Official Transcript of Proceedings ACRST-3308

NUCLEAR REGULATORY COMMISSION

ORIGINAL

Title: Advisory Committee on Reactor Safeguards
520th Meeting

Docket Number: (not applicable)

PROCESS USING ADAMS
TEMPLATE: ACRS/ACNW-005

SISP REVIEW COMPLETE

Location: Rockville, Maryland

Date: Thursday, March 3, 2005

Work Order No.: NRC-265 Pages 1-326

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

TR04

DISCLAIMER

UNITED STATES NUCLEAR REGULATORY COMMISSION'S

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

March 3, 2005

The contents of this transcript of the proceeding of the United States Nuclear Regulatory Commission Advisory Committee on Reactor Safeguards, taken on March 3, 2005, 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)

520th MEETING

+ + + + +

THURSDAY, MARCH 3, 2005

+ + + + +

ROCKVILLE, MARYLAND

+ + + + +

The committee met at the Nuclear Regulatory Commission, Two White Flint North, Room T-2B3, 11545 Rockville Pike, at 8:30 a.m., Graham B. Wallis, Chairman, presiding.

COMMITTEE MEMBERS:

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

ACRS/ACNW STAFF:

JOHN T. LARKINS, Executive Director, ACRS/ACNW

SAM DURAISWAMY, Technical Assistant

MEDHAT EL-ZEFTAWY

MICHAEL SNODDERLY

PANELISTS:

GREG GRECHECK, Dominion Resources

WAYNE HARRISON, Chairman, Westinghouse Owners Group LBLOCA Redefinition Working Group

TONY PIETRANGELO, NEI

NRC STAFF:

LEE ABRAMSON, RES

GOUTAM BAGCHI, NRR/DE/EMEB

DAVID BESSETTE, NRR

STEVE DINSMORE, NRR/SPSB

LAURA DUDES, NRR/DRIP

RICHARD DUDLEY, NRR Rulemaking Section

MARK ERICKSONKIRK, NRR

PETER GRIFFITH, NRR

GARY HAMMER, NRR/DE

ALAN HISER, RES/DET/MEB

RALPH LANDRY, NRR/DSSA

JAY LEE, NRR

CLIFF MUNSON, NRR

JACK ROSENTHAL, NRR

NRC STAFF:

MARK RUBIN, NRR/SPSB

MICHAEL SCOTT, NRR

BRIAN SHERON, NRR/ADPT

ROBERT L. TREGONING, Office of Research

MICHAEL TSCHILTZ, NRR/SPSB

A-G-E-N-D-A

Opening Remarks by the ACRS Chairman 5

Revised Draft NUREG on Expert Elicitation on Large-Break LOCA Frequencies 7

Proposed Rulemaking Package for Risk-Informing 10 CFR 50.46 81

Draft Safety Evaluation Report Related to North Anna Early Site Permit Application 176

Technical Basis for Potential Revision of the Pressurized Thermal Shock Screening Criteria in the PTS Rule 239

Adjourn 326

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

8:27 a.m.

CHAIRMAN WALLIS: Good morning. The meeting will now come to order. This is the first day of the 520th meeting of the Advisory Committee on Reactor Safeguards.

During today's meeting the Committee will consider the following, draft NUREG on Expert Elicitation on Large-Break LOCA Frequencies, proposed rule-making package for risk informing 10 CFR 50.46, draft safety evaluation report related to North Anna early site permit application, technical basis for potential revision of the pressurized thermal shock screening criteria in the PTS rule, and the preparation of ACRS reports.

Several of these are particularly significant items. And I think we're going to be quite busy. 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 from members of the public regarding today's sessions.

We have received requests from Mr.

Pietrangelo, NEI, and Mr. Harrison, Westinghouse Owners Group for time to make oral statements regarding risk informing 10 CFR 50.46.

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

Before we get started there are some items of current interest. In the handout of items of interest you'll note that there's an SRM that states that the ACRS or ACNW should continue to review major research projects addressing nuclear safety issues.

So we continue to do that with an SRM. And there's also, you'll note, a couple of presentations by Commissioner Merrifield in here. Now, you probably know that Mag Weston, who has been with the ACRS staff for five years, is retiring on April the 1st.

And, on behalf of the Committee, I'd like to thank her for her outstanding technical support of the Committee in reviewing several technical issues, including reactor vessel penetration cracking, reactor vessel head degradation, reactor oversight process, the mitigating systems performance index program, and construction authorization requests for the MOX fuel fabrication facility.

She was also responsible for coordinating the preparation of the ACR's action plan and the subsequent revision. I note that she also did several other things not listed here.

Thank you Mag, and good luck in your future endeavors. Also, I believe you all know that this is the last meeting of the ACRS that Peter Ford will attend as a member.

I'd like to express our appreciation of his contributions to the Committee and our pleasure having him as colleague. Thank you Peter. Now we will proceed with the meeting.

And the firs item, Draft NUREG on Expert Elicitation on Large-Break LOCA Frequencies, I'll ask Professor Apoltolakis to take us through that, please.

MEMBER APOSTOLAKIS: Thank you Mr. Chairman. The purpose of our meeting today is to review the revised draft NUREG report on estimating LOCA frequencies through the expert opinion elicitation process.

And, of course, this report was developed in support of the risk-informed revision to emergency core coolant system requirements 50.46. We issued --

we reviewed the version dated November 4^{th}, 2004 of the report.

And we issued a letter in December, December 10^{th} of last year. We received an EDO response on February 4^{th}. There were four, I would say, major -- although they're not all of the same significance -- issues that we raised in our report of December 10^{th}.

The first one had to do with our explanation of what the objective of the expert opinion elicitation was, what -- we saw the word genetic frequency a lot, and, in particular, whether plant-to-plant variability was considered in the estimates.

The second comment in our report had to do with whether all the experts understood the questions that were posed to them. And there appear to be some confusion from some of the experts that were present in our deliberations here regarding the flow rate.

The third one appears to still be a point of disagreement between the authors of the report and at least some members of the Committee. And it has to do with the averaging method -- the method that is used to average the individual member opinions and estimates.

And the final comment, the fourth comment, had to do with our request that the authors of the report state clearly, if they could, that the distribution they developed, based on all the sensitivity studies they did, that that distribution of the frequency of LOCAs represented the expert community's views and not just that annex.

Because, this Agency makes decisions based on the state of the art, not on what six people think, even though these six people might think very prominent.

I was looking again at the revised draft rule -- this morning in fact. And it seems to me that even though we may disagree on several things that the report does, the overall contribution to the revision of 50.46 is good in the sense that the proposed transition break size in the revised rule is greater than the sizes that correspond to 10 to the minus five frequency that you get in the report independently of what method you use.

In other words, what the Staff is going with is the conservative estimate of TBS. So, on the one had, we might say there is a positive contribution of the report in the sense that now we know that, no matter how one process the information from the experts, the regulatory staff is proposing a break size that is higher.

So that's good to know. On the other hand, given that this report might be used in the future by other people and so on, one would have to be more careful about the methods that are used and what is proposed.

So, the disagreements then have to be resolved. So, with these happy notes and observations, I'd like to turn the meeting over the Staff.

And I understand Dr. Alan Hiser wants to make a few comments first, please.

DR. HISER: Good morning, Dr. Apostolakis and Committee members. My name is Alan Hiser. I'm the Chief of the Component Integrity Section of the Office of Research.

As you described, we are here to discuss our revised draft NUREG. I guess what I would note is that this would be, over the last twelve months, our sixth briefing of either sub-committee or the full committee.

This report has been reviewed by ACRS. We've had two external peer reviewers, NRC internal peer review. We are here today to discuss two parts of the NUREG.

One are the changes that we've made since the Committee last reviewed the report and also to discuss our responses to the ACRS letter. At the end of our presentation we'll be seeking to release the NUREG for public comment.

And we'll request a letter from ACRS to that effect. With that, we look forward to a constructive dialogue this morning. And I'll turn it over to Rob.

MR. TREGONING: Thanks Alan. Good morning audience and Dr. Apostolakis and the rest of the ACRS. I wanted to thank you for providing us the opportunity to come in front of you today and, further, additionally discuss some of the, I'll say, remaining issues that we may have to try to resolve prior to, hopefully, our release of this document for public comment.

I'm just repeating the objectives I think George and Alan really summarized pretty well. But the objectives of this presentation, one, as Dr. Apostolakis mentioned, you have reviewed a preliminary version of the draft that was dated November of '04.

We want to walk you through what the major changes in this latest version is so that when you do your subsequent review this hopefully will allow you just to focus on pertinent areas.

So, the first thing we want to do is just very systematically walk you through what's different, the major things that are different. And then probably the more meteor portion of the talk is going to be the discussion of the ACRS comments that we received in your letter dated December 10th, and then our subsequent response to those comments in the letter as Dr. Apostolakis mentioned, dated February 4th.

And, as Dr. Hiser mentioned, we are here. And the ultimate objective is to hopefully we can come to a successful resolution of these differences or at least an agreement on the best path forward so that we can move forward with releasing this document for public comment.

I think Dr. Hiser mentioned this, that we've been in front of ACRS numerous times throughout the elicitation process. It has been our goal to keep ACRS fully informed as we -- not only as we develop the process, but as we started to work through it.

So, this is just a continuing dialogue that we've tried to maintain with ACRS throughout the whole process. And, because of that, we're really just, you know, don't want to revisit old ground here.

I think we've discussed a lot of these issues fairly extensively. And we just want to focus on -- I'll say there's really only a few areas of disagreement that we have right now.

Now, since we were last in here, you see about the bottom of the slide, I just wanted to indicate what we've done with respect to the program and what milestones we've completed since we were last in here in December.

We have completed the draft NUREG that we supplied to you for review prior to this meeting. And, in this draft NUREG, we incorporated revisions in an attempt to address comments that we received in the December 10th letter from ACRS.

And we submitted that revised draft NUREG for both NRR and ACRS. I just want to -- I'm going to mention the comments that we got in the letter up front.

And then I'm going to walk you through the major changes. And the reason for mentioning these up front is, when we look at the changes, we'll say this change was to address ACR comment whatever.

So I just wanted to enumerate what those comments were. Again, Dr. Apostolakis stated these comments already. But I just want to make sure that we're clear what we're talking about today.

The first comment was that the report should include a better explanation of what a generic frequency value for the plants means, and to what extent plant-to-plant variability affected the results.

The second comment in the letter was that the report should state clearly what the understanding of the experts was when they answered questions about LOCA size categories.

The third comment was this practice and the practice that was being discussed is geometric averaging as it varies with the methods employed in references five through seven.

And those references are NUREG 11.50, the EPRI document on the seismic PRA, the hazard determination, and then also a companion report that talks about expert elicitation procedures with respect to the seismic hazard curve analysis.

So, the practice is at odds with those references. And all of those references used an arithmetic type averaging method to construct probability distributions of expert opinion.

And then the fourth comment was that the final distribution reported in the executive summary should be the composite distribution that the analyst -- and by analysts they mean authors of the report -- based on the sensitivity analyses, believe represents the expert community's current state of knowledge regarding LOCA frequencies.

So, these were the four comments. And, again, Dr. Apostolakis has already indicated what they are, has already summarized these. So, the next few slides will just walk you through what changes we've had.

And this first slide really deals with the areas that we have really minimal changes. The first bullets up there just is -- you know, this is probably a nuisance point.

But we've re-lettered all the sections. So we had executive summary previously lettered as section A. Well, that's up front now. So then all the sections go up one.

If you were comparing section H previously, which was quantitative results, that's section G now. So we apologize for that nuisance. And hopefully it hasn't caused too much consternation.

I just wanted to make that clear. Most of the sections in the report we -- you know, between the

11/04 draft and the draft that you've recently received, there's really no changes.

Or I would categorize them as minor editorial type changes. And that includes the background section, the objective and scope section, the base stage results section, the qualitative results and discussion section, and then the section where we talk about ongoing work.

So those chapters of the draft NUREG are essentially unchanged. Now, section C, which is the section on elicitation approach, we did go back in and add some clarification specifically to address the second comment which was, you know, understanding that the experts were -- what were they providing answers to with respect to break sizes.

So, we added some discussion, and specifically in section C7, which deals with the development of the flow rate correlations and how they were used within the elicitation.

So, when you review that section, you should -- I mean, this should be clear. And that new language is in there to make sure it's very clear how the elicitation was structured.

Now, later one, we're going to specifically address the ACRS comments. And I'll go into what exact language we use. Right now I'm just trying to provide an overview.

CHAIRMAN WALLIS: This flow rate thing always seemed to me backwards. These are experts in pipe rupture, aren't they? And the question they're going to ask is will this pipe break?

They're not going to ask, will I get 10,000 gallons per minute. That means nothing to them.

MR. TREGONING: Of course.

CHAIRMAN WALLIS: So it seems very strange to define the problem in terms of flow rate. The problem is in terms of -- should be defined in terms of will a pipe break, how will it break, and what kind of a hole are you going to get when it does break?

MR. TREGONING: Right. And when we define the LOCA categories, realizing there's a lot of historical context involved in how LOCA categories have been defined.

They've been historically defined on a flow rate basis because the flow rate distinction is more important because it has implications in terms of what system performance is required.

You know, are you going to need HPIS, LPIS pumps? You know, what the system response is going to be. So, when we define the elicitation category, we stuck with those historical definitions, expanded them somewhat so that we could more definitively evaluate large break LOCAs, I'll say, with a finer -- a larger amount of categories.

But we certainly realize that the experts that we had, they are experts in degradation mechanisms. There were no plant systems expertise with respect to thermal hydraulic response for mitigating breaks.

So that's why we needed to develop the correlations and relate those categories to effective break sizes that the experts then took and used in their elicitation.

CHAIRMAN WALLIS: And then the peculiar thing --

MR. TREGONING: But we did want to tie them back to those historical definitions.

CHAIRMAN WALLIS: You converted them to single-ended breaks, as if the pipe is going to break and only have one end. It seems, again, a very odd thing to do.

MR. TREGONING: No, it's not.

CHAIRMAN WALLIS: Most --

MR. TREGONING: It's not a single-ended

break. It's, again, the way the correlations were developed is the initial definitions were based on flow rate.

We related the flow rate to pipe to break areas. And then all we did is we took those areas and calculated and effective break diameter.

CHAIRMAN WALLIS: For a single --

MR. TREGONING: Assuming that those areas are --

CHAIRMAN WALLIS: For a single --

MR. TREGONING: It's an effective break hole.

CHAIRMAN WALLIS: One hole?

MR. TREGONING: One hole.

MEMBER APOSTOLAKIS: Yes, in fact -- are you coming back to it?

MR. TREGONING: Yes.

MEMBER APOSTOLAKIS: To this issue later?

MR. TREGONING: Yes, I'll talk exactly about the language we use.

MEMBER APOSTOLAKIS: Because I, in the EDO response of February 4th, we I suspect you guys have something to do with, there is a sentence that is not clear to me.

Thus the LOCA frequency associated with

each LOCA size category relates to the cumulative frequency of a single-ended break of the site's size, and all larger breaks, including double-ended breaks of that size and larger pipe.

I'm having a problem understanding this. What does that mean?

MR. TREGONING: Well, again, realizing how the categories were defined in the elicitation, we were asking for frequency contributions for that size and height. So, the frequency --

MEMBER APOSTOLAKIS: Independently of whether it's double break or -- it's just a size.

MR. TREGONING: It's a size.

MEMBER APOSTOLAKIS: It's a size, okay.

MR. TREGONING: It's a size. So, if you look, let's say, you know, category 3, which was a flow rate of 5,000 GPMs --

MEMBER APOSTOLAKIS: Right.

MR. TREGONING: We're looking for -- and it's greater than 5,000 GPM flows. So we're -- I think we, for PWRs, that ended up being a three to four inch break size.

So, we're looking for frequency contributions for breaks of that effective diameter and higher. So that's what's meant by that statement, that it incorporates not only -- so, if you had a reactor.

Let's go to the biggest category, category 6, which is essentially -- to get to the biggest category you need to have some failure in the main recirculation piping.

Okay, so when you go to category 6, that would incorporate not only, I'll say, a single hole -- let me put it that way -- a single hole in the reactor piping, but it would also incorporate a double ended guillotine break as well. So that's what was meant.

MEMBER APOSTOLAKIS: Of smaller size?

MR. TREGONING: Of a larger size. Well --

MEMBER APOSTOLAKIS: Oh, a larger size?

MR. TREGONING: Yes.

VICE CHAIRMAN SHACK: That's a cumulative distribution rather than a density distribution.

MR. TREGONING: Right.

MEMBER APOSTOLAKIS: But, if all pipes broke with two ends, and you said -- used your method, it seems to me you'd always be displacing the coordinates by a factor of two in terms of size because you wouldn't have a single-ended break.

So your single-ended break area would have nothing there. It would have bigger things, which the first point would be twice as big. But that would be plotted as if it were the single-ended break.

So all the points would be displaced by this factor of two when your --

MR. TREGONING: I'm having trouble seeing that. Because, if you had -- look, the type of break -- if you truly had a double-ended guillotine break, you know, depending on the system, that would -- you could get dramatically different --

MEMBER APOSTOLAKIS: Suppose that you have 5,000 gallons and that corresponds to a five inch pipe, one end broke. And they asked the question, what's the frequency of pipe breaks of that size or bigger?

That's your question. Well, suppose that when five inch pipes break they only break with double ends. Then there's no point of five inch. The first point is at twice that. Well, you could plot it as if --

MR. TREGONING: You could have --

MEMBER APOSTOLAKIS: -- it were the single end. You see what I mean?

MR. TREGONING: Right. You could have -- you could potential -- and this is a George question. You could potentially have smaller pipes that --

MEMBER APOSTOLAKIS: Had two ends.

MR. TREGONING: That had double ended breaks that would be --

MEMBER APOSTOLAKIS: So you might have some real points as well.

MR. TREGONING: As well.

VICE CHAIRMAN SHACK: Or you could have a large crack in a larger diameter pipe.

MR. TREGONING: That's right. So it includes partial breaks as well.

MEMBER APOSTOLAKIS: I understand that. It's just that, this isn't how you do this. This isn't independent of the way in which pipes actual break.

And the way in which pipes actually break has a potentiality to move things around a bit.

MR. TREGONING: Yes. No, that's exactly right.

MEMBER BONACA: I think it's the way that the break is selected in the rule that gives that sense, that you're bounding -- you're really -- you're taking, for example, the largest pipe attached, so, for example the -- and so, it gives you the sense that you have a double ended, but in reality, that's not the case.

MEMBER APOSTOLAKIS: They do say in the revised rule that they consider the largest pipe attached, which is from the pressurizer, right? They say they consider only one side.

MEMBER BONACA: They consider only one side?

MEMBER APOSTOLAKIS: One side. Because that's what matters from the hot --

MR. BISHOP: Excuse me, this is Bruce Bishop from Westinghouse. I was a member of expert panel. And I just want to reinforce something that Dr. Shack just said to contradict an impression that was stated earlier.

And that was the probabilities of having double ended break are very, very small for all pipe sizes, except the very small pipe sizes due to, typically, vibration of socket welds.

The probability primarily come from small slits in bigger pipes. And those are much more probable. Again, they are very small. But they are still much more probable than a double ended break.

And, at least for the PFM team members, and we shared our results, okay, with the other teams also. And there is no database -- in the database there are no double ended breaks.

Okay, there are lots of leaks. So, even the people that were, you know, the experts on the database, had to make some transition from leaks, the probabilities of having leaks, or big leaks, to breaks.

Again, I think that point is very important. A primary contributor is the small slits much less than -- again, to get -- maybe like you were talking about a 5,000 GPM leak rate.

In a reactor coolant system piping you may only need a flaw that's ten percent of the circumference to give you that flow rate. And the probability of having that flaw is much larger than having a double ended break, even of a six inch pipe or something like that.

The other point is that one of the things that the expert panel was asked to take out to consider is how many pipes of a given size contribute to that overall leak rate because that also factors into that cumulative that Rob was talking about.

It's not just one pipe. You may have multiple pipes that could break. And so, the probabilities have to add up. And so, you have to start excluding certain pipe sizes as you go up in size.

And I think a number of us, that's where the double break sort of got -- that's the way you could exclude a pipe size. If physically the leak rate was greater than that of a double ended break then you didn't have to consider that pipe size as being a contributor.

You could exclude that from the cumulative numbers you had to add up. Now, that's a lot of things to keep in your mind. But those were just some of the considerations that I know were discussed with all the panel members.

Now, I can't say that everybody agreed with that. But at least we all discussed that together and talked about that. So I think we were all sort of aware of that.

CHAIRMAN WALLIS: That was very helpful to me, thank you.

MR. TREGONING: Thank you Bruce. So, this next slide deals with -- we're starting to deal with the sections that we have more substantive changes since the November '04 draft report.

This slide deals with section E. And that section deals with the analysis of the elicitation responses. We really had two types of changes that we have here.

The 11/04 draft, we had some inconsistencies between the description in section E and the 11/04 document and what was actually done and presented in the quantitative results section.

So, it was just -- we had some inconsistencies that we have to fix. And that's represented by these first changes made in these first three sections, sections E34, E341, E341, on summing distributions, calculating means, and the calculation of the variance and percentiles that we subsequently present in the quantitative results section, section G.

We also added some new sections which describe either additional or modified sensitivity analyses that we also have the result of in section G.

And those sections which were either modified or added include sections on the mean determination, correlation structure, the aggregation parameters, and the mixture distribution aggregation.

Again, we're not -- we hadn't planned to go over the changes today. Some of them are relatively minor. Some of them are more substantive.

But, I just wanted to alert you as you do your review what sections possibly to focus on. Section G, the quantitative results section, as I mentioned previously, the results that we provided you in 11/04 did previously reflect the current analysis methodology.

So, there's no change in the results that we presented between 11/04 and the draft NUREG that you've got in front of you now. We did, in keeping with sections that were added or modified in section E to reflect either additional or modified sensitivity analysis, we have corollary sections in section G that we've either added or modified on mean determination correlation structure, mixture distribution aggregation, and a new section on summary results.

We also added a new section, the summary results section. And this was in response to ACRS comment 4 to provide a recommendation as to what we thought, I'll say, the best encapsulation of the elicitation results were.

We've added a section called summary results, which are based on the overconfidence adjustment using the error factor scheme, however, aggregated currently with the geometric mean approach.

So, I know we're at odds with you on that. And, again, our opinion is those are the best or the improved group LOCA frequency estimates. We also highlighted these summary results in the executive summary.

Those are the results that we use in the new report. And all the comparisons with historical results that we make in section G are with respect to those summary results.

So, there's consistency at least there between what's in the executive summary, comparisons with historical results, and then this summary results section that's in section G.

MEMBER APOSTOLAKIS: So the baseline results do not have any adjustment for overconfidence or anything? And you are not reporting them in the executive summary?

(No verbal response.)

MEMBER APOSTOLAKIS: Okay. What you report there is what you believe after the whole thing is the current distribution of the frequency.

MR. TREGONING: That's exactly correct. And I should have made that point. So I'm glad that you made if for me.

MEMBER APOSTOLAKIS: And the overconfidence adjustment has to do only with the lower part of the distributions, right?

MR. TREGONING: Well, again, just to refresh your memory on how we did those -- how we did that adjustment, we looked at all the error factors associated with each category that we were trying to get quantitative results for.

And we calculated -- so we had, let's say, eight or nine experts that weighed in on a given question. We determined the mean error factor from those eight or nine experts.

And then experts which were below the mean, we adjusted their error factor only, not their middle response, but their error factor.

MEMBER APOSTOLAKIS: And you brought them up to the mean.

MR. TREGONING: We increased their uncertainty. We brought it up to the mean.

MEMBER APOSTOLAKIS: Okay.

MR. TREGONING: But those that were above the mean, we just left them there. We didn't correct them down.

MEMBER APOSTOLAKIS: Right. And the reason was that you felt that the guys with the lower error factor were overconfident?

MR. TREGONING: Yes. Based on -- and Lee may want to weigh in here. But, based on a lot of elicitation work, overconfidence adjustment is a well-known phenomenon.

And, as I mentioned earlier, we have initially planned on doing some sort of correction for everybody on overconfidence. When we started to look at some of the uncertainty regions that we had for some experts, it became clear to us that they may not have actually been overconfident.

In some ways, many of them could have actually been under-confident. But, we didn't decide to correct back that way.

MEMBER APOSTOLAKIS: Do you remember how many experts were overconfident? I mean, according to this.

MR. TREGONING: Well, by definition, I mean, if you had eight experts and you calculated mean --

MEMBER APOSTOLAKIS: Because you went with the --

MR. TREGONING: You'd have four that you'd correct with, approximately four.

MEMBER APOSTOLAKIS: Do you remember what their affiliation was?

MR. TREGONING: There was no --

(Laughter.)

MR. TREGONING: You know, I don't think it asks this question. I did ask this question quite often. Did we notice any, I'll say, organizational effect on either the mean results or the uncertainty results?

And I will say usually no. If I looked at all the experts, there was no systematic differences between organizations. The only thing I will say is with respect to the uncertainty analysis.

There was probably a weak correlation that the industry participants probably tended to be a little more confident than some of the rest. But, it's a very weak correlation.

I wouldn't read too much into that comment.

MEMBER APOSTOLAKIS: Confident in the sense that they are giving you --

MR. TREGONING: That they --

MEMBER APOSTOLAKIS: -- tighter distributions.

MR. TREGONING: Yes. So it's not -- so, again, just realizing for each question we ask for their mid-value responses and then their uncertainty about the response.

So, it wasn't that there was any clear difference in mid-value responses as a function of organization on the expert panel. There was a stronger correlation, again, albeit, it was still relatively weak between their uncertainty associated with that value.

MEMBER APOSTOLAKIS: But, again, if you look only at the error factor, you really don't care where the distribution is, right? The error factor is a ratio for the square root of the 95^{th} to the 5^{th}.

And I wonder -- I mean, this is another example of, you know, the hundreds of ways that one can process this information. Because, you might say, yes, a guy was over-confident.

He gave a narrow error factor in that sense. But he placed the distribution way up there, you know. He was very conservative of where he put it.

So, by adjusting his error factor, I do not know, maybe you're doing some injustice to his estimates. In other words, overconfidence has to include some measure of location too, where the distribution --

MR. TREGONING: That's --

MEMBER APOSTOLAKIS: Not just the spread of the --

MR. TREGONING: That's a valid point. And that's one of the reasons why we settled on the approach that we did. We looked at some of the more, I'll say, classical ways to do overconfidence correction. This is not a classical way.

MEMBER APOSTOLAKIS: Yes.

MR. TREGONING: That was a -- that's a point that, you know, we had some -- because what happens, your median doesn't shift, but your mean can shift dramatically.

MEMBER APOSTOLAKIS: Exactly.

MR. TREGONING: Based on overconfidence.

MEMBER APOSTOLAKIS: The abstract.

MR. TREGONING: And, when we did some of these corrections, the mean shifted, because they had been conservatively placed, to frequencies which just weren't physically supportable.

And I think, you know, we've had past ACRS meetings where we talked about some of the reasons for that and what some of the ramifications were.

MEMBER APOSTOLAKIS: Okay.

MR. TREGONING: And that's another reason that we ended up doing this particular error factor correction.

MEMBER APOSTOLAKIS: As long as we recognize that, you know, overconfidence must be related to the location of the distribution, the estimate too, not just the spread.

MR. TREGONING: Right.

MEMBER APOSTOLAKIS: Lee?

MR. ABRAMSON: Dr. Apostolakis -- Lee Abramson. One thing that we could do, as suggested by your remarks, is we could investigate other sensitivity studies, sensitivity analyses, considering other modifications.

We investigate, as you know, a number of possible ways to do the overconfidence adjustment. However, as far as the error factor correction is concerned, we didn't try to investigate any modifications to this.

But this is certainly possible to do. And I do not know how this would turn out. We could, for example, say one way of suggestion is consider modifying this when it's going to drastically change the location of the distribution.

So, these are things that could be done to see what affect this particular, say, form the overconfidence adjustment was. The reason that we used the error factor adjustment was it was a more or less objective way to do it.

We didn't have to make any particular judgments about the level of the overconfidence adjustment that we did. That was some of the things that we did investigate from the targeted and the adjusted ones.

This was an overconfidence adjustment in which the experts themselves determined how much they had to be adjusted by virtue of their relation to the error factors of the other experts.

So, certainly, we could do some sensitivity analyses, which we haven't done yet.

MR. TREGONING: I'm going to maybe slightly disagree with one of my co-authors on that. I think one of the things we have to keep in mind with this error factor overconfidence correction is, you know, when we did that we did look at the location.

We sort of plot it out. And we have some box and -- plots that we show in the report that shows how specific points move. And the thing we have to keep in mind here is it's a relatively modest correction in the grand scheme of things.

Usually factor of two in the mean frequencies or less. So, you know, I think there's a lot of interesting ways, like you had said, that we could look at evaluating and processing these results.

But, you know, to me -- and I think we tried to do that by looking at -- we looked at three or four different overconfidence adjustment schemes. And I think, by that sensitivity analysis, we've really bounded pretty well the amount of, I'll say, results perturbation that you could do to account for overconfidence.

I don't really know that we, you know, given that we're talking about factors of two or less, that any further perturbation in the error factor scheme is really going to be justified at this point.

MR. BISHOP: Dr. Apostolakis, in the NRC SER for the risk informed ISI method, Dr. Fred Simonen at the Pacific Northwest Laboratory did some studies on the variability in the PFM results, some expected variabilities.

And I know several of the PFM members used that because it seemed to make sense. And what it showed was is that for the very high frequencies where you typically have failure data or something like that driving your failure probability predictions, the variability is fairly small.

But, when you start getting down to very low numbers like 10 to the minus six, 10 to the minus eight where you have very little or no data, the relative uncertainties can be very large, several orders of magnitude.

However, you're talking about whether it's 10 to the minus six, 10 to the minus eight, or 10 to the minus 10^{th}. And so, in an absolute sense, if you were using arithmetic mean, that probably wouldn't have much effect.

But, if you were using a geometric mean, it could have more of an effect because the relative uncertainties are higher.

MEMBER APOSTOLAKIS: Coming back to the composite, one of the major conclusions of this other study that you guys refuse to consult --

MR. TREGONING: No, no.

MEMBER APOSTOLAKIS: That was EPRI, DOE, and NRC, reviewed by the National Academy of Sciences. One of the major conclusions there was that precisely because one can do a lot of -- implement a lot of mathematical schemes to process individual estimates, group estimates, and so on, as we just discussed, the ultimate distribution has to come from the experts, a consensus process, from a consensus process.

Did you ask the experts to bless your final distribution, or is it yours, the authors of the report?

(No verbal response.)

MEMBER APOSTOLAKIS: Whose distribution is it, the one that you report in the executive summary?

(No verbal response.)

MEMBER APOSTOLAKIS: You had eight experts, right?

(No verbal response.)

MEMBER APOSTOLAKIS: Did these eight experts look at what you said, finally this is the distribution? And they said, yes, we agree or I don't violently disagree?

(No verbal response.)

MEMBER APOSTOLAKIS: Or, is it Abramson's and Rob's?

MR. TREGONING: Okay. Yes, we --

MEMBER APOSTOLAKIS: That doesn't mean it is bad if it's yours. But I want to understand whose it is.

MR. TREGONING: Yes, you want to understand the process, right. And the way the process works, or the way the process worked, is we got results from the experts, which we went around with the experts individually to make sure they were satisfied with their individual results. There was a lot of back and forth.

MEMBER APOSTOLAKIS: The individuals?

MR. TREGONING: Individually.

MEMBER APOSTOLAKIS: Yes.

MR. TREGONING: Individually. Then we met last -- about a year ago last February or so where we presented all the results, all the individual results and outlined our aggregation schemes to the experts.

We had a lot of discussion then about what was appropriate and was not appropriate. And then we went off, we finalized the aggregation schemes, and we reported those aggregation schemes.

And, in last July we had another two, two and a half day meeting with all of the experts where we presented the results of the various aggregation schemes.

Now, we hadn't done the mixture distribution aggregation yet, which is -- I'll take issue with the fact when you say we didn't consult the work.

I think we consulted that work quite extensively. And the mixture distribution is in line with what some of the prior work would recommend.

We didn't have that distribution. However, we had the arithmetic mean type of aggregation, which is pretty similar. You get pretty similar results to what you do with the mixture distribution creation.

And we had some discussions among the expert panel. And I will say that probably some of the violent discussions among the expert panel. And, as far as the expert panel, they were -- those that were -- I don't want to speak for everyone, but we heard several people in violent opposition to using the arithmetic mean type of averaging schemes because of the reason they didn't think it represented a consensus type distribution for this elicitation.

MEMBER APOSTOLAKIS: Well, but -- two questions. First, did they agree that your distribution is representative?

(No verbal response.)

MEMBER APOSTOLAKIS: Because, you keep talking about the scheme. Well, it's one thing to talk about the method, and quite another to say, guys, this is it.

This is what we're going with. Did they have a chance to say, yes, this is fine?

MR. TREGONING: Sure. During that meeting they had a chance to weigh in on which aggregation scheme they proposed. Although, you know, we took their recommendation with somewhat of a grain of salt because, again, these are experts in -- these aren't experts in elicitation or aggregation of expert results.

So, we certainly wanted their input. But, I think Lee and I, you know, we wanted to withhold final judgment to do what we thought was right as well.

But, in all honesty, the experts largely agreed with the scheme that we were recommending at the time, the geometric mean aggregation was the most acceptable one that we presented.

The other thing they were violently opposed to was overconfidence correction. And that's a good thing. They should have been violently opposed to that.

MEMBER APOSTOLAKIS: But you did it anyway?

MR. TREGONING: We did some anyway, sure.

MEMBER APOSTOLAKIS: So, you could have done the same thing with the aggregation scheme?

MEMBER APOSTOLAKIS: We could have done the same, of course. Look, that's the role of the integrative facilitator, of course. But, I'm going to mention this later.

We were sort of clear throughout all the elicitation that we were looking to develop consensus type estimates. And that's something --

MEMBER APOSTOLAKIS: When the experts agreed with the distribution that you presented, what was their view? What kind of distribution -- whose opinion did this represent?

Just the group's? Or did they feel that it represented that of the community at large?

MR. TREGONING: Just the group's.

MEMBER APOSTOLAKIS: Does this Agency make decisions based on a group of eight people?

MR. TREGONING: It was a group of 12 experts.

MEMBER APOSTOLAKIS: Twelve people, 20 people. We never do that. We are based on the state of the art. So, the experts should have told us, this is -- if you go out, you know, this is what the community thinks.

MR. TREGONING: We're going to get to this point later.

MEMBER APOSTOLAKIS: Okay.

MR. TREGONING: And, I understand where you're coming from. I think this is state of the art, to be honest. And we'll get to --

MEMBER APOSTOLAKIS: That's not what your response says. And that's not what the report says. The report says that its' impossible to say what the state of the art is.

Which I -- if I were a Commissioner I would be really very upset.

MR. TREGONING: No. I don't think the report says that. The report says it's impossible to say what the expert community -- what the community at large thinks.

MEMBER APOSTOLAKIS: Yes. Because, if I select the experts carefully -- which I think you did, you did do it carefully -- I should be able to figure out from those experts, if I ask the right questions, what the community at large thinks.

MR. TREGONING: Well, let's --

MEMBER APOSTOLAKIS: Yes, we'll come to that. Okay.

MR. TREGONING: We'll come to that. And I think my -- this may be -- you know, I'm going to be optimistic here. This may be a semantic thing as much as -- what you're calling the expert community and what we're saying, you know, we think our panel represents.

So, we're going to discuss that more fully later.

MEMBER APOSTOLAKIS: All right. Keep going.

MR. TREGONING: Okay. So, changes to the draft NUREG abstract conclusion, executive summary statement. I think I mentioned a lot of these already.

The table and figures now reflect the revised summary results. And this was in --

MEMBER APOSTOLAKIS: Yes, I have another -- there were two issues. One is with what we just discussed, the community at large.

MR. TREGONING: Yes.

MEMBER APOSTOLAKIS: But there is the other statement that you keep making the report. And maybe we need to clarify that as well. The key requirement for aggregation is that the group opinion must be somewhere in the middle of the group.

I don't understand that. I really don't. Are you going to -- maybe when we talk about the expert community --

MR. TREGONING: Yes, we'll talk about that.

MEMBER APOSTOLAKIS: All right. Fine, let's go.

MR. TREGONING: Okay, so the executive summary again. The table and the figures in the summary now reflect these revised summary results.

And this is our recommendation that you requested that we do an ACRS comment number for.

We tried to clarify in the executive summary what we meant by generic frequencies. Again, I'm going to specifically tell you what we did here in the next slide.

That was ACRS comment number one. We tired to summarize a rationale for using the geometric mean again and why, at least in the author's opinion, the mixture distribution aggregation is not appropriate, at least for the revised summary results.

And that's your ACRS comment number three. And, again, tried to clarify our opinion that the study results are designed to best represent the expert panel state of knowledge regarding LOCA frequencies.

Now, we still have this issue of, does the expert panel represent the community at large. And then the abstract and conclusions have been revised to make everything consistent with the executive summary.

So, let's get into specifically what we did. And then I think we'll be -- this will lead obviously to the conclusions discussion about the expert community and some of the other issues that Dr. Apostolakis has raised.

But, let's get -- we wanted to get through the first couple of comments first because I thought hopefully we had pretty good agreement that we've handled those correctly now here.

Comment number one, you'd asked -- again, just to refresh your memory -- better explanation of what the generic frequency means. And this was the staff response to the letter as well as we've tried to clarify the executive summary to make this clear.

We had instructed the expert panel to develop generic or average type values. However, they did consider the service history. The service history comes from all plants.

So, by definition, the service history has information about plant specific variability. But, because we asked them to give us the average, really, the only factors that influence a large number of plants, you would expect to significantly influence the average.

And that's why we had given the panels clear instructions to only account for very broad plant specific factors and not specific individual plant to plant variability.

So, by broad plant specific factors, you're looking at factors which may affect a handful of plants, five to ten plants. You get into things like design differences, vendor differences, some of the bigger grosser distinguishing characteristics of plants.

But, you didn't get down to the level of a specific environment or operating history of one specific plant. And, again, we clarified the executive summary to reflect this understanding.

MEMBER APOSTOLAKIS: Is that how we regulate?

(No verbal response.)

MEMBER APOSTOLAKIS: Are the regulations intended to address the average plant? It's unclear to me.

MR. TREGONING: LOCA frequencies have always been developed historically with that in mind, yes. And that was another reason that we tried to be very clear there.

We wanted to be consistent with how LOCA frequencies have been developed and utilized in the past.

MEMBER FORD: Could I follow upon that Rob?

MR. TREGONING: Sure.

MEMBER FORD: So far we have been having

a very useful discussion on the process of how we ascribe to various uncertainties in how the decisions were made.

Much bigger uncertainty, however, is the specifics over the degradation mechanisms. As you know, there's been a whole range of these. And there's going to be a big distribution of what's the likelihood of a crack, for instance, what's the likelihood of various cracking mechanisms?

And these are not taken into account, because you are looking at the generic plant, generic BWR, generic PWR. These are not taken into account.

Those specific degradation uncertainties are not taken into account. Brian Sheron at the last meeting -- I forget -- the last full meeting, resolved that problem for me by saying that, yes, the TBS that you come up with is the average.

But, plant specific issues, such as a BWR on different water chemistry, PWR at different temperatures and things of this nature, if they have a pipe or component lodged in the TBS, then they have to, still in a plant specific basis, apply a 1174 to show that the risk is not going to be -- for that specific plant.

Is that -- did I hear Brian Sheron

correctly?

(No verbal response.)

MEMBER FORD: Because that resolves my problem with all these discussions of uncertainty.

MR. TREGONING: Yes, I don't want to interpret what Brian said. But, he's here, so maybe he would feel so compelled to --

MEMBER FORD: Do I understand this correctly? There's a back -- to this, a plant specific basis, if you have a BWR operating under something like this, then they can make the appropriate case for the larger pipe sizes and TBS to locate? Is that what you said?

MR. SHERON: In other words -- I'm trying to understand what --

MEMBER FORD: The problem I have is that you're defining a TBS for a generic plant.

MR. SHERON: Right.

MEMBER FORD: And anything above that you get exemptions. But, the problem is that if you have a plant which is operating under different water chemistry conditions, for that specific plant they have to make the safety case for those larger pipes or components. I think that's what you said.

MR. SHERON: For plants -- let's put it

this way, we selected a transition break size, which is a generic number, okay, or a generic one. And it's based on the largest attached pipe to the primary system.

MEMBER FORD: Right.

MR. SHERON: So, that is a bit of a plant specific factor. We have said that if a plant, for example, proposes to run at conditions -- I think we used, like for an example, at an up-rated power level, where you might have higher vibration levels, higher temperatures and so forth.

They would have to provide a rationalization for continuing to use that -- in other words, to show that the transition break size hasn't been adversely affected from a probabilistic standpoint by running at these higher conditions.

MEMBER FORD: Right.

MR. SHERON: Does that make sense? That's what I was, I think, trying to get across at the meeting, that we were not just given a blanket okay, you know.

The parameters that were used in the study had to be consistent with the parameters licensees running their plant at.

MEMBER FORD: Thank you. That's the --

MEMBER BONACA: So that you then would address plant to plant variability?

MEMBER APOSTOLAKIS: Well, it is plant specific the way they specify. But that question should come up again.

MEMBER BONACA: I think your question before about regulation was very valiant.

MEMBER APOSTOLAKIS: I don't mean you, I mean the revised group.

VICE CHAIRMAN SHACK: The plant specificity seems to have very little to do with degradation, you know, the size of your largest attached pipe has virtually no connection whatsoever with any degradation mechanism that you do have.

On the other hand, it seems to me that I wouldn't blow this up too much. I mean, the way we run plants today, the variations in water chemistry from one BWR to another, you know, is almost at the limits of measurement of the water chemistry purity.

The specifications are fairly tight. We're dealing with such a limited database. I mean, you know, we are extrapolating -- we're looking for probabilities of six inch holes when, you know, your database, you know, is largely on leaks of a few gallons once you get beyond steam generator tube ruptures.

As you consider the data and you consider the restrictions that these plants are operating in, I'm not sure how I would distinguish between my fleet variability and uncertainty and my plant to plant variability and uncertainty.

I think you're just slicing the bologna finer than you can make it, if you really think that you can get it any finer than that.

MR. TREGONING: But, just to follow-up, I mean.

MEMBER FORD: But all you need is one.

MEMBER APOSTOLAKIS: But remember also that the report claims that safety cultural is not important.

MR. TREGONING: No, the report does not claim that at all. That is not claimed --

MEMBER POWERS: An entirely accurate perception.

VICE CHAIRMAN SHACK: That's not what it says George. It says that the safety culture is not likely to change dramatically. They've built in an assumption about safety culture.

But they don't think it's going to be allowed to get worse. And that's very different.

MEMBER APOSTOLAKIS: But they also say, I think, that variability in safety culture could affect the results significantly.

MR. TREGONING: For a given plant.

MEMBER APOSTOLAKIS: Right.

MR. TREGONING: By all means.

MEMBER APOSTOLAKIS: Which makes it now plant specific. But that effect we are ignoring in this analysis.

MR. TREGONING: Just to follow-up a little bit on what you had said. You know, when we had talked about degradation mechanisms, Dr. Ford, we did talk about the variability.

For instance, PWSEC, we talked about the effect of temperature. And, I know when the -- so, even though we did generic considerations, a lot of the testimony that we go tended to make rather conservative assumptions for how they were estimating the rates of degradation and things like that based on, again, sort of a maybe a more conservative set of operating conditions.

So, I know for PWSEC that was the way it turned out. For IGSEC, like you had mentioned, where we have a lot more knowledge, the more generic considerations probably held, a lot more knowledge and a lot more uniformity, as Dr. Shack had said.

