonKirk

Materials Engineering Branch

Donnie Whitehead, Nathan Siu, Mike Junge

Probabilistic Risk Assessment Branch, Sandia National Laboratories

David Bessette, Bill Arcieri

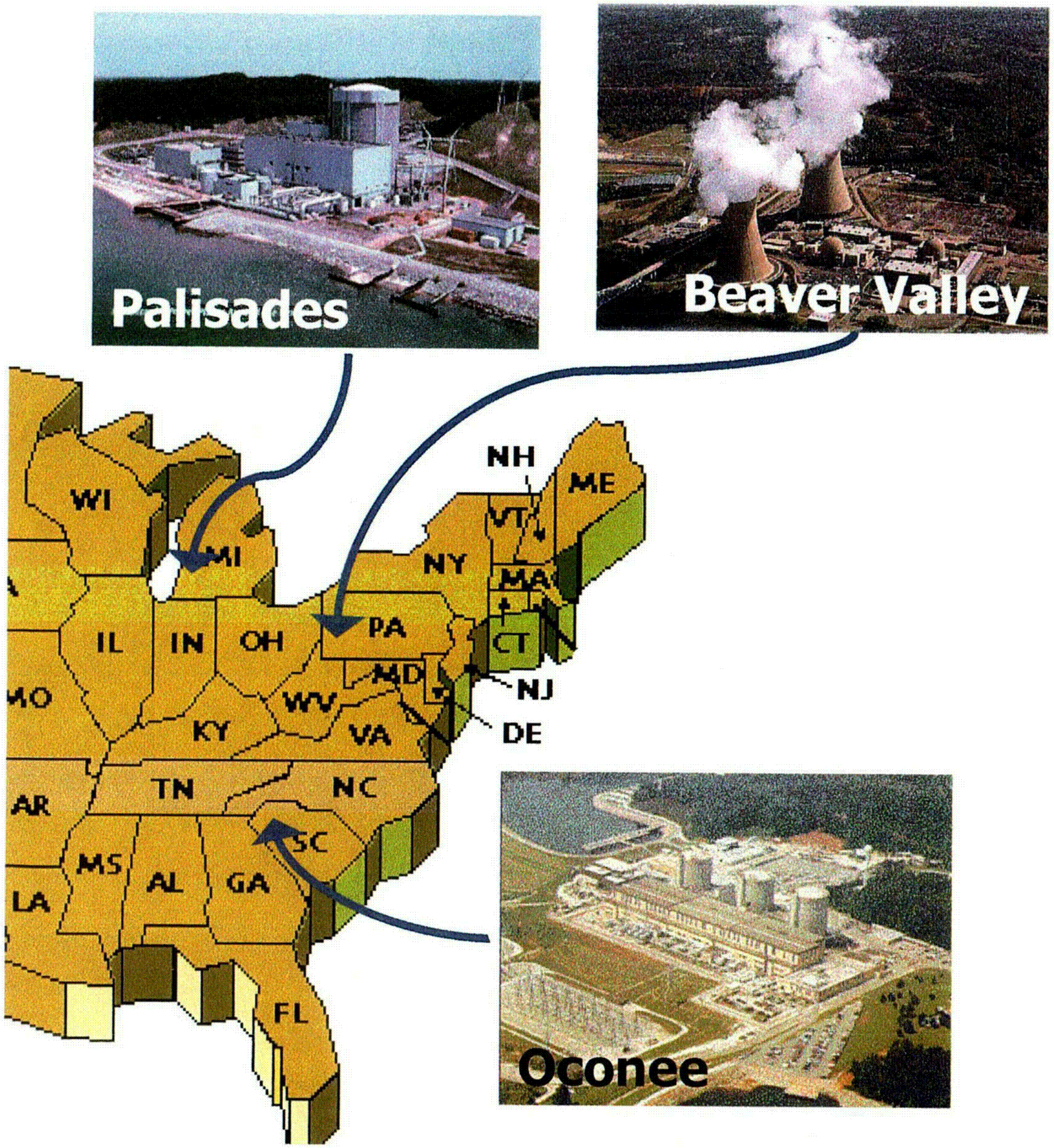
Safety Margins and Systems Analysis Branch, Integrated Systems Laboratory

ACRS Briefing
NRC Headquarters • Rockville, MD • 2nd December 2004

Overview

- Scope of analysis performed
- Factors that contribute most significantly to vessel failure probability
  - Material factors
  - Transient classes
- Generalization of findings to PWRs *in general*
- Proposed RT-based screening limits consistent with RG1.174 guidance on LERF
- Comparison of operating PWRs with these screening limits
- Conservatisms / non-conservatisms that underlie screening limits

Scope of Analysis

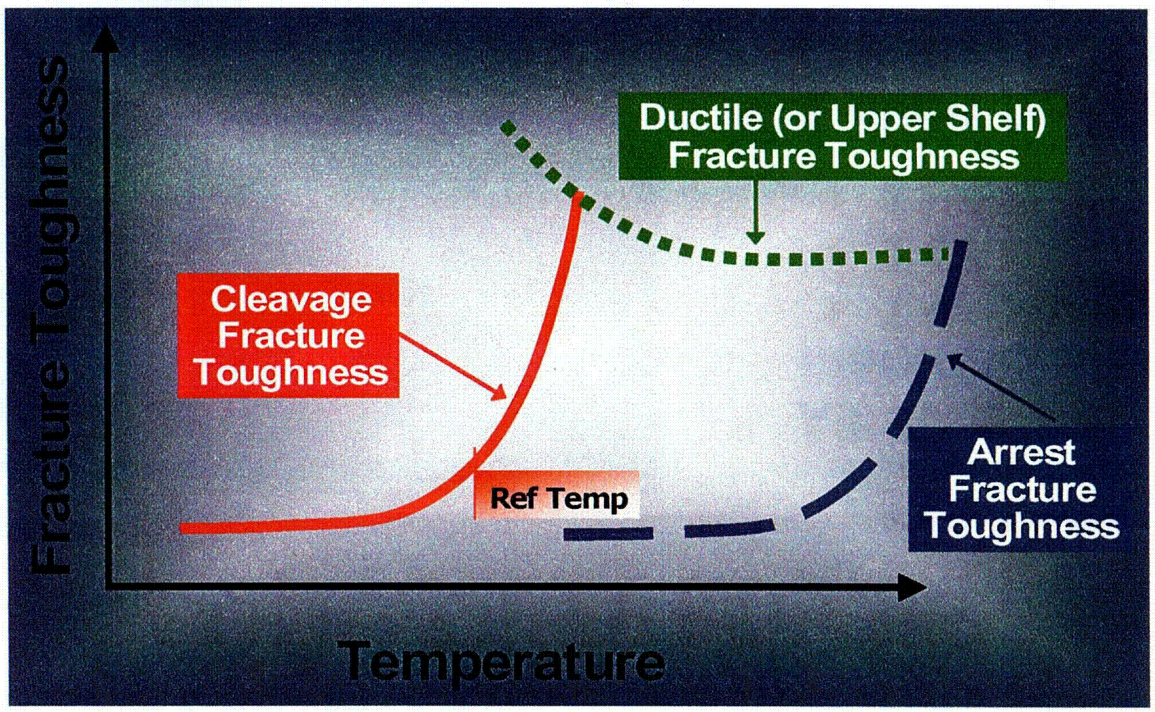


- **All PWR manufacturers**
  - **1 Westinghouse**
  - **1 CE**
  - **1 B&W**
- **1 plant from original (1980s) PTS study**
- **2 plants very close to the current PTS screening criteria**
- **Generalization to all PWRs**
- **TWCF criteria consistent with RG 1.174 guidance on LERF**

C01

Material Factors Controlling Vessel Failure

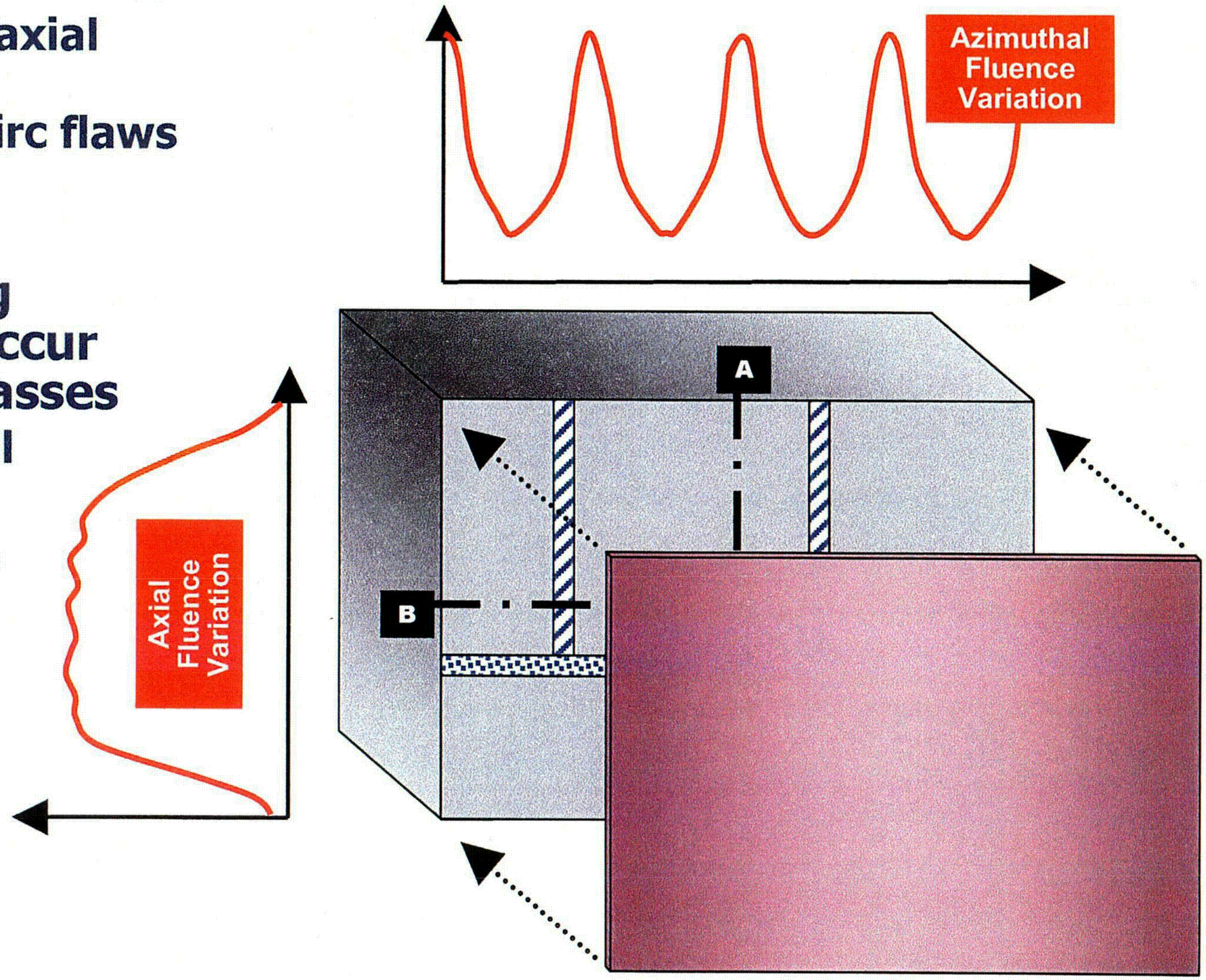
- To correlate / predict vessel failure the toughness properties at the flaw location need to be known
- A reference temperature (*RT*) characterizes all of the toughness properties of interest
- So flaw locations are needed to determine the reference temperature(s) that control the vessel failure probability