MEMBER APOSTOLAKIS: I think we're falling behind. This issue will come up again in the next session. And I'd rather have the next session go overtime than --

MR. BISHOP: But let me just make a point. The point that was said, okay, is that plant to plant variability and so forth was not considered. That is not a true statement because we were asked to provide a best estimate value which was a medium value which -- to represent sort of like the fleet average if you want to call it that.

But we also asked to provide five and 95 percent values. And those tend to catch both the high and the low outliers. That was specifically discussed.

Okay, that's why we were asked to do that, was to catch -- yes, not all plants are going to have, you know, welding fabrication problem or high residual stresses or, you know, forgot to stress relieve their welds, or whatever that problem may be.

But there is still is a chance that maybe happen. And that's why we were asked to estimate five 95 percentiles also on all our estimates.

MEMBER APOSTOLAKIS: Okay. I think we're

done with them.

MR. TREGONING: We discussed this comment. I don't think we need it. So, let's get into the --

MR. SNODDERLY: Yes, Rob, it's Mike Snodderly. So, we've got a half hour left. Because we really need to end this presentation at 10:00, and three comments to go. So, let's try to --

MR. TREGONING: Two comments to go.

MR. SNODDERLY: Two, great.

MEMBER APOSTOLAKIS: What do we have at 10:00?

MR. SNODDERLY: A break at 10:15. Then we start the discussion which is going to be --

MEMBER APOSTOLAKIS: I thought you said industry presentation.

MR. SNODDERLY: That's why we need the extra time, for the next presentation.

CHAIRMAN WALLIS: Do we have an industry presentation on this topic.

MR. SNODDERLY: No, for the next topic, the Rule Making Package.

MEMBER APOSTOLAKIS: This topic will end at 10:00. So where are we now? Slide 11?

MR. TREGONING: Yes. So now we're at sort of the mead of the disagreement or the mead of the comments here.

MEMBER APOSTOLAKIS: Yes.

MR. ABRAMSON: Addressing your third comment on this, and just to -- as we stated again, this practiced geometric averaging is at variance with the methods employed previously in which the arithmetic averaging method is applied to the probability distribution of the experts.

And our response went along the following lines, first of all, fundamental consideration in the elicitation was to aggregate such that the final results represent the opinions of the panel as a whole.

And, let me just digress from this or just amplify this a little in response to your comment there about our statement that it's important in the report that the results represent the center of the group.

What we kept in mind at all times, of course, is this is an expert elicitation. And what's the rationale for doing this? Well, there's been a lot of experience with this, as you all know.

And the indication is -- or there's a lot of evidence that there's some wisdom in the group and that the experts each bring different perspectives, experience, intuition and so on and so forth, and that the group is better than any individual expert could be.

I should emphasize that the purpose the elicitation is not to try to identify one or two good experts. If we could do that we wouldn't have to have the elicitation in the first place.

Now, what do we mean by a group opinion? Well, it seems too axiomatic that a group opinion has got to be somewhere in the center of the group because, if it's near the high end for whatever reason, or the low end, then it's not a group opinion.

Most members of the group would not agree that this is a consensus opinion.

MEMBER APOSTOLAKIS: Are you talking about the point value now?

MR. ABRAMSON: I'm talking about if you're taking what he had -- we had, what, for BWRs we had eight, for PWRs we had nine experts who weighed in on this.

Say, for the eight, what we did for the purpose of the report for summary, we had them summarize these eight values or nine values so they -- so, to replace them, to summarize them by a single point, a single value for whatever it was, for the mean, the median, the 5th percentile, whatever it was, and that this necessarily was, as a group opinion, had to be somewhere in the center of the group.

Because, if it was near the high end, it was like the 8th highest value or the 7th highest value, most members of the group would say, that's not a group opinion.

MEMBER APOSTOLAKIS: But that assumes that you have to work with the estimate, say, of the 95th percentile. Another way of looking at this is the consensus is sought at the distribution level.

MR. ABRAMSON: Well, we didn't choose to do this.

MEMBER APOSTOLAKIS: You did not?

MR. ABRAMSON: No. What we did is our emphasis in the report -- the parameters of interested --

MEMBER APOSTOLAKIS: Right.

MR. ABRAMSON: -- directly, specifically the mean, the median, the 5th, and the 95th percentile. And we did not try to estimate the distribution as a whole, just these particular parameters, which, you know, if you say in the report, are the ones that are used for regulatory decision making purposes.

MEMBER RANSOM: Part of the problem seems to be with what do you mean by center?

MR. ABRAMSON: Well, when I say center --

MEMBER RANSOM: I mean, the center of a log basis or what is --

MR. ABRAMSON: When I say center I mean center so that -- well, a center could be the median, for example.

MEMBER RANSOM: Right.

MR. ABRAMSON: It would be the halfway point.

MEMBER RANSOM: Or it could be the arithmetic average.

MR. ABRAMSON: Well, it depends. If you have -- in some cases we had where the difference between the low and the high value was several orders of magnitude.

The arithmetic mean would be between the highest and the next highest value. It would not be at the center of the group. And then, when I say the center of the group, it should represent in both from the point of view of the panel and also, of course, from the analyst, a group opinion, and not something that's skewed either high or low.

And what this should be would depend on the particular circumstances, I would say, of the situation.

MEMBER RANSOM: Well certainly, it would make a difference whether you considered the actual values or the log of the values, for example, to define the center.

MR. ABRAMSON: Well, what we did -- no, it wouldn't. It wouldn't because you're just making a monotonic transformation of the -- if you take the median and you take the logs, you're going to get the same value.

It doesn't make any difference. The median is the center whether it's spread out or it's compressed with the log scale. It makes absolutely no difference.

MEMBER RANSOM: You mean the mean or the median?

MR. ABRAMSON: Well --

MEMBER RANSOM: The median just divides half higher and half lower.

MR. ABRAMSON: Exactly. That's right. And by the center -- if you took the -- it depends on the value whether the arithmetic mean or the geometric mean, or some other kind of mean is going to be close to the median or not.

By the center of the group I mean

something around the median.

MEMBER RANSOM: The median.

MR. ABRAMSON: Right.

MEMBER RANSOM: Okay.

MR. ABRAMSON: In the sense that you have -- well from a -- I guess from a mathematical point of view, it's one that I would say it's around the 50^{th}, maybe the 60^{th} percentile or the 40^{th} percentile.

But it's not the 90^{th} or the 95^{th} percentile or the 5^{th} percentile. That's point one. And point two is, of course, as Rob emphasizes, you all know we had extensive feedback and iteration with the experts.

It's one of the experts as a group should feel is a consensus opinion. And Rob already described how they weighed into this. I don't want to say that it's -- for example, we didn't use the median in our report, although we did in some of our preliminary evaluation, we did use the median because it was easy to calculate.

And we presented that to the experts. But we did not choose to use this as the final result. The median by definition is the center of the group. But we didn't use that.

But it should be something close to this for the purpose, again, of having this be accepted by the panel and also, obviously, by the analysts as a group opinion.

And that's the key in this. That's their assumption, that we want to get a group opinion. And this necessarily --

MEMBER APOSTOLAKIS: The fundamental difference.

MEMBER RANSOM: Wouldn't that affect what you consider to be the 95th percentile, for example?

MR. ABRAMSON: No. Because what we're doing -- remember what we're doing is we're estimating the 95th percentile. So we have 95th percentile from all eight or nine experts.

So, we want to know what is the group opinion about the 95th percentile. Well, we have these numbers here and we just take, you know, what we did, the geometric mean, whatever we did.

MEMBER RANSOM: Okay.

MR. ABRAMSON: Okay. So that's the fundamental philosophy behind. Now, the -- we outline, as I said, as Rob emphasized in the report, we took, you know, a lot of -- paid a lot of attention to explaining this to the experts, this philosophy.

We got what we call a consensus type

estimate, which means the other center individual --

MEMBER APOSTOLAKIS: Let me -- it seems to me that, you know, as we have already said several times, given eight experts who are providing distributions, point values, or whatever, there are many, many ways that one can process that information.

So, what really should matter at the end is not whether one use a geometric or arithmetic and so on. In fact, as you guys did, doing a lot of sensitivity analysis informs the process.

So, what really matters at the end is, is the distribution that you guys are proposing in the executive summary a distribution that represents what we know now about the frequency of various size breaks so that the decision maker like the Staff or the Commission can base its decision on what you have produced?

That really should be the final thing because to argue whether we are in the middle -- I mean, you know, Lee has a point, maybe I have a point, somebody else has another point.

All these analyses, it seems to me, inform the process, and ultimately we form a judgment in our mind, and we say this is it. So, the final question really should be, the distribution that you are proposing in your exhibit, what does that represent?

And I think our disagreement is now whether it represents a community or just the eight experts.

MR. ABRAMSON: I'm going to come to this in a minute.

MEMBER APOSTOLAKIS: Okay. Why don't we focus on that, because we can discuss this forever.

MR. ABRAMSON: Okay.

MEMBER APOSTOLAKIS: I mean, this is just one way of doing it. You also did the mixture of distribution. I mean, I look at all these things. In my mind I form a distribution, right?

So the question is, at the end, can the Commission feel that, yes, if I look at this distribution, and we go with that the Staff proposes regarding the TBS we are concerned?

MR. ABRAMSON: Okay, let me skip then.

MEMBER APOSTOLAKIS: This much, because we are running out of time.

MR. ABRAMSON: Let's skip the next slide. The ACRS comment number 4, which is what you're saying, the final distribution should be the composite distribution of the analysts based on the sensitivity analysis, represents the expert community's current state of knowledge regarding local frequencies.

Okay. Our response is this, the main point, the first point is, the elicitation did not attempt to determine the state of knowledge of the expert community.

By that I mean we did not explicitly tell the -- I don't think so -- the experts that they were to -- obviously they all recognized they were a part of the expert community.

They wouldn't be there otherwise. But we didn't specifically ask them to try to tell us what the expert community to be a stand in or to their opinion what the expert community felt.

So, they were not there as representatives of the -- or as assessors of the expert community opinion. They were there for their own opinion. Now, again, saying the study represents the expert panel's current state of knowledge regarding LOCA frequencies.

So I would say, certainly everything we did was we tried to make sure that we fairly in an unbiased way as we possibly could, in as accurate a way as we possibly could, have the experts make sure that the results we got from the experts represented their opinion.

And then, of course, from the point of

view of the aggregation, we tried to make sure that the -- tried to aggregate in such a way that the -- what the results we finally came up with represented the panel as a whole.

So that's what we did. But we're talking about the expert panel. Now, because these -- the panel was not asked -- to ask as a stand in for the expert community, we certainly cannot claim the study represents the state of knowledge of the expert community.

We can't claim that. We have their personal opinions, but not their perception, the expert community's opinion.

MEMBER DENNING: Can we ask them --

MR. ABRAMSON: However -- okay, I'm sorry.

MEMBER DENNING: Maybe you're going to get there. Make your point.

MR. ABRAMSON: Okay. However, the panel -- this is of course very, very important. The panel selection was designed to represent broad organizational, experiential, and international differences within the community.

We very deliberately made that. This is not necessarily a -- we did not try to get a random sampling in any sense from the community. So we very carefully chose and obtained relevant diversity.

And, therefore, the diversity of the experts would tend to accomplish the full breath of views in the expert community. So we felt that we had the full breath from whatever in this industry, academia or the regulatory point of view of the expert community.

It's just that we did not explicitly identify them as representation or representative of the expert community. So, from that perspective, we can say that the results may very well represent the results of the expert community.

But, we didn't make that assessment. It's up, I think, to you and the Commission and so on in deciding to what extend these results are going to be relevant and valid.

MEMBER APOSTOLAKIS: But your words now are much more softer than what you have in the report. The report is absolute. No, we didn't do that.

Come on, you selected these guys, as you say, to represent the broad spectrum of use. You know, if I --

MR. ABRAMSON: I think these words are in the report. These particular words are in the report.

MEMBER APOSTOLAKIS: Yes, but, the report is big, right? They can be down here in --

MR. ABRAMSON: Certainly in the executive summary. I believe these words are taken from the executive summary or they are in the executive summary.

MEMBER APOSTOLAKIS: Well, let's see with Dr. Denning.

MEMBER DENNING: Well, I just think this is semantic. I think really that what you've done has really looked at the community that's out there and sampled.

You didn't go out intentionally to sample like that. But I think that the saying that it's not really representative is an over -- I mean, these words are okay here.

MR. ABRAMSON: I'd like to make another point. Maybe Rob was going to make this. I'll jump in. The community -- the expert community is a rather small community.

And, therefore, our petition is that this panel of 12 is a pretty good chunk. I do not know how big of a chunk, but a pretty good chunk of the expert community.

So, from that point of view, it's already fairly representative, although it's not necessarily a random sample.

WITNESS RICE: It doesn't have to be random. In fact, I wouldn't want it to be random. I want the best guys on the --

MR. ABRAMSON: Of course. And I would certainly oppose, you know --

MEMBER APOSTOLAKIS: But that's not --

MR. ABRAMSON: A random choice is not the one you want to make anyway.

MEMBER APOSTOLAKIS: Let me ask again. If the Commission bases its decision on what you guys propose in the executive summary, would they be basing their decision on the best state of the art right now regarding these frequencies?

MR. TREGONING: My opinion is yes.

MEMBER APOSTOLAKIS: So, why don't you say that in the executive summary? Why do you keep talking about random samples and this and that? I mean, just say it.

Okay, you made a mistake -- if you can call it a mistake -- in the sense that you didn't ask the experts to actually try to figure out the state of the art.

But, the care that went into selecting them, all this stuff, all these analyses, all this stuff, you're damn close, it seems to me. I mean, what else can we do right now?

You know, maybe form -- is it possible to have a review group of equally qualified experts that would look at your work and the expert opinions that you collected and come up with the expert community's distribution?

And would that be significantly different from what you already have? Especially on the high side, that's really what worries, I think, the regulator.

MR. TREGONING: Yes, if you formed another group, you would essentially be replicating the elicitation at that point.

MEMBER APOSTOLAKIS: No, I don't want to elicit again, I would have them review what you guys have done. But, do you think that is possible and would that give any results that would justify the expense drastically different?

MR. TREGONING: My opinion is no. And I think that's one of the reasons. The other way we're trying to tap into the expert community here as well is by going out for public comment, by doing the reviews of the NUREG that we've done with ACRS internally and otherwise.

We're hoping to get some of that review and some of the comments and feedbacks that we've received. And, you know, when we went out for the external review panel, that was one of the objectives of that as well.

And I think, throughout this review process, we've received very valuable comments, including comments that we've received from ACRS that we're trying to use to inform us on how this report needs to be structured and presented.

And I think, just following up on your remarks, I think what reaction we'll take out of this is we're going to look at the executive summary yet again and make sure that we do, I'll say, in keeping with words that are on the slides here, to make sure that we, maybe more accurately and fairly represent what's been done here in a very concise manner.

MEMBER APOSTOLAKIS: Yes, and --

MR. TREGONING: To be consistent with some of the concerns that you've raised.

MEMBER BONACA: And I think, particularly the second last paragraph, the way it's written, you know, it says, you know, arguing about why the geometric mean was chosen.

It says mixed distribution aggregation can lead to significantly higher mean in 95th percentile estimates. And then you go into a long discussion to explain why you want to have that.

It almost seems as if you want to have a lower mean. But that's not really what they intend to do. And I think if there is some rewording here to reflect better this discussion, I think that should be appropriate.

MEMBER APOSTOLAKIS: Yes, exactly. The point is not which distribution gives me something or which method gives me something that I like. We do all the methods.

In fact, you did. What matters at the end is the group that I had, plus you, of course, because you are acting as the integrator. Having seen all these results, you know, if I do the arithmetic thing, I get this.

If I do the other thing, I get that. If I have error factor adjustment, I get something else. Having done all these, having looked at all this stuff, now, what do we think as a group?

And that's really what matters at the end. And it should be emphasized, not one method against the other. Maybe they decided at the end, you know, I looked at the arithmetic average, I think it's a little stretching it too much.

You know, so their consensus distribution did not really go close to that. But that's fine. That's up the experts.

CHAIRMAN WALLIS: George, it occurs to me there's something else here too. I mean, you can ask the experts for all these opinions and stuff and what's their best conclusion.

That's rather different than asking them what should the authority use as a distribution in order to make decisions. That might be a different question.

MEMBER APOSTOLAKIS: it's a very different question.

CHAIRMAN WALLIS: That's not the question being asked.

MEMBER APOSTOLAKIS: They should --

CHAIRMAN WALLIS: But I think that's the question you're trying to ask.

MEMBER APOSTOLAKIS: No. What I'm trying to answer is, is this the distribution of what the expert community -- that means what the state of the art is?

I don't want to have an expert who's working in some obscure laboratory somewhere in

Germany come back and give us evidence that this distribution, for example, is optimistic.

I want to have this warm feeling that, yes, this distribution -- look, experts may disagree, you know, by a factor of two here and there.

But, by and large, we have captured what we know now as a community. And this is really what we should be using in regulatory decisions. Now, one way of doing that is to ask the experts at some point explicitly to consider the community.

Now, these guys admit they didn't do that. But now the next question is, are we really far off? And, you know, the selection of the experts and so on, I tend to agree with you that we really aren't because we were careful how we selected the experts.

We were careful, you know, with the process and so on. We did a lot of -- we, I mean you did a lot of sensitivity analysis and so on. But I'm not getting it at this stage into the question of how these results should be used.

No, this is up to different people who will come before us at 10:15.

CHAIRMAN WALLIS: No, but George, the thing is, for certain purposes you might want to use different distributions because there's a good reason for using that particular distribution for that purpose.

MR. TREGONING: And that's why we tried to be very clear. And that was one of the reasons we were hedging about having a set of summary results in the executive summary.

MEMBER APOSTOLAKIS: Well, yes. And, as you recall, in the draft of November they said, you know, you go and read the report and decide what you want to use. And we objected.

CHAIRMAN WALLIS: Well, George, can I ask you, you've asked all these questions, are you not satisfied that they have a reasonable cross section of the expert community?

MEMBER APOSTOLAKIS: I am.

CHAIRMAN WALLIS: That the expert community is rather small and they have a fairly good, you know, fraction of that community is being captured here, that it is sufficiently diverse and all that. Are you satisfied with all that?

MEMBER APOSTOLAKIS: Yes.

CHAIRMAN WALLIS: All those answers to those questions?

MEMBER APOSTOLAKIS: Yes.

MEMBER KRESS: Yes. I think he just says

that it ought to say that in the report.

MEMBER APOSTOLAKIS: It's the words.

MEMBER KRESS: The words, yes.

MEMBER APOSTOLAKIS: Well, let me tell you what I think. First of all, if I combine this with what we're going to hear in the next session, the way the Staff is proposing to select TBS, I think what they have done is fine.

The stuff is going a little higher. That's fine. Now, if there is any discussion at some point of going to lower transition break size, lower than eight inches for PWRs, then you are entering now the range of sizes of the experts are giving me.

Then I would probably have to rethink about it. The thing that really bothers me is that we do not seem to be building on the work that this Agency has sponsored in the past.

In fact, if I look at the citations on the revised report, chapter E, section E, this joint effort by EPRI, NRC and DOE is not even close. And that bothers me.

Because, in the future I'm sure people will go to this report and say this is the latest on expert opinion and LOCAs and so on and they will use this.

And the question of expert community, for example, I don't want it to disappear. I want in the future to be more aware of the fact that we're really after the expert community's distribution, not just the expert panels.

Okay? And that's what bothers me with it. But in terms of revising 50.46, I don't think there is a problem.

CHAIRMAN WALLIS: I do not know quite what you mean by expert community. Each of these communities is sort of a pyramid. And if you take your expert community and make it too big, they're no longer experts.

Your experts are usually fairly select group.

MEMBER APOSTOLAKIS: You know what I mean. I mean the state of the art.

CHAIRMAN WALLIS: Well, it's often behind the experts. The standards --

MEMBER APOSTOLAKIS: This --

CHAIRMAN WALLIS: -- used by engineers in the field is often way behind the expert knowledge in the field.

MEMBER APOSTOLAKIS: And it's interesting to me that yesterday, in fact, the whole methodology that these guys used was on the early -- was in fact based on this joint effort on expert opinion elicitation. Anyway, you have a slide 15?

CHAIRMAN WALLIS: George, you may a predictive statement, which was deterministic, which was that we will finish by ten o'clock.

MEMBER APOSTOLAKIS: We will.

MR. TREGONING: I certainly hear what you say. We're going to go back and look a the executive summary as well as some other areas to make sure.

It sounds like it is semantics that we're talking about in making sure that the semantics and the way we characterize the elicitation is clear with respect to the state of the art and what was done.

I mean, we're taking that as an action to go and do further revision at this point on the executive summary.

MEMBER APOSTOLAKIS: Are we going to see this report after the public comment period.

MR. TREGONING: Yes.

MEMBER APOSTOLAKIS: We will again?

MR. TREGONING: Certainly, yes.

MEMBER APOSTOLAKIS: Okay. Do you have any closing comments?

MR. TREGONING: Closing comments, again,

the reason that we're here is we're requesting a letter from ACRS essentially allowing us or recommending that we proceed for public comment with the draft NUREG report.

MEMBER APOSTOLAKIS: Any comments, questions from the members?

(No verbal response.)

MEMBER APOSTOLAKIS: Anybody else?

(No verbal response.)

MEMBER APOSTOLAKIS: Well, Mr. Chairman, we finished six minutes earlier.

CHAIRMAN WALLIS: Very good. We'll expect this to be maintained, this performance George. Let us take a break until ten after ten.

MEMBER APOSTOLAKIS: No, 10:15.

CHAIRMAN WALLIS: I guess we can't stop -- it's just I'm trying to leave enough space for the examination subject, which I think is going to take some time. Okay. We'll take a break until 10:15.

(Whereupon, the above-entitled matter went off the record at 9:50 a.m. and went back on the record at 10:10 a.m.)

CHAIRMAN WALLIS: Come back into session, please. The next topic is 50.46, and I'll hand it over to my colleague, Dr. Shack to get things going.

VICE CHAIRMAN SHACK: Okay. We're here today to review a proposed draft or a draft of the proposed revision to 50.46 to risk inform the rule. In December, we reviewed a previous draft of a proposed rule change.

There have been a number of changes in this new rule that we're going to be seeing today. The three most important ones that I could identify is the transition break size now is a single-ended rupture of the largest attached pipe in the recirculation piping system.

The previous rule prohibited bundling of unrelated changes when we were assessing essentially changes in risk when we were making changes here. Now the new rule will permit bundling of unrelated changes, so that's a substantial change in the rule.

And they've also removed some of the detail from the acceptance criteria for changes under 50.46. That is the sort of Reg Guide 1.174 stuff that was built into the rule has been now -- some of that has been removed and there's basically a number of high-level requirements left but some of the details have been gone. And I guess there's a suggestion there will be a regulatory guide that will provide more detail to that.

The rule still requires that you be able to mitigate all breaks up to the DEGB. However, when you do that, you do not need to assume loss of off-site power for an independent single failure, and you can credit non-safety grade equipment. And, again, the requirement is that you maintain coolable geometry and provide long-term cooling. The notion will be that there will be somewhat relaxed limits on the amount of damage that it can tolerate. But, again, the requirement that you can only operate in configurations in which this capability has been analyzed and credited is still maintained in the rule.

And Richard Dudley will lead us through a more detailed discussion of some of these changes and the staff's reasoning behind the changes.

MR. DUDLEY: Good morning. I'm Richard Dudley. I'm the rulemaking project manager for the risk-informed 50.46 rule. Today, I'd like to accomplish two things. We'd like to accomplish two things in our talk. First, as Dr. Shack said, we'd like to update the ACRS on what we've done to change the rule since we were last here on the 2nd of December. And, secondly, we would like to ask the ACRS for a letter so that we can go forward with putting the proposed rule forward to the Commission.

And we'd like the letter hopefully by March 11. And I'll show you later in our schedule why that's important to us.

When we were here last on December 2, we received a letter from the ACRS on the 17th with three major comments. The first comment was that we should maintain mitigation of accidents up to and including the largest double-ended break of a reactor coolant system pipe. The proposed rule had that mitigation, and the current rule has that mitigation, so we have made no changes in that area.

The second comment was that for the transition break size we should consider the single-ended break versus a double-ended break. As you have heard, we have looked into that and decided we should change the TBS to a single-ended break.

And the final comment from the ACRS was that we really hadn't done what's necessary to quantify the risk benefits of a smaller TBS and that additional studies and work would be necessary before that relationship was properly known. And so we're doing some studies on that that we'll talk to you about in a moment.

Again, the TBS now is a single-sided break. Gary Hammer, of our Mechanical Engineering

Group, is going to talk to you in some detail about the changes that we've made to the TBS and why we've made those.

We've initiated thermal-hydraulic studies, both the NRC and the industry, to investigate the risk benefits of smaller technical break size. Ralph Landry, of our Reactor Systems Group, will talk to you in some detail about those studies and the parameters and the other things that we're looking at.

And also we've made a number of changes. In addition to changes to bundling, we've made some other changes in the risk assessment requirements that we had in the proposed rule. These would be the requirements that would be used to determine the acceptability of facility changes that are enabled by the revised 50.46 ECCS requirements.

CHAIRMAN WALLIS: I thought, Ralph, doing risk benefits a smaller break size, but, presumably, if you back off on the requirements for the large breaks, then the risk associated with large breaks goes up?

MR. DUDLEY: I guess that would be the case.

CHAIRMAN WALLIS: Are you looking at that risk at all?

MR. DUDLEY: If you optimize your ECCS design for smaller breaks, which are more likely, you could have the net effect be the overall risk to go down.

CHAIRMAN WALLIS: You could, but you don't know. But you can't ignore the other effects on the larger breaks while you're doing that.

MR. DUDLEY: Yes, that's correct. You would have to factor that in and weigh that off against any increases. That's correct. Again, we're going to talk about that in a moment.

VICE CHAIRMAN SHACK: Again, on this 1.174 type requirements, we make all sorts of decisions on changes to licensing basis using 1.174 now. Why do we have to have new requirements in the rule for these particular licensing basis changes?

MR. DUDLEY: I think Mike Tschiltz will go into that perhaps later on. My understanding is that we had Reg Guide 1.174. It had a number of recommended items of guidance in there. And in addition to that, as the staff went through the Reg Guide 1.174 review for risk-informed changes, there were additional things that the staff, I guess, performed or considered or looked at or there was a level of detail that perhaps wasn't in the reg guide that we used when we reviewed risk-informed changes. Again, I'm going to have to let --

VICE CHAIRMAN SHACK: Why these changes to the licensing basis deserve that consideration and other changes to the licensing basis are okay to get by with an inferior version of 1.174.

MR. DUDLEY: Well, the inferior version of Reg Guide 1.174 is not a requirement at all; it's just guidance.

VICE CHAIRMAN SHACK: But it's an acceptable way to make licensing changes.

MR. RUBIN: Well, if I could point out -- this is Mark Rubin from the staff -- the base of 1.174 was licensing changes that met all current regulatory requirements. Here we're making substantial changes to some of the fundamental safety requirements that were promulgated 20, 30 years ago. And so as a consequence, 1.174, the general approach to 1.174, while it's being significantly retained, it's being expanded to fill into the context of supporting a major regulatory change. As a consequence, some areas a little more detail is being provided to provide clarity and to ensure that adequate safety is maintained.

MEMBER APOSTOLAKIS: I thought part of the reason was that as long as it's a regulatory guide you really don't have to follow it. But if you put it in the rule --

VICE CHAIRMAN SHACK: Yes, but you have that problem with every licensing basis change. They don't have to use Reg Guide 1.174; they just do.

MR. RUBIN: But they have to either follow the regulatory guide or provide an alternate acceptable method. Here there are requirements in the rule that have to be satisfied, and there will be a regulatory guide that will provide one way of meeting those requirements.

VICE CHAIRMAN SHACK: Yes, because you've chosen to do that for these changes to the licensing basis.

MEMBER KRESS: I think part of the problem is that when you change this rule there are enumerable changes that can be made to the plant that changes the licensing basis as a result of the rule change. It's relatively impossible to a priori know how many plants will make how many of those changes. Therefore, to go up front and say, "Apply 1.174," it's not going to be very easy because you have to somehow make judgments about all of those changes that are going to be made and how each of them affects each plant. So I don't see how they can --

VICE CHAIRMAN SHACK: They can't make any change without coming in and presenting it.

MEMBER KRESS: Well, after the fact they will come in and use 1.174 to track the result of those changes. I think they're using it -- I don't think you can use it as a basis for judging the pipe size or the rule. You can use it as a control of the effect of the rule once it's in place.

VICE CHAIRMAN SHACK: The rule, as I understand it, will not change anything that's in place. If a plant wants to change anything in response to the new rule, they're going to have to come in and apply for a change to their licensing basis.

MEMBER KRESS: And I think they will use 1.174 like criteria for that.

VICE CHAIRMAN SHACK: But why can't they just use 1.174?

MEMBER KRESS: They probably could have. Every plant would have had to come in and do it.

MR. DINSMORE: This is Steve Dinsmore from the staff. I think we couldn't just reference 1.174 in the rule. We wanted to put enough in the rule to provide the framework with which we had to work with.

And so that's why we actually put some of the 1.174 criteria into the rule.

MEMBER APOSTOLAKIS: In other words, you don't people to propose an alternative approach.

(Laughter.)

If it's a regulatory guide, they can. Now you're putting it in the rule.

VICE CHAIRMAN SHACK: Now they will have no alternative.

MR. SHERON: Dr. Shack, if I could also add, if you remember that what the rule allows is beyond the transition break size, okay? There are a number of things that are currently regulatory requirements, for example, consideration of a single -- or assumption of a single act of failure occurring, picking parameters at their worst case conditions. For example, as I said, we assume infinite operation for decay heat along with the assumption of a maximum peaking factor which those two can't occur, basically, at the same time, yet those are requirements that currently exist.

If a licensee were to come in, you know, and as Mark said, the 1.174 is a risk-informed reg guide but licensees still have to meet the regulatory requirements that exist. What we're doing is we're changing the regulatory requirements in this case, okay? When we apply 1.174 to other situations, licensees still have to meet the regulations regardless.

In this case, if a licensee, for example, were to come in and say, "I want to change my ECCS analysis, and I want to use Reg Guide 1.174," unless they used, for example, infinite decay heat, 1.2 times ANS, et cetera, and the like, they would have to request an exemption from the regulation. They would still have to meet 50.46 requirements. That's the difference. And this is allowing that we are changing 50.46 requirements. We're backing off from them, and what the 1.174 does --

VICE CHAIRMAN SHACK: But with the new rule in place, with 50.46(a) in place, why can't he now come in under 1.174 and say, "I want to change my diesel start time" and present an analysis with a 1.174 analysis? He'll do exactly the same thing except the requirements are in the rule versus the reg guide. If we decide in our infinite wisdom sometime that we need to change 1.174, we now are faced with the fact that we'll have things built into the rule rather than the 1.174. So we're --

MR. SHERON: Well, again, the difference, I think, as Mark said, and that is that 50.46 is an enabling rule. We want to have that control, basically, over changes in risk, because we believe that if a license were to come in and propose changes under 50.46(a), they could result in substantial changes to public health and safety from reduction in risk.

As you said, 1.174 is merely -- it's a guide, it's an acceptable way to meet the Commission's rules and regulations. It's not the only way. That puts more of a burden on the staff from the standpoint is a licensee wants to deviate from 1.174 we have to consider it, we have to -- it basically becomes the burden is on us to say why something's not acceptable. I think the approach we're trying to promulgate here is to put some consistency in the regulatory process in how licensees come in and justify changes to their plants. We've probably beat this enough to death.

MR. DUDLEY: Well, you'll get another chance toward the end, and Mike Tschiltz, the Branch Chief of the Probablistic Assessment Branch, will be talking to you about the changes in the risk assessment that follows.

Now, I'd just like to talk about the schedule for issuing a proposed rule. We're at the point where we've just completed office concurrence and we've received concurrence or comments from a number of different offices. On March 10, our current internal schedule is to resolve any open issues associated with the concurrence or concurrence comments. And now I'd like to kind of go to the end of the schedule. On the March 31 date when we're supposed to have this proposed rule to the Commission, in order to do that, working backwards, we have to provide it to the EDO on March 23. And to get it to the EDO on that date, we have to start the concurrence process around the 17th or the 18th of March.

So it's important for us to get your letter somewhere very near March 11 because if it contains any items that we need to address, either in rule language or in the Federal Register notice, we will need to make those changes before we start the concurrence process. This is why we're asking for the letter by a particular date.

And the last two slides are on what we call a planning schedule. This gives you just an overall idea of how the schedule for the proposed rule and the final rule would go. The purpose of these slides is not to specify the schedule we'll actually use because it's all contingent on many things we have no control over, but the purpose of these slides is to show you that we'll be coming back to the ACRS on numerous occasions as we continue to go through this rulemaking process.

If we assume that the Commission is able to issue an SRM in two months, and that's just an assumption, that's a pretty optimistic assumption, quite honestly, but if that were the case, then we would issue the proposed rule somewhere around the middle of June. We're already working on the reg guide.

MEMBER APOSTOLAKIS: What SRM would that be?

MR. DUDLEY: We'll put forward the proposed rule to the Commission and if the Commission gives us an SRM that tells us to issue the proposed rule --

MEMBER APOSTOLAKIS: Oh.

MR. DUDLEY: -- towards the end of May, then we would publish the proposed rule in mid-June.

We're already working on the reg guide, and we have an internal date of the 30th of June to complete the first internal draft of that reg guide. So in the summer of 2005 we'll probably initiate discussions with the ACRS on the reg guide, most likely with the subcommittee. In late summer or early fall of 2005, we'll publish the reg guide for comment and it will be a 75-day comment period, the same comment period we believe that we'll use for -- that we know we'll use for the proposed rule, and we think we'll use the same period for the reg guide.

In September of 2005, the proposed rule comment period would end. Shortly after that, in the fall of 2005, the comment period on the reg guide would also end. In winter 2005-2006, we're looking to complete the final rule package in the reg guide, the final reg guide. So we'll probably meet with the ACRS at least one more time in the winter of 2006 to discuss the reg guide and the final rule, maybe in one meeting, maybe in separate meetings.

CHAIRMAN WALLIS: It's interesting that you're putting the reg guide and the rules together here; they go out as a package.

MR. DUDLEY: That's correct. That's our goal.

CHAIRMAN WALLIS: Whereas what we've got today to look at is a rule --

MR. DUDLEY: Right.

CHAIRMAN WALLIS: -- with great vagueness about what might be in the reg guide, I think deliberately because you haven't done it yet.

MR. DUDLEY: Yes.

CHAIRMAN WALLIS: It gives you freedom to put in what's appropriate. But we've only got one of those things today.

MR. DUDLEY: That's correct, yes. But you will be seeing the reg guide at least two more times.

And in the spring of 2006, we would be in a position to put the rule forward to the Commission. Again, I want to emphasize, and there's an asterisk on all the planning schedules, that these dates are not official dates. They're contingent on many things we have no control over. And they're just kind of for ballpark planning purposes only, and the elapsed times on the rulemaking items are based on typical rulemaking schedules for other goals, rules that we've worked with.

MR. SHERON: Dick, could I add one thing that I think Dick didn't cover? The industry has indicated their desire to develop let me call it an evaluation or an implementation guide document, perhaps similar to what they did for Generic Issue 191. We have agreed that we think that's something we encourage them to do. I don't know their schedule right now. Maybe that's a question you might want to pose to them when they come up and speak, but the thought is is that somewhere down the road they will have their own guidance document which the staff will review, and presuming we find it acceptable we would then probably endorse it as another alternative method for implementing the 50.46(a) rule. We would endorse it through our reg guide. So that's another piece which you'll probably become involved in.

MR. DUDLEY: Now I'd like to introduce Gary Hammer from the Mechanical Engineering Branch, and he'll talk about the revised selection of the transition break size.

MR. HAMMER: Yes, good morning. In way of a little brief background on the selection of the TBS, as you remember, we were here in late 2004 on a couple of occasions to discuss this with you before where we outlined the basis for the TBS selection at that time, and we discussed that we had based that on several considerations, foremost the expert elicitation frequency estimates. Together, with that, we wanted to incorporate consideration of uncertainties and sensitivities that might need to be considered, and we also wanted to try to account for adjustments that might further need to be incorporated, such as any considerations due to heavy loads other than during normal operation or the sizes of actual attached pipes that are configured in the plants.

And as we discussed, ultimately, we based the size of the TBS on the size of the largest attached pipe in the RCS loop, and those size pipes roughly have the frequency of the 95 percentile of ten to the minus 5th per reactor year. Piping larger than that, larger than those attached pipes, tends to be quite a bit larger and has quite a bit of smaller frequency, such that you have this jump, if you will, which forms sort of a natural decision point, if you will.

At that time, we were postulating that the TBS be considered as double-ended since it was an actual broken pipe, and that it would be applied as a double-ended break at the limiting location; that is, it would have to be moved around in the main loop just to see where the limiting location was.

MEMBER APOSTOLAKIS: No, wait. Go back. Let's go back. I think the first sub-bullet under the first bullet is a little misleading. The frequency, actually -- eight inches I think is the smallest diameter, right?

MR. HAMMER: It would depend on how you aggregate the data. The aggregation had a big change

--

MEMBER APOSTOLAKIS: No, no, no. The pipes attached to the RCS main loop, I think the smallest size is eight inches?

MR. HAMMER: Oh, yes.

MEMBER APOSTOLAKIS: Forget about the expert opinion. I'm talking about the plants now.

MR. HAMMER: Okay.

MEMBER APOSTOLAKIS: It's about eight. I think the frequency of the whole equivalent diameter of eight inches is much lower -- it's lower than ten to the minus five. It's not ten to the minus five, as this sub-bullet implies. And that was your choice is a little more conservative than this.

MR. HAMMER: I'm not sure --

MEMBER APOSTOLAKIS: Attached piping has 95th percentile break frequency of about ten to the minus five?

MR. HAMMER: That's roughly --

MEMBER APOSTOLAKIS: It's not an accurate statement.

MR. HAMMER: It's not exact.

MEMBER APOSTOLAKIS: It's lower. The frequency is actually lower.

MR. HAMMER: Well, if you look at the 95th percentile, those numbers were of course a little bigger breaks. And if you look at the LOCA categories covered some range between LOCA Category 3 and LOCA Category 4 or 4 to 5. And so all of these pipes fell roughly in that range.

MEMBER APOSTOLAKIS: I think it's lower.

MR. HAMMER: Coupled with that, the next bullet, which is that the next larger pipe has a much lower frequency, so --

MEMBER APOSTOLAKIS: So what you're doing here, for my own benefit, if I go to the 95th percentile of the frequency failure, of the distribution of the frequency failure, then I have a bunch of expert opinions, right? Then I will also go to the 95th percentile of the expert opinion variability, and that's the ten to the minus five you're using?

MR. HAMMER: I'm not sure I understand what you're saying. We were only working with one curve, but the curves were aggregated in different ways.

MEMBER APOSTOLAKIS: But this one curve you are using is from the executive summary from the report? Is that what you're using? You say you're working with one curve. Where did that curve come from?

MR. HAMMER: Well, you mentioned 12 experts, but the experts were aggregated into one curve. That's what I meant.

MEMBER APOSTOLAKIS: Right, from the executive summary.

MR. HAMMER: But there were several of those curves.

MEMBER APOSTOLAKIS: I know. And which one did you pick?

MR. HAMMER: We tried to consider that there was some sensitivity involved in which curve you picked, so we took that into consideration.

MEMBER APOSTOLAKIS: Did you pick the one that the previous speakers in the previous session feel is the best consensus curve or you picked another one?

MR. HAMMER: Actually, the base case was the geometric mean curve --

MEMBER APOSTOLAKIS: Yes.

MR. HAMMER: -- that you heard about earlier. There were also the aggregations of the mixture distribution or the arithmetic mean, and we looked at all of those and tended to pick whatever number came up as the larger of the group.

MEMBER APOSTOLAKIS: Oh, okay.

MR. HAMMER: So this is -- realizing that there's not uniform agreement on the exact aggregation anyway and --

MEMBER APOSTOLAKIS: Okay.

MR. HAMMER: -- and we wanted to consider that.

MEMBER APOSTOLAKIS: So you went with the most conservative estimate that you could find.

MR. HAMMER: Well, yes. I mean of course 95th percentile is arbitrary, so in some person's mind that might not be the most conservative.

MEMBER APOSTOLAKIS: But the point is that --

VICE CHAIRMAN SHACK: We're supposed to finish at 10:55.

MEMBER APOSTOLAKIS: But this is an important point. I don't know why the other guy hasn't bothered to come up with their best distribution.

VICE CHAIRMAN SHACK: Well, he's going to pick a different break size anyway. He's conservative, George.

MEMBER APOSTOLAKIS: I know he is. All right.

VICE CHAIRMAN SHACK: Let's move on.

MEMBER RANSOM: Is there a slight disconnect here? They're focusing on attached piping, and I thought the elicitation was for cracks in piping and more or less of a continuous distribution.

VICE CHAIRMAN SHACK: This is the size.

MEMBER RANSOM: Right.

VICE CHAIRMAN SHACK: They're picking the size based on the sciences.

MEMBER RANSOM: But why pick it based on attached piping? Why not pick it based on just on the probability of occurrence regardless?

MR. HAMMER: Well, we looked at that. I mean you could have holes in the system of various configurations. We felt like one of the ways that -- since the bigger pipes tend to be thicker and more robust, then there was a greater likelihood that if you had a break of a given size, it might be in the attached pipe. Because the wells are oriented in a circumferential fashion, so if you have a crack of a given length, it tends to affect you more that way than in some other way.

MEMBER RANSOM: But we heard from the elicitation that the double-ended or guillotine break was more unlikely than, say, cracks in piping and things like that, which might open up, which then leads you to a continuous distribution of sizes of the break, even though it's single-ended type of thing. And I would think that your choice of TBS would be based on the same type of consideration.

MR. HAMMER: Well, I heard the discussion earlier. I'm not sure I exactly agree with it, but we wanted to capture what we though were the important things in terms of the actual configurations. And so we felt like the attached pipes were a major consideration.

VICE CHAIRMAN SHACK: But, again, this size does bound all those other holes that could appear in the system --

MR. HAMMER: Right.

VICE CHAIRMAN SHACK: -- which is consistent.

MR. HAMMER: Right. Right. And I'll get into that a little bit. We looked at how we might do something regarding varying the size of the break with regard to location, and I'll touch on that a little bit. We did investigate that.

After the last RCS meeting in December, we set about investigating ways that we might able to better estimate the TBS, make it smaller or more accurately estimate it. We looked at primarily two issues. The first was could we vary the size of the TBS with respect to the location, and I think this gets into your question a little bit.

One of the things that we specifically wondered, and this is kind of maybe just one example, but we felt like it was an important one, on PWRs you have hot legs and cold legs that operate at slightly different temperatures. Might be 40 degrees F or 50, 60 degrees F, whatever it is. Anyway, it's substantial, perhaps, in terms of the degradation mechanisms being somewhat different, at least theoretically.

So we thought -- and cold leg breaks tend to be limiting thermal-hydraulically in the analysis. So we thought, well, okay, we're basing this TBS on the largest attached pipe, which is actually the surge line, and the surge line is attached to the hot leg. Do we need to make that same size break in a cold leg? Maybe it doesn't logically follow.

So we went through that though process, and we said, well, can we further parse or subdivide some of the information that was in the elicitation data, in some of those estimates, and see if we could come up with some difference like that or some better estimate, which might be beneficial in terms of making the break somewhat smaller in the cold leg?

But when we started to do that we found that we really couldn't adequately quantify such difference, because the elicitation responses were in terms of overall frequencies of a certain size aggregated over a significant population. So if you start to break that data out in that way, it really -- you're doing something and it really wasn't generated for, we didn't feel like. So we felt like we're introducing a lot of additional uncertainty in trying to make that type of formulation.

And so we felt like that what we would do is just stay with the size of the largest attached pipe and apply that from all locations. But --

MEMBER RANSOM: That's what you're intending to do, apply it in all locations.

MR. HAMMER: Right.

MEMBER RANSOM: Okay.

MR. HAMMER: Well, in all locations, but --

MEMBER RANSOM: In cold legs?

MR. HAMMER: Right. Right. Right. The other question we had was something that the -- yes?

VICE CHAIRMAN SHACK: Two minutes we'll

finish up?

MR. HAMMER: Two?

VICE CHAIRMAN SHACK: Two.

MR. HAMMER: Okay. I'll run quick. All right. The other question was something that the ACRS had specifically asked about that we though was a good question, whether it needed to be modeled as a double-end. There's several considerations about that, and I've listed them there. Ultimately, we felt like -- I guess the most important bullet there is that the, as you heard this morning, expert elicitation really estimated frequencies of certain size holes in the system, and our further consideration of doubling that size hole was essentially double counting that would be inappropriate, in large part.

And even if you look at the full break of pressurizer surge line, which does simultaneously empty the pressurizer contents in addition to flow out of the hot leg, the primary effect is what's coming out of the hot leg, not what's coming out of the pressurizer. And so -- let me see if there's anything else there.

CHAIRMAN WALLIS: You had something about manways.

MR. HAMMER: It's essentially bounded --

CHAIRMAN WALLIS: We were happy that the double-ended break sizes seem to bound the manway break, but the single-ended break probably does not bound the manway break anymore.

MR. HAMMER: I'm sorry, Dr. Wallis.

CHAIRMAN WALLIS: The manway and the steam generators and so on, if they come off, that area is I think equivalent to the double-ended break you had before. I think going with a single-ended break you no longer cover the manways.

MR. HAMMER: Because the manway itself would be bigger than this size, you mean --

CHAIRMAN WALLIS: Yes, right.

MR. HAMMER: Yes. But in looking at the manway failure, I think we felt like that was a lower frequency than what was being targeted here. You'd have to fail multiple bolts simultaneously.

CHAIRMAN WALLIS: It has a possible cause, which would be human error. That's why it's a little different from the other breaks. It has a possible cause, which is overtightening of bolts. Human error could lead to manway failure. That's why we like the idea in our letter that you were covering that, and now you're not. So I just noticed that in passing.

MR. HAMMER: Okay. And for the proposed rule, I guess just to summarize, we're proposing that it be based on the largest attached pipe, similar to before, and that it would be applied at the limiting location, wherever that would be, and that it would be modeled as a single-ended break.

MEMBER BONACA: Since you're not using any more double-ended discharge, I mean to continue to link the transition break size to a pipe is misleading. I mean I understand and now I can see it's a single-ended, whatever, but by referring to break size it just raises the question. It seems as if we try to model a limiting break in real terms when we didn't. I mean, yes, it's a size of the pressurizer line but then we're only using one side of this charge. So it really is not related to that. Anyway, just a comment. I can live with that.

I think this linkage is a remnant of the previous version of the rule where we have double-ended discharge, and it stays in but it's unrealistic so therefore is not representative of what happens if you really had a double-ended break on that.

VICE CHAIRMAN SHACK: I think one of the conclusions of the elicitation process was that if you wanted to get an eight-inch hole, the way that you'd most likely get it would be a break of an eight-inch pipe rather than an eight-inch hole in a 24-inch line.

MEMBER BONACA: I understand.

VICE CHAIRMAN SHACK: So there is a logical connection, I think, between the pipe and the hole.

MR. HAMMER: And if you remember -- I didn't go back over all of this, remember we had initially just come up with a nice, big, fat, round number, 14-inch on PWRs, 20-inch on BWRs. But then we started to look at, well, if we're looking at pipes that break, they don't have those exact dimensions, and as a matter of fact those attached pipes vary from plant to plant, so shouldn't we customize it a little bit for that?

MEMBER BONACA: Okay.

MR. HAMMER: Okay.

MR. DUDLEY: Okay. Next, Ralph Landry will talk about the thermal-hydraulic calculations that we're having done.

MR. LANDRY: Okay. One of the interesting questions that has come up from the Subcommittee, the full Committee and our own internal discussions as we've gone about formulating this regulation is that what are some of the safety benefits, what are the changes in risk from a potential change in the rule of the break size?

Subsequent to the meeting which we had in December with the Committee, we met with the industry, the Westinghouse Owner's Group, which included Westinghouse, Framatome and General Electric, and discussed what could be a set of calculations which could be performed by both the industry and the NRC to try to define or determine in some way a risk-benefit. Now, this is not a definitive work, it is not all-encompassing. We due to time could only focus on one particular area, so we have defined, in conjunction with the Westinghouse Owner's Group, a set of calculations which are going to be done by the industry and in parallel by the NRC.

We are going to do reactor coolant system calculations, in other words, the LOCA calculations. The industry is going to perform these calculations, and the NRC is going to perform calculations. We're going to use a more or less generic model for the Westinghouse four-loop, 12-foot core plan. We're are going to use the same basic model for both the industry and the NRC so that we see how the different codes compare.

We are going to do containment calculations, both the industry and the NRC, and the industry is going to take a plant-specific PRA and make modifications in the PRA based on the results of some of these thermal-hydraulic calculations and try to determine what is the change in risk from these operational changes that we're talking about.

Okay. The reactor coolant system calculations which we're going to perform are basically five break sizes. We're going to look at what has been traditionally the worst case, small break LOCA. We're going to look at a hot leg break of the pressurizer surge line, and we're going to look at the cold leg, taking the Accumulator/SI line, but we're going to place that break on the bottom of the pipe, which is traditionally the worst case to have a cold leg break. And then we're going to take that Accumulator/SI line break size and increase it by 20 percent and decrease it by 20 percent, so that we can see if there's an effect from a slightly larger or slightly smaller break size.

These five breaks will then be run in two conditions. We're going to use the normal emergency diesel generator start time of ten seconds, and we're going to use a delay in the start time up to 60 seconds, so that we can see is there a change in the thermal-hydraulic response due to a delay in the diesel generator start.

Now, when Wayne Harrison gets up from the industry, Wayne is going to talk more about how they're going to quantify the effect of change on the PRA and change of reliability --

CHAIRMAN WALLIS: You're expecting a safety benefit from this?

MR. LANDRY: Well, we want to see if there is. These calculations are being designed to tell us for an initial cut is there a change in risk from such things as changing the diesel generator start time? As I said, this is not an all-encompassing set of calculations. This was only one that we determined initially we could use as a starting point.

CHAIRMAN WALLIS: But you might look for an optimum start time would make some sense, wouldn't it?

MR. LANDRY: That's a possibility to optimize, to iterate or perturb the start time till you find what is the optimum tradeoff between change in thermal-hydraulics versus change in reliability.

We had to select an arbitrary set of conditions to get the calculations started, and that's why we've selected these as an arbitrary initial set, and we may vary other things at a later date.

CHAIRMAN WALLIS: Are you looking at risk here or are you looking at core damage?

MR. LANDRY: We're going to look at the change in the thermal-hydraulic conditions from a diesel generator delay. And then that change in start time can be translated into a change in reliability which can be then put into the PRA and determined from the PRA what is the change in risk.

MEMBER BONACA: Would the PRA model also the double-ended guillotine break with less capable PCCS system or less capable, I mean, simply with maybe single train rather than two?

MR. LANDRY: That would be an additional calculation for a later date. This is just -- as I said, this is the initial attempt to try to quantify a change in risk.

MR. SHERON: Mario, this is Brian Sheron. The PRAs I don't think go into that level of detail. And I'll have to turn to Mark or Steve here but my understanding is that, for example, they will have a success criteria that says if the thermal-hydraulic calculation says you mitigate, the event would say two accumulators or three accumulators, and your PRA says therefore for those scenarios it's success, it's not core melt. It doesn't get into the question of how much did I increase risk by decreasing margin. We just don't get down to that level.

MEMBER BONACA: In fact, you don't get into the issue as long as it's coolable.

MR. SHERON: Yes. The intent here is -- I mean we have heard for a long time that these fast starts of diesels and the testing required actually may be causing more harm than good, and so the whole idea here is that if we can allow a longer start time for the diesels, there's I think a pretty obvious safety benefit in terms of reduced wear and tear on diesels, and that's what we're trying to see what that benefit is.

MEMBER KRESS: You have to come up with a new reliability number for the diesel?

MR. LANDRY: Wayne Harrison is going to talk about how the industry is approaching that.

MEMBER KRESS: Okay.

MR. LANDRY: And he presents after us.

Okay. We are also going to look at the containment response in a couple of ways. One is we are going to use a generic GOTHIC containment model for what we're calling a generic large dry containment. We're going to use that model to build a contained model also, so that we can look at GOTHIC and contained within the staff. The industry is using just GOTHIC.

And with the containment analyses, we want to use the mass energy releases we get from the thermal-hydraulic calculations and then look at varying the spray actuation time. Instead of using an automatic containment spry actuation, can we delay the spray actuation, and what is the effect on RWST to some switchover from changing the spray actuation time? What is the change in washed-out debris? What is the change in the effect on ECC pump and PSH from the sump from this delay?

CHAIRMAN WALLIS: It's interesting that you seem to be looking at the consequences of a decision to be made, and the decision's going to be made before your evaluation of the consequences is available.

MR. LANDRY: We plan on sharing the results of these analyses with the appropriate subcommittee as they become available.

CHAIRMAN WALLIS: That's very interesting. I think it's very interesting. I'm just interested in the fact that you're looking at the consequences of the decision, and yet your analysis isn't going to be available before the decision is made. It's just an interesting way to do business. It may be in this case very appropriate, I donÆt know.

MEMBER APOSTOLAKIS: When are the results of these analysis going to be made?

MR. LANDRY: That's my last slide.

MEMBER APOSTOLAKIS: Okay. Keep me in suspense.

CHAIRMAN WALLIS: Yes, we're a long way from the final rule. Maybe by the time we get to the final rule you will have this, and that would be very helpful.

MR. SHERON: Dr. Wallis, again, let me just reiterate, this is an enabling rule. It does not say that licensees will -- this rule allows licensees to go automatically off and do this. Even though we do these calculations, individual licensees are going to have to demonstrated, for example, if they want to go to manual action for the sprays, they're going to have to show why the timing, why the operators are trained, why this can be done reliably.

CHAIRMAN WALLIS: That's very important, I think. The rule doesn't allow all these things to happen automatically, and therefore the kind of thing that is being done here is going to be very helpful and you're assessing the applications from industry resulting from the rule.

MR. SHERON: Yes.

MR. LANDRY: The quick answer, George, is the spring.

MEMBER APOSTOLAKIS: Oh, that's fine.

MR. LANDRY: The PRA, which is being looked at by the industry, is going to look at multiple effects. As we talked about with EDG reliability changes, do the longer start times improve reliability is it less demanding on load sequencing, et cetera? Those effects can be looked at within the PRA. But with respect to the containment, as we already talked about, does changing this switchover time from RWST to sump affect the reliability of the human factor by giving the operator more time in which to make a switchover? Does it reserve water?

MEMBER APOSTOLAKIS: What kind of -- the change, what is it? Because if it's only a few minutes, I donÆt think you're going to see anything.

MR. LANDRY: We were talking about the spray actuation time could be changed on the order of hours.

MEMBER APOSTOLAKIS: Hours. Oh.

MR. LANDRY: The initial discussions which we've had with industry indicate that this could be hours, more than 40 minutes.

MEMBER APOSTOLAKIS: Because, as you know, the human reliability models are not that sensitive to changes in time. But if you go to hours --

MR. LANDRY: That's what the staff had said when we started talking about this, that if it's only a matter of minutes, it's not going to make a change. If it's 40 minutes, an hour or more, then it may have an effect. We don't know that until we run the calculations.

MEMBER BONACA: And still maintain the capability to mitigate beyond TBS?

MR. LANDRY: Downstream. Another phase in this analysis work is that we are planning on doing work with our Office of Research looking at the effects of changes in mitigation strategies, mitigation requirements, what analyses can show --

MEMBER BONACA: The reason why I'm asking that question is that you want to delay the start of the spray as long as you can, but you still have constraints of mitigating beyond the transition break size which may impose some requirement. I don't know what it's going to be. So that's why there's a tradeoff there how much you can gain in the delay of the time.

MR. LANDRY: All right. That gets into a whole different area, because then you start weighing which plants have safety-grade air coolers, which don't. If they have safety-grade air coolers, they may not need sprays for a very long time. This becomes very plant-specific, but right now what we are doing is a first attempt at attempting to quantifying what are some of the risk changes, the safety benefits.

MEMBER BONACA: Yes. All I'm saying is that in quantifying the safety benefits you can't assume that you're going to have all latitude to change these things. You still have the constraints coming from the mitigation necessity beyond transition break size that will limit how much of this can be gained.

MR. LANDRY: Right. We're arbitrarily limited ourselves to the TBS, to the range that would still be the design basis accident, the range which would still require the conservative assumptions for the analysis, single failures, et cetera. We are not looking at the range beyond the TBS to the double-ended guillotine break where we would relax the requirements and say you could use full ECC, you don't have to take single failure or single failure, et cetera. That would be another stage in trying to study and quantify what the safety benefits are.

MEMBER BONACA: If that's true, then we're not independent.

MR. LANDRY: We realize that.

MR. DINSMORE: Dr. Bonaca, this is Steve Dinsmore. I think what you're asking is whether we're going to select a change and fully implement that change into the PRA so that all the plus and the negatives of this change are reflected in the results. And I believe that's the plan.

MEMBER BONACA: I'm only saying that if you say that I can delay my actuation of the spread by one hour, it's a great gain and all that kind of stuff, and then when I do the actual analysis I find that I can't do beyond ten minutes because I have to deal with still this defense-in-depth capability beyond transition break, then we get the wrong picture of the results. We get some results that give us comfort and they may not be correct. That's all I'm saying.

VICE CHAIRMAN SHACK: We have to finish up here in about a minute.

MEMBER BONACA: Understand that, but that's important, I think. Otherwise we mischaracterize the benefits of the change.

MR. LANDRY: Okay. Our schedule is to complete these calculations in May of 2005. We wanted to have these calculations available to support the development of the reg guide. So we're pressing to have these calculations done in May and, again, we do want to share the results with the appropriate subcommittee. As the results are reviewed and we are sure the results are right, we would like to come forward with you all and share the results and discuss them with you.

VICE CHAIRMAN SHACK: Next is Mike Tschiltz.

MR. TSCHILTZ: Go ahead and go to the next slide, please. Next slide, please. Thank you. This slides provides a summary of the four significant changes involving the risk assessment that have made to the proposed rule since the staff last spoke with the committee. Next slide. You'll get a chance. The slide goes into them in detail.

The first issue is late release frequency. I'm trying to be sensitive to the time issue here. The proposed rule has been changed to no longer provide a specific late release frequency acceptance criteria, although a later release frequency calculation will still be required for changes that have an impact on containment performance. It will be evaluated as part of the defense-in-depth assessment to ensure that a reasonable remains between core damage prevention, containment failure and constant mitigation.

Why did we make the change? The staff felt that the best place to evaluate the late release frequency was in the consideration of defense-in-depth. More specific guidance will be developed and provided in the associated reg guide, and guidance will provide for consideration of both qualitative and quantitative information.

We still need the calculation of late release frequency for changes to the facility where CDF and LERF metrics are not sensitive to the change, such as changes to the containment spray system.

If you recall, an inconsequential change has been defined as one when considered by itself and when considered in combination with all other inconsequential changes --

MEMBER APOSTOLAKIS: Let me understand something here. It seems to me when you say that LRF will be evaluated when considered in defense-in-depth, in essence what you're saying is we will leave it up to the judgment of the decision maker whether LRF plays any role or not. Is that true? I mean considering defense-in-depth is really a judgment call, and you are removing explicit criteria.

MR. TSCHILTZ: Yes. And I think there had been a great deal of work done in the early '90s on late release criteria, and I think it becomes very complicated as far as coming up with criteria that don't usurp the other criteria that are directly linked to the QHO, CDF and LERF. So I think that the judgment here was that this was a complicated enough metric that it needed to have a careful assessment as opposed to an arbitrary type of metric with a set limit, that we needed to consider a number of factors in the decision.

MR. SHERON: The other thing, Dr. Apostolakis, is that we looked and we said why is this unique to 50.46 as opposed to 1.174, in general? So I think the though was is that at a future revision of 1.174 we would consider a late release frequency in a more global context rather than just single it out for this rule change.

MR. TSCHILTZ: Okay. Back to the

definition of the inconsequential change. It's one that when considered by itself and when considered in combination with all other inconsequential change remains insignificant. It does not become significant. For those type of changes that can be quantified, we've set the limit as one E to the minus seven CDF and one E to minus eight LERF, but we expect most inconsequential changes that are quantifiable will be much less than these limits.

Why did we make the change? The staff felt that requiring licensees to track the cumulative risk of inconsequential changes was overly burdensome and unnecessary and that there were other measures that remain that assure that the facility risk remains acceptably small.

Why is the change acceptable? The proposed rule requires submittal of a 24-month report by licensees that provides a list of all inconsequential changes. The staff will use this report to evaluate whether the provision for allowing inconsequential changes is being properly applied by licensees, and particularly it will allow us to identify inappropriate parsing of changes where numerous inconsequential changes are being made that should have been considered as one change.

The proposed rule still requires the quantification of the inconsequential change where possible, although there are many changes that may not be quantifiable from a risk perspective. Next slide.

Okay. We reduced the level of detail in the rule that was basically a direct excerpt out of 1.174. Why did we do this? Well, when we discussed this before, we felt that since Reg Guide 1.174 was guidance and not legally enforceable that some of it needed to be incorporated into the rule. I think our first attempt we basically directly excerpted sections from 1.174 into the rule. Upon further consideration we determined that this level of detail was not necessary or appropriate for the rule itself and that a lot of the guidance -- or a lot of the information could be incorporated in the associated reg guide.

Why is this acceptable? What remains in the proposed rule are what we consider to be the high level requirements that provide sufficient control for safety and risk. The requirements that remain in the rule that are related to Reg Guide 1.174 include, first, a requirement concerning the PRA scope and quality. The proposed rule requires that licensees quantitatively address risk from all sources that would affect the regulatory decision in a substantive manner. And for issues that are addressed qualitatively, the proposed rule requires that the analysis be conservative enough to provide a high confidence in the decision.

Second, a requirement that specifies the risk acceptance criteria. The proposed rule provides high-level criteria that will be spelled out in greater detail in the associated reg guide, and it requires that the risk from 50.46(a) change is small and that baseline risk to the facility remains relatively small.

And, third, a requirement that specifies that as a part of the PRA updates licensees must submit a report to the NRC when changes to a licensee's PRA result in either a greater than 20 percent increase in the baseline risk or a greater than one E to the minus six CDF or one E to the minus 7 LERF, respectively. Next slide.

Bundling. Changes that are enabled by 50.46 or changes that are associated with ECCS performance or associated with the consequences of the LOCA, bundling will allow the tradeoff of risk reductions associated with unrelated changes with risk increases associated with changed enabled by 50.46(a). We only envision this to be necessary or useful in situations where the 50.46(a) changes, the cumulative effect of the changes exceed the acceptance criteria. In these cases, it provides licensees with the incentive to reduce the overall risk of the facility by making other unrelated changes.

MEMBER KRESS: Will you allow administrative changes to offset changes in hardware?

MR. TSCHILTZ: Administrative changes as far as -- I'm not seeing how an administrative change --

MEMBER KRESS: Some procedure on how an operator does.

MR. DINSMORE: This is Steve Dinsmore from the staff. Essentially, the way it's written out is that it would allow that. We'd have the opportunity to review each one individually, because these bundled ones have to come in for review.

MEMBER APOSTOLAKIS: So the defense-in-depth consideration, though, probably will veto it.

MR. RUBIN: Let me add that excessive use of programmatic methods is discouraged in 1.174, and we will carry that same philosophy through here. So if it relied heavily on a programmatic method for a significant risk reduction, it's likely we would not accept it for bundling.

MR. TSCHILTZ: This concept is allowed in 1.174. It's described as an unrelated change in consideration of a combined change request. And, basically, 1.174 requires the reviewer to examine the relationships between the proposed changes. Where one proposed change may have a high degree of uncertainty associated with it, the reviewer is supposed to consider that in the decision. The same would apply here to the example, I think, that you gave.

MEMBER APOSTOLAKIS: No, but I think Mark is right. Excessive reliance on programmatic means is discouraged. And that will be part of the defense-in-depth evaluation, which is separate from the quantitative comparison with criteria.

MR. RUBIN: It will all be part of the decision process of whether that particular bundling package was acceptable.

MR. TSCHILTZ: Allowing bundling will result in changes that have a result and a net decrease in risk or smaller net increases than would occur if bundling weren't allowed. Next slide.

Limitations on bundling. One of the premises of risk-informed regulation is that facilities are built and operated in accordance with requirements. Therefore, if a change were necessary to bring a facility in compliance with NRC regulations, it could not be bundled with other changes. An example of this would be where a licensee discovered a section of piping that was required to be seismically qualified and they made the modifications to the plant that brought it in compliance and seismically qualified the pipe. There would be an associated risk reduction with that change. They could not bundle that with other 50.46(a) related changes in order to meet the risk criteria.

There's additional limitations on the use of bundling that have been derived directly from Reg Guide 1.174. Specifically, bundled changes must not increase risk from significant accident sequences, cause lower rank accidents to become more significant or create new significant accident sequences.

MEMBER KRESS: Do you have a quantification of the word, "significant?"

MR. TSCHILTZ: No. It's not quantified in 1.174, as I'm sure you know, and it's not quantified here.

MEMBER KRESS: Yes, but we always have to ask this question.

MEMBER APOSTOLAKIS: You're using the

language that -- I don't understand why do you have to say, "must not." Why don't you soften it and say that these considerations will be part of the defense-in-depth evaluation as well? In other words, it will be part of the judgment of the decision maker. That makes much more sense. Because you can have an increase in risk from significant accident sequences, but overall that's acceptable if you consider everything else.

MEMBER KRESS: In fact it's more likely.

MEMBER APOSTOLAKIS: Yes. I mean this "must not" is kind of too strong.

MR. TSCHILTZ: I don't know whether those words are taken directly out of 1.174 or not.

MEMBER APOSTOLAKIS: Well, you know, 1.174 didn't come down from the mountain.

(Laughter.)

MR. DINSMORE: This is Steve Dinsmore. The "must" is from the rule because it was written in the rule like that. If we changed it to "should," I'm not sure how that affects the rule language.

MEMBER APOSTOLAKIS: From the rule. Which rule is that?

MR. DINSMORE: Well, the proposed rule. I think we have flexibility in defining "significant" and that kind of stuff, but -- I think we could change the rule, but I don't know the impact of that.

MR. RUBIN: I think the actual process is exactly what Dr. Apostolakis is asking for, is implying. But the word language I think was driven, as Steve said, by our attorneys. But we do of course have the flexibility of determining both significant, what the significant accident sequences are. These aren't defined in the ASME standard either, and that's an issue.

MR. SHERON: I was just going to say that we normally don't put "shoulds" in rules, okay? It's "must" or "shalls." Shoulds go to reg guides.

MEMBER APOSTOLAKIS: Couldn't you say, "must be considered in the defense-in-depth evaluation"? Then you still use "must."

MR. RUBIN: This isn't just defense-in-depth, this is directly impacting the risk profile.

MEMBER APOSTOLAKIS: Well, everything is defense-in-depth.

VICE CHAIRMAN SHACK: We need to move on, George.

MEMBER KRESS: Can I ask one more question of these guys? I was a little disturbed to hear that you backed off the late containment failure criteria. Does this mean you're now going to ignore total number of deaths and the total impact of land contamination in your criteria? Because those aren't really covered. Well, to some extent CDF addresses them, but they're not covered by the quantitative health objectives. Those are individual risks.

MR. TSCHILTZ: Well, I think the reason we want to have the late release frequency in there is because we recognize that a significant amount of the dose to the public from an accident would occur from a late release. That's why we're including it in our decision. The ability to come up with a meaningful metric that we could live with forever or close to forever in the time frame that we are developing this rule is a challenge.

MEMBER KRESS: I understand that, and it's a lot like the safety goals, and those were like pulling teeth. I suggest you give this some thought before the next revision of 1.174. I think that's something that is badly needed, some quantifiable risk acceptance metric that deals with societal risk.

MR. TSCHILTZ: I think we were already planning on doing that as part of our next review in Revision 1.174, because this -- when we were doing the work for this rule, we recognized that we could use additional guidance here.

MEMBER APOSTOLAKIS: Is it necessary to have the last bullet in the rule? Take it out. But if you have to use "must," then take the whole thing out. Nobody's forcing you to put that in the rule.

MR. DINSMORE: This is Steve Dinsmore. But then it --

MEMBER APOSTOLAKIS: Because this is awfully detailed. A minor increase in the risk from significant accident sequences must not. Leave it up to the decision maker to decide whether it's important.

MR. DINSMORE: This is Steve. But we have to have some reason to -- we have to have some authority to request that and to deny it based on this type of information.

MEMBER APOSTOLAKIS: They have a lot of freedom.

MR. DINSMORE: Yes, that's a lot of freedom, but it's also difficult to fully justify the -- but if we have this type of language in the rule, it's clear.

MEMBER APOSTOLAKIS: But what is a minor increase? This says, "must not increase," period.

And the increase is ten to the minus ninth. This says, "must not."

MR. SHERON: I think you've raised a good point. We can look at the words. I mean I wouldn't want to use the word, "significant," twice in the same line, but we could say, "should not significantly increase the risk from significant accident sequences." I think that's what you mean, really. But you're right, there could very small increases that are inconsequential where "must" would -- and I think we've suffered with that with the NOED policy.

MR. SNODDERLY: Excuse me, Mike. Can I follow up on Dr. Kress' question about late release frequency? So is it correct to say then from the period early to, say, 24 hours the design basis of containment now would be driven by the transition break size? In other words, after early, say, two to four hours, to 24 hours, in that time period, what would be the design basis of containment? Would it be governed by the transition break size? I'm trying to figure out what --

MR. TSCHILTZ: Which is the most limiting?

MR. SNODDERLY: Right. What would be the design basis for containment? It no longer would be the double-ended guillotine break, right?

MR. TSCHILTZ: Yes, but the containment would still need to withstand the double-ended guillotine break.

MR. SHERON: It still says they have to mitigate up through the double-ended guillotine rupture, which means that the containment has to remain in tact.

VICE CHAIRMAN SHACK: Yes, but if you take a transition break size with a design basis pressure, will that be more limiting with a large break with a realistic failure criteria? That's the question that Mike is after, if I can understand it.

MR. SHERON: The double-ended guillotine is going to produce the largest mass and energy release into the containment and will produce the largest challenge to the containment.

VICE CHAIRMAN SHACK: Right, but as I read it, you're going to have different -- you no longer can have the design basis pressure for the containment.

MR. SHERON: We said we would look. I think if I remember correctly we would look at whether or not it was acceptable to allow increases, say, above the appropriate ASME code service level. For example, if the containment design pressure is 55 psi and let's say a licensee comes in and proposes an uprate in power such that the mass energy release goes up to 60 psi, I think what we said -- help me, Gary, if you remember -- we said that we would take a look at that and as long as we were preserving substantial margin with that, then we would probably allow that. But we were not going to just give up on the design basis for the containment at all. Does that make sense?

MR. LANDRY: That's another one of those plant-specific calculations, because when we talk about the service levels for containment, it's for a particular containment design. The design pressure, the yield pressure and the ultimate pressure for a large dry are significantly different than from a freestanding shell. So that we have to be very careful when we talk about changing allowable pressure limits for a containment. What containment design are we talking about here before we start saying we can allow these changes.

VICE CHAIRMAN SHACK: Yes. I mean you're going to still have assurance of the containment integrity, but it's not clear to me that the design basis will always be the large-break LOCA, the DEGB. It may, it may not be; I just don't know.

MR. LANDRY: The steam line break is still in the design basis. And the main steam line break on pressure is only slightly below the LOCA. It's only a couple psi less than a LOCA for pressurization. It is in virtually all cases the limiting event for temperature in all containments. So simply changing the LOCA requirement or LOCA limitations really isn't going to affect significantly the containment requirements.

VICE CHAIRMAN SHACK: We're going to have to move on now.

MEMBER SIEBER: Well, the leak rate would be higher with the larger break, which is also the design requirement. It's possible you may move to a different service level for containment.

MR. LANDRY: Leak rate is a function of service level and pressure.

MEMBER SIEBER: Right. Right.

MR. LANDRY: The leak rate doesn't go as a stop function with service level. It's a linear function.

MEMBER SIEBER: Right.

MR. LANDRY: As you go up in pressure, the leak rate is going to keep going up.

MEMBER SIEBER: It will go up with it,

right.

MR. LANDRY: When you go from Service Level A to B, you don't have a step function change in leak.

MEMBER SIEBER: Right.

MR. DUDLEY: Dr. Shack, Brian Sheron has some concluding remarks he'd like to make.

MR. SHERON: Well, I just wanted to thank the Committee for allowing us to come down and make the presentation. I just want to point out we've worked kind of long and hard on this. If you counted the number of hours we agonized over this, this was not an easy rule. We think that based on the letter we got from the Committee I think last December, we've actually moved the rule closer to meeting your comments.

VICE CHAIRMAN SHACK: Except for containment failure.

MR. SHERON: I'm sorry?

VICE CHAIRMAN SHACK: Except for late containment failure.

MR. SHERON: Well, what we said, I think, is that we agree with you that -- and I agree with Tom that it's something that needs to be e considered. We need to do it in the context of 1.174. It's not a unique parameter or metric just for this rule, okay? And I think we've said that we would -- you know, as we go forward with 1.174, it is something we will explicitly consider. And to the extent that we change 1.174, it would probably be retroactively applied to this rule as we go forward. But in the same sense, as you heard, we're not ignoring late containment failure considerations when we look at the risk analyses here.

I'm going to be mercenary and say we would love to get a positive letter so we could get this up to the Commission and like to go forward with it and at least get the public comment period started. So with that, I'll close.

MEMBER APOSTOLAKIS: So we're way ahead of time.

MR. HARRISON: You ready?

VICE CHAIRMAN SHACK: Yes.

MR. HARRISON: Well, I guess it's still morning. Good morning. I want to thank the ACRS for giving us this opportunity to status the industry's efforts at evaluating the proposed change to 10 CFR 50.46. Ralph Landry covered a number of the things that I was going to discuss, so I will be brief.

The first slide was intended to put this work in context and I think we've discussed this to some extent. The point that I want to make here is that we view the proposed rule as a key part of the change in the regulatory structure that will serve the industry and the regulator for the long term. These are example safety benefits. They're not the primary purpose or necessary desired outcome of this proposed rule change.

And I'd like to also point out we think the proposed rule is the right thing. We believe that what we're seeing is that the proposed rule is safe, preserves the safety of the plants. It's consistent with the vision that's up here. It is an optional rule, we want to reiterate that, which makes it easier for the industry and to regulator to implement. And I think it establishes the environment for going forward to identify changes in the future.

I think as Ralph mentioned, we met with the staff in January and had a very effective discussion on how the evaluation should proceed and what kind of information the NRC would need from the industry in supporting their evaluation. And today, we focused on the two examples of safety benefits.

It says here we are supporting development of the implementation guidance. That's still in its very early conceptual stage, so I'm not going to spend any time on that today. And the discussion on the two examples is going to be qualitative because we donÆt have the final quantitative results that have been vetted through all our stakeholders.

Ralph discussed how we were doing the modeling with the diesel generator start requirements. We expect the longer start times to have an increase in diesel reliability, and we have been doing quantified evaluations of that. We've introduced station personnel that are familiar with diesel reliability. Their response has been very positive with regard to extending start times from the ten seconds to something like 30 seconds or a minute.

And we've also reviewed INPO EPIX data from diesel generators for the past eight years, about 800 diesel generator reports. And the preliminary results are showing a decrease in start failures, decrease in run-time failures due to the reduced wear and tear of fast starts and the potential for decrease corrective maintenance that you have to take to address those start run failures, which clearly affects the availability of the diesel.

We're taking those results and we're going to run those through several plants PRAs. As you would know, plants vary in their susceptibility or sensitivity to the loss of off-site power events.

MEMBER APOSTOLAKIS: What results are these, preliminary results? What results? I mean where do they come from? You said preliminary results?

MR. HARRISON: Preliminary results indicate we have started to take some of the -- we've begun to try to quantify the effect of this interview with the station personnel and --

MEMBER APOSTOLAKIS: So these are the results of interviews?

MR. HARRISON: Interviews and looking at these 600 cases up there on what effect were these cases attributable and how many of these case could be attributable to issues related to fast starts of the diesel.

MEMBER APOSTOLAKIS: Which is also a matter of judgment.

MR. HARRISON: Well, certainly, the evaluation of the individuals performing those evaluations, yes.

MEMBER APOSTOLAKIS: So you would say, let's say, from the 600, I don't know, 425 were due to the fact that we started within ten seconds.

MR. HARRISON: Or however many there are.

MEMBER APOSTOLAKIS: So now if I didn't have to do that, what would you do? You would eliminate the 425 failures from the pool?

MR. HARRISON: You would evaluate whether that failure could be eliminated from that pool. I don't have the exact details on how they have addressed those values, and that would be part of our report.

MEMBER APOSTOLAKIS: Okay.

MR. HARRISON: The containment spray results, as Ralph has indicated, the changes that could affect the LOCA accident progression, as we mentioned before, are to reduce the potential for human error in performing the manual actions for going to recirc. And they minimize or eliminate major debris transport mechanism to the containment sump. Of those two, the one that we're quantifying is the first one, which is the potential for human error in performing the manual actions.

Also, for smaller LOCAs, you have the potential for using normal shutdown cooling as a long-term stable state to maximize that.

CHAIRMAN WALLIS: I think it would be very good for the industry if you could show that this rule would enable you to do something about the containment sump.

MR. HARRISON: Well, I think qualitatively just looking at what we have to do to the models on that is right now the models are based on the existing sump size assumptions and failure probabilities. And you would say, okay, then if I now assume I don't have to initiate containment spray, this is a change that we wouldn't have to make to the model. We haven't really looked at how we would quantify that, so this has just been a qualitative assessment at this point.

MEMBER SIEBER: With respect to debris generation and transport, have you tried to estimate how much debris generation and transport comes from the actual jet impingement of the break as opposed to the effective containment spray, which typically has much less energy content?

MR. HARRISON: I think there have been -- I'm sure there might be some people who can address what the -- and you all have probably heard the discussions on the modeling that has been done. My understanding is that the containment spray transport is a lot of what washes down from loose stuff inside the containment.

MEMBER SIEBER: Lose all the dust.

MR. HARRISON: But it also adds to the

volume and velocity that goes into the sumps. And the other detriment that containment spray provides here is the water that's used for containment spray can't be used to inject into the core. So you're competing with safety injection on core cooling.

MEMBER SIEBER: Well, I'm struck by the word about halfway down there, "eliminate major debris transport." If you have a break, you're going to have debris transport.

MR. DUDLEY: You'll have debris transport, but the way it's currently done, Jack, it really is a contributing factor to the amount that makes it to the sump.

MEMBER SIEBER: I agree with that.

MR. DUDLEY: Yes.

CHAIRMAN WALLIS: I donÆt think he's eliminating debris transport. He's eliminating one of the major mechanisms.

MR. HARRISON: Right. That's correct.

VICE CHAIRMAN SHACK: It's a major debris transport mechanism.

MR. HARRISON: That's my understanding, it is a major contributor.

VICE CHAIRMAN SHACK: Now, you're going to still address Dr. Bonaca's question of how much of this benefit you can get and still mitigate.

MR. HARRISON: Well, that's true, and I'm glad you asked it. That was one of the comments that I wanted to make, and I'll go ahead and make it now. We had the question, does the risk from the large-break LOCA increase, and I'd point out that there certainly is no change until a licensee actually makes a change to their plans. That's the first thing I'll point out.

For the standby diesel generator, I'd comment that it probably -- changes, it probably makes no difference in the core damage frequency because the ten-second assumption, remind you, is an arbitrary deterministic time, and we don't -- within the PRAs we don't say that you have a loss offsite power at the time of the break. So I would anticipate that there will be no change in the core damage failure probability for the larger breaks.

MEMBER BONACA: My question was a different kind. I just simply said that you do not have freedom in modifying your parameters, such as price set points and things like this. It's too bound by some requirements that comes from the beyond transition break, and you don't know what they are yet.

MR. HARRISON: Right. Now, when you go -- on sprays, that's a good point. We've always made that statement that -- diesel improvements, I think, are more applicable across the board to more people. And like I said, it varies with your sensitivity to loss of offsite power scenarios. Containment spray is more plant-specific. It varies a lot with the design of the plant, the size of the containment, what you depend upon sprays for, whether you have safety-related reactor containment fan coolers and so forth.

So whether you would change the contribution for the larger breaks for containment spray is going to depend upon your plant design, and it may vary from essentially none for a plant like South Texas, I think we would probably see no change where containment spray is not a contributor to core damage frequency, to other plants, smaller plants where containment spray is credited and they would not see the same benefit. In any case, I think it's going to be zero to very, very small.

MEMBER POWERS: Do you run into a Part 100 problem to laying the spray?

MR. HARRISON: I think the short answer to that is no. The source term would already be addressed by, what is it, 50.67, the source term. There may be, I think, opportunities to use the source term in conjunction with this rule, the alternate source term. If you still have to assume a -- certainly, for Part 100 in consideration of offsite dose, you'd still have to consider a deterministic source term.

MEMBER POWERS: I guess I don't understand. Your worst two-hour concentration is guaranteed to be higher, isn't it?

MR. HARRISON: I'm sorry?

MEMBER POWERS: Your worst two-hour concentration of suspended radioactivity in the containment atmosphere is guaranteed to be higher if you delay the spray.

MR. HARRISON: That is right, and that's why I'm saying you may need to credit alternate source term.

MEMBER POWERS: I don't think that will give you any advantage at all, because the amount of particulate that you're going to have in the atmosphere is going to be pretty significant if you don't have that spray operating. Two hours you've gotten everything that you're going to get out of the --

MR. HARRISON: I can only cite the initial results that we have been able to do in South Texas. The initial results that we have at South Texas suggests that with the -- that we do not -- with the alternate source term, we will not need containment spray for dose. Again, I would stress that this is a plant-specific analysis. It may be that not everyone can use the same results or achieve the same results.

MR. PIETRANGELO: But that's one of the limiting factors we talked about before, I think.

MR. HARRISON: Right.

MR. PIETRANGELO: You have to meet that. You cannot get out of that by doing this.

MR. HARRISON: I think I made all the points I was going to make on that one.

The summary I'll stress that the preliminary results are positive, that the valuations for both examples are showing a safety benefit. I stress again the results are going to be plant-specific.

And, again, just for context purposes, that these are example cases, and we're really looking for the rule to establish the framework to identify additional safety benefits for future applications --

operational benefits.

CHAIRMAN WALLIS: So there are some benefits, but I didn't see you speaking as if they were spectacular or so you're saying that they're wonderful benefits and that -- they are benefits.

MR. HARRISON: They are benefits. I think of the two that the diesel generator reliability will be the more significant of the two benefits. I think that's implied, if not almost specifically stated here.

CHAIRMAN WALLIS: Is there some way to quantify that benefit so we know how big it is? How big is it?

MR. HARRISON: We're in the process of quantifying that. Again, that's not been -- we don't have the final results, but it will be --

CHAIRMAN WALLIS: How big is it likely to be? I mean you must have some idea of the order of magnitude.

MR. HARRISON: I'm not even going to try to --

MEMBER APOSTOLAKIS: Let me understand this. To what extent a statement like that depends on our ability to quantify these things?

MR. HARRISON: Well, it depends upon the

--

MEMBER APOSTOLAKIS: I mean I question where we are --

MR. HARRISON: You need to be able to quantify and make the relationship between the data --

MEMBER APOSTOLAKIS: Sure.

MR. HARRISON: -- that we're evaluating. In other words, if we say, "Well, we're going to increase diesel generator reliability by five percent or ten percent," then we need to be able to use the data that we have to say that these data support that change in diesel generator reliability. We can make that relationship between those data.

MEMBER APOSTOLAKIS: Right. It seems to me in both cases there will be a considerable use of judgment just to the impact on the safety benefit. On the one hand, as we said earlier, we have to decide which failures of the diesels that have been reported were actually due to the fast start time.

MR. HARRISON: Right.

MEMBER APOSTOLAKIS: And then use some judgment to say, "If I didn't have that, something would happen." And with the human reliability, as we discussed with the staff earlier, unless you go to hours, the current models really will not be able to tell you, "Boy, this is really better, because you increased it by 15 minutes."

MR. HARRISON: Well, as they're saying, 15 minutes doesn't help very much, but if you increase it by an hour, you could probably increase human reliability by maybe a factor of five or an order of magnitude, perhaps. And that can help some plants.

MEMBER APOSTOLAKIS: So you think the main benefit is the diesel reliability.

MR. HARRISON: That's my judgment. And the reason I say that is because I think that it would be more broadly applicable to more plants.

MEMBER APOSTOLAKIS: Oh, I see. I see. Thank you.

MR. HARRISON: And that concludes my discussion. If you have any questions --

MR. DUDLEY: May I ask a question? Obviously, we're interested in things that are potential safety benefits. As far as the economic benefits are concerned, is it obvious to you which things you would go after now? I mean is it clear if this is enacted that you're going to go and ask for some changes to the plant that would involve very small increases for economic purposes?

MR. HARRISON: We have a pilot plant

that's ready to make an application. I think that we have quantified some business cases for this. We've looked at, for instance, some of the testing requirements on the diesel generators. We think it's an advantage to us. One of the things that the jury's still a little bit -- still out on is the analytical savings that we would see from not having to do detailed large-break LOCA analysis to the same degree we had. So one of the goals of the implementation guidance is that we don't create a process where we have to do a risk-informed beyond design basis evaluation that looks and has the same impact that the current large-break LOCA does. But I think we're seeing certainly some potential savings in that area.

The fuel savings that we've talked about, that's going to be plant-specific. It depends on whether you're large-break LOCA limited. If you're large-break LOCA limited on peaking, you may have an opportunity there, but I think we all recognize that there are other fuel design limits that may give you a challenge, like DNB or actual offset anomaly or what have you.

So, again, I want to say that we're establishing a framework here that will remove what's been a barrier so that as we move forward in time that we can gain some of these additional benefits.

CHAIRMAN WALLIS: I think my colleague asked you about economic benefits, and the regulatory analysis that we saw came up with a major benefit being the potential for power uprate. Is that something that you see from your perspective to be a major benefit?

MR. HARRISON: My personal view is this will facilitate power uprates. Power uprates obviously require a lot of other analytical things that you have to consider. I think that this will at least make the large-break LOCA evaluation certainly simpler and much less of an obstacle for a power uprate.

MR. SNODDERLY: Excuse me, Mr. Harrison? Mike Snodderly back here.

MR. HARRISON: Oh, Mike.

MR. SNODDERLY: The staff told us they anticipated completing their analyses in May 2005 and their reg guide by June 30, 2005 and then initiating discussions with us in the summer of 2005. Can you give us some idea of your schedule or if you think it will be compatible with the staff's? In other words -- because I think when we review the staff's analyses and their reg guide, we'd ideally like to be able to compare it to what you've developed.

MR. HARRISON: We're working with the staff's schedule, so our intent and plan is to support the staff's schedule with our evaluations and actually to give them -- perhaps if we can to even precede their schedule so they'll have something to look at ahead of time.

MR. SNODDERLY: Okay.

VICE CHAIRMAN SHACK: Tony?

MR. PIETRANGELO: Before I get into some perspective on the proposed rulemaking and some of the other stuff, I did want to offer a few remarks on behalf of the BWR Boiling Water Reactor Owner's Group. They couldn't be here today but they did send me some stuff to ask me to include in the remarks here, and I did want to do that.