C02

Locations of Simulated Flaws

- Embedded weld flaws follow weld fusion lines
  - Axial welds → axial flaws only
  - Circ welds → circ flaws only
- Surface breaking cladding flaws occur between weld passes
  - Circumferential
- Plate flaws have no preferred orientation



C03

Flaw Location Specific Reference Temperatures …

… are needed to characterize accurately toughness properties at the different flaw locations

$$RT_{MAX} \equiv MAX\left\{\left(RT_{NDT(u)}^{plate} + \Delta T_{30}^{plate}(\phi t_{FL})\right), \left(RT_{NDT(u)}^{axialweld} + \Delta T_{30}^{axialweld}(\phi t_{FL})\right)\right\}$$

$$RT_{AW} = \frac{\sum_{i=1}^{nfl} RT_{\max}^{i} \cdot \ell_{FL}^{i}}{\sum_{i=1}^{nfl} \ell_{FL}^{i}}$$

Failure of axial weld flaws controlled by axial weld or plate toughness properties & by the fluence along the axial weld fusion lines

Failure of circ weld flaws controlled by circ weld or plate toughness properties & by the peak fluence in the vessel

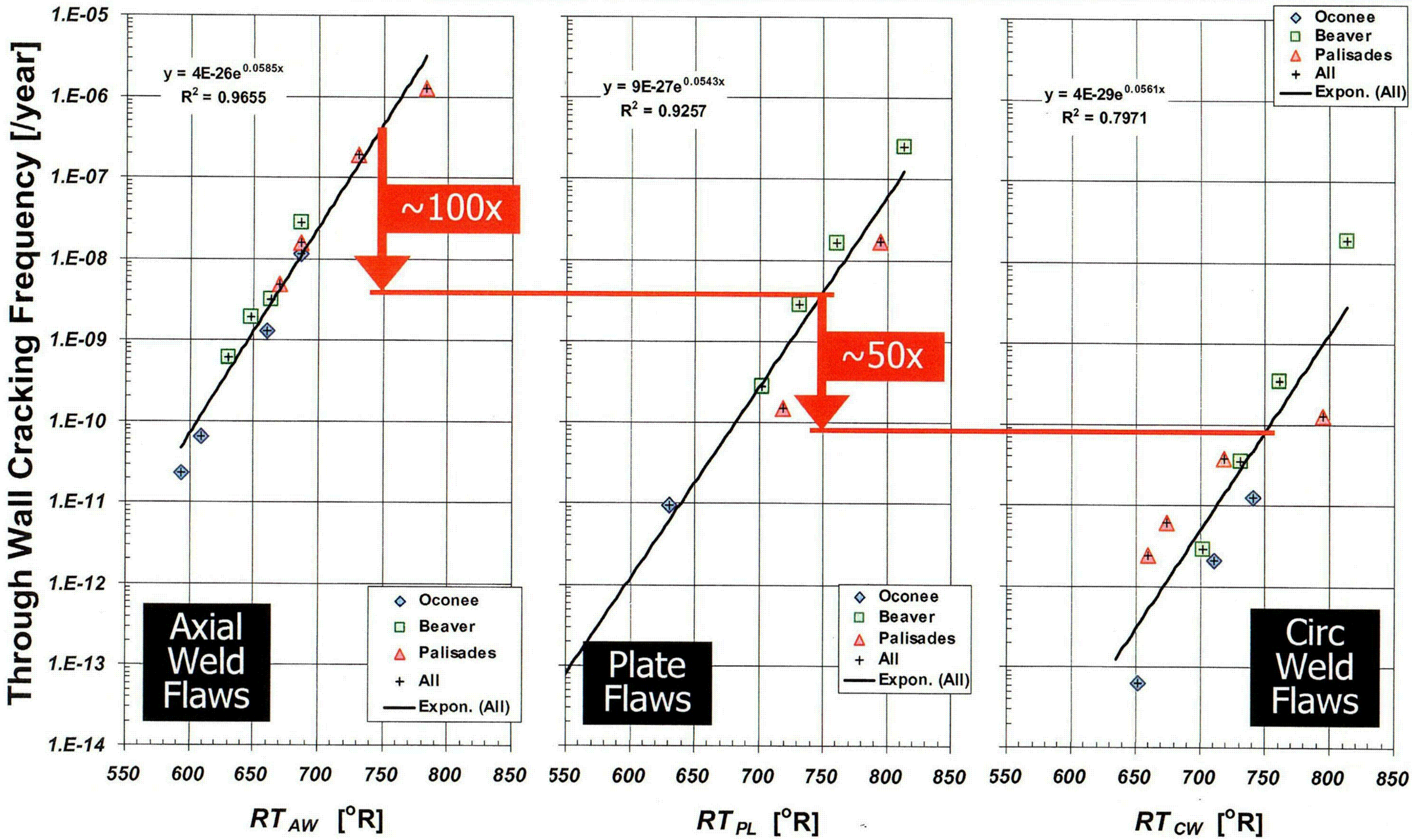
$$RT_{CW} \equiv MAX\left\{\underset{i=1}{\overset{ncw}{MAX}}\left(RT_{NDT(u)}^{i} + \Delta T_{30}^{i}(\phi t_{MAXID})\right), \underset{j=1}{\overset{npl}{MAX}}\left(RT_{NDT(u)}^{j} + \Delta T_{30}^{j}(\phi t_{MAXID})\right)\right\}$$

Failure of plate flaws controlled by plate toughness properties & by the peak fluence in the vessel

$$RT_{PL} \equiv \underset{i=1}{\overset{npl}{MAX}}\left(RT_{NDT(u)}^{i} + \Delta T_{30}^{i}(\phi t_{MAXID})\right)$$

C04

Materials Factors Controlling Vessel Failure



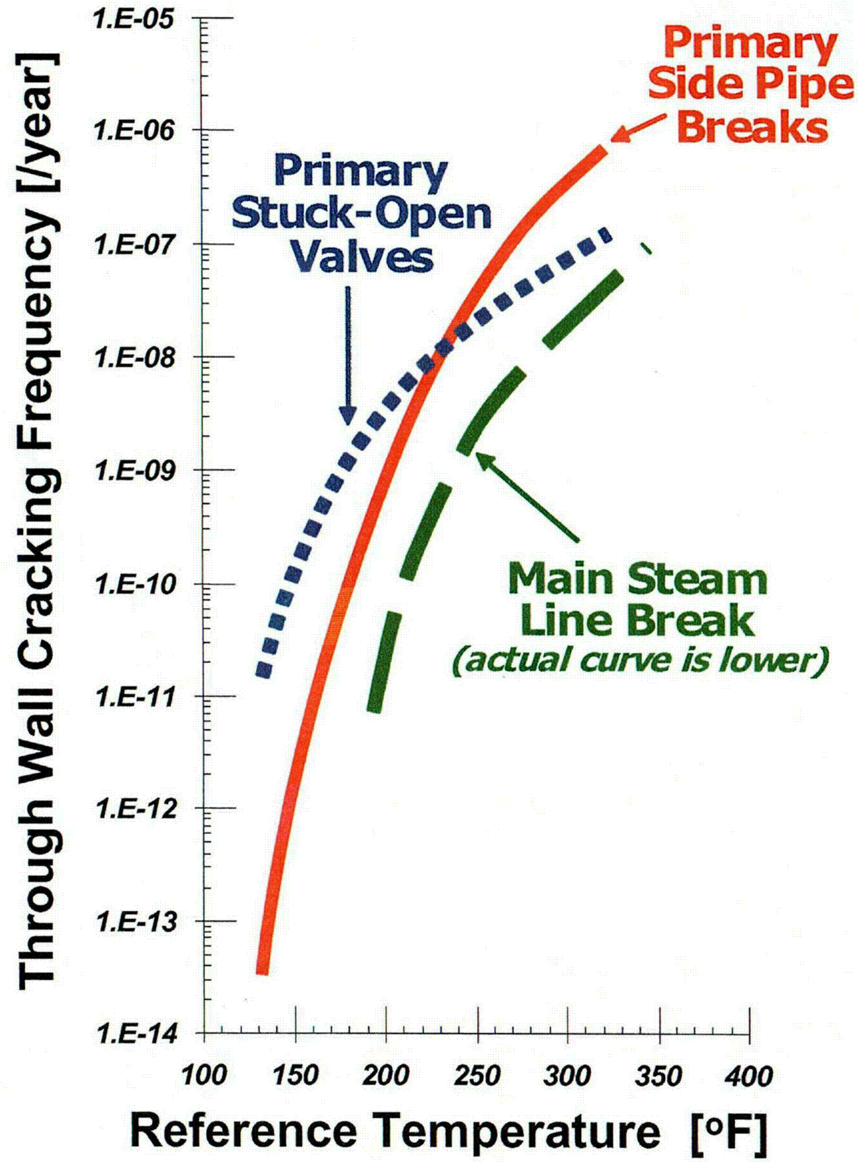
VG 7

C05

Transient Classes Controlling Vessel Failure

- **Primary side failures dominate risk (75% or more)**
  - **Low embrittlement:** stuck open valves that later re-close
  - **Higher embrittlement:** medium & large diameter pipe breaks
- **Secondary side failures**
  - main steam line breaks
  - stuck open valves

  of much smaller consequence, & only at extremely high embrittlement levels



COC

Transient Classes Controlling Failure

- **Secondary side breaks much less damaging than primary side**
  - **Initial cooling rate similar, scales with break size**
  - **Minimum temperature much higher for secondary breaks (212°F) than for primary breaks (40°F)**

- **Operator action "credits" have small influence on overall results**
  - **Pipe break: no operator actions possible**
  - **Stuck-open valves (primary circuit): Only very rapid action has any effect**