Obviously, we haven't seen what's in the proposed rulemaking package with regard to the specific rule language. The first version of the rule specified the 14-inch and 20-inch for BWR double-ended break. I believe, if I could surmise correctly, that the current version says something like single-sided of the largest attached pipe.

In the case of the BWRs, that doesn't do them much good, because it's still 20 inches with their recirc piping and their RHR lines. So from terms of enabling anything with regard to boiling water reactors, this rule does not do that. And, again, on behalf of the boilers, they do think that there is in the neighborhood of something less than 14 inches, consistent with the expert elicitation results, would allow them to accrue the same types of safety benefits as well as other benefits that they could get with their current topical report that was submitted last year on the separation of loss of offsite power from the large-break LOCA.

Now, that's been in the staff. That's been deferred because of this rulemaking plan, but this rulemaking, given that the GDCs don't apply beyond the transition break size, could accomplish the same purpose that the boiling water reactors were included in the ruling.

So in terms of being enabling, it doesn't do it for the boilers. They did submit comments to the staff in September as part of the regulatory analysis input following the workshop late last summer. I know it's too late for the staff to do anything with the current package and probably even for the Commission to do anything at this point, so this is obviously something that's going to be commented on in the proposed rule stage, but I'd ask you to -- I'm planting that seed now because we're going to come back to this point when we have further deliberations.

So the boilers think there's a case to be made for their inclusion as being enabling in this rule with regard to break size, and there's lots of benefits like the ones that Wayne talked about diesel generator reliability, on optimized DCCS performance on enhanced decay removal capability as well as simplifying some of the text spec surveillance requirements. The same kind of safety benefits we're trying to quantify here we could do the same thing now if the boilers could play in the sandbox, if you will. So I just wanted to offer that on their behalf.

Okay. Turning to the -- let me start at a really high level. Why are we doing this rulemaking? What is the purpose of this rulemaking? What are the success criteria for this rulemaking? What do you really want to get out of it? And I guess I could go around and poll each ACRS member, but let me just suggest one to save time.

If at the end of the day this rule doesn't provide the option at least to get licensees and the NRC to focus more on safety-significant matters, it's a failure. It will be perceived as a failure. I mean that is the intent. That goes back all the way to the definition of risk-informed regulation. Focus on things more that matter, more of the stuff that doesn't matter or that's less significant. So that's what this has to achieve at the end of the day.

Now, we just talked about enabling beneficial changes. That to me is a sub-tier. It has to -- if you can't do anything that's beneficial as a result of the rule, it's a failure. It's just out there, people won't pick it up. It we go through all this work, staff went through all this work, industry went through all this work, nobody picks it up, it's a failure. So it has to enable beneficial changes. I think that's why the boilers want to be included in this.

CHAIRMAN WALLIS: I noticed you said safety-significant matters were beneficial, but how about the power uprates? There are benefits which are not related to safety.

MR. PIETRANGELO: There are.

CHAIRMAN WALLIS: That are enabled by this rule.

MR. PIETRANGELO: There are. There are. But at the end of the day, you still have to be able to make a case that you're focused on safety more than you were before. I you can get some economic benefits out of that and do the same thing, great.

Now, there's another element of this, and I think consistent with the history of risk-informed regulation you see this, and that is, well, how do you control the potential changes that this thing enables? And I think that's where a lot of that part of the rule that the staff worked on comes from. And I understand that. From a regulatory perspective, you don't want to enable something that could have a significant increase in the risk profile or decrease safety at the plant. So I perfectly understand that.

My point is that at the end you have to have a balance, okay, that you can't burden licensees on looking at things that are inconsequential or burden the NRC staff with amendment requests on things that are inconsequential or review of things that are inconsequential, because if you do that, you won't meet the higher-level purpose of focusing on things that matter more. So there's a balance that has to be achieved there. I understand the regulatory perspective, but there's an attention and resources perspective that also has to be balanced.

CHAIRMAN WALLIS: Well, that's what I'm

waiting for really is the consequential things. I think that there are a lot of inconsequential things. I'm not really interested in those. But if you can show there are some really consequential changes which matter, then that will be great. I don't think we've got to that point yet.

MR. PIETRANGELO: I'll get to that in a second. To me there's three basic issues wrapped up in this rulemaking, okay? The first has been the focus on the break size. A lot of -- that's the whole expert elicitation, three years worth and even before that talking about it has been focused on this expert elicitation. So when that effort's over, I mean you've looked at it six times now, you're going to get a seventh shot at it later, I think we're going to have a pretty sound rationale for saying this is it. And it will be reflective of the expert community.

Part of the safety benefits calculations that Wayne talked about and that Ralph Landry talked about before are really aimed, I think, at trying to give us some more confidence that when you put the TBS at a certain spot consistent with that expert elicitation, you can in fact enable beneficial changes. You don't want to set it so high that it doesn't enable anything. So those calculations, those quantifications will help to inform that.

But I've got to tell you, I don't need a PRA calculation that tells me if I increase the diesel start time from ten seconds to 60 seconds, I don't need a calculation to tell me that's better.

CHAIRMAN WALLIS: It's better, but how consequential is it?

MR. PIETRANGELO: Doesn't matter. Doesn't matter. Doesn't matter.

CHAIRMAN WALLIS: But you said you used --

MR. PIETRANGELO: I don't need to have it quantified.

CHAIRMAN WALLIS: Don't want it to be an inconsequential thing.

MR. PIETRANGELO: Why? I know, and I think the qualitative data will tell you that it's better. To delay containment spray, and Dr. Powers brought up the part about the Part 100, I mean we already have to assume that you have a degraded core in order to scrub the containment spray. But in delaying containment spray it doesn't mean that there can't be some operator actions that look at actual radioactivity levels in the containment post-accident or have interlocks with radiation monitors that would actuate containment spray versus let's just assume it is per the current design basis and just flood the containment with all that containment spray, bypass the core, wash all that debris down in the screens.

I mean, intuitively, I know that it's better if we do it smart, and we can quantify what the delay and emptying the RWST is and the delay to switch over and how much that will improve the reliability in doing that. And we'll do it. But I don't have to do it to know that it's better. And there's thousands of examples like that. I don't have to know that if the diesel starts in 11 seconds instead of 10 today I've got to tear the diesel down and go fix something to get it to start at ten seconds. That takes the diesel out of service. It's unavailable, okay? Is that good for safety?

MEMBER RANSOM: But the real question is is 60 seconds any better?

MR. PIETRANGELO: Right.

MEMBER RANSOM: Significantly better.

MR. PIETRANGELO: Right.

MEMBER RANSOM: Because starting a diesel engine it takes maybe an hour to bring it up to thermal --

MR. PIETRANGELO: Right. But even beyond just the diesel itself there's the sequencing of the loads, and most of these are done right up to the max of what those buses can handle. So I think by allowing those loads to come on more gradually, okay, that you can actually improve the reliability of the whole ECCS. And we don't have time to go do calculations on all that different stuff, but, intuitively, and I think if we apply expert opinion and judgment to this, we can say it's better. So we're going to do the quantifications and I hope to get some of the other owners' groups in on this because I think there are benefits associated with this and it makes a strong safety case. Again, the rule has to enable that.

The second part of the issues or the second issue to me that's important with this rulemaking is this demonstration of mitigation capability, and that's what Dr. Bonaca raised before. You're going to change the design basis of the facility from this double-ended largest break in the RCS to something smaller, the TBS, all right? To me, a big part of the defense-in-depth is this mitigation capability all the way up to that largest break. We still have to demonstrate that.

There's been next to no discussion, even in the industry or with the NRC staff, on what's good enough or that demonstrating of the mitigation capability. And I won't be able to answer Dr. Bonaca's question sufficiently until I know what's good enough there, because then I'll know what leeway I have between my new design basis and what's good enough for this demonstration of mitigation capability.

At least from my perspective, this is probably the most important part of this rule, because that's what's different. If I'm a licensee and I'm going to opt for the new 50.46, okay, for up to my TBS I'm going to use the same method, same rule, same requirements that I was using before; nothing changes. What changes is I've got this other thing, this demonstration of mitigating capability. I don't know whether the staff wants to review and approve it, I don't know what to do for current code. There hasn't been any discussion on that. So we need to have that.

But if the licensee ops, I'm guessing that staff's going to be interested in what their mitigating capability is, because that's going to be part of the license. It won't be the design basis, but it will be part of the licensing basis. And you're going to be asked to maintain that going forward. So that's a significant piece.

MEMBER BONACA: I'm sorry, I thought, however, reading the statement of consideration, that there is a significant intent or an accession. I mean there is a lot of concessions being done. Now, clearly, it's not fully defined yet in the reg guide.

MR. PIETRANGELO: Right. Right. And that to me is the focus of the rule, should be the focus of the reg guide, all that stuff.

MEMBER BONACA: Yes. But I'm saying that on that issue the door is open, it seems to me.

MR. PIETRANGELO: I hope so, yes. I hope it's open. Yes, because we haven't had any discussion, we haven't see that. So I'm glad to hear you say that.

MEMBER BONACA: Oh, okay.

MR. PIETRANGELO: I haven't seen it. Okay.

Now, the third issue wrapped up in this is one I alluded to before, this kind of change control. Now, one of the kind of principles we've always used in risk-informed regulation is we try to build on the existing regulatory framework before you invent something new. And if you're going to invent something new, you'd better have a really good reason why you've got to go it differently than what the current framework tells you to do.

So what do we do today for change control? Well, we've got 50.59. Been in place since the mid-60s. It was significantly improved, I think, in the late 1990s. Licensees have been using it every day. Every change that's for something that's described in the FSAR and even some that's not described in the FSAR are run through this 50.59 process. The SAR's updated as appropriate, the safety analysis report. These changes are reported to the NRC periodically. And you don't have to do any risk assessment on any of these changes. You don't. That's what we have in place today.

Now, we're going to do this new TBS for the 50.46. Was PRA used as the basis for this change? I don't see any. I do know that any change I make going forward I still have to meet the current design basis, the SAR analysis up to that transition break size. I still have to demonstrate that I have the mitigating capability for up to the double-ended -- so we will have change control in place with the current framework.

Now, a lot of the talk has been about we have to do more than 1.174 and this and that. Well, those are for risk-informed license amendments, when you have to come into the staff, and even in 50.59. We give examples in the deterministic guide to what a more than minimal increase in risk is or consequences. That's when you have to come in. But 1.174 has a similar threshold about what's small and very small. But all 1.174 is is a broad framework for risk-informed decision making on amendment requests and changes to the current licensing basis. And it tells you you've got to look at all the sources of risk. And it tells you how to input defense-in-depth and safety margins and risk insights. And it's worked pretty darn well, I think.

And a lot of the changes I think that the staff's concerned about are things that are necessarily going to involve amendment requests. You can't do a power uprate without coming into the NRC. You can't change your technical specifications without coming into the NRC. And I'm hard pressed to think of any of the changes the staff would be concerned about that wouldn't drive an amendment request. And in that case, we have guidance on submitting amendment requests. And even if the licensee doesn't use a risk argument as part of that amendment request, the staff has the leeway to ask for risk information if they think it's important to that amendment request.

So at least from perspective, the framework's in place to handle this already, without trying to redo it as part of this rule.

CHAIRMAN WALLIS: Are you saying we don't need a rule at all?

MR. PIETRANGELO: Well, this is supposed to be an enabling rule that incorporates this insight about big pipes don't break as often as little pipes. And that's the insight, okay without any of the quantification and all this other stuff. And it's not -- at least it wasn't our intent when we began deliberations with the staff to turn this into the configuration control change we'd use in risk and codify all that in the rule. Now, it's evident from the staff's presentation --

CHAIRMAN WALLIS: Tony, I want to ask my question again.

MR. PIETRANGELO: Okay.

CHAIRMAN WALLIS: You seem to be saying quite eloquently that we've got a lot of stuff in place already, 50.59 --

MR. PIETRANGELO: Right.

CHAIRMAN WALLIS: -- and 1.174. And you seem to be questioning whether we need any rule at all. That seems to be where you're going.

MR. PIETRANGELO: No, I didn't say we don't need a rule at all. I'm talking about this portion that deals with change control.

MEMBER APOSTOLAKIS: The 1.174 part of the rule.

CHAIRMAN WALLIS: Oh, that part. It's that part.

MR. PIETRANGELO: That part. That part.

CHAIRMAN WALLIS: Okay.

MEMBER APOSTOLAKIS: Tony, what you're saying is that that is not needed at all.

MR. PIETRANGELO: Well, we haven't had a lot of discussion with the staff on this. I really haven't heard a case yet that tells me why I need this all other stuff in the rule. I think the changes that the staff are concerned about are things that are in the current license, that are in tech specs, that you've got to come in with an amendment request anyway.

MEMBER APOSTOLAKIS: Why does it bother you that it's in the rule? I mean it's just redundant.

MR. PIETRANGELO: If it's in the rule? Why add extraneous stuff? I mean that's just a bad practice.

MEMBER APOSTOLAKIS: I mean is it just the beauty of the rule?

MR. PIETRANGELO: No. To me the rule was supposed to be about enabling beneficial changes and getting focused on safety significance. This it doesn't. Look at the staff lines about inconsequential changes and reporting all that and bundling. Is that what the rule was supposed to be about? It's supposed to make you focus on the more safety-significant things. And I don't want to reinvent a process that's worked, whether it's 50.59 or 1.174.

MEMBER APOSTOLAKIS: But isn't the inconsequential part the equivalent of 50.59? I mean that's what they're trying to do. They're trying say, "Well, look, we don't want to review everything."

MR. PIETRANGELO: No. Well, they just told you to report them all. And if they're quantifiable, you should do it and put it in your risk model. Now, I'm not saying that's a bad practice at all. I already report all my changes, whether they're inconsequential or not, under 50.59.

MEMBER APOSTOLAKIS: That's what I'm saying.

MR. PIETRANGELO: So why do I have to

repeat it in this rule?

MEMBER APOSTOLAKIS: So it's really the elegance of the rule that bothers you.

MR. PIETRANGELO: No, it's not the elegance. It's people see -- they're used to a certain way of doing it, and if you're not intending anything differently, don't create something new that makes them do the same thing, because they'll read the words differently, they'll intend something differently, and I already talked about developing additional regulatory guidance

And any specific application like whether it's power uprate or even some of these tech spec things typically what we do is we do application-specific regulatory guidance, especially if it's a risk-informed one. What parts of the PRA am I going to tinker with to show the delta CDF, the delta LERF, late release, whatever? It will be on an application-specific basis. We'll probably develop the guidance and ask the staff to endorse it. We'll even clip it to make sure that everybody does it in a template that the staff's familiar with and facilitate the changes.

So it's hard to say at the outset of this rule how many of those I'm going to need or try to guess on what I need to put in the rule to cover all those things. I understand the urge to do it, I'm just not convinced that the basis is there to do it yet, because no one's shown me that the current framework won't work.

Now, again, I know that's not going to be changed in the current version. We will comment on it when it comes out. I'm not trying to delay the current thing, but we will have this discussion again some day, and I just want to get on record our concerns. And it's obvious there's been movement since the last time. Evidently, the staff took a lot of the prescriptive stuff that was in 1.174 and in this rule and taken it out, so I think it's a step in the right direction. That's a good thing.

One last piece about -- I think I've covered it. I've probably said enough. Thanks.

CHAIRMAN WALLIS: Could I comment on what you said, Tony?

MR. PIETRANGELO: Sure.

CHAIRMAN WALLIS: Listening to you, a great deal of what you said, not all of it, but a great deal of what you said I felt could have been said by an ACRS member. We have the same sorts of questions and concerns that you have. You maybe are freer to be more outspoken and eloquent in expressing it, but I was struck by the fact that a lot of these concerns really are things we've mulled over too.

MR. PIETRANGELO: I'm sorry that you feel constrained to speak your mind in here, Dr. Wallis. That wouldn't have been one of the attributes I thought was yours.

(Laughter.)

MEMBER APOSTOLAKIS: I don't pay attention to that. I don't feel constrained.

MEMBER SIEBER: Thinking is a protected activity.

MR. PIETRANGELO: Well, I'm glad to hear that.

CHAIRMAN WALLIS: The problem is, you see, if I say something that's too outspoken, you will get criticize it, and it will get in the newspaper, but you can say anything you like and I can't criticize you quite the same way.

(Laughter.)

MR. PIETRANGELO: I think the discussion -- this Committee is absolutely essential to this activity.

CHAIRMAN WALLIS: I think it was very good to have your input, and maybe I'm not speaking for the

Committee at all, but personally I felt a lot of the things -- the questions you raised are ones that we have raised ourselves and mulled over too.

MEMBER APOSTOLAKIS: I'm looking forward to debating the last point that you made, because I still think you worry about elegance.

MR. PIETRANGELO: No, it's no.

MEMBER APOSTOLAKIS: Well, you're concerned that maybe these new requirements, which really are intended to be the same as before but now they're qualified in the rule, they might be misinterpreted by people who are already doing this work. Isn't that what you said?

MR. PIETRANGELO: Again, I haven't seen what's in the -- I'm --

MEMBER APOSTOLAKIS: No, I understand that.

MR. PIETRANGELO: Yes. And there may be a need to put something in the rule. But we've already got -- even if it just points you to the existing framework, that's better than trying to repeat a lot of the other stuff.

MEMBER APOSTOLAKIS: Okay. That's a good statement. But you are not -- I mean the final conclusion from your speech is that you are not objecting, based on what you've heard, to having this released for public comment. In fact you are looking forward to submitting --

MR. PIETRANGELO: Because of the schedule there's been precious little opportunity for interaction, and maybe once the proposed rule's out that we can actually engage on what should be in the regulatory guide and that kind of thing. So we want to get on with it. There are certain things that, again, I haven't seen it, that we might want differently --

MEMBER APOSTOLAKIS: Very good.

MR. PIETRANGELO: -- in the proposed rule, but I know, trying to be practical, that trying to change it now isn't going to speed up this process at all. But I would hope that we keep open mind to changes to the proposed rule once everybody can really engage and weigh in.

VICE CHAIRMAN SHACK: Any more comments or questions from the Committee? Turn it back to you, Mr. Chairman.

CHAIRMAN WALLIS: Thank you. I was trying to finish on time but we just missed. We will now take a break for lunch until 1:15, and I'd like to thank all those who contributed to our discussions this morning. Thank you.

(Whereupon, the foregoing matter went off the record at 12:12 p.m. and went back on the record at 1:11 p.m.)

CHAIRMAN WALLIS: The topic we will consider now is the draft safety evaluation report for the North Anna early site permit application.

I'll turn to my colleague, Dana Powers, to lead us through this one.

** MEMBER POWERS: "Lead" may be too strong of a term.

We're going to talk about an early site permit. As most of you are aware, approval of early site permits is a statutory obligation of the committee. All of this playing around on pressurized thermal shock, that's a sidelight. This is the real line business.

This is the first of the early site permits that come in, and for those of you that thought we would get it for enough time to study it, to devise procedures, to test procedures and whatnot, I'm going to have to apologize. The subcommittee was mean enough on yesterday's subcommittee meeting that Laura Dudes promised that she would get even by inflicting about three of these on us at two-month schedules, and that any further obstreperousness on our part, she would invent four or five more to inflict on us.

What we're going to hear is a synopsis of discussions that were presented at a subcommittee meeting yesterday. All of the speakers had promised to attenuate the use of geological jargon in their presentations, though they equally promised that if we're too obstreperous they will lapse back into "geologicese."

What the staff has done is receive the application and prepared a draft safety evaluation report, following a review standard that has been developed, and they're asking from us for an interim letter which would be rather similar to the interim letters that we prepare in connection with design certification.

There are still a few outstanding open items and discussions of conditions on the license that are going on. Apparently there was a meeting today.

MR. GRECHECK: There will be a letter sent in today.

MEMBER POWERS: And so things are going on, but by and large, I would say that the safety evaluation report and the application are pretty complete and pretty well done.

The rules are fairly prescriptive for what the staff has to do once they receive these applications. It is prescriptive on what the application should contain, and consequently fairly prescriptive sense of analyses, and it looks to me like they're pretty well through all of that process. So it's more of a mopping up operation than were made to be done.

So unless any of the members of the subcommittee have points to add, and I don't see any, let us start with a presentation from Dominion by the Vice President, Gene Grecheck.

** MR. GRECHECK: Good afternoon. I'm Gene Grecheck, Vice President of Nuclear Support Services for Dominion.

And what I'm going to do in the next few minutes is just to try to give you a quick overview of what the ESP application is and then also a little bit more about the North Anna site if you're not familiar with it.

First, the reason that we made the application to start with was to determine the suitability of a potential site without having gotten to the point of determining a specific technology that we would like to deploy there. The benefit of the ESP process, at least in theory, is that you can resolve the siting issues early, before you have spent a great deal of resources trying to finish the design of a particular technology.

So that's what we're doing. We've been working with the staff for about the last year and a half on the site itself, and we still have not made a decision or a final decision on a technology or whether we would submit a COL application for this particular site, but at least we're working through the siting options.

The next slide.

Just a little bit about the North Anna Power Station. The site that we are proposing is within the North Anna site boundary. North Anna was originally planned as a four unit site back in the 1970s. Two units were Westinghouse three-loop PWRs. Those were licensed in 1978 and 1980.

Adjacent to that construction permits were issued for two additional BNW units. The construction had actually started. There was actually the steel frame for the containment buildings were actually erected at both of those, when first Unit 3 and then -- well, first Unit 4 and then Unit 3 were canceled, one of those in the last '70s and then Unit 3 was canceled in the post TMI contraction.

All of the above ground hardware that was installed as part of that construction effort was removed. The base mats for the containment are still there down at the bottom of the pit somewhere, and you'll see on the picture shortly that the intake and discharge structures for those plants still exist, and we are studying whether to use those existing structures as part of a proposed additional unit.

The next slide is a 50-mile overview of the North Anna site. North Anna is in western central Virginia south of Washington here. You can see right at the center is Lake Anna. Lake Anna was formed by damming the North Anna River in the early '70s. That dam was built for the purpose of constructing a cooling water lake for the plant.

Within this 50-mile circle, you can see off to the west Charlottesville is about 40 miles or so due west. Richmond is to the southeast about 45 miles or so.

CHAIRMAN WALLIS: What is it, South Anna?

MR. GRECHECK: South Anna?

PARTICIPANT: Another river.

MR. GRECHECK: There's a North Anna River and a South Anna River.

CHAIRMAN WALLIS: I thought you said "nuke."

MR. GRECHECK: Oh, NUG, N-U-G, that's a non-utility generator. There's an independent merchant power plant there.

CHAIRMAN WALLIS: It's not N-U-C.

MR. GRECHECK: No.

All right. The next slide is a little bit closer view. This is a ten-mile view of the site. You can now see the lake. Down at the very bottom there where you see the North Anna River designation, that's where the dam is, and you can see that the Town of Mineral is about seven miles or so from the site.

The Town of Mineral, I think, at the latest population estimates were about 400 people.

MEMBER SIEBER: It has a post office.

MR. GRECHECK: Yes, it does.

The lake is quite popular for recreation use over the years since the plant was installed. You can see just to the northwest of the plant is a state park, Lake Anna State Park, that has a large, transient population of boaters and water skiers that come in through there.

And also there has been a significant amount of residential development around both shores of the lake.

The next slide is zeroing in on the site itself. This is the exclusion boundary of the site. Right in the middle where you see the red X, that is North Anna or Unit 1. The exclusion boundary is measured as a 5,000 foot radius around that, and then off to the left there, that cross-hatched area is the ESP site. That is the site that is being examined for the application.

The area that is right in the center immediately to the left of the two plants where -- as a matter of fact, where the words "Unit 2 Containment" are -- that is the location of the previously proposed and started construction of Units 3 and 4.

We extended the site a little bit off to the west there to provide room for the cooling tower.

CHAIRMAN WALLIS: Now, the center of that circle is not at the red X.

MR. GRECHECK: It's intended to be. Okay.

MEMBER POWERS: It may not be germane either.

(Laughter.)

MR. GRECHECK: And the next slide is a

close-up of the proposed early site permit slide. Again, the rectangular, roughly rectangular space right in the middle of the figure is where North Anna 3 and 4 were, and it is most likely the location of the units if we were to proceed with building them, and then off to the left is a large open area that would be the location of cooling towers if they were to be built.

Next slide is a photograph. This is a photograph of Units 1 and 2. You can see immediately to the left of Units 1 and 2 is a pit. That pit is where the Unit 3 and 4 construction was. Actually there was another construction project, and as a matter of fact, you can see some concrete there at the bottom of that pit. There was a rad waste handling facility that construction had begun in the mid-'80s, and then that project was also terminated. So that area has had several stops and starts, but that would be the area.

But one of the things I wanted to point out on this picture is you can look in this area here. This area right in that area is where the Units 3 and 4 intake is. You can see that there's a cofferdam or a, you know, embankment that's been built there to keep the lake out of that pit, but that would be removed, and that would be the intake for Units 3 and 4.

And the discharge for Units 3 and 4 is up here on the right that would discharge into the existing discharge canal that comes out.

MEMBER KRESS: Are there any dry storage on the site?

MR. GRECHECK: Yes, there are, and that is about right here.

And the final picture in this set is just a very conceptual idea of a generic plant built on that site. That's not intended to represent any design that you might be able to recognize.

All right. The next slide.

This is a little bit about the chronology of the application that was submitted in September of 2003. We have submitted three formal revisions to the application as you can see on those dates. Revision 2 was primarily an environmental, responding to various environmental requests for additional information. Revision 3 was mostly answers to the various safety related questions.

The Revision 2 is also significant because we did modify in that revision the cooling design of Unit 4, and I'll get to that a little bit later, but that was where we officially change the design.

NRC issued the draft SER in December of 2004. That's what the staff will be discussing with you, and later this afternoon, we will submit the response to all of those open items but one. So we will pretty much have all of those open items resolved today.

There are a few items I just wanted to point out to you. I'm sure if you've read the application you've seen that we used something called the plant parameter envelope. This is just a way to represent a potential unit without having specifics about what that unit looks like.

What we have proposed is two 4,300 megawatt conceptual units that could be built at this site, and that envelope envelopes six different reactor technology designs.

CHAIRMAN WALLIS: Is this allowed to be built now? That seems to be awfully big in megawattage.

MR. GRECHECK: Yes, they would be allowed to be built.

CHAIRMAN WALLIS: I thought there was a limit.

MR. GRECHECK: We had that discussion

yesterday, and we're not aware of any --

CHAIRMAN WALLIS: I wasn't here.

MR. GRECHECK: I mean, I think there perhaps was some de facto limit based on the plants that were being built at the time, but most of the advanced designs, if you look at the G.E. BWR, for example or, as a matter of fact, Framatome is currently marketing the EPR; all of those units are significantly larger than the previous one.

CHAIRMAN WALLIS: You're actually very specific when you say 4,300.

MR. GRECHECK: Well, that was based on the plant parameter envelope of the designs that were provided.

MEMBER SIEBER: These are megawatts thermal.

MR. GRECHECK: That's correct.

MEMBER SIEBER: So you basically divide by three to get electric.

MR. GRECHECK: In general we're looking at about 1,400, 1,450 megawatt electric plants.

And when you look at the conceptual units, include the designs, for example, of a pebble bed or a gas turbine GTMHR, which means that these units as defined as 4,300 megawatt thermal could be composed of multiple modules of smaller units and --

CHAIRMAN WALLIS: Would they be put in?

MR. GRECHECK: Yes, they would.

CHAIRMAN WALLIS: It would be an awful lot of pebble beds to get 4,300.

MR. GRECHECK: There would be, but the site does accommodate that, and that site boundary, we have a layout that shows how they could fit on that particular site.

Finally, there have been several issues during the review. Again, we believe that all of the remaining issues that the staff will discuss from the draft safety evaluation report are resolvable, but there has been a tremendous amount of discussion about seismic issues, and I know that we've promised not to talk about that too much, but it has been the first application or the first time we've used the revised NRC guidance that came out during the 1990s about using a different methodology for approaching the design seismic of a plant, and it has been a learning experience, I think, for all parties trying to work through that.

I did want to make a point that one of the issues that is resolved or discussed during an early site permit process is the emergency planning or major features of emergency planning. Clearly, we do have two existing units here, and we have referenced that existing emergency plan and would use all of the features of that existing emergency plan if these units were built.

And finally, Lake Anna water usage has been an issue here because as we indicated, the lake was originally built for four units, and if you go back and look at the licensing history of Units 3 and 4, there was some uncertainty about the overall effect of four large units on this lake, and there were some questions that were left open during the construction permit phase.

As we went through that process for these units, we did make a determination that we would use the lake as cooling for a proposed Unit 3, but for Unit 4, the issues of both thermal effects on the lake, but even more importantly than thermal effects would be water consumption and thereby water level of the lake. Those issues seemed a bit steep for Unit 4.

So in the application we do propose the use of a dry atmospheric cooling tower for Unit 4. So Unit 4 does not use any water from the lake other than for miscellaneous make-up.

Again, I look forward to the discussion, and if there are question I can answer, I'd be happy to do that, but I think that just gives you a good overview of what the application looks like.

CHAIRMAN WALLIS: How do you consumer water from the lake if you're not having cooling towers and things? You don't consume much of it. It doesn't disappear.

MEMBER SIEBER: Evaporation.

MR. GRECHECK: Well, the majority of the water leaving the lake is by evaporation. If you had a cooling tower you have to make up to the cooling tower, and that is a significant drop in --

CHAIRMAN WALLIS: Why is it so much?

MR. GRECHECK: It's actually more usage than a once through cooling system.

MEMBER SIEBER: And a dry cooling tower, so to speak, would have to have a tremendous amount of surface in order to operate a unit.

MR. GRECHECK: It would require a great deal of surface. It would also require motive force with fans.

CHAIRMAN WALLIS: With fans, yeah.

MEMBER SIEBER: Yeah.

MR. GRECHECK: And it would be a rather significant use of electricity in order to make that happen. So our thought is that it is not likely that a lightwater reactor would be built on this site using that cooling system, but there are other reactor technologies included within the PPE that have much less thermal effect, and if one of those were ever built on this site, it's more likely that that would be the way we would go.

MEMBER SIEBER: Well, my question is: have you looked at the size of the site to accommodate such a cooling --

MR. GRECHECK: Yes. That large area that I showed you on the diagram will accommodate that.

MEMBER SIEBER: Yes, okay. Thank you.

CHAIRMAN WALLIS: They'd have less cooling effect because they're more efficient?

MR. GRECHECK: Well, they don't use a water exchange as the cooling medium. The heat rejection is to the air directly.

CHAIRMAN WALLIS: But it would still have to reject it.

MR. GRECHECK: Yes, but it's rejected to the atmosphere.

CHAIRMAN WALLIS: So it would still have to take the same mass of air through something.

MR. GRECHECK: That is correct.

CHAIRMAN WALLIS: So you would still have to have fans and all of that.

MR. GRECHECK: Yes. But I think what I'm saying is that with other reactor technologies, their thermal discharge to the environment is less because they're more thermally efficient.

CHAIRMAN WALLIS: That's correct. So you would have less heat to reject and there would be a smaller cooling tower as a result.

MEMBER POWERS: Of the many elements of the application, which did you find the most difficult to do?

MR. GRECHECK: Again, I would have to say seismic because I think that was --

MEMBER POWERS: It was seismic?

MR. GRECHECK: What has happened with seismic is that many -- and we had some of these discussions yesterday -- many of the paradigms and the rules that many of us remember from many years ago about what a design basis or what an SSE is and how you select that acceleration, much of that has changed, and as a result of that, it's a learning process to understand what's significant and what isn't and how do you define that SSE and how do you define what geological features are significant and how do you handle those.

And I'm sure that even once we complete the ESP process, should we get into a COL process at a later date, I'm sure many of those questions will come up again.

MEMBER POWERS: Which of the many elements were you frustrated the most with?

MR. GRECHECK: I think for us it was probably most surprising and what was most frustrating was the review of emergency planning. As I indicated, we did reference an acceptable in-place emergency plan that's been in place for many, many years, which is periodically exercised and inspected and verified, and verified not only by the NRC, but also by FEMA for the off-site processes.

And I think we were a bit surprised to find that the review standard as it's currently in place seems to require a detailed re-examination of many, many things in that plan which, you know, down to the level of -- as a matter of fact, we had requests for additional information talking about how many hospital beds are available in various hospitals and how the equipment in various state and county emergency centers is configured, and some of that seemed to be, first, misplaced in terms of timing, given that the plant would be built many years from now, but in addition to that, again, we're talking about existing plans that would not have to be appreciably modified for the additional units, and yet there was this extensive review required.

And I think I would certainly suggest that as part of any lessons learned process that would come out of this, we would have to take a look as to why does that seem to be necessary in this review.

MEMBER POWERS: Which of the sections do you think you did the best job on?

MR. GRECHECK: Well, I wouldn't want to make any --

MEMBER POWERS: Oh, come on.

MR. GRECHECK: I wouldn't want to make anybody feel they --

MEMBER POWERS: Well, you did an excellent job on all of them. Now, which one is a little more excellent than the others.

PARTICIPANT: First among equals.

MR. GRECHECK: Right. Well, I think going into the application, I think we suspected that there would be lake usage issues, and I think we spent a lot of time on that and a lot of effort, and I'm rather proud of the work that was done in terms of reconstructing the thermal models that existed from the previous applications and then updating those and making some sense of all of that.

So I think that was probably a significant work that we're proud of.

CHAIRMAN WALLIS: You have about a three-page theses on geology.

MR. GRECHECK: Yes.

MEMBER POWERS: That's actually required explicitly in the requirement, in the regulations. They had no choice but to.

CHAIRMAN WALLIS: Can't you go back billions of years and everything?

MEMBER POWERS: Well, that's a feature of geology, is it goes back billions of years.

Any other questions?

(No response.)

MEMBER POWERS: Okay. Let's turn to the staff. Ms. Dobbs --

MS. DUDES: Dudes.

MEMBER POWERS: -- are you going to give an introduction or are we going to go straight to beating on Mike?

MS. DUDES: Well, I'd like my introduction to include beating on Mike, but I'd like to just do an introduction, and I know I did this with the subcommittee yesterday. So I'll try and make it brief.

MEMBER POWERS: So you should be practiced, right?

** MS. DUDES: Yeah, yeah. We'll change it up a little bit.

First and foremost, my name is Laura Dudes. I'm the Section Chief for New Reactors. I wanted to introduce Michael Scott, the Senior Project Manager. I'm probably introducing him for the last time as a New Reactor staff member, but I'm sure you'll all get used to seeing Mike around here shortly.

So that's the bad news for us, good news for the ACRS. The good news for the North Anna project is Ms. Belkys Sosa will be taking over as the Senior Project Manager for the North Anna ESP.

MEMBER POWERS: They might want to check with the Canadians before they celebrate too much.

MS. DUDES: Well, I think regardless, the Canadians were pretty happy, and I know ACRS was pretty complimentary of her work for our pre-application review on that. So we're very lucky to have her step in at this critical time in this project.

And I say "critical" because the early site permits are first of a kind projects. We have come to an interim milestone, which is the completion of the draft safety evaluation report, which we have provided to all of you, and I must say the introductory remarks were correct. They do plan on bringing two more of those to you in two-month intervals.

MEMBER POWERS: What did we do to you?

MS. DUDES: Nothing, but I was thinking of a mitigative strategy last night in terms of if we step back a little bit and look at some of the activities that are going on nationally in Congress and other things, we are now planning and looking at a much higher level of new reactor activities, including combined license applications.

Another design certification is expected in June, and more early site permits. So I think one thing that we can do to maybe help the committee, and you'll have a pretty good support system with Mr. Scott next week, and we'll be able to maybe figure out with him how we can get you more information in a timely manner is once we docket these applications, the applications are 2,000 pages. They're big.

The staff review is slightly smaller. So we could probably get you the applications much sooner and try and condense and point out some critical areas so that we're not waiting until the last minute when we're handing you the draft safety evaluation report.

So as I said, the Clinton Exelon application should be -- these applications were all received within about a month of one another in 2003. We staggered the reviews by two months to make efficient use of resource teams because we just physically couldn't review all of them simultaneously, and I think we're learning lessons as we go through this.

So Mike is going to go through the North Anna ESER now. Two months later we'll see Clinton and then two months after that Grand Gulf, and then just in case, you know, you're afraid that we're going to let you have a little bit of a breather, we'll be back again to do the final safety evaluation for North Anna.

MEMBER KRESS: One question.

MS. DUDES: Yes.

MEMBER KRESS: Did you guys, the same group, review the environmental impact statement or is that a different group?

MS. DUDES: We're within the same division in NRR, but it's a different section, yes, that does the environmental impact statement.

MEMBER KRESS: Should we be hearing from them also on these?

MR. SCOTT: I don't believe so because the statutory charter that was mentioned earlier is that you all report on safety aspects of the application.

MEMBER KRESS: And there are no safety aspects in the environmental impact statement?

MEMBER POWERS: Well, the questions you were asking, Dr. Kress, about the severe accident and doses, whatnot, is all in the environmental part of it, and as portrayed yesterday, it's all there. And as portrayed yesterday, the potential dose to the public is all dominated by the existing reactors. New reactors have very low core damage frequencies.

MEMBER KRESS: I think that's a good think, yeah, as long as the constraints are there that says these have to be one of the new reactors.

** MR. SCOTT: If we can get started, I'd like to, first of all, defend my lengthy slide show. I have taken some comments already before we even started on it, but I would ask you all to be a little patient with me. There are really only 21 slides here and the rest are all back-up, and some of the 21 slides we should be able to get through quickly because they are somewhat repetitive either to what Laura said or what Dominion said earlier.

In addition to the slide package, you have two individual pieces of paper there. One of them is a brightly colored map of the area and another one is the seismic source zone map. Those are also in your slide show as the very last two pages, but I was a little concerned that there might be a vision test issue with those. So the separate copies are just larger font so that you would be able to see them if you wish.

And I don't plan, unless you all have a particular question on any of the back-up material to get into that back-up material. We discussed it with the subcommittee yesterday.

So moving into the presentation, the purpose, of course, is to brief the committee on the draft safety evaluation report and support your view and the ultimate issuance of an interim letter to the Commission.

Next slide is the agenda, which I'm anticipating we would spend approximately 30 minutes on.

Slide No. 4, as was mentioned earlier, Subpart A to 10 CFR 52, Part 52 governs what we're doing here, and Part 52, of course, references Part 100, and we talked about the ACRS does have a statutory role in this, and Laura mentioned already this is the first one you're getting. So we can move right on.

The subcommittee asked us to come back with the purpose of an early site permit, and Dominion came back with the purpose from their perspective, and we developed a slide here that shows the purpose of an early site permit, more generically speaking. It separates to the extent feasible; ideally it would be completely feasible to separate, but it turns out that there are some cases where it's a little difficult to draw the line, as we discussed with the subcommittee yesterday.

In any event, the intention is to separate the review of the site from the review of the design, and that allows the resolution of site related issues before the applicant has spent significant resources either developing the design or actually constructing the plant.

And it allows the early site permit holder who is successful to bank the site for up to 20 years for future use. So if the applicant anticipates they may want to build a nuclear power plant but isn't in an immediate rush to build one right now, then the early site permit could facilitate a step-wise review for them to reach the finish line.

Next slide.

Dominion talked about the past milestones. I'd like to talk a little bit about the future milestones. Laura, of course, referred to some of these.

Our schedule assumes an interim letter from the ACRS this month. Staff provides the final SER to you in late May. It will be in close to final form, and then we will issue the FSER, the final safety evaluation report, in the middle of June. Hope to have a letter from you all, your final letter, in July. We have a nominal date here, but of course, just some time in July.

And then we will incorporate the ACRS letter and issue the final safety evaluation report as a NUREG, and that schedule date is August 29th, '05.

Once the SER is issued and the EIS, the final EIS is issued, and the ACRS letter is received, then that will trigger the remaining events that will take us to a mandatory hearing, which we assume will begin in the fall of 2005.

There will be a contested hearing, as we discussed with the subcommittee, because there is currently one environmental contention that is before the Board, and of course, the Atomic Safety and Licensing Board keeps its own schedule. So these are only assumptions on our part as to when the hearing would actually occur.

And also have an assumption, as you see in the bottom bulleted slide that the Commission would make its decision in mid-2006, but that's, again, just a staff assumption.

Slide 7, this has largely been covered by Dominion. I'd just mention here they are seeking authorization for limited work in accordance with 10 CFR 52.17. The applicant for this early side permit is a company that, like Virginia Power, is owned by Dominion Resources, Incorporated, but the applicant is not the same identical entity as the one that owns North Anna Power Station. That has some import in the review that's discussed in the safety evaluation report.

Slide 8. Dominion talked about what they're asking for capacity-wise. They mentioned the fact that a unit might be one large reactor or multiple smaller reactors. They mentioned the fact that they have submitted a plant parameter envelope.

The point that we would make there is that when an applicant submits a plant parameter envelope, they are retaining additional flexibility that they might want to choose their reactor design later instead of choosing it at the early site permit stage.

The down side to that is that we do not issue -- if we do issue an early site permit to an applicant who submits a PPE, that permit will not speak to any particular reactor being approved, and our review of the PPE values at the early site permit stage will be limited to whether they are reasonable or not.

And then the combined license applicant is burdened with showing that their actual chosen design falls within the PPE. For cases where it does not, then the issue needs to be reevaluated at combined license.

Slide 9, this is additional information that we provided in response to a request from the subcommittee. Of course, this is a rock site. There are regional geologic faults and the very colorful drawing that you have there that I mentioned that's separate shows the faults in the vicinity, and

Dominion did develop their application ultimately for the seismic hazard using Regulatory Guide 1.165 method and the low and high frequency earthquakes that are noted there.

Should you be interested, the drawing that shows the resulting safe shutdown earthquake is in the back-up slides on page 27 -- I'm sorry -- 26.

CHAIRMAN WALLIS: This earthquake M7.2 is Charleston, is it?

MR. MUNSON: Yes, that's correct.

MR. SCOTT: That was Cliff Munson speaking for the staff.

Next slide.

I believe Dominion talked about their cooling system. I won't address that again. They do plan if they elect to place a unit on the site that requires an ultimate heat sink, they plan to provide an underground ultimate heat sink which also has had some import on the review as is discussed in one or two of the staff's open items.

Slide 11. Talked about the draft safety evaluation. Of course, this is the first of a kind. It has, therefore, been an interesting review for the staff, just as I'm sure that it was interesting and challenging for the applicant in developing a first of a kind early site permit application.

We did have a generic issue resolution process that we used prior to the receipt of any early site permit applications to attempt to resolve as many generic issues as we could identify before the applications came in.

As you can imagine, while we were successful in identifying a number of issues, others popped up. We actually got to look at an application, and so some of those, a few of those are being resolved as part of what's going on with the review of these three applications, and I'll speak briefly to that in a minute.

Slide 12 shows the review areas for the safety review and the staff reviewers. As you can see there, we have an able group of reviewers, many of whom you all have previously interacted with. We also have some very important contract and consulting support in the hydrology area. We received contract support from Pacific Northwest Laboratory. They also supported the site hazards review. Geology and seismology we were assisted by the U.S. Geologic Survey, and in the emergency planning area, the staff consulted extensively with the Federal Emergency Management Agency.

Next slide.

I'd like to talk briefly about a few issues that came up during the review of the early site permit application for North Anna. Some of these are more generic in nature, but of course, we do have the three applications before us. So they affect those applications.

The first one is regarding emergency planning. Of course, Gene Grecheck referred to their concerns regarding emergency planning, and we have accumulated some lessons learned from the review in this area.

Dominion, like the other two applicants has elected to seek acceptance of major features, which is authorized by 10 CFR 52. The concept, however, is not to find in detail, and when we got into the review of these three applications, we ended up having discussions regarding what is finality when you have limited information presented to you on a given subject.

And what we've concluded is that the staff, of course, must be able to make its required findings at the combined license stage. So if we receive information on a major feature, we can approve and provide finality for the review of that major feature, the description of the major feature at a high level.

However, the implementation details underneath that major feature are open to additional valuation at the combined license stage. And this, as was mentioned, perhaps, was not what was expected going in. So this has been a bit thorny.

Slide 14, I mentioned in an earlier -- yes?

MEMBER POWERS: Let's come back to this. As I read the regulations, which, I mean, doesn't say very much, but I get the impression that what they were looking for on the emergency plans was a much more high level sort of thing than what hospital beds. I mean, they were looking at are there any changes that are going to change the evacuation routes that are going to be a problem, not the more microscopic features in the emergency plan.

Am I wrong in reading it that way?

MR. SCOTT: Oh, no. You are correct. I believe that, again, Gene Grecheck referred to that. This applicant and -- well, let's just say this applicant -- Dominion did submit emergency planning information that included a reference to the existing emergency plan and the evacuation time estimate for the North Anna Power Station.

The staff had previously dealt generically with the question of what do we do with submittal of preexisting information, information previously submitted to the NRC, and we absolutely communicated with the Commission on that in the approval of RS002, their early site permit review standard.

When we got into the reviews, the staff did choose to do a review in some detail of both the on-site and off-site emergency plans and the evacuation time estimate, and as we remarked to the subcommittee yesterday, that is an area in which we have accumulated some lessons learned that perhaps next time it will be different.

MEMBER POWERS: As long as we're going back, at the subcommittee we did not go into much of the detail on population projections. Safe to say that you did them. Could you talk a little bit about population projections?

MR. SCOTT: Population projections figure into the safety side review both in the emergency planning area and in the Part 100 area, and there are, as we mentioned yesterday, there are some regulatory guides that provide a methodology for determining actually whether population density is adequate or not or excessive or not.

The review standard provides guidance on doing a population projection, and as we mentioned, the population projections that were done run out to a total of 60 years, which would be the 20-year assumed period for the early site permit, and then assuming an application is submitted towards the end of that period and a plant is built, then we assumed another 40 years on top of that.

And when we looked at the and when the applicant looked at the resulting population density figures, they were all the way out to 2065, I believe is the end year. They were within the criteria for a population density that the regulatory guides provide.

If you want details on what the numbers are in the regulatory guides, I have somebody here who can answer that.

MEMBER POWERS: I'm more interested in the resources available to make those projections.

MR. SCOTT: Can you clarify, please?

MEMBER POWERS: Yeah, how do you know? I mean, have you got a crystal ball that --

MR. SCOTT: What's the basis of the projections?

MEMBER POWERS: Count the number of women?

MR. SCOTT: Okay. The first place I'll go to ask that question is the tech staff over here. Jay Lee, can you speak to that? Yeah, that would be your area, I believe.

Did you understand the question?

MR. LEE: Yeah, yeah, I do. Perhaps maybe applicant can address that better than I can. They use the special formula they developed projecting future population distribution.

MR. SCOTT: And we looked at their method and found it to be acceptable.

MR. LEE: Right.

MR. SCOTT: Okay. I don't know if Dominion would have anybody here that could address that question. Do you happen to have?

PARTICIPANT: We don't have a way to do that in detail, but it was --

MR. SCOTT: It's documented in the application, I believe.

MEMBER POWERS: There's a lot written on it.

MR. SCOTT: Marvin Smith, I believe, from Dominion wants to say something.

MR. SMITH: It's Marvin Smith from Dominion.

It is documented in the application as to how that was done, but use the 2000 census as a basis point and then you have formulas that project population trends over time that were applied to the population and the area around the early site permit site.

But, again, the details would be, I think, pretty well described in the application.

MR. SCOTT: And referenced in the safety evaluation report.

Jay, what section of the SER is that? Is that 2.1.3?

MR. LEE: Correct, yes.

MR. SCOTT: So that information is, we believe, contained in there.

MEMBER POWERS: There was an ulterior motive.

MR. SCOTT: Okay.

MEMBER POWERS: And it is you can project on population, but you don't project on weather.

MR. SCOTT: That's correct.

MEMBER POWERS: They would seem equally challenging to me.

MR. SCOTT: I'm going to have to say that we have no new information for you on the subject of forecasting the weather based on what was said yesterday.

MEMBER KRESS: Point of clarification on the siting rules on population density.

MR. SCOTT: Yes.

MEMBER KRESS: There's a number in there, I guess, a certain number of people per square mile, right?

MR. SCOTT: Well, there's --

MEMBER KRESS: A limit.

MR. SCOTT: -- a population center distance and there is a number per square mile taken out to certain radiuses, yes.

MEMBER KRESS: Now, my question about that one, that part of it.

MR. LEE: Right. Population density guidance is 500 persons per square mile.

MEMBER KRESS: How is that determined? Do you take a ten-mile limit and get the area and divide by the number of people, divide that into the number of people in there?

MR. LEE: No, no. We use 20 miles from the site.

MEMBER KRESS: But you use the full area of the 20 and the total number of people?

MR. LEE: Right, average, average.

MEMBER KRESS: And the number of people there?

MR. LEE: But average population density.

MEMBER KRESS: Okay. It doesn't involve the wind rows or bunches of people at given spots in that 20 miles?

MR. LEE: Well, that's included, transient population, as well.

MEMBER KRESS: But that's an average in the full 20 miles?

MR. LEE: Right. Twenty miles. So you have the area and then you project so many population including weighing the transient population. Then you divided that number by area.

MEMBER KRESS: That's what I thought.

MR. LEE: To come up with --

MEMBER KRESS: Thank you. That's what I thought it was.

MR. SCOTT: It's concentric rings, right?

MR. LEE: Right.

MR. SCOTT: Are we ready to move on?

Slide 14. We did identify some issues in the seismic area. As was mentioned earlier, Dominion ultimately used the NRC approved method in Regulatory

Guide 1.165. They had come in with a performance based approach, which is a new approach the NRC has not yet evaluated, and therefore, we informed the applicant that use of this performance based approach would likely result in a delay in completion of the review, and so the applicant revised its application to --

MEMBER POWERS: But it would seem to me they'd still use the EPRI-1, but they just noted that it bounded the Reg. Guide 1.165.

MR. SCOTT: Well, that's correct. If we can flip back to Slide 27, please, or 26 rather. Can you take us there?

If you used the NRC approved method, you come up with an SSE that's addressed by taking the higher of the blue and the red lines that you see on this figure. When the applicant used their performance based approach, they came up with a line that exceeds or is equal to those blue -- the higher blue and red curves throughout.

So the NRC found it acceptable because by our standards it's conservative, but they could have chosen another number and used another method, and it still would have been conservative.

So while we accept their choice of SSE, we did not accept it on the basis of a review of the performance based approach.

As we mentioned yesterday, the second of these applications you're going to see from Entergy, they have chosen to retain a performance based approach, and so the staff is reviewing that. So you'll hear considerably more about the performance based approach next time around.

MEMBER POWERS: I have to admit that that is the most confusing language. I mean, the idea of a performance based approach, I think, I could imagine somebody in Japan coming to me and saying, "Well, I've got a performance based approach to earthquakes," but the East Coast of the United States?

MR. SCOTT: Cliff Munson can correct if I'm wrong here. I believe that the performance based approach refers to other aspects of the methodology, doesn't it?

MR. MUNSON: It refers to the performance of systems, structures, and components undergoing ground motion.

MR. SCOTT: Which is not the way we've done these evaluations in the past. So I think that's what they had in mind rather than it's based on a large series of earthquakes and what happens to equipment, you know, in that kind of thing.

Let's see here. Okay. The bottom bullet here, another issue that's come up, and this will end up being a combined licensed item to be addressed. As I mentioned, North Anna is a rock site. So the site, safe shutdown earthquake exceeds the design safe shutdown earthquake for the applications that have been either certified or submitted for certification to date.

That is depicted graphically on Slide No. 27, if you're interested in looking at that, and we fixed Slide 27, by the way. The legend was backwards yesterday. It's now on straight.

So that issue, the applicant has defined a safe shutdown earthquake and once the open items are all addressed, if presumably the staff finds it acceptable, then that will be adequate for the early site permit.

And then the comparison of that safe shutdown earthquake with the design will be a function that we'll need to happen to the COL.

Slide 15.

MEMBER POWERS: I mean, it still raises the question of once again we run into this finality issue that now if you open up the design, the certified design to say, okay, you've got to X this thing in order to put it on this site. How much do you open it up?

MR. SCOTT: Well, I guess I don't see that as the same thing as some of these other considerations. The SSE as specified for the site will be final, subject to the provisions of 10 CFR 52.39, and the design SSE is a design issue, and our purpose here is not to resolve design issues at the ESP stage.

So I don't see that as a finality issue so much as an item of matching the site and the design, and in the perfect world, you would have those two match up. The site would fully bound the design, and so at combined license, the applicants' task would be easier, but because that's not the case here, if they don't come in with the design that is bounded by the site at that stage, then they're going to have to demonstrate that the design can be safety put on the site, and that will be subject to all full consideration at combined license.

Slide 15 speaks to another question that's come up, site characteristics versus design inputs. We have given Dominion credit in our SER for appropriate consideration of the most severe and natural phenomena that have been reported for the site with allowance for margin and uncertainties, which is, of course, the language that they will ultimately need to comply with in General Design Criterion II, although GDC II largely does not apply at the ESP stage.

The staff was of the objective that if the applicant has been able to partially demonstrate compliance with a rule that will apply at combined license, we should give them credit for that, and we did where appropriate.

However, Dominion was concerned about the language in our safety evaluation report that refers to design bases, and they wanted to clarify that site characteristics are not necessarily the design bases. Site characteristics are the minimum design bases, and an applicant can always choose to use more conservative design bases for their actual design, and the staff is all right with that.

Slide 16. I mentioned earlier that the interface between site and design, which we would like to separate the review of the site and the design to the extent we can because that, of course, is the purpose of the step-wise process in Part 52. There are some cases where it's not quite clear how we do that, and some of the examples that we've come up against in this evaluation you see in front of you.

For most of these we have worked through it and determined a site characteristic that can be suitable for addressing the issues involved. The one that we're still under discussion with in the staff is potential interferences between new and existing plants.

The subject who actually brought this up was the fact that the normal service water discharge for the new plants will run underneath the safety related service water piping going to and from the ultimate heat sink for the existing plants, and we have wrestled with how do we insure that the impact of the construction of the new plants is appropriately addressed.

The applicant believes that that should be addressed under Part 50, that it's not necessary to be part of the ESP considerations, and the staff is still evaluating that.

Now, other examples of these are discussed in the back-up slides, but I don't propose to address them today unless the committee would like to discuss any particular one of them.

MEMBER POWERS: Let's go through the

frazil and anchor iced again.

MR. SCOTT: Okay. The issue there -- well, I'll tell you what. Rather than me go through it, I'll just get Goutam to come up here. Goutam, are you back there?

Would you please speak to the open item regarding frazil ice and anchor ice?

MR. BAGCHI: The staff was looking for some kind of criterion to insure that frazil and anchor ice is considered as a characteristic of the site that would be incorporated in the future design of the intake and the screen and so forth.

MR. SCOTT: And what we ended up concluding the right thing to do at this stage is to have a site characteristic simply that there are conditions that could arise at the site that would cause frazil or anchor ice to occur.

There was not, to the best of we could determine, a site characteristic that we could rely on that would say this is what will bring about frazil ice because there's a combination of conditions, and so what we are simply stating is that at ESP frazil and anchor ice could occur, and that will mean that when we stated that, that the combined license applicant will need to provide appropriate design features to deal with that.

CHAIRMAN WALLIS: Have you got frazil ice in lakes, do you?

MR. SCOTT: Yes.

CHAIRMAN WALLIS: I've seen it in rivers. It just floats around in a lake?

MR. BAGCHI: Well, in the application itself they accept that it can occur in lakes, lakes and rivers, yes.

CHAIRMAN WALLIS: But rivers, it's moved by the river. So it's mixed up with the water in the river. In the lake I would think it would float to the surface.

MEMBER POWERS: Well, the application itself defines a turbulent condition to get the necessary mixing.

MR. SCOTT: The actual combinations of conditions that would result in that occurring at Lake Anna, Virginia are not going to be common.

MEMBER POWERS: Yeah, basically, as I interpret the argument, it is that if the Units 1 and 2 are operating, you don't get cold enough to get ice. If they're not operating then there's not enough turbulence to mix any ice up, and so that it's a relatively rare occurrence.

MR. SCOTT: As I recall, the issue could emerge if you've had a large number or say all of the units shut down and now you're getting ready to start one up. The cold water is there, and no you have the turbulence.

MEMBER POWERS: But you handle it just by saying, yeah, it can occur.

MR. SCOTT: It can occur, and so the COL applicant is going to need to provide design measures to deal with it, and that is not something that's unprecedented.

MEMBER POWERS: Oh, yeah, yeah.

MR. SCOTT: And this was one of those kind of lessons learned again. Do we ask the applicant at the early site permit stage to show us what design feasibility is out there?

And ultimately we concluded that that's not the role of an early site permit review.

MEMBER POWERS: Yeah, because I mean if nobody had ever had frazil ice before in the world, you might well want to look at that for feasibility, but since Wolf Creek, we're all attuned into frazil ice. You know, there are ways of handling it.

MR. SCOTT: Right. Slide 17 just speaks to largely the collection of items that we've given to you in the back-up slides. We do have some open items. There are about 30 of them. Twenty of them are in the emergency planning area and half of those are related to the fact that some of the requests for additional information responses came in late.

And then there are another ten or 15 that are related to various site issues, and as Gene Grecheck mentioned, we are working through those, and the applicant expects to provide most of that information today.

So we're anticipating that, and we'll have the staff reviewers looking hard at how the applicant, how Dominion is resolving those.

MEMBER POWERS: You tantalized us by saying all save one. Do you happen to know what the one is?

MR. SCOTT: The issue is, yes -- let me see if I can find it.

MEMBER POWERS: He's a dirty guy. He leaves me curious for long periods of time. I know he did it deliberately. He's grinning back there.

MR. SCOTT: A whole lot more credit than it's due.

CHAIRMAN WALLIS: Are you going to revisit seismic or are you going to go to the end?

MR. SCOTT: Well, actually as it happen, the item that they're going to be a little late on is seismic. If we could -- say again? -- go to page 35, actually I'm going to say it's 36.

Thirty-six is open item 2.5.2, which is to incorporate site specific geologic properties and their uncertainties into the determination of the SSE. Dominion has provided their method for determining the SSE at a hypothetical rock outcrop, which is consistent with NRC guidance on the subject, and as noted on the slide here, the staff has no questions on it, but the actual results of the method will not be provided to us until the end of this month.

CHAIRMAN WALLIS: Now, this is a rock site.

MR. SCOTT: It is a rock site, yes.

CHAIRMAN WALLIS: Yet you have concerns with the liquefaction in the --

MEMBER SIEBER: Yes.

CHAIRMAN WALLIS: How does that come about?

MR. MUNSON: This is Cliff Munson.

They have a thin layer of soil. It's considered a rock site. There is a thin layer of soil at the top. This will be removed when they build a reactor. It will be excavated and removed, but they did do a liquefaction analysis propagating the ground motion up through the site, and that included this weak soil layer

CHAIRMAN WALLIS: That's going to be removed?

MR. MUNSON: Right.

CHAIRMAN WALLIS: So liquefaction issue goes away?

MR. MUNSON: Right.

MR. SCOTT: That's a permit condition, too, that we're planning to propose.

MEMBER SIEBER: Actually they're going to do a couple of things. They're going to improve the soil that's located not under safety related structures.

MR. SCOTT: Right.

MEMBER SIEBER: And remove the soil where safety related structures would be. So there's a lot of shoveling.

CHAIRMAN WALLIS: Now, do these pipes go through the rock or through the soil?

MR. SCOTT: Are you speaking of the service water piping?

CHAIRMAN WALLIS: Yes. Do they go through the rock or through the soil?

MR. SCOTT: As I think Mr. Grecheck mentioned, Dominion is planning to use the existing service water structure to the extent possible. I don't know. Cliff, can you speak to whether it's in the rock?

MR. MUNSON: I have no idea.

CHAIRMAN WALLIS: Presumably, it's a seismic response of the piping?

MR. SCOTT: Dominion, do you have any insight on this?

CHAIRMAN WALLIS: It depends on what it's in?

MR. GRECHECK: First, the piping that's being referenced --

MR. SCOTT: That's Gene Grecheck.

MR. GRECHECK: Yes, this is Gene Grecheck.

The piping that's being referenced here is the circ water piping for condenser cooling. That's non-safety related, and that's the large cooling structure. That is through soil. That is not.

But this soil at this site is a mixture of soil and then something called saprolite, which is a crumbled rock type material, but the excavation --and part of the reason that we are seriously looking at using this existing piping is because all of this excavation in construction was done some years ago, and if we can reuse that, there's no reason to do all of that again.

But the rock layer, the safety related structures are founded on the bedrock underneath all of that. So when we're talking about what we'd do is remove that cover material, found the structures on rock, and then refill it, and much of the discussion that we have about seismic response is the response of that fill material and how that interacts with the structure.

MEMBER POWERS: And as I read your application, you had agreed to backfill not with the existing soil but with a different soil.

MR. GRECHECK: And with an improved material.

CHAIRMAN WALLIS: You have safety related pipes. You have an ultimate heat sink and things like that. Presumably you have safety related pipes that go through this soil.

MR. SCOTT: If they use an ultimate heat sink.

CHAIRMAN WALLIS: Do you do a seismic analysis of these pipes then?

MR. SCOTT: Not at this stage.

MR. GRECHECK: For the existing station, for North Anna 1 and 2, there is safety related piping that does run through the soil, but that piping is anchored at various points, and there is a seismic analysis that discusses how that would response.

MR. SCOTT: But that would be outside our scope here.

In addition to the open items, there is a confirmatory item. Just briefly, it's regarding use of the Internet for information supporting safety related analyses, and the applicant addressed that, and the staff has inspected it and has no additional questions on it.

COL action items. There are a number of items which, again, are in the back-up slides here. There are items that are site related, but for various reasons the staff believes will more appropriately be addressed at the combined license stage.

Just as an aside, as part of reviewing the responses to the open items discussing these issues with the applicant, the staff has considered and there's some chance that some of these combined license action items may be revised or deleted by the time we're complete with the final safety evaluation report.

For example, we have one speaking to this separation distance, and it doesn't make sense given the actual physical condition or configuration of the site.

Finally, we have a number of permit conditions. Again, these are in the back-up slides as well. These are items that we believe are applicable to the ESP holder, and there will be constraints on the ESP holder if an ESP is issued for the site.

CHAIRMAN WALLIS: To go back to seismic, what's the effect of seismic on the dam that retains the lake?

CHAIRMAN WALLIS: Okay. Again, the lake is not the safety related ultimate heat sink for the site, for the early site permit site.

CHAIRMAN WALLIS: They don't need the lake.

MEMBER SIEBER: No.

CHAIRMAN WALLIS: For safety purposes.

MR. SCOTT: That's correct.

CHAIRMAN WALLIS: So if you lost the lake, it wouldn't matter.

MR. SCOTT: Well, it wouldn't be good.

MR. BAGCHI: Well, that's right.

This is Goutam Bagchi.

We did look at that for availability of water, and the dam failure is postulated.

MEMBER SIEBER: On the other hand, the ultimate heat sink is that big pond.

MR. SCOTT: That's correct.

CHAIRMAN WALLIS: No, no.

MR. SCOTT: Well, there's an underground facility if they use one, correct, Goutam?

MEMBER SIEBER: That's for the new.

MR. SCOTT: The new ones, yes, as opposed to the old ones.

MR. GRECHECK: Again, this is Gene Grecheck.

Just to clarify that, remember on the picture there was that pond. That is the service water reservoir, and that is the ultimate heat sink for Units 1 and 2. For the ESP units, we are proposing if an external ultimate heat sink is required, then it would be an underground width band.

CHAIRMAN WALLIS: I wonder if it's underground what do you do. You have welds or something? Is that what you mean?

MR. SCOTT: No, the make-up would come from the lake.

CHAIRMAN WALLIS: But the lake is gone in my scenario.

MEMBER SIEBER: Well, you fill it first.

CHAIRMAN WALLIS: You fill it first. It's an underground pond. Is that what it is, rather than groundwater? It's actually underground reservoir?

MR. BAGCHI: It's a very large tank. It's 230 feet by some 100 feet by 50 feet.

CHAIRMAN WALLIS: So it's an actual tank.

MR. BAGCHI: It's an actual tank buried inside the ground.

MEMBER SIEBER: Big.

MR. BAGCHI: Very big.

MR. SCOTT: The next slide, Slide 18, please.

CHAIRMAN WALLIS: -- from the tank on the surface. It's just a tank of water.

MR. SCOTT: Yes. The DSER, being the first cut at the safety evaluation report and having open items associated with it, defers general regulatory conclusions regarding site safety and suitability to the final safety evaluation report, which I mentioned we will plan to issue in June.

However, there are some sections of the report for which there are no open items, and in those sections we have reached conclusions that are shown here. As you will note, the applicant has provided appropriate quality assurance measures equivalent to those in 10 CFR 50, Appendix B.

Part 52 does not require compliance with Appendix B, but the staff has clearly stated to the applicants that we need for the ability to have confidence in the review findings, that the measures the applicant applies be equivalent in substance to those in Appendix B, and Dominion has done so, and the staff has accepted that.

Site characteristics are such that adequate security plans and measures can be developed. As I understand, the committee is not evaluating security. So we'll move on from that one.

CHAIRMAN WALLIS: We just note that it is on a lake.

MR. SCOTT: It is on a lake.

MEMBER POWERS: Our specific charter is to look at the items related to safety, and the Commission has expressed no interest in advising them on security issues with regard to these early site permits. So we've kind of said, okay, we won't do that.

I think we have enough to do without it.

MR. SCOTT: Additional conclusions. We talked about this earlier. The population center distance is defined in 10 CFR 100.3. Meets the criteria for being one and a third times the distance from the reactor to the outer boundary of the low population zone, and is compliant with the applicable regulations.

The applicant has also established appropriate atmospheric dispersion characteristics to support its radiological calculations, radiological dose consequence evaluations.

And based on that information, as well as the PPE value --

CHAIRMAN WALLIS: I'm curious about this population center distance. How do you decide what the distance is? Is it the outer boundary of the population center or is it the center of the -- if it's a big area, how do you decide how to measure the distance?

MR. SCOTT: Jay, can you speak to that, please?

MEMBER POWERS: It's mineral.

MR. LEE: The distance is from the reactor.

CHAIRMAN WALLIS: That's easy to define,

but what's the other end of the distance?

MR. LEE: That's the one and one-third times --

CHAIRMAN WALLIS: Yeah, but what's the --

MR. LEE: -- the distance to the LPG, which is 6.8 miles.

CHAIRMAN WALLIS: I understand.

MR. SCOTT: I think he's asking what the population center is.

CHAIRMAN WALLIS: What is the location of the population center? Is it the outer boundary or what? We have a city. Is it the distance to the first suburb or is it the distance to the city limits, City Hall?

MR. SCOTT: What is the definition of a population center is where he's going.

MR. LEE: I don't think we defined that.

CHAIRMAN WALLIS: It seems to me important because the city could be bigger than one and one-third times the distance.

MR. SCOTT: I think it is dispersed.

MEMBER POWERS: It could be, but it's Mineral, Virginia. So --

MEMBER SIEBER: Yeah, Mineral is not --

MEMBER POWERS: You could take either one of them. It's the same distance.

CHAIRMAN WALLIS: It's like a small town in Vermont.

MEMBER POWERS: It's not quite that big.

MR. SCOTT: There are criteria for this and we can get back to you on that as to what those criteria are. I mean, there is a method for doing this that we went through in this evaluation.

MEMBER POWERS: Well, the first population center has to have a population of less than 25,000, and unless it's an extremely peculiar 25,000 city, there's not going to be a huge amount of distance between the outer limits and the town center.

CHAIRMAN WALLIS: Well --

MEMBER POWERS: A town of 25,000?

PARTICIPANT: Oak Ridge would be a huge area.

MEMBER POWERS: Unless it's extremely unusual. I excluded that. There's a possibility on the off chance you might bring up Oak Ridge, which by definition is a very eccentric place.

PARTICIPANT: You're right.

MR. SCOTT: The nearest relatively large town in the vicinity of this site, as Mr. Grecheck mentioned is over 30 miles away.

Slide 20. The staff also concluded that potential hazards associated with nearby transportation routes, industrial-military facilities pose no undue risk to a facility that might be constructed on the site. In other words, we evaluated the hazards in the area, and did not find issues related to significant hazards, off-site hazards.

Slide 21. This is just a wrap-up on the presentation. The staff has, of course, issued a first of a kind DSER. We expect today to have open item responses for most of them. We are working through some issues that we've talked to you about.

We're looking forward to seeing the interim ACRS letter and to coming back -- well, to Belkys coming back in July and bringing you again on the final safety evaluation report.

And we are identifying a number of lessons learned related to these three reviews. As you can imagine, first of a kind, it's fertile ground for identifying things that you didn't expect to identify, and we plan to revise our guidance in the future to address these lessons learned and that which supports review of any future early site permit applications that might be submitted.

And there is some industry discussion that there may be additional early site permits, although we do not currently have a commitment letter from any particular entity for seeking one.

MEMBER POWERS: I think we'd be interested in working with you on that, the lessons learned activities. We can help you provide input from our perspective, but not to -- you know, if it's not too terribly much of an imposition on you, once you get your thoughts together, maybe come down and give us a chat, and we can give some feedback, and maybe we can put something together kind of jointly on this.

MR. SCOTT: We would appreciate your input.

MEMBER POWERS: You know, I mean, in the spirit of what is efficient and good guidance and is efficient or review is possible and things like that. So I think we'd be interested in working with you on that.

MR. SCOTT: Thank you.

CHAIRMAN WALLIS: That would be very useful. It would help us to know what to focus on next time around and that sort of thing.

MR. SCOTT: Sure. That concludes --

MEMBER POWERS: I think it's going to be possible. I mean, it sounds like they're going to exercise us pretty good on this, and if we're just careful on keeping track of where we find rough spots and things like that, and then we get together with them and get their notes and where they found rough spots and we might be able to put together a pretty good story here.

I'm quite sure the Commission is very anxious for us to work like that, in a, you know, cooperative fashion like that.

Similarly, I would invite comments, Gene, from your crowd, too, just you know, some input on what you found easy, difficult, hard, and things like that, and confusing or whatever. I just think it would be useful.

MR. SCOTT: That concludes my prepared remarks, subject to your questions.

MEMBER POWERS: Do you have any questions for the speaker?

(No response.)

MEMBER POWERS: Well, for those of you who have not had a chance to look at the massive documentation sent to us primarily, I think, in electronic format, it's actually -- the application is impressive, but the SER is a fairly readable document, and if take a chance to look at it if you haven't.

Now, are there any questions the members have of either set of speakers?

(No response.)

MEMBER POWERS: And I'm not aware of anybody from the public wanting to make comments. So I'll thank you.

MR. SCOTT: Thank you.

MEMBER POWERS: And welcome aboard, Mike.

MR. SCOTT: Thank you.

MEMBER POWERS: And thank all of the speakers and turn it back to you, Mr. Chair.

CHAIRMAN WALLIS: Thank you.

So we have gained some time, but we can't use it because we're not allowed to start until three o'clock. So we will take a break until three o'clock.

(Whereupon, the foregoing matter went off the record at 2:24 p.m. and went back on the record at 2:56 p.m.)

CHAIRMAN WALLIS: Let's come back into session.

We're going to hear about pressurized thermal shock rule. We're very much looking forward to what we hope will be the end or almost the end of this process. I will hand the chair over to Bill Shack to get things going.

** VICE CHAIRMAN SHACK: Okay. You know, we've had a number of meetings to discuss pressurized thermal shock. At our last meeting since we reviewed much of the documentation which really provides the technical basis for pressurized thermal shock, and we said, you know, this project was out to develop the technical basis. It really comes down to the reports that were available.

And today we'll be talking about another one of those reports covering the thermal hydraulic evaluation of thermal shock. And again, you know, there's a PRA part. There's a thermal hydraulic part, and a probabilistic fracture mechanics to PTS.

The thermal hydraulic calculations have been done with RELAP, and being a structures guy, I never understand exactly how this works when you do these things with RELAP.

CHAIRMAN WALLIS: It's magic.

VICE CHAIRMAN SHACK: Its magic. They used 2D models with their axial azimuthal segments here. We deactivate the momentum flux in the downcomer because otherwise we get unrealistic circulations, but --

MEMBER POWERS: And that part is wrong anyway, right?

VICE CHAIRMAN SHACK: Six azimuthal regions. We looked at NUREG 1806 last time. There are comparisons with experiments in NUREG 1806, and they focused on comparisons of the pressure and the fluid temperature in the downcomer in experiments and RELAP calculations. Those were fairly good.

However, there were no comparisons of the wall temperature or the heat transfer coefficient H, and in reality it's really the wall temperature that controls the pressurized thermal shock.

There was some sensitivity studies that showed that the downcomer fluid temperature is relatively insensitive to H, and again, that's not totally unexpected, but it's really the wall temperature that we're worried about. RELAP uses the maximum of the Churchill-Chu or the Dittus-Bolter correlations to compute age for the baseline calculations, and they use plus or minus 30 percent on those values for an uncertainty analysis.

MEMBER POWERS: Why 30 percent?

VICE CHAIRMAN SHACK: Well, we'll let them discuss that.

In 1806, they did some sensitivity studies, Petcherkoff-Galinski, with the Swanson-Catton multiplier for buoyancy opposed mixed convection, and when they did those calculations, they got a through wall cracking frequency for the 12 transients they did increase or change by factors ranging from .4 to 1, with an average of about five.

And so if you take a simple minded point of view, you might say that if you use those correlations you would increase the through all cracking frequencies you were getting by something on the order of a factor of five.

Now, that's interesting. That would still leave a significant margin for plants at the end of license renewal. So it's not the end of the world, but it certainly would be different than the kind of values that we've had.

We have a new report now, NUREG 1809 that's intended to provide further information on the comparison of RELAP with experiments. One of the things that I'd like to get out of this discussion is the basis that we should find acceptable either way of calculating age that we use, either the conventional baseline RELAP calculations or the Petcherkoff-Galinski with Swanson-Catton multiplier.

And so what evidence do we have that either one of those provides a realistic value of H? Which H correlation should we be using? The baseline calculations have been done with one. We have an alternative sensitivity calculation with another, and, again, any more insight on how much difference it really makes.

And I believe Jack Rosenthal wants to.

** MR. ROSENTHAL: Thank you.

I'm Jack Rosenthal. I'm the Branch Chief of the Advanced Reactor and Regulatory Effectiveness Branch in the office of Nuclear Regulatory Research.

I've been given the opportunity to provide some opening remarks.

This February we provided our report, NUREG 1809, entitled "Thermal Hydraulic Evaluation of Pressurized Thermal Shock," and that was intended to summarize our work and answer questions. Dr. Bessette is our principal spokesman today to summarize the report of which he's really the author and to respond to questions.

Dr. Kirk also is at the table. He's from Materials Engineering Branch, and he will actually start the discussion to try to put what we have to say in perspective.

Roy Woods is in the room, and he's from the Probabilistic Risk Analysis Branch should questions arise.

And Professor Griffith and Professor DiMarzo, who are consultants to the staff, are next to me to answer questions should they arise.

We've been doing thermal hydraulic work for over four years in this area, and we've had an extensive analytic effort and experimental program, and we think that we've made significant progress over what we knew 20 years ago, in part due to increased understanding and in part due to the fact that we now have computers that just allow us to do multiple, multiple calculations.

We have performed assessment of our code against experiments, and find it surprisingly predicts rather well, and you'll hear an explanation of why.

Using the tools we've performed hundreds of calculations to examine a spectrum of transients and accidents relevant to PTS, ranging from a stuck open safety valve which subsequently receives to a large break loss of coolant accident.

We've performed extensive sensitivity studies of the thermal hydraulic aspects alone, as well as coupling the thermal hydraulics and the fracture mechanics, and the body of work provides confidence that we've addressed what we believe are the significant issues.

We've had the benefit of peer review both by the ACRS and an independent peer review committee in which we spent days going over the details and have had the benefit of their wisdom, and I believe that we've addressed their comments.

I believe our effort at this point is complete. While questions may exist and you can always make refinements, we believe that the work is now technically robust and provide the technical basis to move forward with rulemaking.

With this, Mark.

MR. EricksonKIRK: Okay.

CHAIRMAN WALLIS: Could I say something here?

MR. EricksonKIRK: Sure.

CHAIRMAN WALLIS: Yes, we've heard a lot about your calculations and the effect on each and temperature distributions and all of that sort of thing. The bottom line is: how does this affect PTS?

And you know, seeing temperature distributions in the wall is very interesting, but if they have no effect on PTS, there's no useful conclusion.

So I'd like us to eventually get to that bottom line, as what is the effect on all this stuff, on crack initiation growth and the real sort of issue with PTS.

** MR. EricksonKIRK: Okay. Well, I've got the easy part here because I've only been asked to explain one slide and then Dave gets all of the hard questions.

CHAIRMAN WALLIS: You're not going to show us that big scatter plot again, are you?

MR. EricksonKIRK: I'm going to make a big copy of that for your wall at home, but I'll be here to answer, you know, questions about fracture mechanics calculations and so on.

But just to orient people, and I think this is all fairly familiar in terms of overall how we conduct the analysis. We begin with a PRA and then sequence analysis, and that defines for us both the sequences of things that could go wrong that would lead to an overcooling event, perhaps with repressurization, perhaps not, and also the frequency with which those events would occur.

Those sequences of bad things would then be passed to the thermal hydraulics code RELAP, which would then -- and since I'm a structural analyst, I don't understand what goes on in there either. So I have some sympathy for Dr. Shack, but something happens inside and out comes pressure, temperature, and heat transfer coefficient, all varying versus time.

That is then passed to our probabilistic fracture mechanics code, which takes that information in combination with information on the vessel material properties, the flow distribution within the vessel, refluence, and out of that code comes a conditional probability of through wall cracking, and it's called conditional because it's conditioned on or premised on the fact or the assumption that a certain transient has occurred.

Of course, those transients occur with certain frequencies or probabilities. So the last step in the calculation is to actually multiply the frequency with which we believe these events occur with the probability of generating a through wall crack, presuming that they occur, and that gives us our yearly frequency of through wall crack.

And we then perform those analyses for a number of different plants at a number of different embrittlement levels, and use that information to develop proposals for materials based screening limits, and we would then recommend to our colleagues in NRR for their use.

So that's the overall scope of the calculation, and now we're going to focus in on the thermal hydraulics part.

CHAIRMAN WALLIS: Can we also at some time discuss the effect of uncertainties, fluctuations and so on in the thermal hydraulics on the favor code? How robust is the favor code when fed uncertainties in the thermal hydraulics? Can we address that at some time?

MR. EricksonKIRK: Yeah, I can. I think that will come up, but I can take a shot at it just right off the top.

I think if we were asking Favor to analyze the response of the probability of a vessel failing relative to one specified transient, then these small differences that Dave will show you between what RELAP predicts and what reality is could, in fact, be very troublesome, and I can just give you some thought experiments to tell you why.

For example, you'll see figures like RELAP is off or can be off by ten degrees C. Is ten degrees C. a big difference? Well, it could be a very big difference if, say, the -- and, again, these are comments restricted to analysis of a particular transient and its effect on the vessel.

If RELAP predicted values that were systematically ten degrees C. too high so that the real transient was ten degrees C. lower and, therefore, the fracture toughness was lower and the thermal stress was higher, and so in the real transient you actually got a failure probability, but in the analyzed transient the driving force was too low and the resistance was too high and you didn't get a failure probability. You'd then have a difference between reality when you actually have some finite, albeit small, failure probability and the analysis or representation of reality where you calculate a zero, and that's obviously --

CHAIRMAN WALLIS: Because you have a critical event. You're either above it or not.

MR. EricksonKIRK: That's right. That's right.

CHAIRMAN WALLIS: And thus your uncertainties begin to really matter.

MR. EricksonKIRK: That's right, and that's just a natural consequence of the material.

But all of those comments were with regards to one particular transient, whereas in the PT analysis coming out of the PRA are sequences of events where we analyze anywhere between 30 and 100 different events for their PTS significance.

And what the assessment results that you've seen before and Dave will summarize again show is that, you know, yes, RELAP can be a bit off by something of the order of ten degrees C. and similarly small values in pressure. But it's neither systematically high nor low. Sometimes it's high; sometimes it's low.

And you know, I can't give you a proof that this is so, but the fact that it's sometimes high, sometimes low gives me, you know, as the guy that's sitting in the third blue box a reasonable degree of confidence that since we're analyzing a family of different events that are sometimes going to be predicted high with respect to reality, sometimes predicted low, that on average my results out the end will be a reasonable representation of reality.

If we were in the other situation where I was asked to analyze one particular transient, then I must admit I'd be getting much more wrapped around the axle about these small differences.

CHAIRMAN WALLIS: But that's okay for temperature. Now, when we talk about heat transfer coefficient, I think you would agree if heat transfer coefficient is big enough it doesn't matter what it is, and the question then would be, well, suppose it's infinite. Does it really make a difference whether it's 3,000 or any --

MR. EricksonKIRK: I think in concert the same comments apply to heat transfer coefficient in that if RELAP is systematically always one way or the other relative to the reality of heat transfer coefficient, that's a bad thing.

CHAIRMAN WALLIS: Is it a bad thing or does it matter if it's big enough?

MR. EricksonKIRK: If it's big enough, it doesn't matter, but I think now we're getting into the point where --

CHAIRMAN WALLIS: It does make a difference. He's going to tell us it does matter.

MR. EricksonKIRK: Yes.

DR. ROSENTHAL: I think now we're starting to get ahead of ourselves. We'll bring it up again in about Slide 8, and then we'll bring it up again when we talk about the heat transfer coefficient, and I would remind you that you have to think it through, the transients, the small break LOCAs, the large break LOCAs because what's important changes, and of course, the commensurate frequency.

CHAIRMAN WALLIS: The reason I'm asking

these questions is that the draft report we have from Dave has a lot of thermal hydraulics in it, has very little of the coupling of that to the fracture mechanics, and that's why I'm asking questions now about that coupling.

MR. EricksonKIRK: I'm just going over there to be comfortable.

CHAIRMAN WALLIS: Perhaps we'll come back to that later.

MR. EricksonKIRK: Yeah.

CHAIRMAN WALLIS: That's the bottom line really.

MR. EricksonKIRK: Well, yes, that's the bottom line, but it's also true that even before you get to that bottom line you need to, you know, we all need to convince ourselves that the thermal hydraulics models are either right or adequate.

CHAIRMAN WALLIS: Or it doesn't matter.

MR. EricksonKIRK: But I would respectfully disagree because the sensitivity or insensitivity of a result coming out of a fracture mechanics code to input says nothing about whether the input is right or wrong. I think we have to start by saying that we believe what's going in.

VICE CHAIRMAN SHACK: Well, would you

agree with my sort of extrapolation from the sensitivity results you do present in 1806 that if we change the heat transfer correlation, we would be talking about changing --

MR. EricksonKIRK: Yes.

VICE CHAIRMAN SHACK: -- the failure rate by something like a --

MR. EricksonKIRK: Yes, yes, yes.

VICE CHAIRMAN SHACK: -- factor of five?

MR. EricksonKIRK: Yes.

VICE CHAIRMAN SHACK: And would that bother you?

MR. EricksonKIRK: A factor of five would turn into something like 20 degrees on the screening limit, and yes, that would bother me. So yes. But I think before we get into saying it's a factor of five, we need to first qualify that and say what has produced the factor of five, and is the difference between the base calculation and the sensitivity, are those both credible models?

If those are, indeed, both credible models, then we need to worry about the factor of five. If either of those models is incredible, then the factor of five is meaningless, and that's the thing that I think is important for the thermal hydraulists to establish before we get into structural mechanics.

CHAIRMAN WALLIS: There's nothing universal about this factor of five. If you have a long, slow transient as we have seen in some of the reports where things happen on the scale of 50 minutes or 3,000 seconds, then the wall sort of cools down with the water and nothing much happens. So the heat transfer coefficient doesn't become important.

If it's a long, slow transient, you don't care too much about age I think you'll find.

MR. EricksonKIRK: Well, if it's a long, slow transient, I don't care much about it anyway.

CHAIRMAN WALLIS: If somebody quenched the wall, a double ended guillotine break, things happen very quickly. Then that H assumes a much bigger role. So I think we have to be careful about sort of a factor of five being bandied around. It may be that for certain transients the factor is much bitter. For certain other transients it doesn't matter what H is.

That was, again, not too clear from the report. Maybe it will be made clearer today.

MR. EricksonKIRK: Okay.

CHAIRMAN WALLIS: I'm sorry to hold you up, Dave. I'm sure you're eager to go.

DR. BESSETTE: Take up the whole two hours if you like.

(Laughter.)

** DR. BESSETTE: I have about 15 viewgraphs to go through.

So where we were in December is described the assessment performed to determine the ability of RELAP to predict pressure, downcomer temperature, and part of the presentation was devoted to showing that plumes would not be an issue.

It also showed results of a sensitivity study we did prior to the start of the current PTS reevaluation that showed that even if plumes did exist, they did not materially affect the --

CHAIRMAN WALLIS: Now, were these plumes with 100 degrees of subcooling that you got in the cold leg or are they -- that's a much bigger, stronger plume than no plume.

DR. BESSETTE: Are you speaking of the sensitivity?

CHAIRMAN WALLIS: I'm saying if plumes did exist in fact it was negligible. How strong were those plumes?

DR. BESSETTE: They were 40 degrees C. and 80 degrees C.

CHAIRMAN WALLIS: Because you've got over 100 degrees C. stratification in the cold leg.

DR. BESSETTE: Yes.

CHAIRMAN WALLIS: So you didn't look at the worst plume.

DR. BESSETTE: Well, I think there's no evidence that any experiments or modeling -

CHAIRMAN WALLIS: I know.

DR. BESSETTE: -- that you can get such plumes.

CHAIRMAN WALLIS: But if you're going to make this categorical statement if they exist, the effect is negligible, you're not looking at the worst case. You're looking at something more realistic.

DR. BESSETTE: I am looking at something more realistic, but it was --

CHAIRMAN WALLIS: The first thing you might do is look at the extreme case, and if nothing matters, then forget about it.

DR. BESSETTE: What we looked at in that study was conservative to everything we knew at the time. And the 40 degree case was conservative, and then we did twice that at 80 degrees and still could not see an effect.

So today I've got to --

CHAIRMAN WALLIS: And you concluded that the plumes are no stronger than ten degrees, I think, from the experiments.

DR. BESSETTE: Yes.

CHAIRMAN WALLIS: You haven't seen any plume stronger than ten degrees.

DR. BESSETTE: Not in any integral system test, no.

CHAIRMAN WALLIS: Except on the inner call and the QRA (phonetic) test.

DR. BESSETTE: Yeah. So today I was going to just go over those results quickly. So at the December meeting, I think the main questions that were lingering regarded RELAP's -- the adequacy of RELAP's modeling in the downcomer heat transfer, particularly suggested that RELAP could be nonconservative and what would be the effect.

CHAIRMAN WALLIS: Could we get it absolutely straight at the beginning what RELAP you're talking about? Because there's 1D RELAPs mentioned very often in your report, but the downcomer modeling is 2D always, right?

DR. BESSETTE: Well, when I spoke of RELAP as 1D, I spoke of it in terms of the formulation of the transport equation.

CHAIRMAN WALLIS: When it's 2B, it gets you circulation patterns which are much stronger than the average.

DR. BESSETTE: But for all of our analyses and assessment, we use a consistent two dimensional downcomer.

CHAIRMAN WALLIS: And what do you do for an H then? Because in the circulation pattern, you've got various losses in various places. So what do you say is the H?

DR. BESSETTE: Well, the H is dependent on if you're a free conduction regime, velocity doesn't come into it.

CHAIRMAN WALLIS: Yeah, but when you have circulation patterns in the downcomer --

DR. BESSETTE: Yes.