Findings applicable to PWRs *in general*

- **The transients that contribute the most to TWCF have ≈ occurrence rate and ≈ severity across plants**
- **Operator actions, though modeled, do not influence significantly the calculated TWCF**
- **Similarity of PWR designs**
- **Calculational models robust to credible changes**
- **Conservatisms intentionally left in model**
- **Equivocations**
  - **Forgings prone to sub-clad cracking at high embrittlement levels**
  - **Thick vessels**

TWCF Estimation Based on TWCF *vs.* RT Correlations

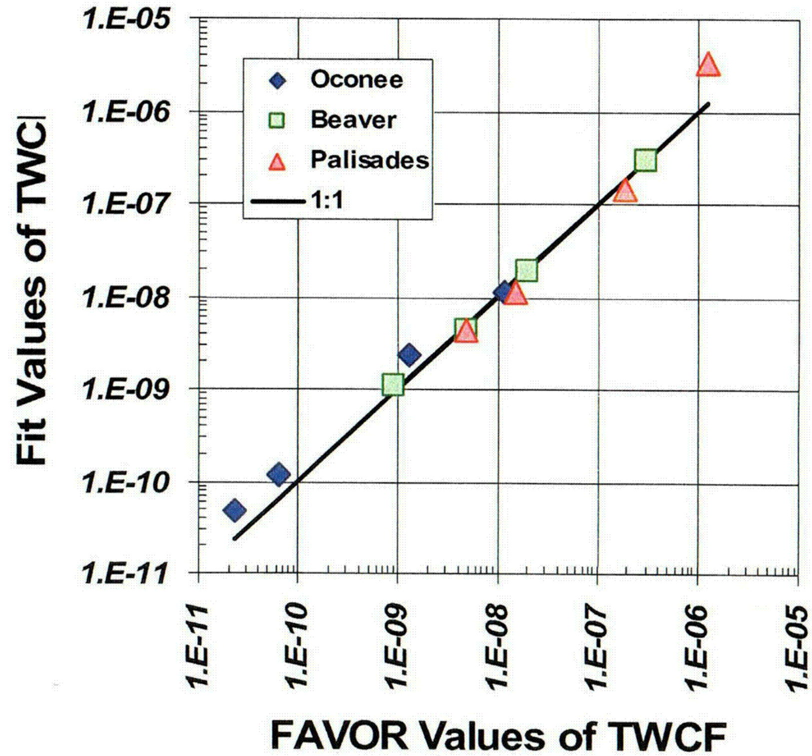
$$TWCF_{TOTAL} = TWCF_{AXIAL-WELD} + \alpha_{PL} \cdot TWCF_{PLATE} + TWCF_{CIRC-WELD}$$

$$TWCF_{AXIAL-WELD} = 4\text{x}10^{-26} \cdot \exp\{0.0585 \cdot (RT_{AW} + 459.69)\}$$

$$\alpha_{PL} = 2 \quad TWCF_{PLATE} = 9\text{x}10^{-27} \cdot \exp\{0.0543 \cdot (RT_{PL} + 459.69)\}$$

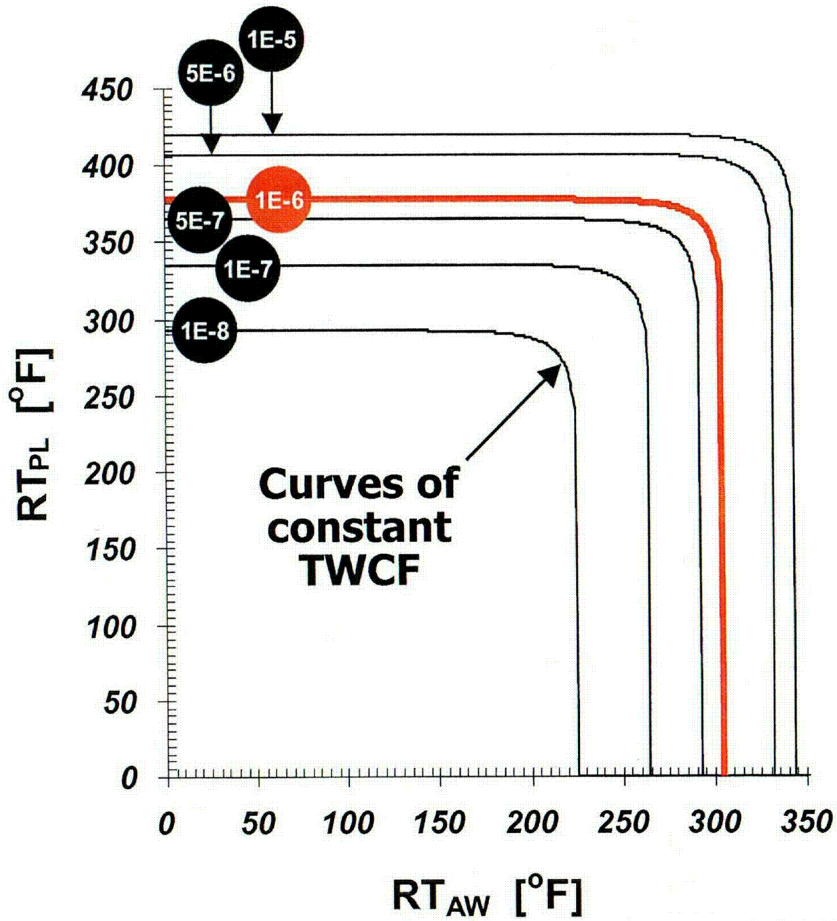
$$TWCF_{CIRC-WELD} = 4\text{x}10^{-29} \cdot \exp\{0.0561 \cdot (RT_{CW} + 459.69)\}$$

- **TWCF due to plate flaws multiplied by 2 to prevent under-estimation of Beaver Valley**
- **Setting $TWCF_{TOTAL}$ = 1E-6 permits derivation of RT-based screening limits consistent with LERF limit**

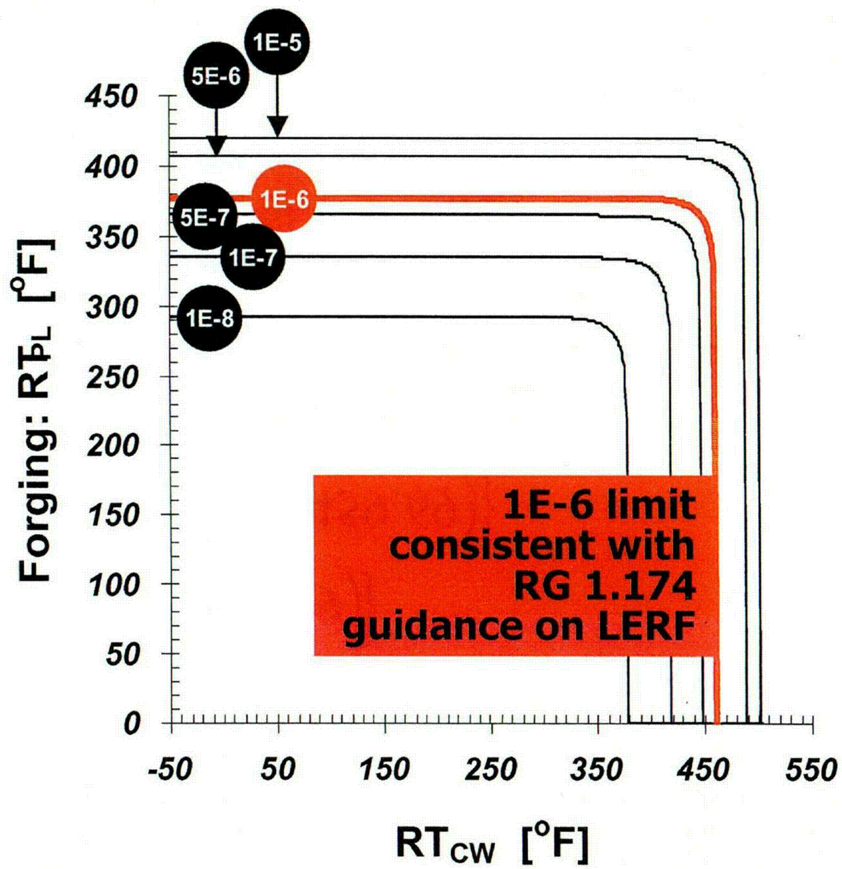


Proposed PTS Screening Limits

Plate Welded Plants



Forging Plants

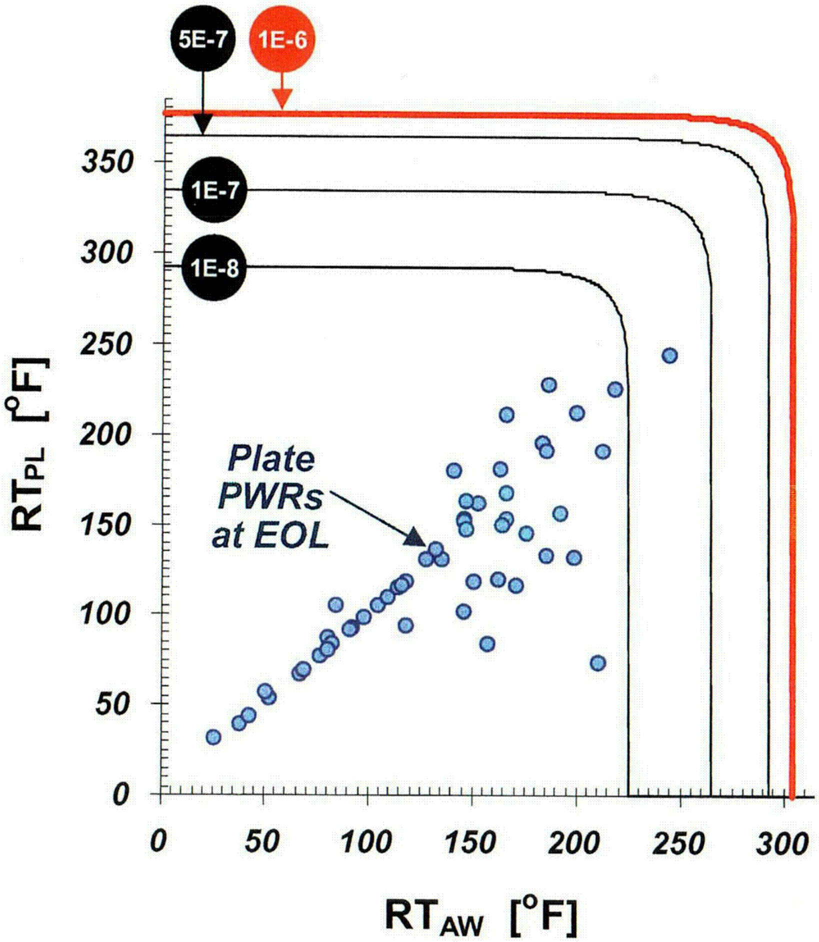


<u>Provisos</u>: Forgings at very high embrittlement & thick wall vessels.