CHAIRMAN WALLIS: -- there are some places where there's no velocity, and there's some places where it's up four and a half meters a second. What do you use for the velocity to calculate H? Do you vary H around the thing or what do you do?

DR. BESSETTE: Well, the way RELAP works is it takes the maximum of free convection and force convection.

CHAIRMAN WALLIS: It takes the maximum H.

DR. BESSETTE: The maximum free. So if velocity dropped to zero, heat transfer does not drop to zero. It drops to a free convection number.

CHAIRMAN WALLIS: Yeah, but when it has got force conduction cells, it takes the maximum H from the force conduction?

DR. BESSETTE: For each cell, it looks at the velocity within that cell and takes the maximum of free and forced convection.

CHAIRMAN WALLIS: I think these are important details I didn't get from your report. Maybe they were buried somewhere or maybe they weren't there.

DR. BESSETTE: Well, maybe it's another level of detail that I didn't go to.

CHAIRMAN WALLIS: But it's important.

DR. BESSETTE: Yeah. So it's not like if you had a zero velocity heat transfer drops to zero.

CHAIRMAN WALLIS: It's very important to know what you're using in this to get age. It's very important to specify clearly so that the reader knows.

DR. BESSETTE: It is in there. I'll give you the page number.

CHAIRMAN WALLIS: This is the document that's going out to the world about how to calculate

PTS and how to calculate --

DR. BESSETTE: Yeah, my only thing is definitely without -- the balance is in there with the equations.

MEMBER RANSOM: And that 2D representation of the downcomer, I gather you had to turn off momentum flux in order to avoid these artificial recirculations?

DR. BESSETTE: Well, let's say 98 percent of the time, for 98 out of 100 transients we analyze, it wasn't a factor.

MEMBER RANSOM: Oh, only once in a while?

DR. BESSETTE: Only once in a while did it turn up as a factor.

MEMBER RANSOM: And I guess you're using a cross-flow approximation to the 2D effects in the downcomer?

DR. BESSETTE: That's correct. You know, it's parallel channels with cross-ros (phonetic) junctions.

MEMBER RANSOM: Now, one thing, the volume average velocity in that case is only an axial average of the velocities computed at the top and bottom, more or less, of the volumes, aren't they?

DR. BESSETTE: I think that's correct,

too.

MEMBER RANSOM: And that's what goes into the heat transfer correlation.

DR. BESSETTE: Yeah, but like I say, you get quite a significant amount of heat transfer in free convection. It doesn't drop to a low value.

MEMBER RANSOM: And that's just based on a Grashoff number correlation.

DR. BESSETTE: Yeah. So that I think the residual questions were mainly focused on the heat transfer because at that time we did not have integrated assessment results of RELAP against experimental data. Since then we performed additional assessment based on data from UPTF, APEX, and we also looked at CREARE.

The comparisons indicated that RELAP heat transfer modeling is appropriate, and secondly, there's another issue that was still lingering in December, was the question of whether we get down to low enough --

CHAIRMAN WALLIS: Can we look back at the CREARE tests where they have a plot? It's in your report, a Dittus-Bolter versus the actual measure of each. Do you remember that?

DR. BESSETTE: Yes.

CHAIRMAN WALLIS: They had to take the average velocity and multiply it by 20 to get all of that stuff.

DR. BESSETTE: Yes.

CHAIRMAN WALLIS: There is an error there, a factor of about two even there, I think, in that box, but this factor of 20, that comes from the two dimensional RELAP calculation?

DR. BESSETTE: No, the factor of 20 comes from the experiments.

CHAIRMAN WALLIS: But it must also come from RELAP. Otherwise RELAP isn't a useful tool.

DR. BESSETTE: Well, yes. RELAP comes out with a consistent -- with a factor of 20 that's --

CHAIRMAN WALLIS: That also predicts the factor of 20?

DR. BESSETTE: Yeah, but the when I quote a factor of 20 and a half and it flows, it's from the experimental data with measurements of flow velocities.

CHAIRMAN WALLIS: But you have to also convince us that RELAP with the momentum flux suppression and all of that is realistic enough to predict the right circulation velocity.

MR. ROSENTHAL: Yeah, and when we compared

RELAP with the data, it was consistent velocities.

CHAIRMAN WALLIS: It also had the 20 times or something close, but not necessarily in the same place.

DR. BESSETTE: If you take a certain point in the vessel, it could be off, but overall obviously it's probably time and spatial varying.

MR. ROSENTHAL: I think that we're discussing what's about Slide 15, and if we let Dave rapidly go through the beginning, it will set the stage, and then we can dwell on the phenomenological issues which are the real reason that we're here.

CHAIRMAN WALLIS: So you think you've required a little more?

MR. ROSENTHAL: Can we just give Dave five minutes?

CHAIRMAN WALLIS: Well, we can probably skip this slide.

DR. BESSETTE: So I just show this just to list the six reports that we've written, and this is in addition to the ESR. I just show this just to remind you.

So when we talk about the main contributors to uncertainty, the thermal hydraulic issues can basically be distilled into how good a predictive tool is RELAP, and from that governing issue, the main subissues included experimental evidence for plumes and the heat transfer modeling in RELAP. I was going to talk about that today.

This is along the lines what Mark was talking about earlier. The overall determination of uncertainties includes contributions from PRA, fracture mechanics, and thermal hydraulics. The bottom line risk number incorporates each of these three sources of uncertainty, and each needs to be considered within the context of the overall analysis.

The PRA uncertainty is reflected in the estimates that have been frequency, which is shown in the left-most histogram. The bin frequency is an estimate of the total frequency of all the individual event sequences that comprise a bin. For example, the medium break LOCA bin includes all of this spectrum of break sizes from four inches to eight inches, different break locations, different decay heat levels either coming out of full power operation or shutdown, winter or summer ECC conditions, and so on.

The middle histogram illustrates the resulting range of behavior that can occur within a given PRA bin so that each PRA bin has a certain family of 100 to 1,000 sequences in it, and you have a variation, an outcome within that bin. We characterize the range of that behavior for the various bins by analyzing a number of sequences or scenarios within each bin that are using RELAP.

In the last histogram, these tended to be qualitatively indicating the actual uncertainty.

CHAIRMAN WALLIS: Those are temperatures and impression.

DR. BESSETTE: Yeah. It represents the uncertainty in the RELAP code itself. It's the physical models in the code. So it says heat transfer and natural circulation.

CHAIRMAN WALLIS: Well, your message is that the thermal hydraulic uncertainties, perhaps because it's scaled this way, are smaller than the uncertainties in defining the event itself.

DR. BESSETTE: Well, I think, yeah, that's the correct conclusion.

CHAIRMAN WALLIS: So as in so many of these things, the uncertainties in the PRA dominate the uncertainties in the physics.

DR. BESSETTE: Believe it or not, the thermal hydraulics code is rather exact compared to the other uncertainties.

CHAIRMAN WALLIS: With the PRA, yeah.

DR. BESSETTE: Yeah.

MEMBER APOSTOLAKIS: Taking advantage of the fact that I came late --

(Laughter.)

CHAIRMAN WALLIS: Did you say something, George?

DR. BESSETTE: The main contributors to hydraulic uncertainties are actually --

PARTICIPANT: Next slide.

DR. BESSETTE: I hit at the button and it didn't go. Wrong button this way. Human factors problem.

The main contributors to thermal hydraulic uncertainty is the boundary conditions. The range of thermal hydraulic response in a given PRA bin is large compared to the predicted capability of RELAP. So, therefore --

CHAIRMAN WALLIS: Could you remind us which of these sequences is most important in determining the fracture potential? It seems to have changed with time over the evolution of this project.

MR. ROSENTHAL: Mark, you explained that to me yesterday, you know, what was important and it depended on what time of life, how much irradiation. Why don't you take that?

MR. EricksonKIRK: The general answer that's true at any point in the embrittlement life of the vessel is the primary side events way dominate over secondary side events, irrespective of embrittlement level.

At the next level of refinement, you'd have to say that at levels of embrittlement that are characteristic of the plants that we have operating today, when you take them out at either the end of their current 40-year license or even the end of license extension at 60 years, it's the stuck open valves that reclose later, and this is sort of a general statement, that would dominate for --

CHAIRMAN WALLIS: So it's the pressurized thermal shock.

MR. EricksonKIRK: It's the pressurized thermal shock. When you get down to the lower levels of embrittlement, the mild thermal shock that comes from the stuck open valve, which is equivalent to punching like a two to three inch hole in the primary is enough to initiate the cracks, but to get it all the way through the vessel, you need that late stage repressurization.

As you get out to the levels of embrittlement that are characteristic of our more embrittled vessels at the end of the 20-year license extension, at the end of 60 years, then you're starting to get into a mode where the medium and large pipe breaks on the primary side are starting to be like 50-50 contributors relative to the stuck open valves with late stage reclosure.

CHAIRMAN WALLIS: And that's with no pressurization presumably.

MR. EricksonKIRK: Yeah, the pressure is what it is, and it's not much when you put that big a hole in the vessel.

DR. BESSETTE: So, by list, the main contributors of the medium and large breaks and the stuck open SRV.

So in terms of the thermal hydraulic response of the plant for these bins, the outcome is mainly a function of the boundary conditions. For LOCA the most important factor is the break size. This affects both the energy removal from the RCS and the rate at which you add cold water to the ECC system.

For stuck open SRV scenarios, the important factor is whether the valve recloses or not, and if it did, how long did it stay open, and when it does close whether the operator throttles HPI to prevent the RCS from going water solid?

MEMBER APOSTOLAKIS: So the size of the break is a random variable?

DR. BESSETTE: Well, it's not known a priori. So you analyze the whole break spectrum.

MEMBER APOSTOLAKIS: But you're saying that it's a random variable that can be anywhere from 1.4 inch to 24 inches?

DR. BESSETTE: Yes.

VICE CHAIRMAN SHACK: But they have different frequencies.

DR. BESSETTE: They have different frequencies, yes. So it's not conclusive or anything. It's not a uniform distribution, but you don't know the size of the break a priori.

MEMBER APOSTOLAKIS: And if you had 100 of these, you would get 100 different break sizes. That's what they're saying.

DR. BESSETTE: Yes, yes.

MEMBER APOSTOLAKIS: Because it's random.

DR. BESSETTE: Yes. It may not be completely random, but because of certain pipe sizes you --

MEMBER APOSTOLAKIS: Essentially it would be random.

DR. BESSETTE: But it's essentially random.

VICE CHAIRMAN SHACK: One of the things that bothered me in the 1809 report is that your measure for the effect is the downcomer fluid temperature, whereas the thing I'm really worried about is the downcomer wall temperature or the vessel wall temperature, and I'm sort of worried whether you're underestimating the effect of the heat transfer coefficient in these calculations because I'll agree that the heat transfer coefficient doesn't do much to the downcomer fluid temperature, but it may have a rather more significant effect on the vessel wall temperature.

And so the measure that you have chosen for much of this on whether something is important or not is the fluid temperature when the reality the thing that drives the rest of this problem is the wall temperature.

DR. BESSETTE: Well, I'll try to show that if you have to choose a single variable in which in this case we had to choose a single variable, fluid temperature is the thing to choose. I mean the wall temperature reflects the fluid temperature and the heat transfer, but so you could choose like a heat flux number, let's say, that would incorporate both --

VICE CHAIRMAN SHACK: I could just choose a wall temperature.

DR. BESSETTE: Or wall temperature.

MEMBER RANSOM: Well, certainly it seems like the most uncertain parameter in this is the heat transfer coefficient itself. You know, the pressure and the temperature are pretty much global or macroscopic variables that their accuracies are more easily determined, I would guess.

But the thing that I think derives thermal stress on the wall is the gradient of temperature at the wall, and the boundary condition that is in force is the heat transfer coefficient times the wall delta T equal to minus K times the gradient of temperature in the wall.

It's the gradient that drives the thermal stress.

DR. BESSETTE: But I think though that we'll try to show that the fluid temperature, and average, an average downcomer fluid temperature is a suitable or the most is a good indicator of the severity of any given transient or comparing one transient to another and comparing the effect of different -- if you're trying to do sensitivity studies to look at the importance of different boundary conditions or physical models in the code, it's the best indicator or certainly there's no better indicator for our purpose than just simply choosing the downcomer fluid.

MEMBER RANSOM: Well, I don't doubt that the fluid temperature -- certainly that's important because that's the heat transfer to the wall, but in terms of uncertainty and, you know, trusting the system calculations, the one that I believe probably has the greatest uncertainty would be the heat transfer coefficient itself.

DR. BESSETTE: I'll try to show the uncertainty in the heat transfer coefficient is similar to the uncertainty effect of the downcomer fluid temperature.

CHAIRMAN WALLIS: The preferred temperature is the key thing. It must mean that the heat transfer is effective because if the heat transfer were very poor, the wall would not follow the fluid.

And it's really significant that what the heat transfer coefficient was, but you're telling me the fluid temperature matters the most. That seems to indicate to me that the heat transfer coefficient is big enough that it doesn't exert much influence.

DR. BESSETTE: Well, you could have a transient with a fluid temperature that went to 300 F., and it does what -- the heat transfer doesn't matter because the vessel doesn't get cold enough. So the key indicator is --

CHAIRMAN WALLIS: It doesn't get cold enough?

DR. BESSETTE: Essentially, no. Three hundred F. is not --

CHAIRMAN WALLIS: How could that be a measure of what's happening then if the vessel doesn't respond?

Well, maybe you're going to go ahead.

DR. BESSETTE: Well, I'll try to proceed and see if I answer the question.

So for a stuck open SRV scenario, the important factor is what -- oh, I went through that.

So anyway, these boundary conditions don't involve the physical modeling capability of the code. They're all associated with the input model of the code.

This is an example of the medium break LOCA bin for Palisades, where I plotted the risk significant transients that fall into the medium break

LOCA bin, and you can see the family of curves here in terms of pressure and temperature, and I hope we can make it out.

These are the error bars or uncertainty bars on the RELAP predictions of pressure and temperature, and the idea, this illustrates that the RELAP uncertainty in predicting these parameters is small compared to the range of behavior, the family of curves that characterize a range of behavior in this particular PRA bin.

VICE CHAIRMAN SHACK: Now, that RELAP uncertainty is what you're getting when you're varying the break flow model uncertainty and the heat transfer coefficient uncertainty?

DR. BESSETTE: No, this is the uncertainty we determined. Well, I guess when I say "RELAP," it's experimental data. So this is the code data comparisons for a bunch of experiments.

CHAIRMAN WALLIS: And just to put this in perspective, the response time of the wall is something like 50 minutes or 3,000 seconds in terms of the wall.

DR. BESSETTE: Yeah, it's about 1,000 seconds or so, or more.

So from here on I'll get more into the

RELAP modeling issues. I hope this shows that the basic idea is that the uncertainty from RELAP itself is small compared to what we're trying to measure with RELAP, what we're trying to characterize with RELAP, which is a good thing. Otherwise, it would be a problem.

MEMBER RANSOM: That's true of PNT, but I'm not sure it's true of H.

DR. BESSETTE: Well, we'll get into that.

MEMBER RANSOM: Which you can't measure.

DR. BESSETTE: I'll discuss that.

Well, we can measure it.

CHAIRMAN WALLIS: So your approach to this is not to say analyzing the system, the important dimensionless parameters are the Froude number, the BO number, the this and the that, and we're going to make sure that we cover a range of these variables.

You're going to say you have integral system tests representative of transients and because the facilities have been properly scaled, these cover the range of interests. That's your argument, rather than a dimensionless group sort of scaling thing. You're going to say all of these experiments suitably scaled, the range of transients we're interested in. That's your --

DR. BESSETTE: Yeah, in a sense, that's kind of --

CHAIRMAN WALLIS: That needs to come across.

DR. BESSETTE: -- a short circuit way of saying it, yeah.

CHAIRMAN WALLIS: -- in the report, too.

DR. BESSETTE: Yeah.

CHAIRMAN WALLIS: How you assured yourself that the experiments covered the field of interest.

DR. BESSETTE: Well, we knew, of course, what the dominant bins were, or at least early on we had some indication what the dominant bins are going to be, and they turn out to be medium break LOCAs.

CHAIRMAN WALLIS: See, if you read your report, there's the one page where it will say the only Froude number of interest is .05, and then you have the table where it goes to 60, and then there's no indication in any of these experiments what the Froude number really was, and the reader is left saying, "Well, now what Froude number is he really interested in?"

DR. BESSETTE: Well, in fact, I did look at the Froude numbers for the cold legs. I thought it was --

CHAIRMAN WALLIS: Well, you see the problem the reader has here, but you actually say the Froude number is an important variable, and you give conflicting values for what it should be, and it's never related to these experiments, and the reader says, well, you know, "What's going on here?" There's something important which never seems to be tied together with the experiments.

DR. BESSETTE: I'm pretty sure it's discussed in the report, but we show that the Froude number -- obviously the Froude numbers in the cold legs indicate stratification for the experiments and for the plant, and indeed for all --

CHAIRMAN WALLIS: Well, what are they in reality? Are they always low?

DR. BESSETTE: They're always low.

CHAIRMAN WALLIS: They're always much less than one?

DR. BESSETTE: Yes.

CHAIRMAN WALLIS: I didn't get that from the report because I have a table which has it going up to 60.

DR. BESSETTE: Well, I sent you a correction to that. There was a --

CHAIRMAN WALLIS: Yeah, but you see it

doesn't tell me what really happens. You have table going up to 60. It doesn't tell me which of those numbers in that table are realistic and which are just academic.

MEMBER POWERS: I'm shocked that you would use such a term.

DR. BESSETTE: So one of the -- this is how we obtained the uncertainty values with RELAP. So what are the objectives for determining the uncertainty due to the physical modeling in the code?

To do so, we assess RELAP against both integral and separate effects tests, and then integral tests were used to assess the code's ability to predict temperature or pressure and heat transfer. We included 12 experiments from UPTF, LOFT, ROSA, APEX and MIST, and these facilities cover a range of scales up to full scale. Their geometrical representations included all three vendor designs, and LOFT and ROSA were based on Westinghouse, APEX on Combustion Engineering; and MIST on Babcock & Wilcox.

So one scaling factor common to all was the power-to-volume, which was the basis of all the LOCA integral system test programs that we performed.

Now, the PTS PERT was used to guide the assessment of RELAP in terms of important phenomena.

The overall conclusion from all this was that the code compared well with the data.

CHAIRMAN WALLIS: Now, can I ask you about that? That's a qualitative sort of statement, and somewhere here I've got an APEX result where RELAP starts off doing fairly well, but ends up being off by 20 degrees in downcomer temperature. Is that good enough or not?

I mean I don't know what you mean by "compared well." How good does it have to be is perhaps the question.

CHAIRMAN WALLIS: Well, all I can say is, you know, we generated the uncertainties using the whole set of experiments, but the answer of how good does it have to be goes back to the question that was posed to Mark a little earlier.

I can tell you how good it is, and I can tell you --

CHAIRMAN WALLIS: I think your measurements of goodness are qualitative statements, aren't they, in your report?

DR. BESSETTE: In terms of comparisons with a separate effects phenomena, I used qualitative indications. In terms of an integral system test, we're actually generating statistics for the pressure and temperature as well as looking in detail comparing phenomena to make sure that we're in the right --

CHAIRMAN WALLIS: You see, I've got here a curve which compares RELAP with APEX CE tests, and after a while it's off by 20 degrees or more, and the APEX is colder than RELAP is predicting.

So that would mean that RELAP is not being conservative. I just wonder if that's important or not.

DR. BESSETTE: Well, like I say, you have to look -- I mean, I've said the one uncertainty in RELAP for temperature is ten degrees C., meaning five percent of the time it's going to be more than 20 degrees C. high or low.

CHAIRMAN WALLIS: I don't know if it matters. You see, if you're very close to fracturing the wall, 20 degrees might make a big difference. I don't know.

VICE CHAIRMAN SHACK: Because if you come back again to his Slide 8 where he's showing his RELAP uncertainty --

CHAIRMAN WALLIS: It's very small.

VICE CHAIRMAN SHACK: -- with all of the variations that he gets from his boundary condition, I mean, he does have three orders of magnitude of scatter in the through wall cracking results. I mean, there's no question there's large uncertainty in the prediction of the frequencies, but you know, his answers here do seem to be dominated by these uncertainties in the boundary conditions.

DR. BESSETTE: And in fact, I think that particular what you're referring to, if I remember, is the fact that we had suppressed circulation in the cold legs. So we constrained the mixing volume that RELAP was using, you know, in terms of a remix type of approach. The mixing volume includes all of the cold legs at a downcomer in the lower plenum.

By suppressing circulation in the RELAP model in the cold leg to prevent circulating flow, we truncated the mixing volume, and I think that was the explanation for that divergence.

CHAIRMAN WALLIS: So the bottom line here is that your 12 integral system tests --

DR. BESSETTE: They were chosen to --

CHAIRMAN WALLIS: -- offered enough of a feel that you really covered everything of interest --

DR. BESSETTE: I think so.

CHAIRMAN WALLIS: -- from the smallest break to the largest break?

DR. BESSETTE: We covered small breaks,

medium breaks, large breaks, like open SRVs, main steam line breaks. The idea was to choose from the best facilities that we had for the same transients that showed up as being risk significant in the PTS analyses.

CHAIRMAN WALLIS: So that what you actually could cite in your report are the significant transients or just some typical transients?

DR. BESSETTE: I cited all transients that we did assessments for.

CHAIRMAN WALLIS: But only in one to two cases did you ever get to the point of giving us any information about whether or not a crack would form.

DR. BESSETTE: Well, in my report I didn't get into the combined analysis. I focus on the thermal hydraulic validation of RELAP.

CHAIRMAN WALLIS: So you didn't get to what's my bottom line here.

DR. BESSETTE: That wasn't really the intent. The intent was to show the validity of RELAP for the PTS analysis.

MR. EricksonKIRK: I think I'd like to just interject a thought experiment here. I really think we need to -- and if the committee wants to see effects on the bottom line, that's a reasonable question, and I think clearly we haven't come prepared to answer that today, but I do think we need to structure the discussion in terms of first establishing what do the relevant topical area experts feel is a credible model and then assess the effect of variations between potential credible models on the bottom line.

And I'll just, you know, throw out this question as a thought experiment, and this applies to any part of the calculation.

Would the committee be prepared to accept a completely ludicrous model as part of the whole if I could show you that it had no effect on the bottom line? For instance, would Dr. Ford let me get away with an embrittlement model that says as I embrittle the material it becomes -- as I irradiate the material, the fracture toughness goes up, if I could show him that it had no effect on the model?

Certainly it wouldn't because it's absurd, and so I think that the focus of Dave's paper and what we need to focus on today is to say: is the heat transfer coefficient model credible? Are there potential alternative credible models that we need to investigate? You know, are plumes credible or not?

And once we establish those answers, then we'll be prepared to move on and say, "Okay. Here's our baseline model. Here are the potential credible alternatives," be they slight variations in heat transfer coefficient, slight existence of plumes or not, and then we can crank those things through the fracture mechanics analysis to see what the effect of potential credible variations is on the bottom line.

CHAIRMAN WALLIS: You see, the reason I keep saying this is I like figures like Figure 420, where you've got a KR versus time versus various Hs, and then there's a statement in the text that if KR gets above one, then you have to worry.

Well, it's quite clear that by varying H by a little bit, you can make KR go above one or not, and so this tells me I'd better get H right.

And that's to me being a much more important message than seeing a whole lot of Hs predicted by RELAP maybe or maybe not agreeing with data. That tells me how well I have to get my H right. I think that's a very important part of the report.

MR. EricksonKIRK: Yes, it is, but you also have to remember that the bottom line that we keep talking about is not the through wall cracking frequency or the conditional probability of failure associated with one particular transient.

If we were trying to predict with high accuracy the response of the vessel to one particular transient, I'd go find myself another job because I know we can't do it.

CHAIRMAN WALLIS: Yeah.

MR. EricksonKIRK: But, I mean, because of the uncertainties and the systematic biases in all of the parts of this analysis, but because we're trying to predict the response of the vessel to a series of different postulated transients, and again, you know, the assessment results showed, some of which are high, some of which are low, and they're not off by that much. You know, I think we can get a reasonable result that can be used in an engineering analysis to set a screening criteria.

CHAIRMAN WALLIS: Go back to the argument. Because we're so uncertain about the PRA results we can be really sloppy about the thermal hydraulics.

MR. EricksonKIRK: I'm not sure I want to agree with that.

MEMBER RANSOM: I thought the report did quite a good job though of pointing out that you can screen out many of these transients because if you don't have any pressure on the vessel, you're certainly not going to contribute to the stress.

So there is a selected break size of importance, and a set of scenarios and pretty much need to just focus on those.

In terms of the heat transfer coefficient, too, I suspect again you can probably show it's not very important because these are very low flow type situations that are not going to result in high convective heat transfer.

So I thought it did a pretty reasonable job of leading you through all of that for us.

DR. BESSETTE: And I'm planning to go through that story today.

This is sort of the bottom line in a way that shows that the statistical results obtained for comparing RELAP with the 12 experiments from the five facilities I mentioned. As you can see, RELAP had a bias of 13 psi in pressure with a standard deviation of 46 psi.

These differences, these numbers are equivalent to about one to two percent of the vessel, the pressure during normal operation. It's less than one percent of the yield stress. So obviously these are small numbers. So these uncertainties are not important.

MEMBER POWERS: When you do a comparison of the code against the experiments and you look at what might be called residuals between the experimental measurements and the "could" predictions, do you try to characterize the distribution of those residuals?

DR. BESSETTE: I'm not --

MEMBER POWERS: Well, you've used the language here as though you saw these residuals as normally distributed, and that's not uncommon. Most people do that. But I wondered if you actually went and tried to verify that, in fact, those residuals came from a normal population.

DR. BESSETTE: I don't think we looked at that.

Is Bill here? We didn't look -- no. No, we did not.

MEMBER POWERS: Is it important to do that?

DR. BESSETTE: Well, I don't think so. I think this first order numbers are adequate for what we're trying to do.

You can see with respect to temperature RELAP had essentially no bias. That's one degree C., and the standard deviation of one sigma was ten degrees C. Heat transfer --

CHAIRMAN WALLIS: This temperature is the downcomer?

DR. BESSETTE: Downcomer temperature, yeah.

The heat transfer, the integral system assessments that we performed showed RELAP to be realistic or conservative.

CHAIRMAN WALLIS: Now, is there some evidence for that? And what you mean by conservative is that the heat transfer in the experiment is always less than what you predicted. Is that what you mean by that?

DR. BESSETTE: The heat transfer coefficient in RELAP, that would be derived from RELAP was higher than the experiment.

CHAIRMAN WALLIS: In every case?

DR. BESSETTE: The cases we looked at. We didn't --

CHAIRMAN WALLIS: The CREARE tests, you got that factor of 20, and Dittus-Bolter. The experimental points are above the predicted.

DR. BESSETTE: Yeah, well, the Dittus -- we didn't actually try to calculate Dittus-Bolter. We calculated APEX and -- I mean, we didn't try to calculate CREARE. We tried to -- we calculated UPTF and APEX.

CHAIRMAN WALLIS: But you see, that's the problem, again, I have with parts of the report. You make this statement, and then I look at that figure from CREARE, and the data are all about a factor of two above the predictions.

DR. BESSETTE: Well, it's about 50 percent higher.

CHAIRMAN WALLIS: Well, at least it's not conservative.

DR. BESSETTE: Yeah, it's consistent with Dittus-Bolter, but lying above the line.

CHAIRMAN WALLIS: The problem I have, again, sort of reading bits of the report, we say, well, is this evidence compatible with the conclusion or not?

DR. BESSETTE: The evidence that it does match -- it does follow Dittus-Bolter with a 1.5 multiplier --

CHAIRMAN WALLIS: It never reached a conclusion like that. It's a very strong conclusion really, and I think you ought to be careful that there isn't something else in the report that's inconsistent with it.

DR. BESSETTE: Well, we chose UPTV, and see CREARE has, let's say, what you might call an atypical geometry. It has a thermal shield in it, and the measurements that were taken that led to where those data came from were just slightly downstream from the entrance to the thermal shield region, and we weren't sure how valid or how applicable those data were.

So we concentrated on APEX and UPTF instead.

VICE CHAIRMAN SHACK: Now, as I read the APEX though, there were only a very limited number of tests in which you actually made the wall temperature measurements.

DR. BESSETTE: Well, they're there for all of the tests, but we had just --

VICE CHAIRMAN SHACK: But you only presented them --

DR. BESSETTE: We only did one test. We only picked one test.

VICE CHAIRMAN SHACK: Oh, so you only made the comparison for one test.

DR. BESSETTE: Yeah.

VICE CHAIRMAN SHACK: So for that test it was okay, and you, therefore --

DR. BESSETTE: We looked at the APEX, at the APEX and UPTF, and they both produced similar results.

CHAIRMAN WALLIS: So that's another thing, is were you extrapolating some very limited results from one test to make a general conclusion about all conditions.

DR. BESSETTE: Well, what I said is for the test we looked at and we compared against data from UPTF and Apex under conditions of loop flow stagnation, and for these tests the code was realistic or conservative.

CHAIRMAN WALLIS: And do you generalize this conclusion to all conditions of interest in that?

DR. BESSETTE: No, I don't think I say that.

CHAIRMAN WALLIS: But you have this conclusion to your report that each is predictive conservatively by RELAP, and I just wanted to find out how broad a base of evidence you have for that conclusion.

DR. BESSETTE: Well, I mean, that's why I didn't go as far as to try to generate statistics and whatnot, is because I figured I didn't have a large enough database to be definitive that in all cases this would be true, but all I can say is we had a short time to do it. We looked at the best data we could find at least from two facilities, and from what we looked at, the code looked okay.

CHAIRMAN WALLIS: Well, I would still ask the question if it's a very limited data set, is it one extreme or the other? If it's for a very slow transient, maybe you don't care what H is anyway and the fact that it's conservative or that's unimportant.

DR. BESSETTE: Yes.

CHAIRMAN WALLIS: But maybe it's for a rapid transient where you do really care about it, and in that case it's conservative. So when it really matters, you've got some evidence that it's conservative.

DR. BESSETTE: Yes.

CHAIRMAN WALLIS: I can't put it in perspective if it's just one test, and I don't know which one it is.

DR. BESSETTE: I'm going to get into that later.

CHAIRMAN WALLIS: Okay.

DR. BESSETTE: A few slides later.

So I'm going to talk about impact of these uncertainties first in terms of pressure, then temperature and then heat transfer.

So by itself, the uncertainty in the RELAP prediction approach was small compared to the range of conditions found in the various PRA bins, and without uncertainty value was considered in terms of the contribution of vessel wall stress. The effect also seemed to be small, as well.

For example, I said the uncertainty in the RELAP calculation of pressure amounts to approximately two percent of the normal operating stress.

CHAIRMAN WALLIS: No problem.

DR. BESSETTE: So off the table.

Now, for stuck open SRV scenarios, the pressure at the time of vessel failure, for predicted vessel failure is determined by the set point of the SRVs themselves, and not by the thermal hydraulic uncertainties. So the most important factor is the timing of reclosure, which is a boundary condition.

Now, with respect to temperature, the heat flux is a function of the downcomer temperature and the heat transfer combined, and from these two parameters the favor calculates the temperature distribution and vessel walls as a function of time. And the vessel temperature distribution, of course, determines both thermal stress and the local fracture toughness.

Therefore, temperature enters twice into the equation and determines the vessel failure probability.

So it showed RELAP calculates temperature with no bias, while the standard deviation is ten degrees C. This standard deviation number of ten degrees C., while it seems small, can still affect the probability of vessel failure, as I think we've been discussing.

However, in context, this ten degrees is small compared to the absolute change in temperature, which gets back to why we chose average downcomer temperature, which during these risk significant transients, the absolute change in temperature is about 200 degrees C.

So the uncertainty of ten degrees compared to the absolute change is about five percent.

CHAIRMAN WALLIS: That's okay unless there's no crack growth until you get to 200 C., and if you get to 210 degrees C. maybe it makes a big difference. I mean, again, I don't know.

DR. BESSETTE: That's why I say it can't be dismissed.

CHAIRMAN WALLIS: I think this is rather

a cliff sort of thing. It's not a continuum where you can say five percent doesn't matter.

DR. BESSETTE: No.

MS. DUDES: It's like going through a door. If you're six feet, six, you go through a door. If you're six foot, nine you hit your head. I mean just the fact that it's a small percent change doesn't really help you.

DR. BESSETTE: It depends where you are.

But secondly, it's small in comparison to the range of behavior that characterizes a given PRA bin, which is typically 50 degrees C. to 150 degrees C. or so.

Now, the impact of the heat transfer coefficient.

So I think the situation is probably clear with pressure and temperature. Now, we turn to the heat transfer coefficient. Now, the change in the heat transfer coefficient has a similar effect to a change in the downcomer temperature as the heat fluxes a combination of the two.

So the impact of an uncertainty in heat transfer depends on a transient, of course, and like I've said, the faster the transient, the greater is the wall to fluid temperature difference. So fast transient has got to be sensitive to uncertainty in heat transfer than slow transients.

So a small break LOCA is slow transients obviously. For slow transients, a downcomer wall attracts the fluid temperature quite closely with a small delta T, and a large break LOCA is fast transients, and the downcomer cools quickly. The fluid cools quickly, and you build up more of a lag between the wall temperature and the fluid temperature.

CHAIRMAN WALLIS: This is one of my questions again. You chose to show only one figure of the effect of H on as pressurized thermal shock parameter, such as K sub R, and that was for a transient of 30 minutes tau, which is much longer than the large break that you show here.

And so my immediate sort of curiosity is, well, suppose you had shown some other curves for a shorter transient. What would it have looked like?

DR. BESSETTE: Well, I didn't choose that. I was taking a historical document and --

CHAIRMAN WALLIS: But you see, it immediately raises the question by the reader: why did he predict this long, slow transient which really isn't that much of a threat to the vessel? I'm more interested in the other ones.

DR. BESSETTE: Yeah.

CHAIRMAN WALLIS: So I'd like to see more figures like 420 for other --

DR. BESSETTE: Yeah, I'll try to address that to some extent today at least.

CHAIRMAN WALLIS: I notice this difference in this large break, the big temperature uncertainties here. Anyway, when you get this 29 degrees C. and you've talked about ten degrees C. not mattering, being where things don't matter, it immediately raises a flag.

DR. BESSETTE: Well, it goes back to putting things in context and showing where things might matter and where things might not matter.

CHAIRMAN WALLIS: That's good, that's good.

DR. BESSETTE: So you get some things off the table and you concentrate on the other things.

MEMBER SIEBER: Yeah, now when you did the through wall cracking sensitivity study with the other heat transfer coefficient, four of the 16 inch hot leg break, you increase by a factor of an order of a magnitude.

DR. BESSETTE: Yes.

PARTICIPANT: So, you know, it was sensitive to --

CHAIRMAN WALLIS: It was significant. It would have been good to bring that out more in the report.

DR. BESSETTE: So you can see here --

CHAIRMAN WALLIS: Well, the results are pretty reasoned, but you can see that for a large break it would be double the heat transfer coefficient. This is equivalent to decreasing the fluid temperature roughly by 20 to 30 degrees C.

So even though a large break is a fast cool-down, you can still boost the heat transfer even more.

MEMBER SIEBER: Yeah, but you can't get the pressure back up, right?

MR. GRIFFITH: Peter Griffith.

I think you should mention here that the probability of those three breaks is not the same.

CHAIRMAN WALLIS: That's right.

MR. GRIFFITH: But you could have another column over there which showed the --

CHAIRMAN WALLIS: That's what he has on the bottom.

MR. GRIFFITH: That's right. The event

frequency for large break is very low to begin with. So --

CHAIRMAN WALLIS: But it might dominate the risk because it might lead much more frequently to disaster, and so I understand that when you go to high levels of embrittlement, this large break LOCA becomes a more dominant thing. So if you're going to come up with a number for probable failure, but if the large break LOCA, even though very unlike is the dominant sequence.

DR. BESSETTE: So you can see from the previous slide that --

CHAIRMAN WALLIS: Then you can't just dismiss it because its event frequency is low to begin with.

MEMBER SIEBER: Well, let me understand. If you have a large break and you get a rapid cool down, because you have the break, you can't repressurize, and so you can't put stress.

CHAIRMAN WALLIS: That's right.

DR. BESSETTE: So there's no pressure. That's right.

CHAIRMAN WALLIS: So it breaks from the thermal stress alone.

MEMBER SIEBER: So why worry about that.

CHAIRMAN WALLIS: The thermal stress alone can break the vessel.

MEMBER SIEBER: That's true, but whether it breaks or not, you know.

CHAIRMAN WALLIS: You can do the experiment by taking a glass.

MEMBER SIEBER: I've done that.

MR. EricksonKIRK: You're getting into the question of consequence after the break.

MEMBER SIEBER: Right. You've got a messed up plant. On the other hand, the consequence from a public health and safety standpoint really doesn't change.

MR. ROSENTHAL: Actually -- Jack Rosenthal -- actually it does.

MEMBER SIEBER: Okay.

MR. ROSENTHAL: Let's just take this in pieces.

MEMBER SIEBER: All right.

MR. ROSENTHAL: We have a large break LOCA. ECCS works or doesn't work.

MEMBER SIEBER: Right.

MR. ROSENTHAL: If ECCS doesn't work, then it's a severe accident, and we're in a different regime and discussion.

MEMBER SIEBER: Yeah.

MR. ROSENTHAL: We're talking about large break LOCA in which ECCS does work. You reflood the core. You don't melt the core or you reflood the vessel and you don't melt the core.

Now, let's say -- and in your event tree, you would write okay at the far right. Now if you do crack the vessel, then you have the initiating event. ECCS did work, but the vessel, should the vessel have cracked, now I may not be able to maintain a covered core, and so I may have a sequence in which even though I had my LOCA and ECCS worked, I'm still in trouble.

So it is a relevant consideration, and the argument would be that it's unlikely that you're going to fail the vessel, even with injecting cold water and successfully mitigating the LOCA.

DR. BESSETTE: So where heat transfer is -- where the outcome is most sensitive to heat transfers for large breaks, and we're dealing with the run frequencies. Current numbers are like ten to the minus seven. It brings it --

CHAIRMAN WALLIS: So we might as well not consider them at all.

DR. BESSETTE: So even if they're

sensitive to heat transfer, it's still, you know.

VICE CHAIRMAN SHACK: Although again, we'll come back to the sensitivity study, I just keep looking at the numbers here. The one that I have the biggest thing tacked on gives me a factor of 30 increase in through wall failure frequency, and that's a two-inch break.

MR. EricksonKIRK: At the risk of contradicting my colleague, the fact remains that large breaks are an important to PTS risk. So you can say that it's a low event frequency, which is true, but when you roll all of the calculations together, they show that medium to large breaks are important contributors at high levels of embrittlement. You're not going to get rid of it.

CHAIRMAN WALLIS: They're important contributors, but the total risk is still very small.

MR. EricksonKIRK: Yes, that's absolutely true, nd that's a true statement across the board. You can say that about anything we discuss today.

CHAIRMAN WALLIS: So what about this sensitivity factor of 30 that my colleague Bill Shack is raising here?

VICE CHAIRMAN SHACK: I'm just looking at another case with a two inch line break which does occur more frequently, and it's got a factor of 30 increase with a change in the age, and that actually strikes me as somewhat plausible, but you know, if I only have a small thermal insult, the question of whether I get that thermal insult from the fluid to the wall is kind of a critical question.

So, you know, with a large break LOCA, the insult is so big it almost doesn't matter what I -- you know, it's going to get to the wall and do me in anyway, but I'd sort of worry about medium and small breaks where, you know, how much I get to the wall really starts to become important.

MR. EricksonKIRK: At the risk of beating a dead horse because I've tried this twice and we keep veering off --

(Laughter.)

MR. EricksonKIRK: I think it's extraordinarily important because you know, the nice thing about calculations is you can make them tell you anything that you want.

I think it's exceedingly important to first establish what the technical area experts consider to be credible variations in the heat transfer coefficient or any other parameter we want to examine, and then we'll do the sensitivity studies.

It isn't at all clear to me that --

VICE CHAIRMAN SHACK: Well, we were just studying impact of heat transfer coefficient.

MR. EricksonKIRK: Yes, yes, and it's big. It can be big, sure.

VICE CHAIRMAN SHACK: Okay.

MR. EricksonKIRK: So the question really is back to we need to reach some consensus between the review committee and the staff as to what a credible baseline model is and what credible perturbations are, and then we can do sensitivity studies with meaning.

CHAIRMAN WALLIS: But the sensitivity studies help to define the requirements for the accuracy of the thermal hydraulics. If it was a factor of 30 in your predictions by getting an error in new transfer coefficients, then it seems to me you would say, "Well, go back and get the heat transfer coefficient more accurately."

I don't think you can just look at how good thermal hydraulics is without asking what are you going to use it for. Then you're not being an engineer.

DR. BESSETTE: What I'm trying to show here is, you know, that your question is concerned with heat flux, and the heat flux is temperature and heat transfer coefficient. If the effect of a factor of two change in heat transfer coefficient is within the uncertainty as to how well you know the temperature, so it's not a uniquely important problem. It's not more important than how well you know the fluid temperature, and we know the fluid temperature to with --

VICE CHAIRMAN SHACK: Well, unless there's a systematic.

DR. BESSETTE: Yeah, and we don't see a systematic -- we haven't seen a systematic error or bias in fluid temperature or in a more limited assessment we did, a heat transfer.

MR. EricksonKIRK: To return to Dr. Wallis' last point, isn't there a question of state of the art? And I'll get this in something that the materials people can understand so that I have a chance.

The uncertainty in fracture toughness data is what it is, and that's the plot with the gas leak scatter that you keep referring to, and members, you know, Shack and Ford cannot like that degree of uncertainty, but it's controlled by physics. I can't make it any better. So we just simply have to deal with it, and can't a similar -- can't an analogous point be raised here regarding the overall fidelity of the thermal hydraulics model? I mean there has to be a question of practical state of the art that puts in that maybe we don't know the heat transfer coefficient better than plus or minus 20 percent.

If that's the consensus of the technical community, then that's what we need to feed through our analysis, but I don't think we've gotten there yet.

I mean, certainly, yes, you're absolutely right. You need to understand the sensitivity of your results on your input, but I'm seeing that we've gone quite a bit further than that and that we're letting the results, be they sensitive or insensitive drive our acceptance of models that either may be at state of the art or may be completely ludicrous.

CHAIRMAN WALLIS: I understand your point, and I think it's a very good one, but inevitably when we look at the results that they present, we sort of say, "What does it matter?" We can't help asking ourselves that question.

DR. BESSETTE: And you know, when you look at a sensitivity studies plot, sometimes it doesn't make any difference. Sometimes you can find a factor of 30, and you just have to look at the bottom line and just put everything in context.

In fact, you can see we're dealing with -- you can see the order of magnitude in terms of the frequency estimates in the last column between the 5th and the 95th percentiles. So within that kind of context, a factor of 30 is certainly within that range.

So one of probably the key issues raised during the peer review that we had of the PTS work was with respect to the buoyancy opposed mixed convection. So if flow velocities were to be sufficiently low, one could get an enhancement in heat transfer over that predicted by the three or fourth convection models in RELAP.

Sine the December meeting, we looked at data from UPTF, APEX and CREARE, the same data we've just been discussing, that provide flow velocity measurements in a downcomer.

CHAIRMAN WALLIS: These are the maximum velocities reported?

DR. BESSETTE: I reported the range. What i have here, this one third to -- we saw velocities. The total range of velocities we saw amongst the three experiments was between one-third of a meter, one foot a second and four or five feet a second.

CHAIRMAN WALLIS: Our cells are probably some places where there's at least some of the time no velocity at all. So you may not --

DR. BESSETTE: That zero velocity, that stagnation point is probably changing the design in space.