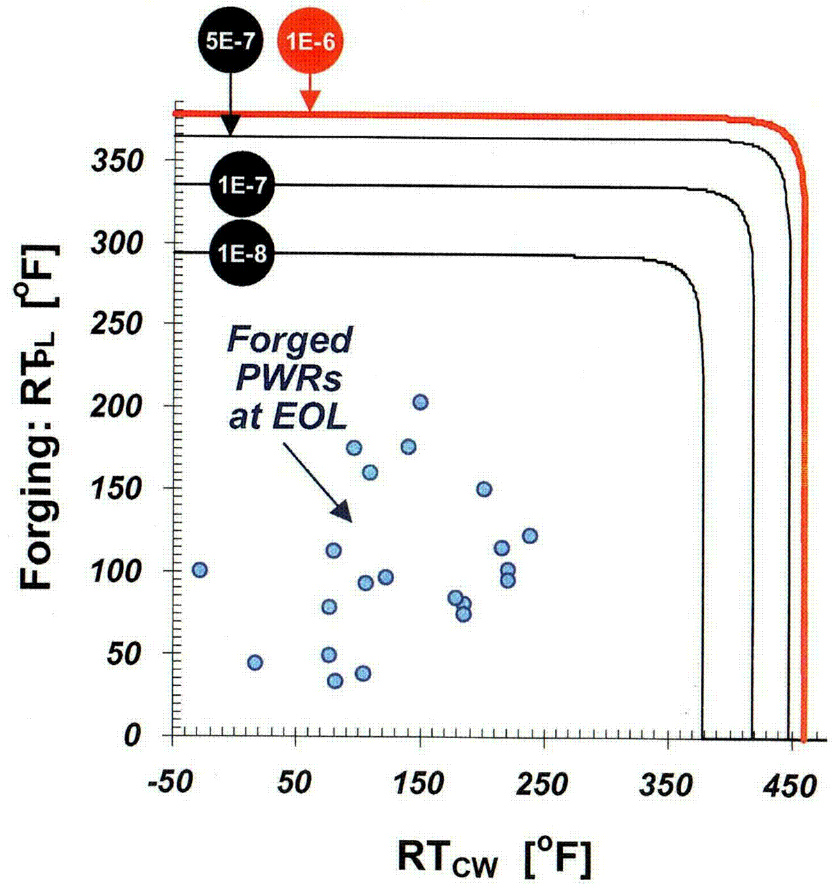
C08

Assessment of U.S. PWRs at EOL Relative to Proposed PTS Screening Limits

Plate Welded Plants



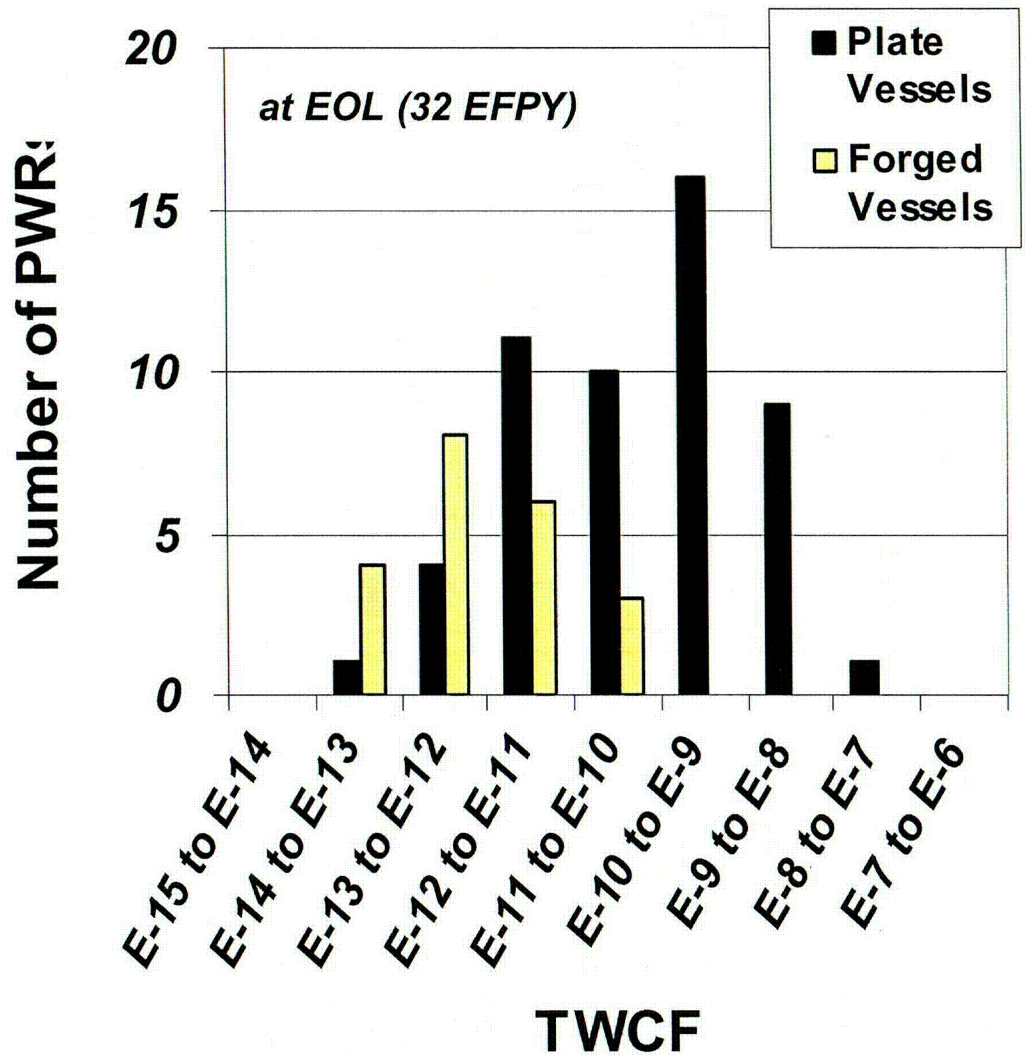
Forging Plants



C09

Proposed PTS Screening Limits & Current Plant Status

- **Plant status**
  - **PWRs all an order of magnitude away (or more) from 1E-6 LERF limit**
  - **At least 60°F (& usually much more) separates any PWR from the proposed screening limit at EOL (compare with <1°F per current regs.)**
  - **Plants move 10-20°F closer to screening limits at EOLE**



C10

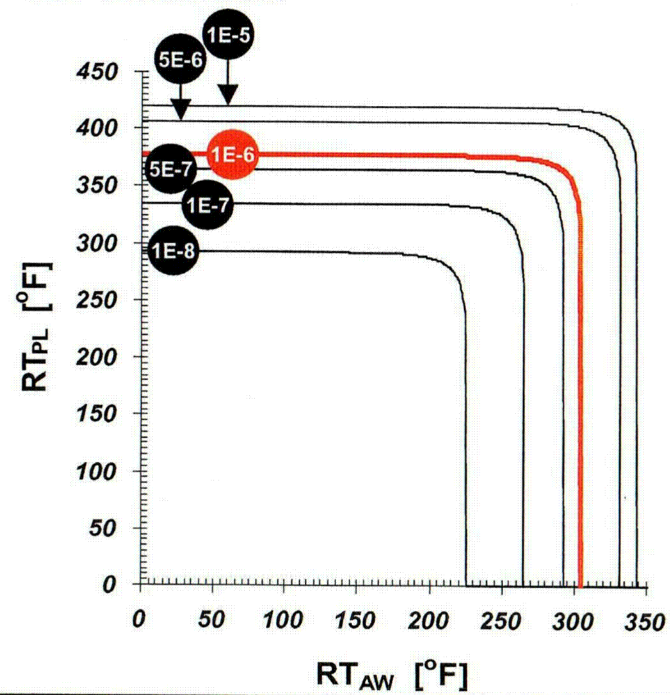
Uncertainties that Underlie Proposed PTS Screening Limits

Conservatisms

- Vessel failure always leads to LERF
- Conservative binning to account for lack of knowledge
- MSLB min temperatures ~40°F too cold
- SO-2 transients conservatively modeled
- Infinite length axial flaw propagation assumed
- Full circumferential crack propagation assumed
- Material variability / uncertainty over-estimated (both chemistry and un-irradiated toughness)
- Neutron attenuation function
- Systematically conservative assumptions made in developing flaw model (e.g., all defects characterized as flaws, etc.)
- Initiation / arrest interdependency model
- Model of RT shift due to embrittlement
- Increasing embrittlement by increasing EFPY

Non-Conservatisms

- Plume effects, if present, have been ignored
- External events ignored
- Heat transfer model
- Through-wall chemistry layering
- Air oxidation



Many more/larger residual conservatisms than non-conservatisms suggests that there is no need for a "margin" term.

C11

PRA Response to Draft Final Peer Review Comments

Donnie W. Whitehead

12/02/04

Sandia National Labs
505-844-2632
dwwhite@sandia.gov

Dr. Murley Comment

- **Comment**

  The assumption that SRV opening size is uniformly distributed seems intuitively wrong.

- **Response**

  Initial response is to agree that we should not make this assumption.

  Additional investigation to be performed.

Backup Slide

A sensitivity analysis to determine the importance of this assumption was performed (set the basic events that represent the SRV opening size to 1.0).

Total TWCF estimates (at 60 EFPY) increased.

- **Oconee: Factor of 1.5**
  8.4E-10/5.5E-10 (sensitivity/original)
- **Beaver Valley: Factor of 1.4**
  9.9E-9/7.0E-9

Should not affect the overall conclusion (i.e., sufficient technical basis exists for rule modification).