CHAIRMAN WALLIS: And probably their velocity meter measured fluctuating velocity, no?

DR. BESSETTE: Well, these velocities, of course, they're measured at fixed locations, a certain number of fixed locations, and --

CHAIRMAN WALLIS: It did vary with time presumably.

DR. BESSETTE: You see, of course, noisy data.

CHAIRMAN WALLIS: I think was it APEX. The heat transfer coefficient fluctuated by a factor of about five. So something is certainly going on there.

DR. BESSETTE: Yeah. Well, certainly if you look, for example, if you look at either velocity data or temperature data, you see fluctuations. That's like the passage of eddies or whatnot.

CHAIRMAN WALLIS: Right. So what you mean here is the maximum velocity when you talk about downcomer velocity?

DR. BESSETTE: What I'm talking about here is the velocities that we saw fell within this range. Sometimes there would be -- it was all within this range. I didn't see anything lower than about a foot a second.

CHAIRMAN WALLIS: So which one are you going to use? You're going to use the maximum one for your heat transfer predictions?

DR. BESSETTE: No, I'm just saying --

CHAIRMAN WALLIS: No?

DR. BESSETTE: What we did, I said this is their range of velocities. Well, the point on this viewgraph is the to say for these kind of velocities, you're well outside the range of buoyancy opposed mixed convection.

CHAIRMAN WALLIS: It's Reynolds number dominated.

DR. BESSETTE: This is Reynolds number dominated.

CHAIRMAN WALLIS: So you want to get the velocity right.

DR. BESSETTE: So, I mean, for these velocities, what we get is downcomer reynolds numbers of 500,000 to three million. So the idea is that this whole issue of buoyancy opposed mixed convection was something of a red herring.

VICE CHAIRMAN SHACK: On this, I noticed on the staff replied review comment number 65, no experiments of measured velocity in the downcomer.

DR. BESSETTE: Well, I was pretty ignorant when I wrote that.

(Laughter.)

DR. BESSETTE: I looked harder and found data.

VICE CHAIRMAN SHACK: Ah, you looked harder. Okay. That solves that problem.

DR. BESSETTE: Anybody can be wrong in this, but there's always a chance for reforming.

CHAIRMAN WALLIS: That business of centimeters, it's just a typo. Centimeters in the second one is a typo.

DR. BESSETTE: That's supposed to be -- that was a typo. That's meters.

CHAIRMAN WALLIS: So RELAP is predicting similar velocities at the maximum, although the cells are not quite the same, and you think that's good enough to give a characteristic velocity on which to base age.

DR. BESSETTE: I think what we can say is that RELAP velocities are consistent with these experiments.

CHAIRMAN WALLIS: See, 2D RELAP without momentum flux is not a very good tool, is it, in general?

DR. BESSETTE: The 2D RELAP with momentum flux off aid these same range of velocities that we saw in the experiments.

MEMBER RANSOM: Well, when they emit the momentum flux, they're not emitting all the other forces, you know, pressure driven forces and that kind of buoyancy and gravitational. So those forces are still included.

If you're in a constant area passage and an incompressible fluid, you don't have any real change in momentum flux.

DR. BESSETTE: But you may be seeing to do is disable a potential demiracle (phonetic) effect.

CHAIRMAN WALLIS: Right. So a user who runs RELAP 2D form in the downcomer for this problem is not going to encounter erratic, whimsical, large velocities, unrealistic just due to the numerics and the running of the code under any circumstance?

DR. BESSETTE: We ran hundreds of calculations. We looked at the output of every calculation and checked for downcomer velocities just to make sure we weren't getting anything.

CHAIRMAN WALLIS: And they were all --

DR. BESSETTE: They were like typically --

CHAIRMAN WALLIS: Erratic ones only come in when you put in some momentum flux terms.

DR. BESSETTE: Yeah, it's like for the whole set of Oconee transients, there's only one transient. When we ran a whole set of 75 Oconee transients with momentum flux on or off, only one out of those 75 was affected.

CHAIRMAN WALLIS: So we're trying to establish a MOX requirement in this, the state of the art. The state of the art is the RELAP can predict this thing, and it can predict it well enough on some basis?

DR. BESSETTE: I think the state of the art is, I think, reflected in these ten degrees C. and the fact that if you change heat transfer by a factor of two, the effect is similar. It's within this ten degrees C. uncertainty.

CHAIRMAN WALLIS: This factor of 20 is also 20 in RELAP or in 16 or 25?

DR. BESSETTE: Yes. Without that factor of 20, you just have flow creeping along at about an inch a second in downcomer instead of what we see of one to --

CHAIRMAN WALLIS: But the story has changed here. The old story I heard was that the reason you get good mixing is because you have flow-through there which is mixing injected flow.

It's not that at all. It's that the injected flow itself sets up cells.

DR. BESSETTE: That's correct, yeah. I think we characterized it as mysterious last time, but --

(Laughter.)

DR. BESSETTE: So this is the issue of, say, temperature distribution in the downcomer, and we looked at the same body of integral system test data that I have been talking about, these 12 experiments, and we looked at the temperature measurements both axially and azimuthally and couldn't find any plumes in any of the integral system test data. I'm speaking of a plume now. I'm speaking of any temperature differences beyond ten degrees C., but typically we didn't even find anything close to ten degrees.

CHAIRMAN WALLIS: All because the stratified flow coming out of the cold leg in some way fixes with about ten times as much fluid and 140 degrees certification becomes ten degrees.

DR. BESSETTE: That's right if you have a mixing --

CHAIRMAN WALLIS: But not insight as to what that mixing process is.

DR. BESSETTE: They have a mixing ratio of ten. Then the 100 degrees becomes ten.

CHAIRMAN WALLIS: There's an awful lot to happen at that one place instantaneously. I agree there's a lot of evidence, but it seems a very strange, extraordinary amount of mixing in one place.

DR. BESSETTE: Well, I think what we --

CHAIRMAN WALLIS: If you look at pictures of salt plumes, they don't show all stirring around and so on.

DR. BESSETTE: Well, I think maybe a part of that is, you know, you see these salt plumes in these separate effects tests. I think there are additional mixing processes going on.

The other thing --

CHAIRMAN WALLIS: My instinct would be that if you had a low Froude number, you'd simply be pouring the stuff down the wall like pouring maple syrup out of a container, and it's running down the container into of onto your plate, and it doesn't mix at all. This stuff would just ooze out and run down the wall. There's no reason for it to mix.

It jumps the gap and impinges on the wall and spreads out, and that's great.

DR. BESSETTE: Well, the CREARE data --

CHAIRMAN WALLIS: The Froude number must have something to do with this.

DR. BESSETTE: The CREARE data, for example, flows up the gap.

CHAIRMAN WALLIS: Then that would be a mechanism for it spreading and getting a lot of mixing.

DR. BESSETTE: Yeah.

CHAIRMAN WALLIS: But that would depend on the Froude number.

DR. BESSETTE: Yeah.

CHAIRMAN WALLIS: And does the Froude number vary a lot between plants? Well, it did between CE plants and Westinghouse.

DR. BESSETTE: Well, see, I think the injection Froude number varies a lot. I mean, CE and Westinghouse have low injection Froude numbers and BNW high,b ut no matter what --

CHAIRMAN WALLIS: How high is high for BNW?

DR. BESSETTE: Like any of the velocities, I can't remember the exact numbers. CE and Westinghouse flow comes in at about a foot a second or so, and BNW comes in at 20 feet a second. So in Westinghouse, let's say the flow comes in, drops to the bottom of the cold leg, and then it spreads out. There's some mixing in the cold leg obviously.

CHAIRMAN WALLIS: It's extraordinary to me. It's not just low velocity. It's being squirted in I thought very rapidly in order to save the core. In fact, it was just dribbling in.

DR. BESSETTE: Well, I'm talking about high pressure injection flow rates, and everything is coming in through the same pipe. So each --

CHAIRMAN WALLIS: High pressure would presumably create high velocity.

DR. BESSETTE: Well, no. High pressure --

CHAIRMAN WALLIS: It goes through a throttle valve or something?

DR. BESSETTE: No, no.

MEMBER SIEBER: Everything is high pressure.

CHAIRMAN WALLIS: But it's into low pressure once the pressure drops down in the system.

DR. BESSETTE: the low pressure pumps a

high capacity. High pressure pumps a low capacity.

CHAIRMAN WALLIS: Well, this is part of the report I thought could b improved, where you talk about Froude number being so important, and clearly it does affect some of these phenomena, and yet you don't then tell us what it is for various plants and various conditions. So we don't have a perspective as to, you know, why it's important, what its range is, whether you've covered the range and all of that.

So maybe you could do that for us when you rewrite the report.

DR. BESSETTE: I will try to clarify it. I thought it was in there. Obviously I'll take another look at it.

Well, about the these dye tests, of course, you know, it's qualitative indications.

CHAIRMAN WALLIS: And also the salt tests at APEX I guess have been thrown out because if you look at them they're quite anomalous.

DR. BESSETTE: I think the uncertainties are so high it's best not to draw anything more than some qualitative indications.

So at any rate I already talked about the sensitivity studies. Earlier we talked about the sensitivities we did on plumes before we started this whole reanalysis, and we used plumes of 40 degrees C. and 80 degrees C., and so almost no effect on the probability of vessel failure.

Nevertheless, we thought it was one of the key reasons we did the whole APEX program, was to make sure that our understanding that plumes were not important was, indeed true, and I think APEX certainly bore that out. We ran more than 20 different tests, and I looked at data from every test we ran, and typically the axial or azimuthal temperature variations were less than five degrees C. Generally they're unobservable.

So in conclusion what I tried to show is that the most important thermal hydraulic uncertainty, and I don't even know if you can call it thermal hydraulic uncertainty. It's the range of variations that characterize any given PRA bin. Within that range the actual physical model uncertainty contributed by RELAP --

CHAIRMAN WALLIS: -- analogy. It's like the break size. You can argue about what model you should use for critical flow out the break, but if the break itself is uncertain over a huge a range, it's not so important that you get your model right.

DR. BESSETTE: Yeah, the break flow

uncertainty may be 20 percent, but when you double the size of the break you don't care about the uncertainty, and that's the whole bottom line.

So since the RELAP modeling uncertainty is small compared to the bin uncertainty, the method we use to characterize the variations within a bin by running a set of RELAP calculations that cover the range of the bin was sufficient to represent the behavior of that bin, the map of the behavior of that bin.

We established the accuracy and uncertainty of RELAP, assessing it against a body of experimental data, and it was also assessed against additional separate effects data for important phenomena identified by the PTS PERT, and I think particularly with pressure and temperature, the agreement is very good, and it can be attributed to the integral nature of temperature and pressure as a measure of energy and inventory, conservation of energy and inventory.

And I think we've addressed the issues of fluid temperature distribution and downcomer and of mixed convection and have showed these two to be resolved or unimportant.

CHAIRMAN WALLIS: There you say RELAP

compared well to data. That doesn't mean anything to me. A well comparison in fracture mechanics, I think I know what that is.

DR. BESSETTE: It's qualitative, but you've got to say something.

CHAIRMAN WALLIS: Yeah, but I think you --

VICE CHAIRMAN SHACK: Well, but you do have uncertainties.

CHAIRMAN WALLIS: You have uncertainties. You have real numbers.

DR. BESSETTE: And quantify the uncertainties to the extent we can.

VICE CHAIRMAN SHACK: I have the same problem with this slide that I do with your report, and that is bullet three really seems to me to be bullet one. You know, the report should have been organized to tell me that RELAP agrees well with experiments, and I can sort of believe RELAP predictions.

Then you can go on and tell me how you can deal with the uncertainties, and the last thing I should hear about is the argument that maybe H variations aren't so important because when you start out with and I start to get to discuss variations on H, then I can run to my sensitivity calculations and

I come up with factors of 30, and it sure is hard to convince me that H is unimportant until you've convinced me that I'm using the wrong H.

DR. BESSETTE: I'll schedule a dry run with you next time.

(Laughter.)

VICE CHAIRMAN SHACK: Well, you know, I should have read the report backwards.

CHAIRMAN WALLIS: No, you shouldn't have done that. You shouldn't have done that because where is the section? There's a section called "Sensitivity of Probabilistic Fracture Mechanics Analysis to Thermal Hydraulic Variations," which I thought was one of the bottom lines, is one page, and there's nothing there or almost nothing there.

Now, this is one of -- it seems to me it's one of the key questions.

DR. BESSETTE: Do you want to handle that again, Mark? Do you want to go for it?

MR. EricksonKIRK: No, I believe I would say the same thing again. Comments with regards to the organization of the report notwithstanding, I mean, you're right. That's an important part of the story, and I think the comments we've received from the committee suggest that some reorganization of the report and perhaps an additional --

CHAIRMAN WALLIS: I think that might be in order. I think that generally speaking you've got enough here to make a case.

VICE CHAIRMAN SHACK: And review that section on the ratio of K applied and K fracture mechanics so that it isn't a stress.

MR. EricksonKIRK: That section will be removed.

VICE CHAIRMAN SHACK: Good.

MR. EricksonKIRK: Because that's not a bottom line.

VICE CHAIRMAN SHACK: Well, it's also wrong.

MR. EricksonKIRK: Yes. Minor issue.

VICE CHAIRMAN SHACK: Minor issue.

MR. EricksonKIRK: That's why it's easy to remove it.

MEMBER SIEBER: So it makes no difference.

CHAIRMAN WALLIS: So if we were to suggest that you rewrite the report, what would be the mechanics of it and the time line and so on?

MR. ELTAWILA: this is Farouk Eltawila from the (unintelligible) staff.

I think we really appreciate the comments that we got from the committee here, and we definitely need to sharpen our message, and reorganize the report, but I think it should not be germane for the committee to write its own report to the Commission about that we have enough information to proceed with the rulemaking so that we can transfer the report to NRR so they can work on it.

So having said that, we definitely are going to go and reorganize the report, and we're putting the message to put more clarity in it, and all the recommendations that you made, we'll incorporate them.

But again, it should not be any conditions for the --

CHAIRMAN WALLIS: And remember that the report doesn't just go to NRR. It goes out in the world.

MR. ELTAWILA: Absolutely.

CHAIRMAN WALLIS: Other countries, other experts are very much interested in this problem. You have to make your case clear so that they can understand it.

MR. ELTAWILA: No doubt about it, but the NRR needs to know now that we have enough technical basis to support a rulemaking, and they can put that into their schedule and they can work on the process, and we will be working on modifying the report, and we can do that in the next few months.

CHAIRMAN WALLIS: And the rule goes out for public comment in time to --

MR. EricksonKIRK: Oh, yes.

CHAIRMAN WALLIS: -- really get the report in shape before the rule is finalized.

MR. ELTAWILA: That's correct.

VICE CHAIRMAN SHACK: Don't forget to change Comment 65.

(Laughter.)

DR. BESSETTE: I'll make a note.

MR. EricksonKIRK: Would you like self-consistency?

CHAIRMAN WALLIS: How are we for time?

MEMBER POWERS: We're just about right on it.

CHAIRMAN WALLIS: We've been on time all day.

VICE CHAIRMAN SHACK: This is unbelievable. I'll turn it back to you, Mr. Chairman.

CHAIRMAN WALLIS: Thank you.

Are there any comments from the other members of the committee? Now is your chance.

(No response.)

MR. EricksonKIRK: Are we -- I'm going to ask my management a pointed question. What are we asking of the committee at this time?

MR. ELTAWILA: I think we are asking for a letter, that the staff has sufficient information to support change to the rule, and whatever additional comments the committee will want to make, that's their prerogative, but we're asking for a letter right now.

CHAIRMAN WALLIS: Okay. So we are ahead of time again. But this time, gentlemen, we don't have something that we have to come back for on time. We can come back early and do our work.

So thank you very much, Mark and Dave. I think you did a good job under --

MEMBER KRESS: Duress.

CHAIRMAN WALLIS: No, under appropriate examination.

(Laughter.)

VICE CHAIRMAN SHACK: Just remember Professor Wallis is always restrained when he has to make his comments at the ACRS.

CHAIRMAN WALLIS: I have to be very careful.

VICE CHAIRMAN SHACK: We'll bring the NEI guys in next time to give you a hard time.

(Laughter.)

CHAIRMAN WALLIS: We will then take a break until five o'clock, and then we'll go to work.

Thank you.

(Whereupon, at 4:36 p.m., the committee meeting was concluded.)

CERTIFICATE

This is to certify that the attached proceedings before the United States Nuclear Regulatory Commission in the matter of:

Name of Proceeding: Advisory Committee on Reactor Safeguards 520TH Meeting

Docket Number: n/a

Location: Rockville, MD

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 MOLLEN
Official Reporter
Neal R. Gross & Co., Inc.

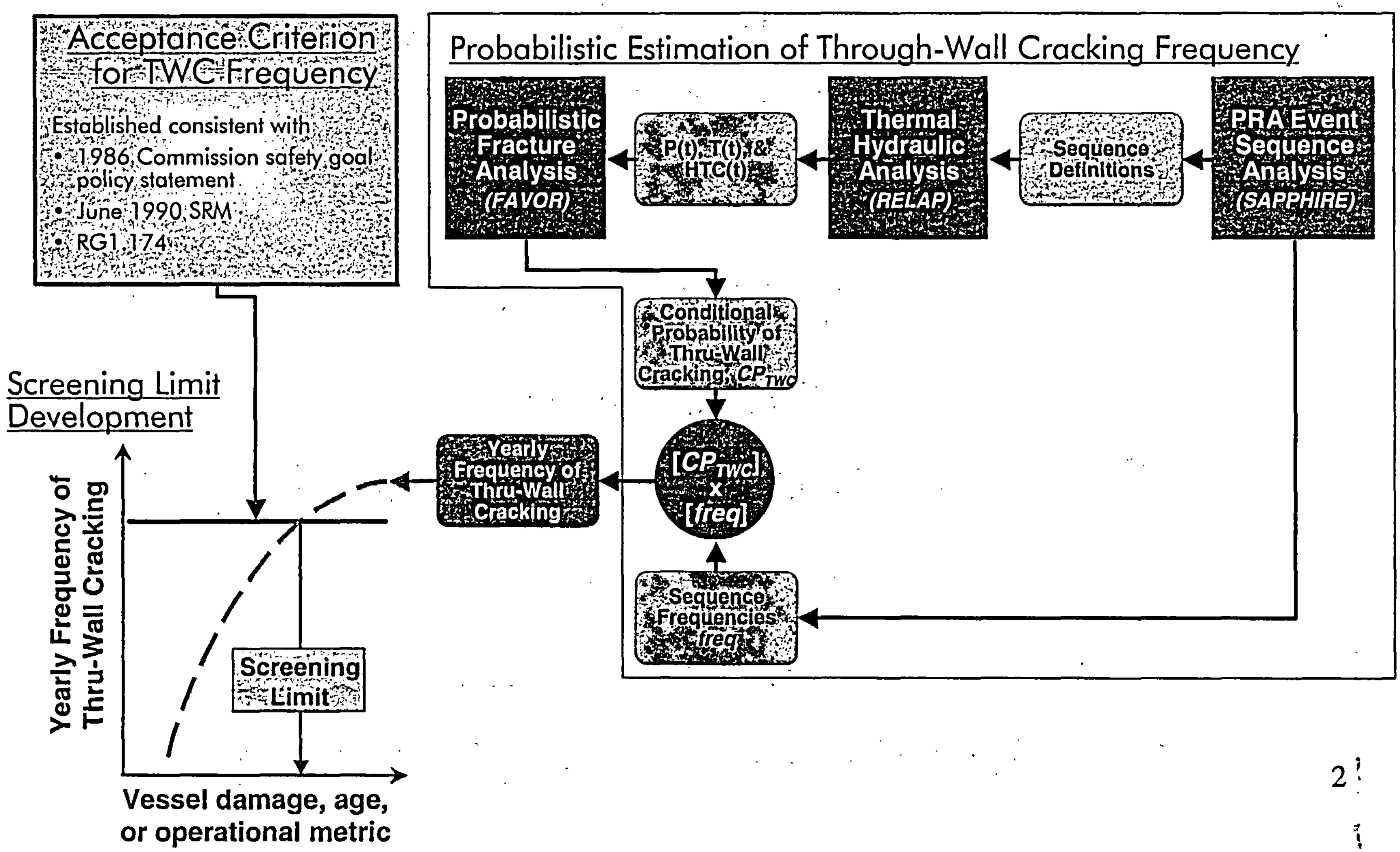
Thermal Hydraulic Evaluation of Pressurized Thermal Shock

Advisory Committee on Reactor Safeguards

March 3, 2005

David Bessette
Mark EricksonKirk
Office of Nuclear Regulatory Research

Overall Structure of PTS TWCF Estimate & How it is used to Establish PTS Screening Limits



Background

- December 2004, presentation included assessment of RELAP5 predictions of downcomer temperature and pressure, and showed the code predicted these parameters well.
- Data were presented that showed plumes to be weak or non-existent. Sensitivity studies conducted using stronger plumes indicated that if plumes did exist, the effect was negligible
- Current presentation reaffirms conclusions, summarizes assessment results, and addresses the issues of downcomer flows and heat transfer coefficient.

Background
Six Thermal Hydraulic Reports Describe Work Performed

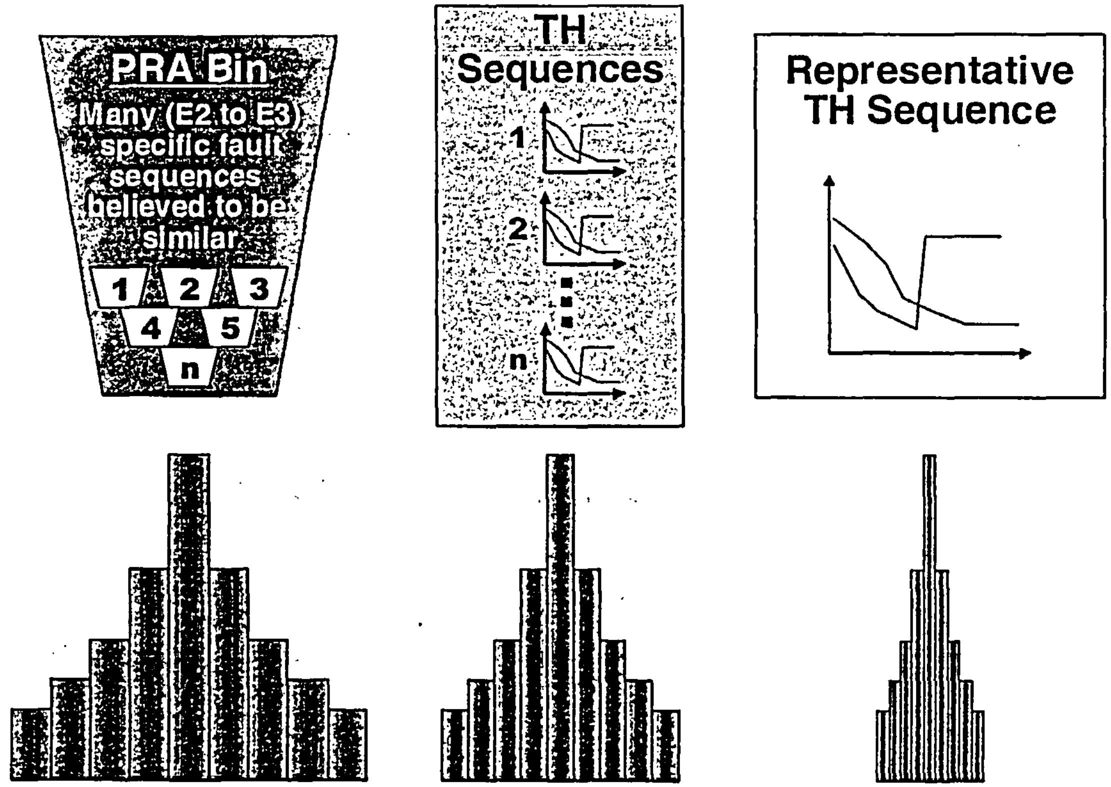
- RELAP5 Applications
  - Arcieri, W.C., Beaton, R.M.S., Fletcher, C.D., Bessette, D.E, "RELAP5 Thermal Hydraulic Analysis to Support PTS Evaluations for the Oconee-1, Beaver Valley-1, and Palisades Nuclear Power Plants," NUREG/CR-6858, October 2004.
- RELAP5 Assessment
  - Fletcher, C.D., Prelewicz, D.A., Arcieri, W.C., "RELAP5/MOD3.2.2γ Assessment for Pressurized Thermal Shock Applications," NUREG/CR-6857, October 1984
- Thermal hydraulic uncertainties
  - Chang, Y.H., Almenas, K., Mosleh, A., Pour-Gol, M., "Thermal Hydraulic Uncertainty Analysis in Pressurized Thermal Shock Risk Assessment," CRR-0401, University of Maryland, October 2004.
- PTS Experiments
  - Reyes, J.N., Scaling Analysis for the OSU APEX-CE Integral Test Facility, NUREG/CR-6731, 2003.
  - Reyes, J.N., et. al., Final Report for the OSU APEX-CE Integral Test Facility, NUREG/CR-6856, October 2004.
- Response to ACRS and peer review comments
  - Bessette, D., Thermal Hydraulic Evaluation of Pressurized Thermal Shock, NUREG-1809, February 2005

Thermal Hydraulic Issues Raised by ACRS and Peer Review

- **Main contributors to uncertainty (slides 6-7).**
- **Overall accuracy and uncertainty in RELAP5 to model thermal hydraulic boundary conditions of average downcomer temperature, pressure, and heat transfer coefficient.**
- **Accuracy of the heat transfer modeling in RELAP5 for downcomer conditions.**
- **Appropriateness of average value with respect to temperature and heat transfer variations around the downcomer (plumes, stratification).**

Premise of TH Uncertainty Treatment

- A single TH sequence is selected to represent to the PFM analysis ALL of a family of similar sequences in a particular PRA bin
- The parameter, modeling, and measurement uncertainties associated with a RELAP5 representation are small relative to
  - Uncertainty associated with the initiating event frequency for a bin, and
  - Sequence to sequence uncertainty within a bin
- These uncertainties are subsumed, enabling FAVOR to treat P(t), T(t), & h(t) deterministically for a particular sequence



Uncertainty in the frequency of event occurrence

Variability in the severity of different sequences

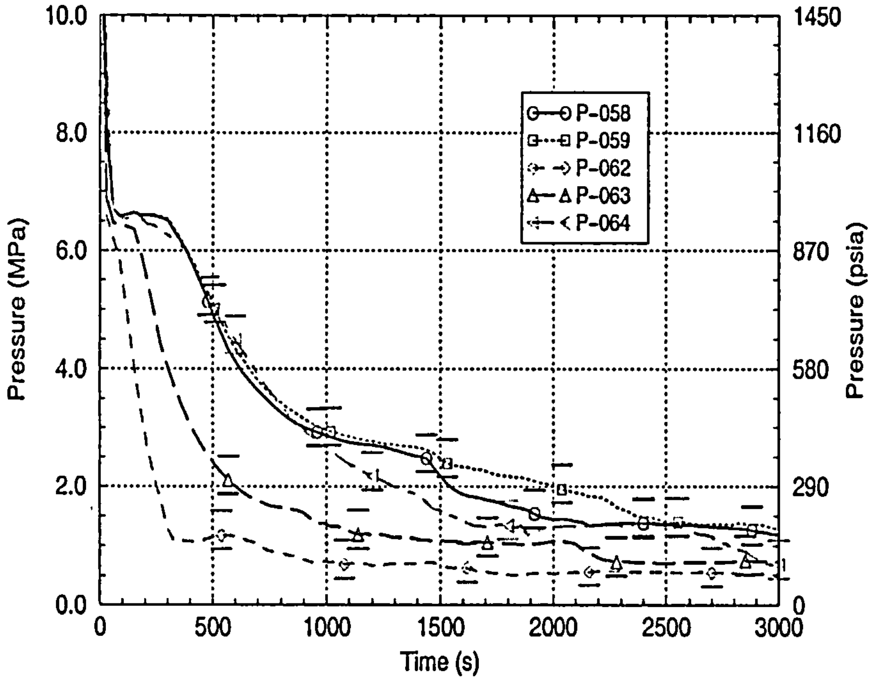
TH parameter and modeling uncertainties

Main Contributors to Thermal Hydraulic Uncertainties are Boundary Conditions

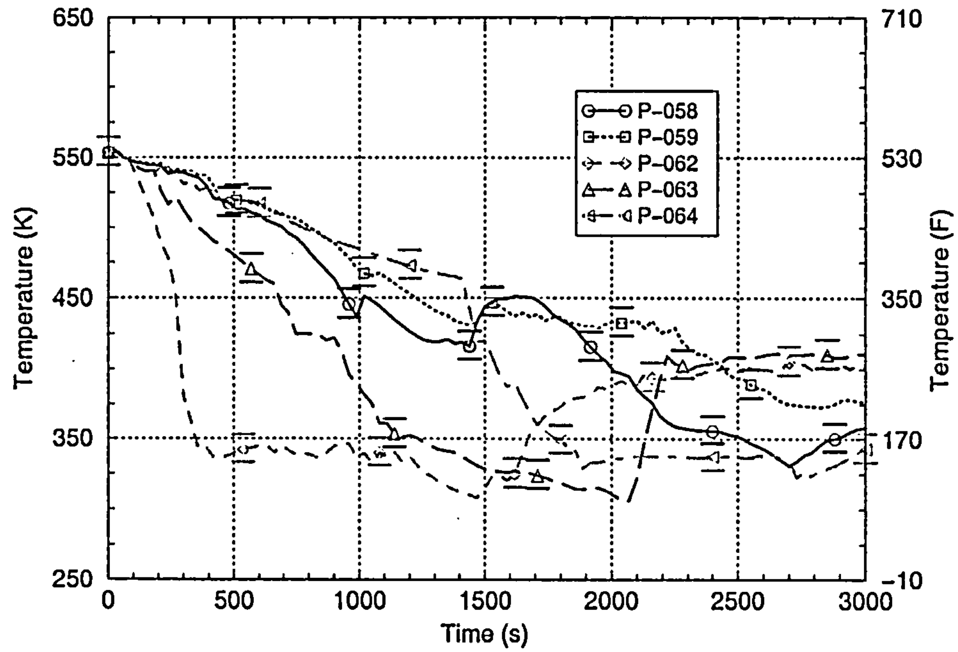
- *Because a bin is defined broadly, the range of behavior that describes a given bin is due mainly to boundary conditions (aleatory) rather than physical models in RELAP5 (epistemic)*
- **For LOCAs, the key factor is the size of the break:**
  - **Small break bin 1.4 inch to 4 inch (factor of 8)**
  - **Medium break bin 4 inch to 8 inch (factor of 4)**
  - **Large break bin 8 inch to 24 inch (factor of 10)**
- **For stuck open SRVs bin, it is time of valve reclosure, number of valves stuck open, decay heat.**

RELAP5 Calculations of Risk-Significant Transients
Palisades Medium Break LOCA Bin

Variations in boundary conditions dominate uncertainty in temperature and pressure



Pressure



Temperature

RELAP5 Physical Modeling Uncertainty Determined Through Assessment

- The applicability and uncertainty of RELAP5 was determined through comparisons to integral systems tests.
- Additional separate effects assessment performed for important phenomena.
- RELAP5 calculations compared well to experimental data.
- Assessment included 12 integral system tests representative of risk-dominant PTS transients.
- Facilities included UPTF, LOFT, ROSA-IV, ROSA-AP600, APEX-CE, and MIST.
- Facilities covered a range of geometries and scaling approaches and included full-scale tests. One scaling factor common to all was power-to-volume scaling, which was the basis for all LOCA integral system test programs.

RELAP5 Physical Modeling Uncertainties
Summary of Assessment Results

Pressure	Bias (RELAP5-experiment)	-0.093 MPa (-13 psi)
	Standard deviation (1 σ)	0.32 MPa (46 psi)
Temperature	Bias (RELAP5-experiment)	-1C (-2F)
	Standard deviation (1 σ)	10C (18F)
Heat transfer	Integral comparisons of RELAP5 with experimental data from UPTF and APEX under conditions of loop flow stagnation show that the code is realistic or conservative. No nonconservatisms were identified.	

Impact of RELAP5 Uncertainty in Pressure

- Bias (-13 psi) and uncertainty (46 psi) between RELAP5 and experimental data in the prediction of RCS pressure are small. The uncertainty of 46 psi amounts to 2% of normal operating stress.
- For LOCAs, pressure is low at the time of vessel failure. The contribution of pressure to wall stress is small. The uncertainty in the RELAP5 calculation of this pressure is small.
- For SRV scenarios, pressure contributes significantly to wall stress, however, pressure is determined by the SRV setting and not by RELAP5

Impact of RELAP5 Uncertainty in Temperature

- **Temperature affects both fracture toughness and the thermal stress in the vessel (and, thereby, the applied fracture driving force).**
- **RELAP5 effectively has no bias (-1C) in the prediction of downcomer temperature.**
- **The RELAP5 1σ uncertainty of 10C, while seemingly small, can still be significant at certain times during certain transients with respect to determining fracture toughness.**
- ***For risk-significant transients, the change in downcomer temperature from initial conditions to the time of vessel failure is ~200C, so the uncertainty is ~5% of the total change in temperature.***
- ***In addition, this 10C uncertainty is small (10% to 20%) compared to the variations in a bin of 50C to 150C and is subsumed by the spectrum of transients analyzed to determine uncertainty.***

Impact of Heat Transfer Coefficient

- **Heat flux is function of h and fluid temperature. A change in heat transfer coefficient has about the same effect on heat flux as a change in fluid temperature.** $q'' = h (\Delta T)$
- **The faster the change in fluid temperature, the larger the wall-to-fluid ΔT. Heat flux is insensitive to the uncertainty in h for slow transients (small break LOCAs and SRV scenarios). Fast transients (large LOCAs) are more sensitive to changes in h.**
- **Difference in ΔT between base case and HTC x 2:**
- **Small breaks: 1C to 7C**
- **Medium breaks: 3C to 10C**
- **Large breaks: 18C to 29C**
- **Only for large breaks does factor of 2 increase in HTC become greater than the fluid temperature uncertainty of 10C. *The event frequency for large break is very low to begin with. (next slide)***

Initiating Event Frequency for LOCAs

- Mean initiating event frequency for large break LOCAs is less than 10^{-7} based on frequency alone, while medium breaks are less than 10^{-6}.
- Range of uncertainty in frequency from 5th to 95th percentile is 2 to 3 orders of magnitude

SECY-04-060 PWRs, per reactor year

Break size	5^{th}	Mean	95th	Range 5^{th} to 95^{th}
1.6"	7 E-6	2 E-4	9 E-4	120
3"	2 E-7	2 E-5	6 E-5	390
7"	1 E-8	2 E-6	9 E-6	800
14"	6 E-10	4 E-8	2 E-7	260
31"	4 E-11	2 E-8	7 E-8	1700

Downcomer Heat Transfer
Mixed Convection Not Relevant

- RELAP5 calculations of downcomer velocities are similar to measured data from UPTF, APEX and CREARE (0.3 to 1.5 m/s)
- Buoyancy enhanced flows produce large circulation cells well-mixed conditions ($Gr/Re^2 < 0.1$).
- Factor of 20 enhancement in downcomer mass flows relative to ECC injection rate seen in data from UPTF, Creare, and APEX-CE.
- Buoyancy-opposed mixed convection not relevant.
  - Downcomer Reynolds numbers range from 500,000 to 3,000,000 (compared to 6,000 to 20,000 for Swanson-Catton experiments).
  - Gr/Re^2 ~0.01 to 0.1 in plant compared to ~0.6 to 2 for Swanson-Catton

Downcomer Heat Transfer and Fluid Temperature Plumes Are Not a Important Factor

- Integral test data show no plumes.
  - Integral system tests more reliable than separate effects tests. Full 3D representation of downcomer, interaction among multiple plumes, upper plenum-downcomer bypass flow path allows in-vessel natural circulation, additional driving forces of core decay heat and heat transfer across core barrel, additional flows induced by break and depressurization
- Separate effects test data exhibited weak plumes (~20C) that decreased in magnitude over the duration of the test.
  - IVO dye tests give a qualitative indication of flow patterns consistent with large mixing cells (NUREG/IA-004). The tests were not intended to be quantitative
- Prior to start of PTS reevaluation, sensitivity studies with stronger plumes (40C, 80C) were performed. Almost no effect on conditional probability of vessel failure (CPF).

Conclusions

- Range of thermal hydraulic conditions in any given bin is larger than the thermal hydraulic uncertainty from physical models in RELAP5.
  - Uncertainties in predictions of pressure, temperature, and heat transfer are subsumed by the range of transients analyzed.
- Plant behavior adequately resolved from the number of thermal hydraulic calculations and corresponding thermal hydraulic bins.
- RELAP5 adequately predicts important phenomena, most importantly the boundary conditions for fracture mechanics analysis.
  - The good comparisons are attributable to the fact that pressure and temperature are global state parameters.
  - Integral assessment of heat transfer in the downcomer showed RELAP5 compared well to data.
  - Mixed convection issue not relevant.
  - Downcomer temperature variations (plumes) are not important

Presentation to the Advisory Committee on Reactor Safeguards

Safety Review of the North Anna Early Site Permit Application

Presented by
Michael Scott
Senior Project Manager
New, Research and Test Reactors Program
March 3, 2005

Purpose

- Brief the Committee on the North Anna early site permit (ESP) application and the status of the NRC staff's safety review of that application
- Support the Committee's review of the application and subsequent interim letter to the Commission
- Answer the Committee's questions

Agenda

• Background and Milestones	5 min
• North Anna ESP Application	5 min
• Draft Safety Evaluation Report (DSER)	5 min
• DSER Issues	5 min
• Future-Oriented Items	5 min
• Conclusions	5 min
• Discussion / Committee questions	

Background and Regulatory Framework

- Subpart A to 10 CFR Part 52 governs ESPs
- Subpart B to 10 CFR Part 100 contains applicable siting evaluation factors
- 10 CFR 52.23 requires ACRS to report to Commission on portions of application that pertain to safety (i.e., Site Safety Analysis Report)
- Purpose of ESP process is to resolve issues related to siting at early stage
- North Anna is first of three ESP applications the NRC staff is currently reviewing - others follow at two-month intervals

Purpose of ESP Process

- Separates, to extent feasible, review of site from review of design
- Allows resolution of site-related issues before expenditure of significant resources
- Allows ESP holder to “bank” site for future use

Future Milestones

- ACRS interim letter to the Commission assumed 03/18/05
- Staff provides final SER (FSER) to ACRS late May 2005 (prior to final division director and Office of the General Counsel concurrence)
- Staff issues FSER 06/16/05
- ACRS letter to the Commission assumed 07/25/05
- Staff incorporates ACRS letter and issues FSER as NUREG 08/29/05
- Mandatory hearings begin fall 2005
- Commission decision assumed mid 2006

North Anna ESP Application

- Submitted for a site wholly within the existing North Anna Power Station (NAPS) site, adjacent to existing North Anna units 1 and 2 and partially overlaying site of canceled units 3 and 4 (partially constructed in early 1980s; most structures subsequently removed)
- NAPS is owned by Virginia Power and Old Dominion Electric Cooperative and controlled by Virginia Power
- ESP applicant, Dominion, is a wholly-owned subsidiary of Dominion Resources, Inc. (as is Virginia Power)
- Dominion seeks authorization for limited work in accordance with 10 CFR 52.17(c) and 10 CFR 50.10(e)(1)

North Anna ESP Application

- Dominion requests site be approved for location of two "units" of up to 4300 MWt
- Each unit may be one large reactor or multiple smaller reactors
- Dominion has chosen not to submit a specific design but instead has submitted a plant parameter envelope (PPE) based on a number of current and future reactor designs
- Staff's review of PPE values in ESP applications limited to whether they are reasonable

North Anna ESP Application

- Rock site
- Regional geologic faults
- Seismic hazard characterized using Regulatory Guide (RG) 1.165 method
  - Low-frequency earthquake M7.2 at 300 km
  - High-frequency earthquake M5.4 at 20 km

North Anna ESP Application

- Unit 3 to use once-through cooling
- Unit 4 to use "dry" closed-loop (radiative/convective) cooling to atmosphere to eliminate/minimize lake temperature increase and water demand on lake
- Underground ultimate heat sink (UHS) if design selected requires a UHS
- Dominion considering use of intake and discharge structure of canceled units 3 and 4
- Dominion seeks 20-year ESP term

DSER

- First-of-a-kind evaluation of safety aspects of an ESP application
- Benefited from resolution of a number of generic issues prior to application submittal
- Review guidance is RS-002, “Processing Applications for Early Site Permits”
- Some “generic” issues arose during application review and needed to be resolved during DSER development

Safety Review Areas and Lead Staff Reviewers

- Meteorology: Brad Harvey
- Hydrology: Goutam Bagchi (contract support from Pacific Northwest Laboratory) (PNL)
- Site Hazards: Kaz Campe (contract support from PNL)
- Geology/seismology: Cliff Munson (support from U.S. Geologic Survey)
- Demography/Geography: Jay Lee
- Emergency Planning: Bruce Musico (consultation with Federal Emergency Management Agency)
- Quality Assurance: Paul Prescott
- Physical Security: Al Tardiff
- Radiological Consequence Analysis: Jay Lee

Issues - Emergency Planning

- Dominion has elected to seek acceptance of "major features" of emergency plans as provided in 10 CFR 52.17(c)(ii)
- Concept is not defined in detail in regulations
- NRC/FEMA have issued draft guidance document, Supplement 2 to NUREG-0654
- Generic industry concern with degree of finality associated with major features
- Staff can grant finality as to the overall description but will need to address implementation details at COL

Issues - Seismic

- Dominion proposed new "performance-based" approach for determining safe shutdown earthquake (SSE) - Not entirely consistent with NRC-approved method in RG 1.165
  - Staff advised Dominion that time required for review of this method would likely result in delay in issuance of staff's review products for the ESP application
  - Applicant ultimately elected to use RG 1.165 method
- Because North Anna is a rock site, site SSE exceeds design SSE at high frequencies for designs certified to date (COL item)

Issues - Site Characteristics vs Design Inputs

- Issue is what is needed and/or appropriate at ESP
  - Staff has given Dominion credit for appropriate consideration of most severe natural phenomena including margin
  - Dominion concerned that ESP should not specify design bases, but rather may specify site characteristics that would serve as minimum site-related design inputs at COL

Issues - Design/Site Interface

- Several examples involving interface between site (intended to be subject of ESP) and design (intended to be subject of design certification and/or COL)
  - Potential interferences between new and existing plants
  - Potential underground UHS in presence of water table near surface
  - Potential for frazil and anchor ice
- These individual items are discussed in backup slides

Future-Oriented Items in DSER

- Open items – Staff needs additional information prior to developing FSER
- Confirmatory item – Staff needs to verify applicant's planned actions as stated in its responses to requests for additional information
- COL action items – Site-related items that are more appropriately addressed at COL stage
- Permit conditions – Conditions the staff proposes be imposed on holder of the ESP should one be issued

DSER Conclusions

- DSER defers general regulatory conclusion regarding site safety and suitability to FSER after open items addressed
- Some conclusions from individual sections without open items
  - Applicant has provided appropriate quality assurance measures equivalent to those in 10 CFR Part 50 Appendix B
  - Site characteristics are such that adequate security plans and measures can be developed

DSER Conclusions

- Additional conclusions from individual sections without open items
  - Population center distance, as defined in 10 CFR100.3, is at least one and one-third times the distance from the reactor to the outer boundary of the low population zone and compliant with 10 CFR 100.21(b) and (h)
  - Applicant has established appropriate atmospheric dispersion characteristics to support radiological calculations
  - Based on PPE and site characteristics, site meets radiological dose consequence criteria in 10 CFR 50.34(a)(1)

DSER Conclusions

- Additional conclusion from individual section without open items
  - Potential hazards associated with nearby transportation routes, industrial and military facilities pose no undue risk to facility that might be constructed on the site