Pressurized Thermal Shock
Technical Basis for Rule Revision

PTS Thermal Hydraulic Analysis

David E. Bessette

December 2, 2004

518th. ACRS Meeting

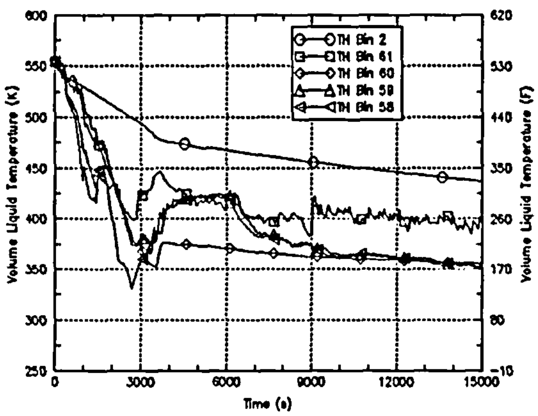
TH-1

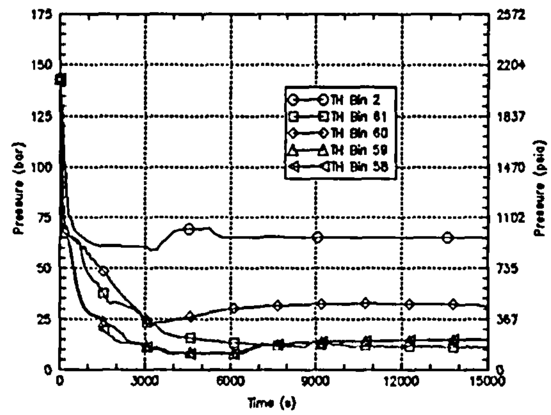
Main Peer Review Group Comments

1. Most parameters in PIRT are system boundary conditions rather than physical models. (e.g break size, ECCS flow, etc.)
2. Effect of thermal stratification and mixing in the cold leg and downcomer from ECCS injection (adequacy of 1-dimensional code)
3. Uncertainty in downcomer fluid to wall heat transfer coefficient and its impact on conditional probability of vessel failure (CPF).

TH-2

Palisades Small Break LOCA Spectrum



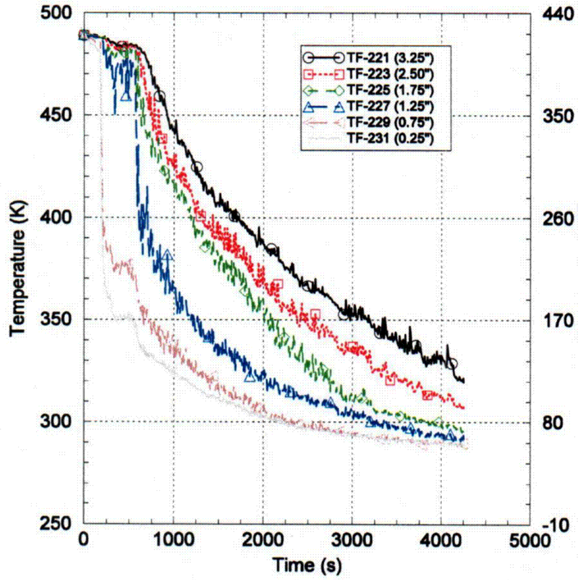


Boundary conditions dominate determination of temperature and pressure.

Adequacy of RELAP for PTS Analysis

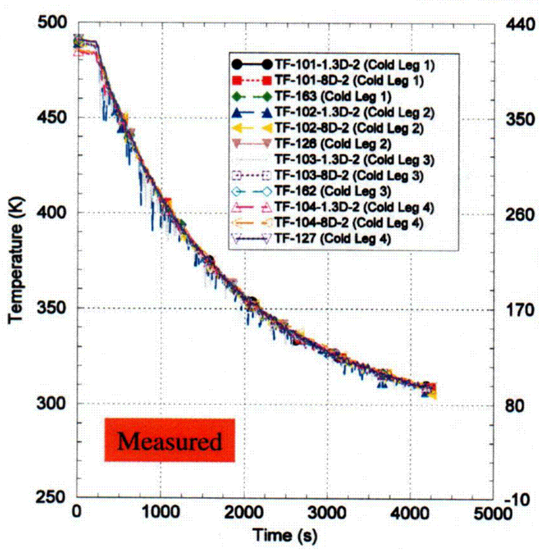
- Issues:
  - Prediction of average downcomer temperature and pressure
  - Possible downcomer temperature variations due to plumes
  - Downcomer fluid to wall heat transfer coefficient
- RELAP compared to best available system test data. Facilities include UPTF, LOFT, ROSA, APEX, and MIST.
  - 12 tests selected that are representative of risk dominant PTS transients.
- Downcomer average temperature predicted to within 4 K of the body of the experimental data with a overall standard deviation of 11K by RELAP5.
- System pressure predicted to within -0.06 MPa [9 psi] with a standard deviation of 0.39 MPa [57 psi] by RELAP5.
- Facility temperature measurements show plumes to be weak or nonexistent (< 5 - 20 K).

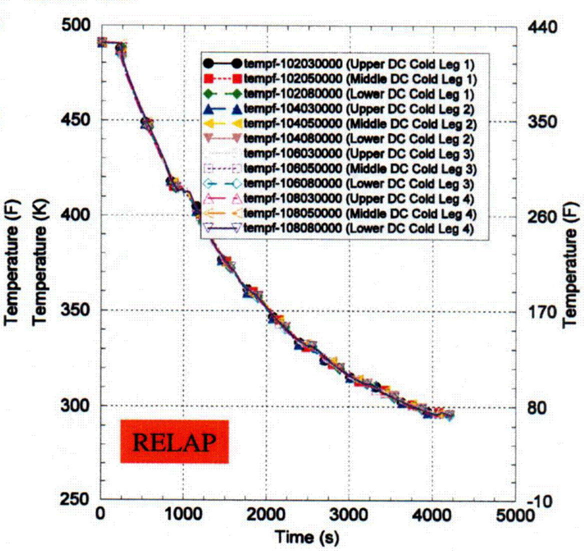
Cold Leg Thermal Stratification
APEX-CE-05



- 50 to 150 K [90 - 270°F] thermal stratification seen in the two instrumented cold legs.
- ECCS injection temperature is 285 K [54°F].
- Maximum possible thermal stratification is 200 K [360 °F]

Axial Downcomer Temperature Variation
APEX-CE-05

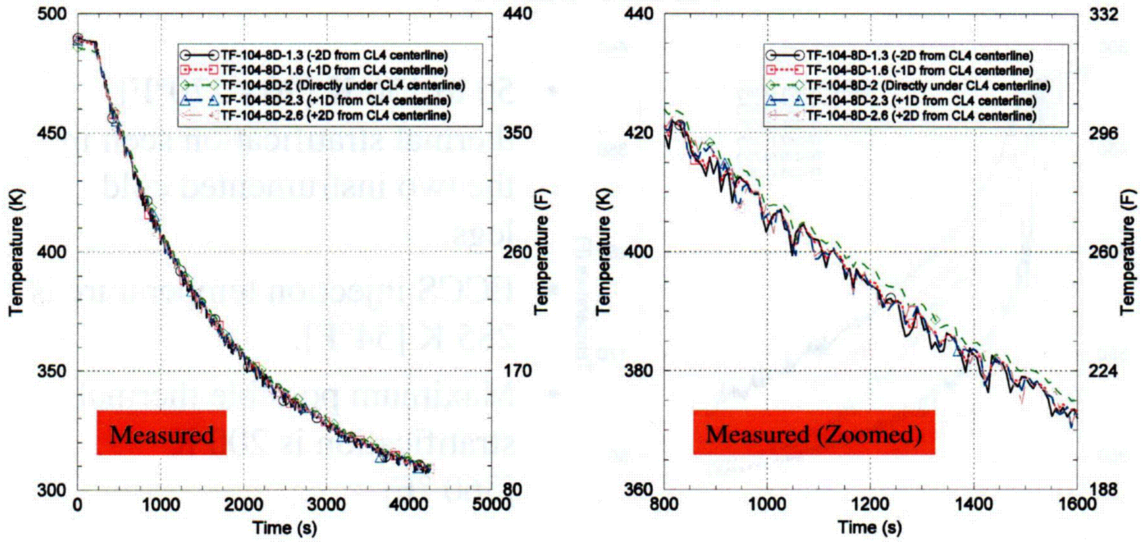




- Fluid temperatures at 0, 1.3 and 8 cold leg diameters axially along each cold leg centerline.
- No evidence of plumes based on the above temperature data

C12

Azimuthal Downcomer Temperature Variation
APEX-CE-05

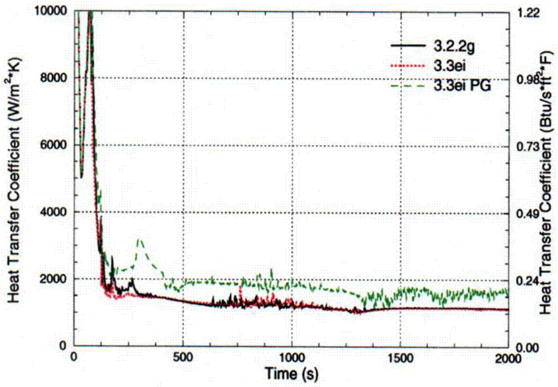


- Fluid temperatures at top of core elevation +/- 1 and +/- 2 cold leg diameters azimuthally from CL4.
- No evidence of plumes based on the above temperature data

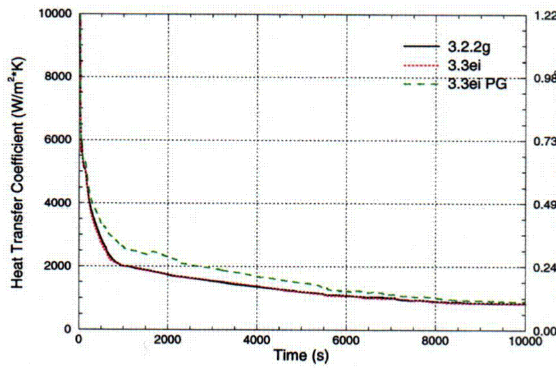
RELAP5 Adequacy for PTS Analysis
Heat Transfer Coefficient

- Dominant heat transfer mode in the downcomer is buoyancy-opposed mixed convection (cold fluid flowing downwards past heated walls).
- RELAP5 applies the maximum of the Dittus-Boelter (turbulent forced convection) and Churchill-Chu (free convection).
  - Churchill-Chu gives higher values of HTC for low-flow conditions.
- Ivan Catton suggested the use of the Petukhov-Kirillov correlation with an enhancement factor (Petukhov-Catton).

Comparison of Heat Transfer Models



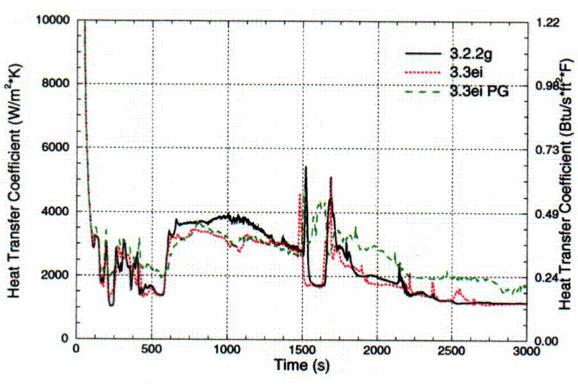
16-inch Hot Leg Break



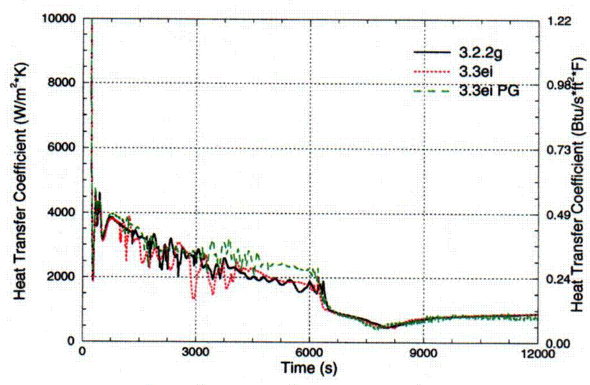
Main Steam Line Break

TH-9

Comparison of Heat Transfer Models



4 inch Surge Line Break



Stuck-open Primary SRV

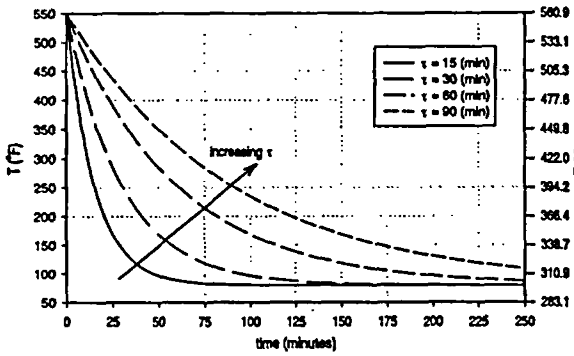
Overall, Petukhov-Catton predicts HTC to be 20% higher than the default RELAP5 models.

Sensitivity study on risk-dominant Palisades transients showed a factor of 3 increase in CPF.

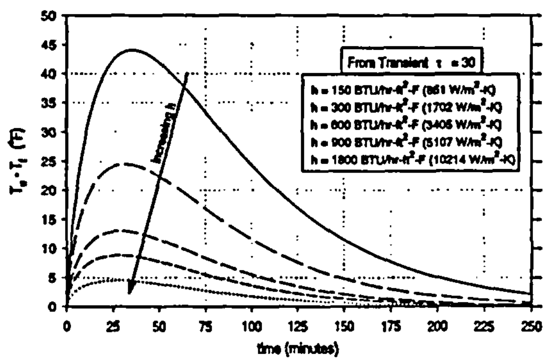
TH-10

C14

Heat Transfer Sensitivity Study



$$T_{dc}(t) = T_{ECC} + (T_0 - T_{ECC})e^{-\beta t}$$

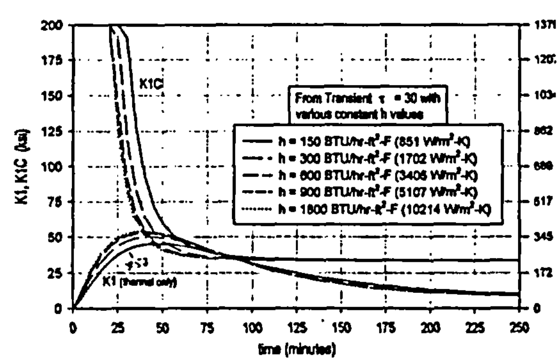


Effect of Varying h

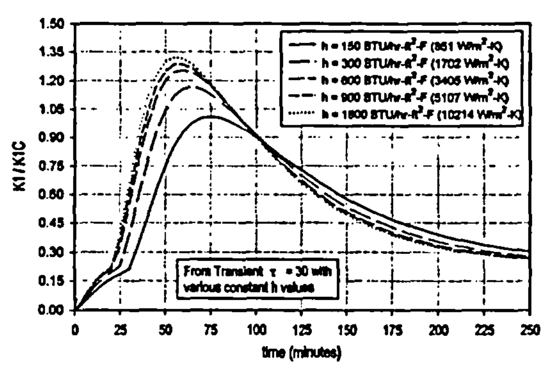
Sensitivity Study by C.F. Boyd and T. Dickson (NUREG-1667, Feb. 1999)

TH-11

Heat Transfer Sensitivity Study



Plot of K1, K1C vs. time



Plot of K-ratio vs. time

TH-12

Conclusions

- RELAP5 predicts pressure and temperature adequately for the PTS analysis.
- Experimental data show large thermal stratification in the cold legs, but nearly uniform downcomer temperature distribution.
- Sensitivity of CPF to heat transfer coefficient uncertainty small compared to the boundary condition variation within a PRA bin.

TH-13

Technical Basis to Support Revision of the PTS Rule (10CFR50.61)

→ *PFM Review Comments, Appendix B*

Mark EricksonKirk

Materials Engineering Branch

ACRS Briefing
NRC Headquarters • Rockville, MD • 2nd December 2004

VG 1

Comment Categories

- **Comments that led to model changes**
- **New (final) comments from reviewers**

VG 2

Comments that led to Model Changes

- 23: Crack face pressure (Schultz)
  - Previously not modeled
- 40: Allowance of upper shelf fracture (VanWalle)
  - Reviewer pointed out unreliability of correlative approach, suggested adoption of a physically-based model [EPRI MRP-101] as both more accurate and more in line with the project's overall modeling philosophy
  - FAVOR modified (FAVOR 04.1) to incorporate upper shelf model per reviewer comments

VG 3

Final Comments

(Schultz)

- Summary
  - *"The work performed shows clearly advancements compared to previous studies. It is well founded in most parts. My major comments are directed to the flaw distribution and connected requirements to the plant specific applicability, as well as some reservation concerning the level of validation of crack arrest."*
- Remaining issues
  - As noted under "summary"
- Recommends
  - Licensees be required to demonstrate appropriateness of the assumed flaw distribution to their vessels.

VG 4

Final Comments
(VanWalle)

- Summary
  - *"The newly proposed PTS-methodology is worked out well and has a logical and acceptable pattern ... The methodology is very well established, explained, and documented in NUREG-1806 ... The reviewer recommends that ... the PFM procedure as implemented in FAVOR 04.1 shall be used in the overall approach of the PTS methodology."*
- Remaining issues
  - Not sampling correlation uncertainties for embrittlement relationships and Charpy to toughness conversions
  - Difficulty in mathematically representing "mixed" uncertainties.
- Recommends
  - Continued in-service inspection to substantiate applicability of flaw distribution to all PWRs
  - Over time, the direct use of fracture toughness measurements made on surveillance specimens instead of correlative approach.
  - Continued / further validation of crack arrest models.

VG 5

Final Comments
(Murley)

- General summary
  - *"The NRC RES staff is to be congratulated for producing the breadth and quality of world class PTS research represented by this material ... While I have some issues & concerns {regarding the PRA, TH, & PFM}" analysis, these concerns do not rise to the level that would seriously challenge the logic of the overall approach or the general validity of the PRA, TH or PFM calculational methods."*

VG 6

Final Comments, Cont.

(Murley)

- **New or remaining issues (PFM only)**
  - **Errors in understanding indicating that improvements in writing are needed**
    - ✓ **Non-conservatism of RT_{AW}**
    - ✓ **Why weld layer model reduces TWCF**
    - ✓ **FAVOR chemistry sampling protocol when multiple flaws are simulated to exist in the same sub region of a vessel**
  - **Need for a more thorough discussion of the residual uncertainties (both conservative and non-conservative) that underlie the proposed RT-based screening limits. Discussion would serve as the basis for determining if "margin" is needed along with the RT-based screening limits.**
    - ✓ **Addressed in this presentation**
  - **Applicability of flaw distribution to all PWRs**

VG 7

Selection of Transition Break Size for 10 CFR 50.46 Revision

Charles Hammer
NRR

Robert L. Tregoning
Lee Abramson
RES

Advisory Committee on Reactor Safeguards
December 2, 2004

Presentation Objectives

- Provide overview of elicitation scope, results, and uncertainty.

- Describe approach for selecting transition break size (TBS).
  - Use elicitation passive-system LOCA frequencies as a starting point.
  - Consider uncertainty and variability in elicitation results.
  - Incorporate adjustments to account for other LOCA frequency contributions.

Elicitation Objectives and Scope

- Develop generic BWR and PWR piping and non-piping passive system LOCA frequency distributions as function of break size and operating time.
  - LOCAs which initiate in unisolable portion of reactor coolant system.
  - LOCAs related to passive component aging, tempered by mitigation measures.
  - Small, medium, and large-break LOCAs examined. Large break category further subdivided to consider LOCA sizes up to complete break of largest RCS piping.
  - Time frames considered: 25 years (current day), 40 years (end of original license), and 60 years (end of life extension).
- Primary focus: frequencies associated with normal operating loads and expected transients.
- Assume that no significant changes will occur in the plant operating profiles.

Other LOCA Risk Contributors

- LOCA frequency contributions occur from other events that were beyond the scope of the elicitation.

- Active system LOCAs.
  - Stuck-open valves.
  - Pump seal LOCAs.

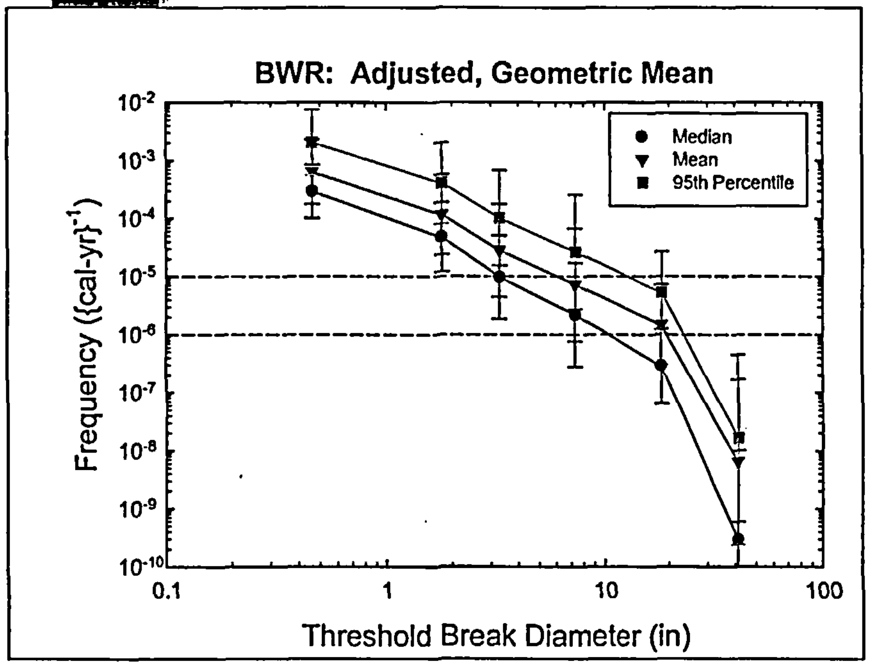
- Seismically-induced LOCAs.

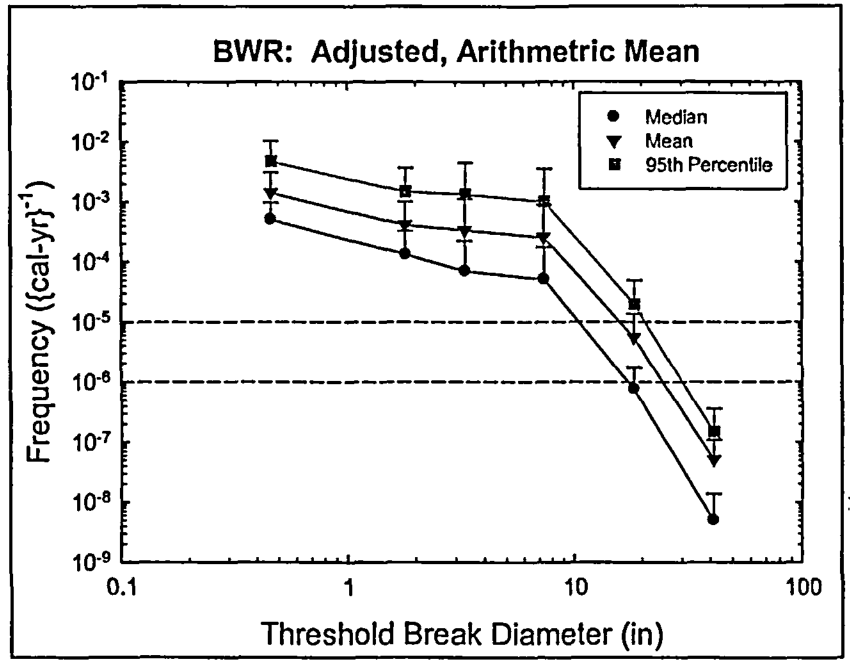
- Other rare event loading LOCAs.
  - Rare water hammer events.
  - Heavy load drops from overhead cranes.

Elicitation Results

- Baseline results developed with measures of both individual uncertainty and group variability.
- Sensitivity analyses conducted in five broad areas.
  - Effect of distribution shape on mean.
  - **Overconfidence adjustment.**
  - Correlation structure of panelist responses.
  - **Aggregating expert opinion.**
  - Panel diversity measurement.
- Baseline results modified by important sensitivity analyses for TBS selection.
  - Results modified to incorporate overconfidence adjustment.
  - Geometric mean aggregation and mixture distribution used to provide range of elicitation results which capture process uncertainty.

Total LOCA Frequencies: BWR Current Day Estimates

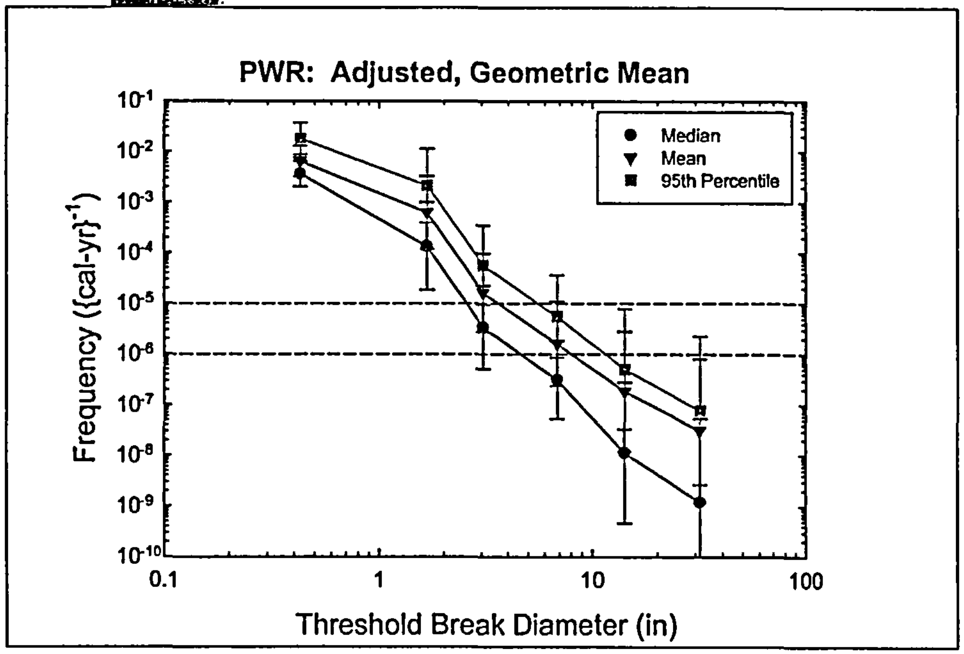


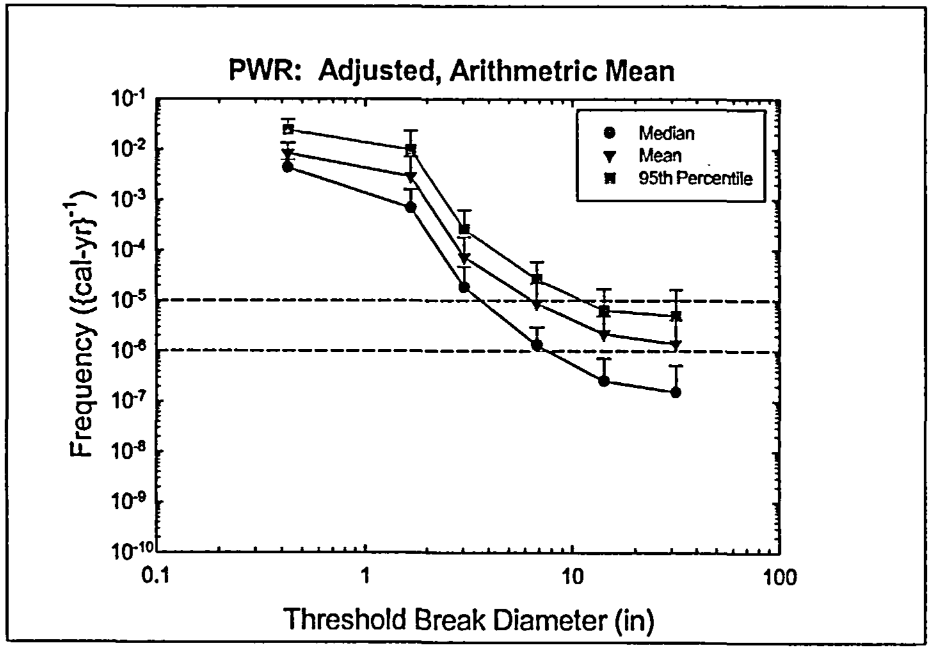


Elicitation Results: BWR Break Size @ 1×10^{-5} ($\{\text{cal-yr}\}^{-1}$)

Aggregation	Mean	Mean with 95% Conf.	95th Per.	95th Per. with 95% Conf.
Geometric Mean	6	16	13	22
Arithmetic Mean	16	19	21	24
Mixture Distribution	16		20	

Total LOCA Frequencies: PWR Current Day Estimates





Elicitation Results: PWR Break Size @ $1x10^{-5}$ ($\{cal\text{-}yr\}^{-1}$)

Aggregation	Mean	Mean with 95% Conf.	95^{th} Per.	95^{th} Per. with 95% Conf.
Geometric Mean	4	7	6	14
Arithmetic Mean	7	10	11	31
Mixture Distribution	7		10	

Selection of TBS

- Use Expert Elicitation results as a starting point.
- There is a range of pipe sizes which correlate to pipe break frequency of 1E-5/cal-yr.
- Selection should accommodate various uncertainties.
- There are other considerations which could impact the TBS selection.
  - Active LOCAs.
  - Load-generated LOCAs.
- Actual plant piping design and operating experience should be considered in final selection.

Other Considerations Not Addressed by Expert Elicitation

- Active LOCAs.
  - Stuck-open valves, failure of seals and gaskets.
  - Generally result in small-break LOCAs.
  - Higher frequency than pipe break LOCAs.
  - Limited in size by the size of associated pipe.
  - Not larger than largest attached pipe to main loop.

- Dropped heavy loads.
  - Studied in NUREGs-0612 and -1774 and in GSI-186.
  - LOCA frequency contribution not significant.
    - Most lifts made during shutdown conditions.
    - Fewer and lighter loads lifted during power operation.

Other Considerations Not Addressed by Expert Elicitation (cont)

- Seismically-induced LOCAs.
  - Seismic events at 1E-5/cal-yr are large magnitude.
  - Some plant sites may have higher failure frequencies especially with piping degradation.
  - Undegraded piping not expected to have significant effect on failure frequency.
  - For small flaw sizes, undegraded and degraded failure probabilities are similar.
  - For worst case flaw sizes, unacceptable increase in failure probabilities.
  - Confirmatory studies are ongoing and to be published.
  - Guidance to be provided by the staff in RG.
  - Licensees need to ensure inspection plans are adequate such that no breaks larger than TBS expected.

Other considerations Not Addressed by Expert Elicitation (cont)

- Water hammer.
  - No water hammers expected during normal operation.
  - Condensation-induced water hammer following SBLOCA (NUREG/CR-3895).
  - Narrow range of SBLOCAs (plant-specific) requiring level drop and higher pressures believed to be low probability.
  - Licensees need to ensure plants are not susceptible to damaging water hammer.
  - Screening criteria to be provided by the staff in RG.

Preliminary TBS Selection and Consideration of Actual Piping

- Certain piping is more susceptible within their size range.
  - Pressurizer surge lines in PWRs (thermal fatigue).
  - Feedwater lines in BWRs (flow accelerated corrosion).
- Preliminary TBS values of 14" for PWRs and 20" for BWRs.
  - Includes necessary adjustments for uncertainties.
  - Includes pipes of most concern.
- These sizes are also similar to the sizes of the largest pipes attached to the main loop piping.
  - Pressurizer surge line in PWRs.
  - RHR suction line and feedwater line in BWRs.
  - Actual sizes do vary somewhat among plants.
- The next larger size pipes are the large main coolant loop piping.
  - Hot and cold legs in PWRs.
  - Recirculation and main steam piping in BWRs.

TBS Selection

- TBS is selected as the size of the largest pipe attached to the main coolant loop.
  - For PWRs, based on the size of the largest pipe attached to the cold or hot leg main loop piping.
  - For BWRs, based on the size of the largest pipe in either of the RHR or Feedwater systems inside primary containment.
  - Next larger pipes are significantly less likely to break.
  - Accommodates uncertainties and provides regulatory stability.

- TBS is actually defined in the proposed rule as twice the cross-sectional flow areas of these size pipes.

Future Plant Modifications and TBS Adjustments

- Expert elicitation did not consider power uprate conditions in estimates of break frequencies.
  - Recent operating experience indicates higher rates of degradation are possible.
  - Licensees need to explain why future uprate conditions do not significantly increase break frequencies.

- The staff will continue to assess pipe break frequencies and update as necessary.

- However, because significant uncertainties are accommodated, staff does not expect to have to adjust TBS in future.

TBS Selection Summary

- Elicitation results used as starting point for TBS selection.
  - Consider individual uncertainty, panel variability, and process sensitivity.
  - Determine break size at $1x10^{-5}$/cal-yr associated with different elicitation metrics.
- Other considerations not evaluated in the elicitation were incorporated to adjust TBS.
- The final TBS selections are risk-informed, accommodate attached piping of most concern, and promote regulatory stability.

Tracking the Cumulative Change in Risk

Stephen Dinsmore
Probabilistic Safety Assessment Branch – NRR
Advisory Committee on Reactor Safeguards
December 2, 2004

Issues Identified by ACRS for Additional Clarification

- The Rule Requires the Licensee to Estimate and Track the Cumulative Impact on Risk of all Changes Related to Redefinition of LBLOCA

- The Rule Prohibits Combining the Risk Impact of Unrelated Changes with Changes Related to Redefinition of LBLOCA in the Cumulative Impact

  - A Related Change is any Change Enabled by the New Rule and not Permitted by the Current Rule

Cumulative Change in Risk

- The Proposed Rule Requires the Combination of the Risk Impacts of Related Changes Made at Different Times
  - Compare the Total Change in Risk from all Related Changes to the Acceptance Guidelines
  - The same concept is utilized in all current risk-informed applications since they provide for measurement and control of cumulative risk from related changes

- Change in Risk - Initial and the Final Risk Must be Estimated Using an Updated PRA
  - (the current facility configuration <u>excluding</u> all related changes)
    - » Compared to
  - (the current facility configuration <u>including</u> all related changes)

Justification for Tracking Cumulative Change in Risk

- There may be a Wide Variety and Large Number of Changes that are Enabled by the Rule

- The Combined Impact of the Changes Enabled by the New Rule should not Result in an Inordinate Increase in Risk or Create New Vulnerabilities

- Consistent with RG 1.174 and Numerous Risk-Informed Applications

- Requirement on Processing and Tracking Changes Precisely Defined Because this is a Rule

Cumulative Change in Risk – Original Applications

- RG 1.174 - the cumulative impact from all risk-informed changes should be available when necessary
  - Cumulative impact of previous changes should be submitted when acceptance guidelines are approached
- RG 1.175: Inservice Testing
  - The cumulative impact of all RI-IST program changes (initial approval plus later changes) should comply with the acceptance guidelines
- 50.69 Categorization
  - If the categorization of SSC's is done at different times, the sensitivity study should consider the potential cumulative impact of all SSCs categorized
  - The results of the risk sensitivity study must be confirmed to still be acceptable following each revision or update of the PRA to ensure that the categorization process is maintained valid

Cumulative Changes in Risk – Rulemaking

- 50.46a Proposed Implementation
  - Must Estimate and Track Cumulative Change in Risk from all Related Changes
  - Changes that Cause the Cumulative Risk Increase to Exceed Sufficiently Small will not be Permitted
  - If the Cumulative Increase Exceeds the Sufficiently Small Guidelines Following PRA Updates the Licensee must take “appropriate action”

Combined Change Requests

- Changes Unrelated to LB LOCA Redefinition Will not be Allowed to Directly Offset Risk Increases from Changes Related to LB LOCA

- If Direct offset was Allowed
  - The Change in Risk - Initial and the Final Risk Must be Estimated Using an Updated PRA would become
    - (the current facility configuration <u>excluding</u> all related **and "selected" unrelated** changes)
      - » Compared to
    - (the current facility configuration <u>including</u> all related **and "selected" unrelated** changes)

Justification for Not Allowing Combined Change Requests

- All Changes at the Facility that Impact the Risk Profile of the Plant may also Indirectly Impact the Risk of the 50.46 Changes – this Impact is Always Included in PRA Updates and the Cumulative Change in Risk Estimate

- There may be a Wide Variety and Large Number of Changes that are Enabled by the Rule

- RG 1.174 Cautions that Staff should not Allow Risk tradeoffs in Combined Change Request that could Create Significant Sequences or Vulnerabilities

- Rule would Require Criteria to Specify what must be Combined and what can not Combined – Significant Challenge to Develop Appropriate Criteria in Rule Language that Provides the Necessary Confidence that such Vulnerabilities are not Created.

- CCR would only be Necessary if the Risk Increase Exceeds the Significantly Small Guidelines and the Staff Believes that many Changes could be Made without Challenging these Guidelines

Combined Change Request Rulemaking

- 50.46a Proposed Implementation
  - Unrelated Changes may not be Combined with Related Changes in the Cumulative Risk Impact
  - However, there may be Situations where a New Plant Improvement Results in a very Desirable Safety Enhancement
  - Licensee could make an Exemption Request using 50.12(a)(2)(iv)

    The exemption would result in benefit to the public health and safety that compensates for any decrease in safety that may result from the grant of the exemption

Regulatory Analysis for 10 CFR 50.46 Proposed Rule

ACRS Briefing
December 2, 2004

Brian Thomas, NRR Financial and Regulatory Analysis Section
U.S. Nuclear Regulatory Commission

10 CFR 50.46 Regulatory Analysis (Redefinition of Large Break LOCA)

- Enabling rule (licensees may voluntarily choose this rule in lieu of current ECCS requirements)
- Licensees can change aspects of facility design and operations
- Intent is to enable flexibility in design and operations; rule can also contribute to improving safety

10 CFR 50.46 Regulatory Analysis

Examples -

<u>Operational Enhancements Identified by Industry</u>

- More power uprates
- Relaxation of emergency diesel generator start times
- Improved fuel management
- Changes in number of accumulators
- Potential for load removal in load sequencing of electrical equipment

10 CFR 50.46 Regulatory Analysis

- Some design and operational benefits are not quantifiable at this time
- Safety benefits will vary on a plant-specific basis (no specifics on what licensees will do)

Regulatory Analysis Approach

- Builds on WOG submittals from 2000 and 2004
  - PWRs to benefit from uprates and EDG tech spec changes
- Uses scenario approach for bounding uncertainty
  - Due to different levels of participation by PWRs and different degrees of possible power uprates
- Assumes all 69 PWRs will obtain license renewals

Quantified Benefits

Value of Increased Power from Uprates

Plus

Labor and Materials Savings from Reduced Scope of EDG Maintenance (mechanical work on EDGs)

Plus

Replacement Power Savings from Reduced Outage Time for EDG Maintenance (cost of replacement power)

Bottom Line

- Although many potential operational benefits – including safety – cannot be quantified,the rule can be justified based on the quantifiable benefits and costs

- The net present value of the proposed rule is positive, regardless of discount rate or scenario, and is estimated at $700 million to about $13 billion

- For any given discount rate, the economic value to society increases as more plants undertake greater uprates facilitated by the rule

Background

Selected Tables

Net Present Value in 2004 in millions of 2004$
Annual Discount Rate = 7%

Quantitative Attributes		Present Value Estimates (Millions of 2004$)		
		Scenario 1	**Scenario 2**	**Scenario 3**
Power Uprating Benefits		$1,151	$3,108	$8,633
EDG Benefits		$237	$213	$178
Licensee Costs	Capital Costs	($185)	($500)	($2,493)
	Initial Licensing Costs	($120)	($108)	($91)
	Recurring Monitoring/PRA Costs	($353)	($317)	($266)
NRC Costs	Initial Regulatory Costs	($21)	($24)	($29)
	Deferred/Recurring Regulatory Costs	($11)	($10)	($9)
Overall Net Present Value		$697	$2,362	$5,923

Note: Totals are subject to round-off error

Net Present Value in 2004 in millions of 2004$
Annual Discount Rate = 3%

Quantitative Attributes		Present Value Estimates (Millions of 2004$)		
		Scenario 1	Scenario 2	Scenario 3
Power Uprating Benefits		$2,148	$5,801	$16,113
EDG Benefits		$429	$386	$321
Licensee Costs	Capital Costs	($215)	($582)	($2,900)
	Initial Licensing Costs	($128)	($115)	($97)
	Recurring Monitoring/PRA Costs	($682)	($613)	($514)
NRC Costs	Initial Regulatory Costs	($25)	($28)	($34)
	Deferred/Recurring Regulatory Costs	($23)	($21)	($18)
Overall Net Present Value		$1,504	$4,829	$12,871

Note: Totals are subject to round-off error

Power Uprate and EDG Scenarios

Scenario	Participation	# Plants	Degree of Power Uprate
1	100%	69	1%
2	90%	62	3%
3	75%	52	10%

Note: Scenario 3 may even be conservative

Summary of Estimated 50.46a Direct Application Costs to Licensee (2004$)

Best estimate of the cost of implementing the proposed rule

Activity	Burden	Estimated Cost
ECCS Re-Analysis based on new TBS (pipe break size)	2,500 hours	$392,500
Describe Proposed Change	700 hours	$109,900
Engineering Analyses	2,500 hours	$392,500
Develop Monitoring Plan	850 hours	$133,450
Synthesize Proposal	540 hours	$84,780
License Amendment Process	384 hours	$60,288
	Upfront Total	$1,173,418
PRA Updates (Reevaluations)	400 hours @ 3 years	$62,800 @ 3 years
Implement Monitoring	1,150 hours/year	$180,500/year

Summary of Estimated 50.46a Costs to NRC (2004$)

	Review Submission	Process License Amendment
Scenario 1	1,248 hours x $88/hour = $109,800 per application	200 hours x $88/hour = $17,600 per application
Scenario 2	2,340 hours x $88/hour = $205,900 per application	200 hours x $88/hour = $17,600 per application
Scenario 3	5,070 hours x $88/hour = $446,200 per application	200 hours x $88/hour = $17,600 per application

Note: Review hours increase as level of power uprates increases

Estimated NRC Costs (2004$)

- Prepare Reg. Guide(s) - $402K
- Review Submissions and Process License Amendments over three years (3% discount rate)

Scenario 1	$24.2 million	(all 69 units)
Scenario 2	$27.0 million	(62 units)
Scenario 3	$33.7 million	(52 units)

- Review PRA Updates - 200 hours per review
- Review LOCA Frequencies - $2.4 million (10-year review)