Presentation Conclusions

- Staff has issued first-of-a-kind DSER for North Anna ESP application
- Most open item responses expected by March 3, 2005
- Because of first-of-a-kind nature of this action, staff is working through some issues identified during the review
- Looking forward to seeing interim ACRS letter and to briefing the Subcommittee and the full Committee this summer on final results of staff's review of this application
- Staff is identifying lessons learned for possible inputs to future rulemakings and revisions to guidance

Backup Slides

Issues - Seismic

- Dominion proposed new "performance-based" approach for determining safe shutdown earthquake (SSE)
  - Not entirely consistent with NRC-approved method in RG 1.165
  - ASCE Standard 43-05 describes this approach
  - Risk-based approach that targets performance goal
    - 1×10^{-5} annual probability of unacceptable performance of Category 1 systems, structures, and components
    - Target seismic risk based on core damage frequencies for existing nuclear power plants

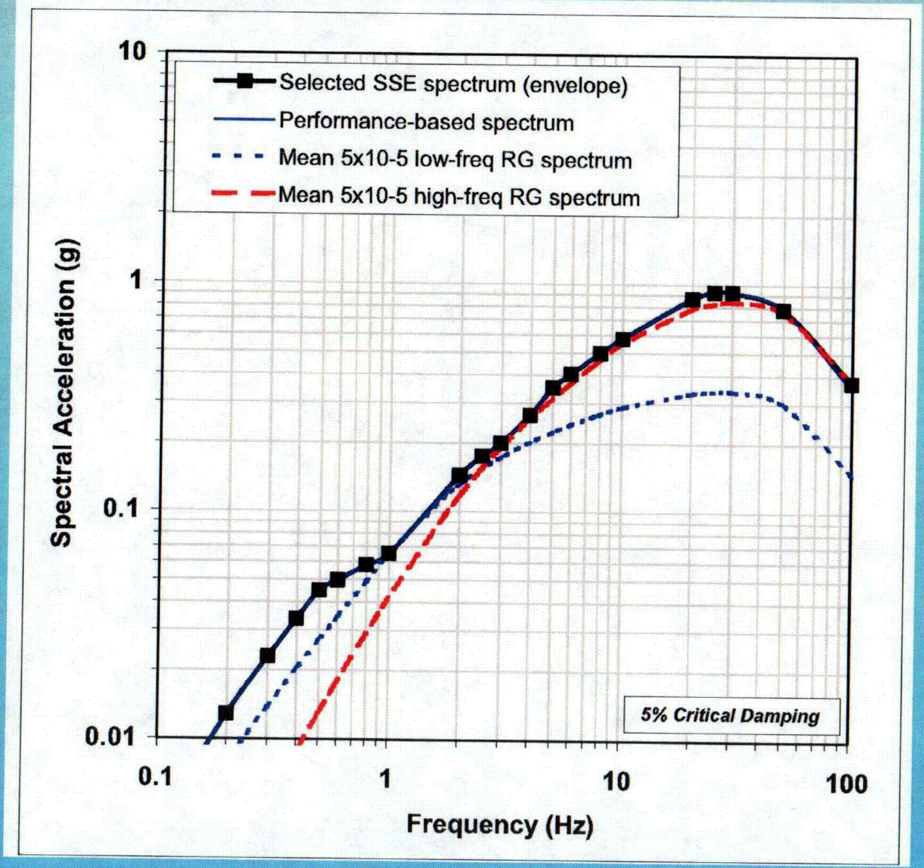
Issues - Seismic

- Because staff had not reviewed or approved the performance-based approach, staff advised Dominion that time required for review of this method would likely result in delay in issuance of staff's review products for the ESP application
- Applicant ultimately elected to use RG 1.165 method with justification for use of reference probability 5×10^{-5} per year

Issues - Seismic

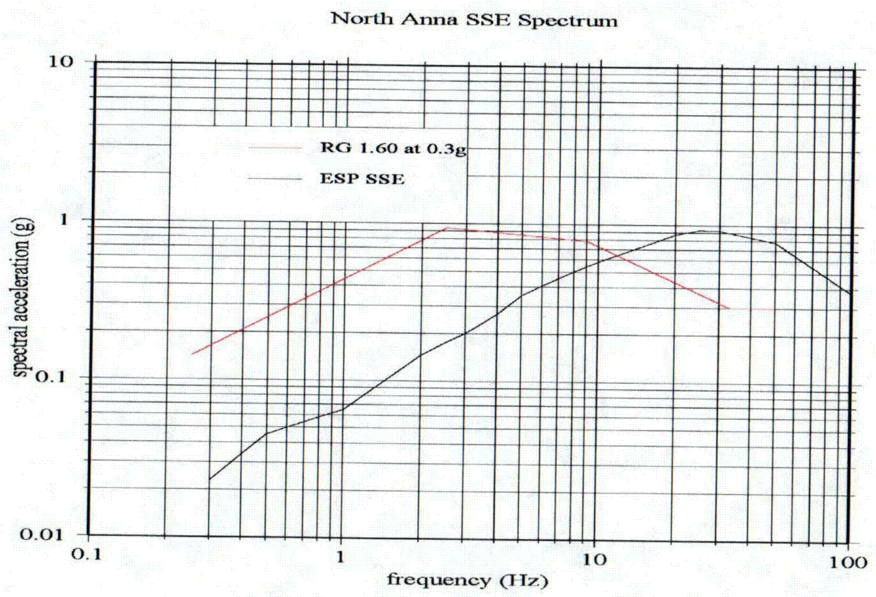
- Because North Anna is a rock site, site SSE exceeds design SSE at high frequencies for designs certified to date
- COL applicant would need to resolve disparity if one exists (dependent on design selected)
- See SSE vs. RG 1.60 diagram

North Anna SSE



C01

SSE vs RG 1.60



C02

Open Items

- 2.1-1, Control of exclusion area
  - Applicant must have control over exclusion area or irrevocable right to obtain control
  - Legal issue being addressed in Office of General Counsel
- 2.3-1, Basic wind speed (fastest mile)
  - Dominion used 100-year return fastest mile value from industry standard
  - Observed data point exceeds 100-year return from standard
  - Dominion has chosen to provide 100-year return 3-second gust in lieu of fastest mile

Open Items

- 2.3-2, Snowpack weight vs snow load
  - Regulatory Guide 1.70 states weight of 100-year snowpack and 48-hour probable max winter precipitation (PMWP) should be used to provide weight of snow and ice on safety-related structures
  - Staff branch technical position provides clarification:
    - Normal winter precipitation load should be weight of 100-year snowpack
    - Extreme winter precipitation load should be weight of 100-year snowpack plus 48-hour PMWP
  - Dominion plans to provide 100-year snowpack, 48-hour maximum snowfall, and 48-hour winter PMP
  - COL applicant will determine how to combine these characteristics for comparison with design for extreme environmental load category unless otherwise justified

Open Items

- 2.3-3, Site characteristic to assess potential for freezing in UHS
  - Dominion plans to submit accumulated degree-days below freezing
  - Issues remain regarding choice of weather station and methodology for calculating
- 2.3-4, Impact of dry cooling on atmospheric temperature
  - Dominion plans to provide qualitative or semi-quantitative assessment
  - Approach recognizes system not designed
- 2.4-1, Coordinate reference system
  - Dominion plans to submit reference system and units of measure

Open Items

- 2.4-2, Minimize distance to existing systems, structures, and components (SSCs)
  - Existing NAPS Units 3 and 4 discharge tunnel likely within 1 foot of Units 1 and 2 service water piping
  - What will happen if COL applicant finds it cannot use existing structure?
  - Dominion states:
    - Not feasible or necessary to specify vertical separation distance
    - Only one of many examples of possible interferences that can and will be addressed at construction stage
    - 10 CFR 50.59 review of changes provides protection for operating plant

Open Items

- 2.4-3, Impacts of low-flow conditions
  - Dominion plans to propose minimum lake level same as for NAPS units
- 2.4-4, Ice jam formation and breakup
  - Dominion plans to show impact bounded by already-analyzed impact of breach of upstream dams
- 2.4-5, Minimum intake water temperature
  - No clear quantitative site characteristic regarding frazil ice
  - Dominion plans to note in application that frazil ice conditions could occur at the site
  - COL applicant would need to describe engineered measures to handle frazil ice

Open Items

- 2.4-6, Stability of underground UHS against ground water pressure head
  - Water table near surface, could lift empty or partially full UHS
  - Absent construction details, would have site characteristic for groundwater elevation
- 2.4-7, Correlate ground water level measurements taken in support of the ESP application with data from long-term piezometers
  - Dominion states they do not correlate well (different purposes and locations)
  - Need to show post-drought data not anomalous
  - Dominion plans to take additional data
  - Dominion will need to assess impact of lack of correlation

Open Items

- 2.4-8, Conservative hydraulic conductivity
  - Dominion plans to provide more conservative method
- 2.4-9, Upward hydraulic gradients
  - Dominion plans to show such gradient is small fraction of horizontal flow and bound its impact
- 2.4-10, Variation in hydraulic gradient
  - Dominion plans to provide additional seasonal data
- 2.4-11, Onsite measurement of adsorption and retention coefficients
  - Dominion plans to use onsite measurements of soil conditions and a lookup table from the Environmental Protection Agency to determine coefficients

Open Items

- 2.5-1, Criteria for ground motion model weighting in the model clusters for the EPRI 2003 ground motion evaluation
  - Dominion has responded to this item
  - Staff has questions regarding evaluation
    - Heavy weighting in one cluster for three ground motion models
    - Seismic attenuation parameter for three models in one cluster
    - Criteria for overall weighting for clusters not clearly explained

Open Items

- 2.5-2, Incorporate site-specific geologic properties and their uncertainties into the determination of safe-shutdown earthquake (SSE)
  - Dominion plans to determine SSE at hypothetical rock outcrop consistent with NRC guidance and determine transfer function
  - Dominion has provided method to staff, and staff has no questions on it

Open Items

- 13.3-1, Offsite laboratories
- 13.3-2, Orange County emergency notification program
- 13.3-4, Reliance on DOE for plume tracking
- 13.3-5, Various additional details on offsite emergency response measures
- 13.3-7, Guidance and authority for exceeding exposure limits
- 13.3-8, Capabilities of hospital and emergency services
- 13.3-9, Qualification for directors of emergency response
- 13.3-10, Cross-references to NUREG-0654 Supplement 2 and review of Orange County emergency response program

Applicant has provided information to address the above open items, and staff has no additional questions on them

Open Items

- 13.3-3, Adequacy of technical support center, emergency operations facility, and operational support center
  - Applicant does not plan to provide details on these subjects and plans to withdraw request for the associated major feature
- 13.3-6, Additional information on evacuation time estimate (ETE)
  - Applicant referenced existing NAPS ETE
  - Staff has a number of questions on details of the plan
  - Dominion is reviewing document against staff questions

COL Action Items

- Identify/highlight work needed at COL
- Similar to established concept in design certifications
- Regulatory standing under discussion (unlike design certification, not written into a rule)
- Not all-inclusive
- Applicant believes some are unnecessary when already required by regulations
- Specific items in backup slides
- Based on staff's evaluation of open item responses, some of these items may be changed or deleted in FSER

COL Action Items

- 2.1-1, Specific unit locations
- 2.1-2, Agency control of water bodies within exclusion area
- 2.2-1, Hazards of nearby industrial area
  - Currently undeveloped
  - Zoning could permit hazardous operations in future
- 2.2-2, Design-specific interactions between NAPS and new facility
  - Depends on layout and design of new units

COL Action Items

- 2.3-1, Dispersion of radionuclides to control room
- 2.3-2, Release point characteristics and receptor locations for routine release dose computations
- 2.4-1, Restriction on operations posed by low-water conditions
- 2.5-1, Additional soil borings
- 2.5-2, Compare plot plans with subsurface profile and material properties
- 2.5-3, Submit excavation and backfill plans

COL Action Items

- 2.5-4, Evaluate groundwater impact on foundation stability and dewatering plans
- 2.5-5, Perform soil column amplification/attenuation analyses
- 2.5-6, Analyze stability of safety-related structures
- 2.5-7, Provide design-related structural criteria
- 2.5-8, Provide plans for ground improvement
- 2.5-9, Verify average shear-wave velocity of materials underlying containment

COL Action Items

- 2.5-10, Provide more detailed slope stability analysis
- 2.5-11, Provide plans for safety-related slopes
- 13.6-1, Provide designs for protected area barriers

Proposed Permit Conditions

- Should an ESP be issued for the site, NRC staff believes the ESP holder needs to be constrained by these conditions
- Based on staff's evaluation of open item responses, some of these items may be changed or deleted in FSER
- May also reclassify some of these as COL action items
- Dominion plans to identify technical concerns with some of these items

Proposed Permit Conditions

- 2.1-1, Obtain authority to restore site before undertaking limited work activities
- 2.4-1, Maintain minimum separation distance from NAPS SSCs
  - This item likely to be revised based on Dominion's response to open item 2.4-2
- 2.4-2, Maximum water budget
  - Dominion believes minimum lake level is adequate limit

Proposed Permit Conditions

- 2.4-3, Design slopes based on drainage without need for engineered drainage systems that can be blocked
- 2.4-4, Locate safety-related facilities above maximum water level from local intense precipitation
- 2.4-5, Minimum free-surface elevation of UHS
  - This item may be revised based on applicant's response to open item 2.4-6
- 2.4-6, Minimum UHS storage capability
- 2.4-7, Design UHS capacity to address potential for freezing

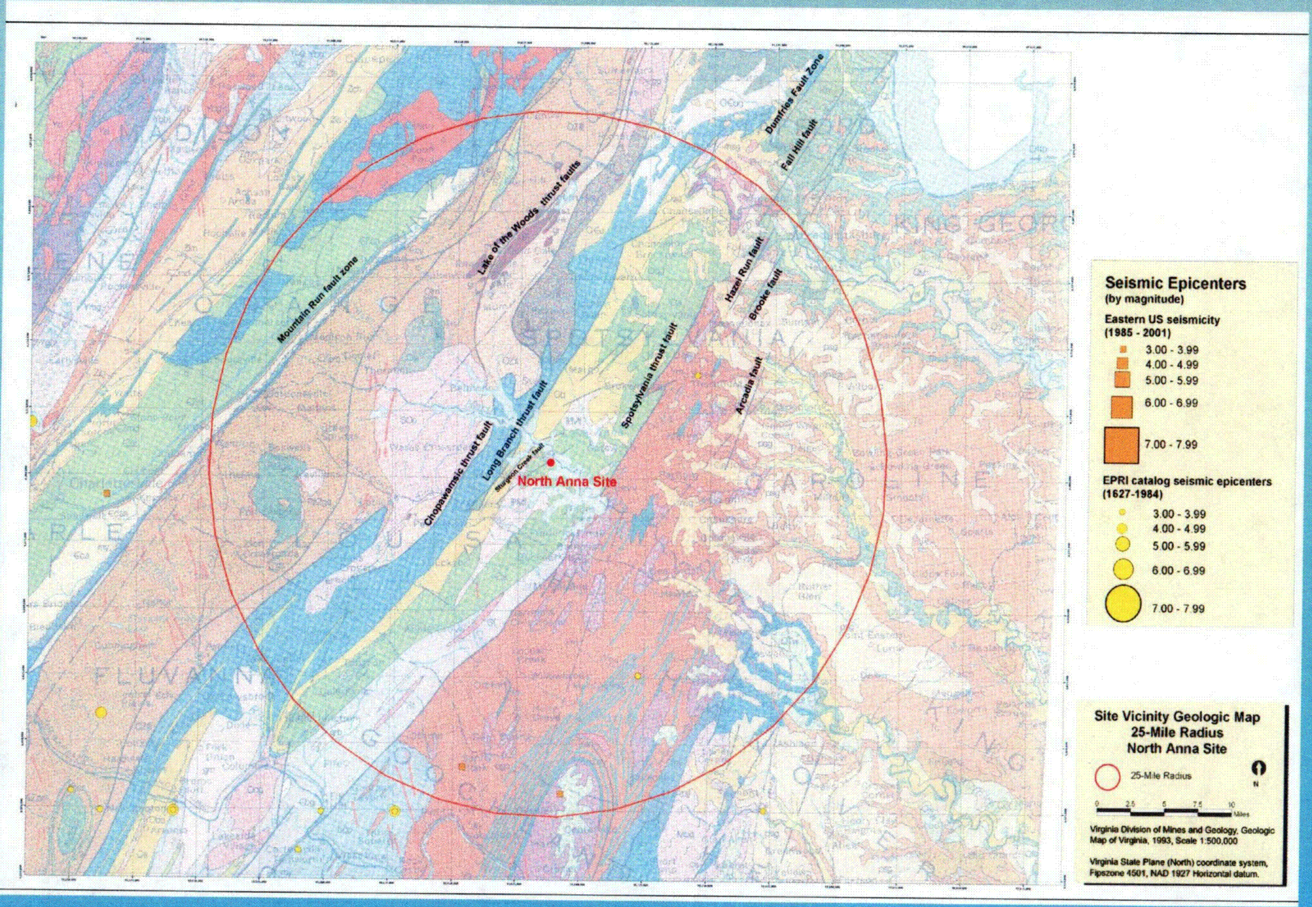
Proposed Permit Conditions

- 2.4-8, No reliance on Lake Anna for safety-related water supply
- 2.4-9, Locate ingress/egress opening for safety-related SSCs above 271 ft MSL
- 2.4-10, Provide erosion protection for slopes at intake
- 2.4-11, No compromise of flood control measures for existing NAPS units during construction of new units
- 2.4-12, Locate new units where ground water level does not exceed 270 ft MSL
  - Dominion believes appropriate condition is distance above water table

Proposed Permit Conditions

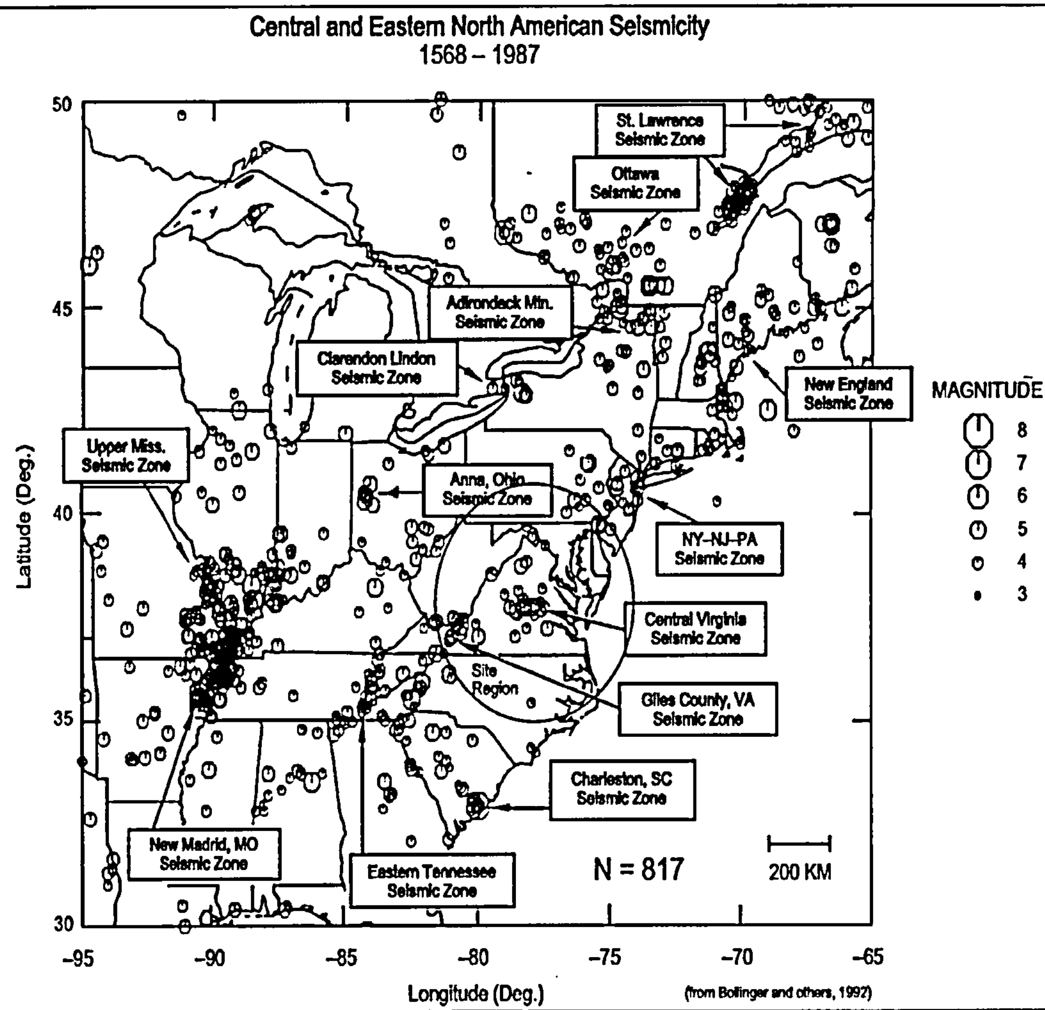
- 2.5-1, Replace fractured/weathered rock at foundations
- 2.5-2, Perform additional borings to identify weathered or fractured rock at foundations
- 2.5-3, Do not use saprolite as engineered fill
- 2.5-4, Perform geologic mapping of future excavations for safety-related facilities
- 2.5-5, Improve Zone II saprolitic soils if locating safety-related structures on them

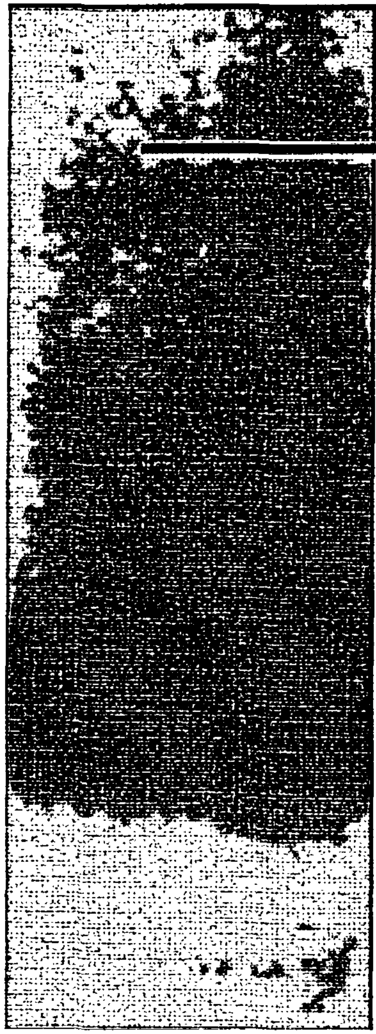
Site Geologic Map



C03

Area Seismic Zones





North Anna Early Site Permit Application

Briefing to
Advisory Committee on
Reactor Safeguards
March 3, 2005

Dominion

Purpose for Submitting North Anna ESP Application

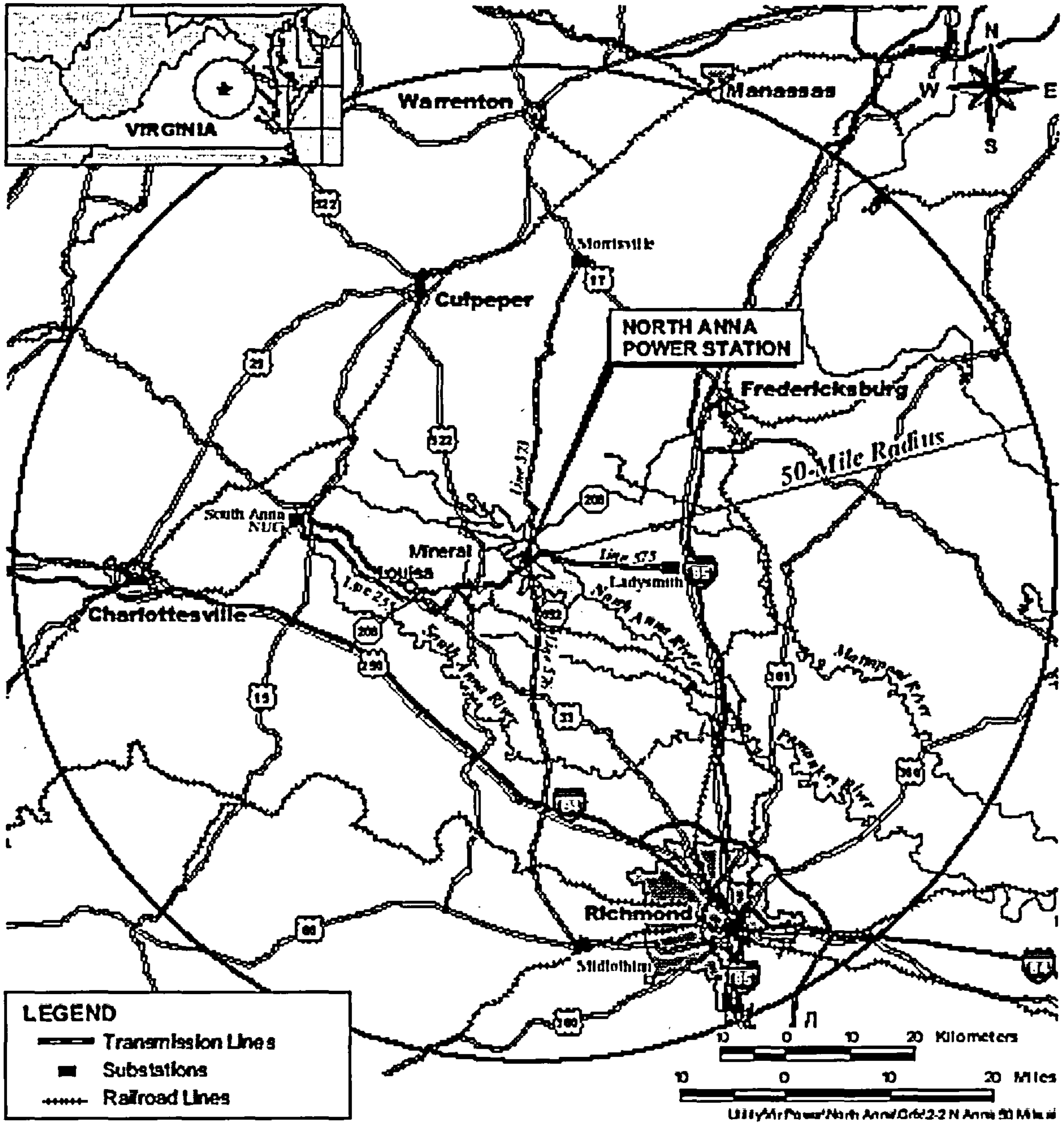
- Determine suitability of a potential site
- Early resolution of siting issues
- Defer technology decision until justified by the business case
- Keep nuclear option open while monitoring and evaluating market conditions

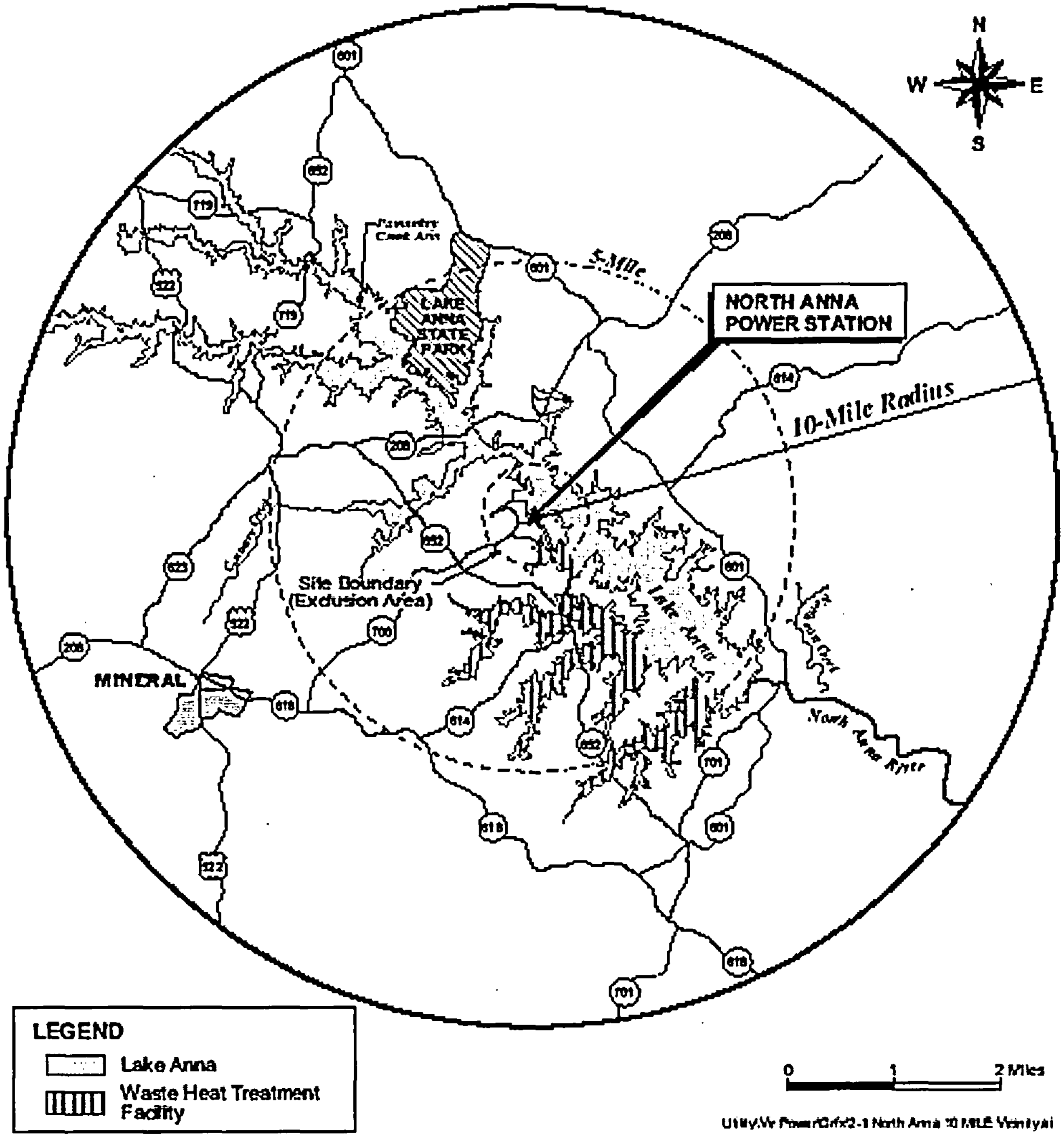
Dominion

North Anna Power Station

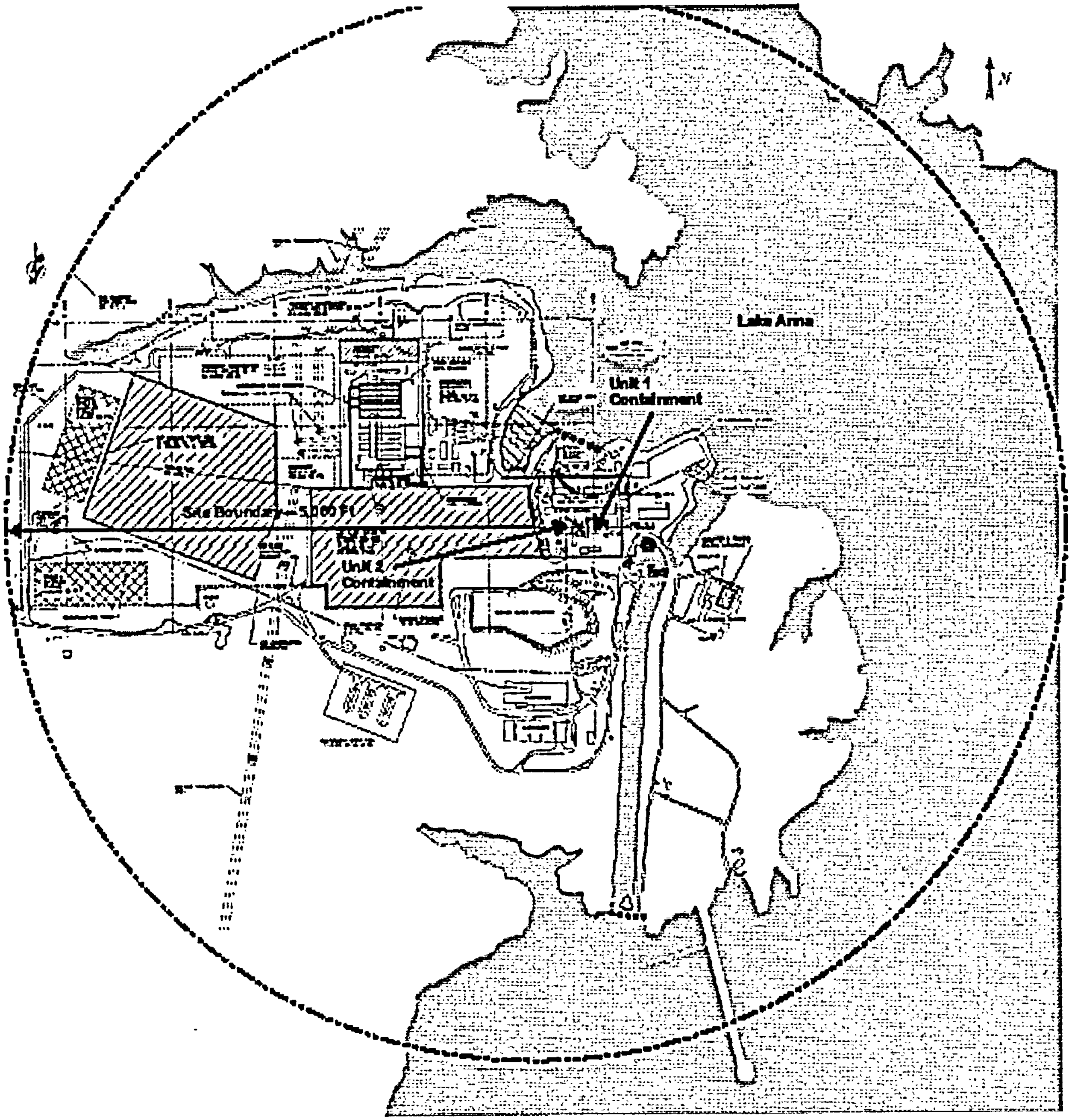
- Originally planned as a four unit site
- Units 1 and 2 actually built
  - Westinghouse 3-loop PWRs
- Operating licenses issued in 1978/1980
- Construction permits issued for Units 3 and 4
- Units 3 and 4 partially constructed
- Units 3 and 4 cancelled and demolished in early 1980's

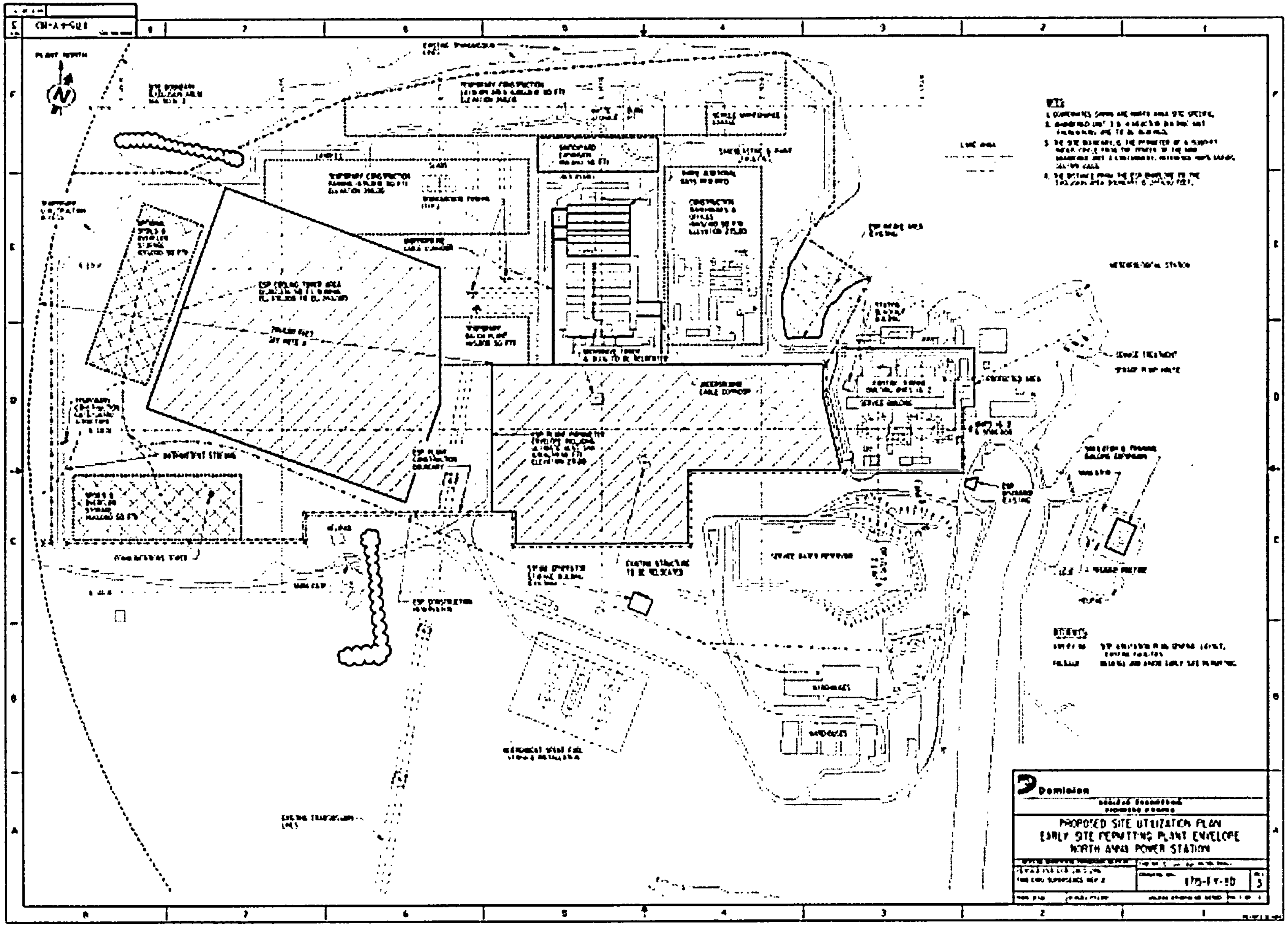
Dominion


VIRGINIA
N
S
E
W
Warrenton
Manassas
Morrisville
Culpeper
NORTH ANNA POWER STATION
Fredericksburg
50-Mile Radius
South Anna NUG
Mineral
Louisa
Line 573
Line 575
Ladysmith
Charlottesville
North Anna River
South Anna River
Mattaponi River
Pamunkey River
Richmond
Midlothian
LEGEND
Transmission Lines
Substations
Railroad Lines
Kilometers
Miles



N
S
W
E
NORTH ANNA POWER STATION
10-Mile Radius
5-Mile
LAKE ANNA STATE PARK
Site Boundary (Exclusion Area)
MINERAL
Lake Anna
North Anna River
601
652
719
208
322
614
623
700
618
701
LEGEND
Lake Anna
Waste Heat Treatment Facility
0
1
2 Miles


Lake Anna
Unit 1
Containment
Site Boundary — 5,000 Ft
Unit 2
Containment
N



PLANT NORTH
SERVICE BUILDING
WAREHOUSES
Dominion
PROPOSED SITE UTILIZATION PLAN
EARLY SITE PERMITTING PLANT ENVELOPE
NORTH ANNA POWER STATION

North Anna ESP Application

Submitted ESP Application	Sept. 2003
Revision 1	Oct. 2003
Revision 2	July 2004
Revision 3	Sept. 2004
NRC Issued Draft SER	Dec. 2004
Response to DSER Open Items	March 2005

Dominion

Plant Parameter Envelope Approach

- Defers technology decision
- PPE defines a set of surrogate plant parameters
- At COL, applicant would demonstrate that chosen design fell within PPE envelope and evaluate anything outside the envelope
- Dominion has proposed a PPE that consists of two 4300 MWt units
- Each unit represents one or more reactor modules, depending on design

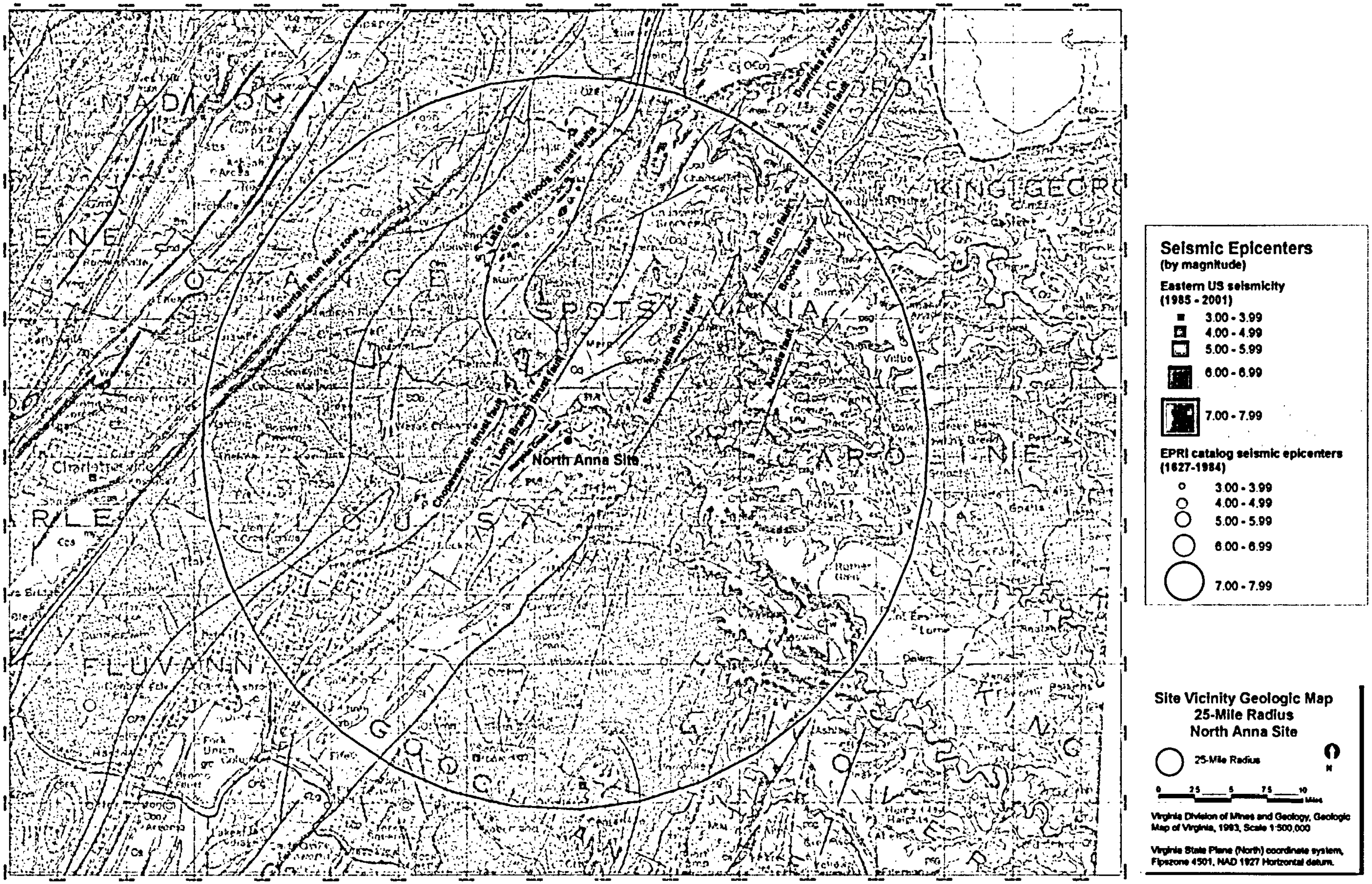
Dominion

Issues of Interest

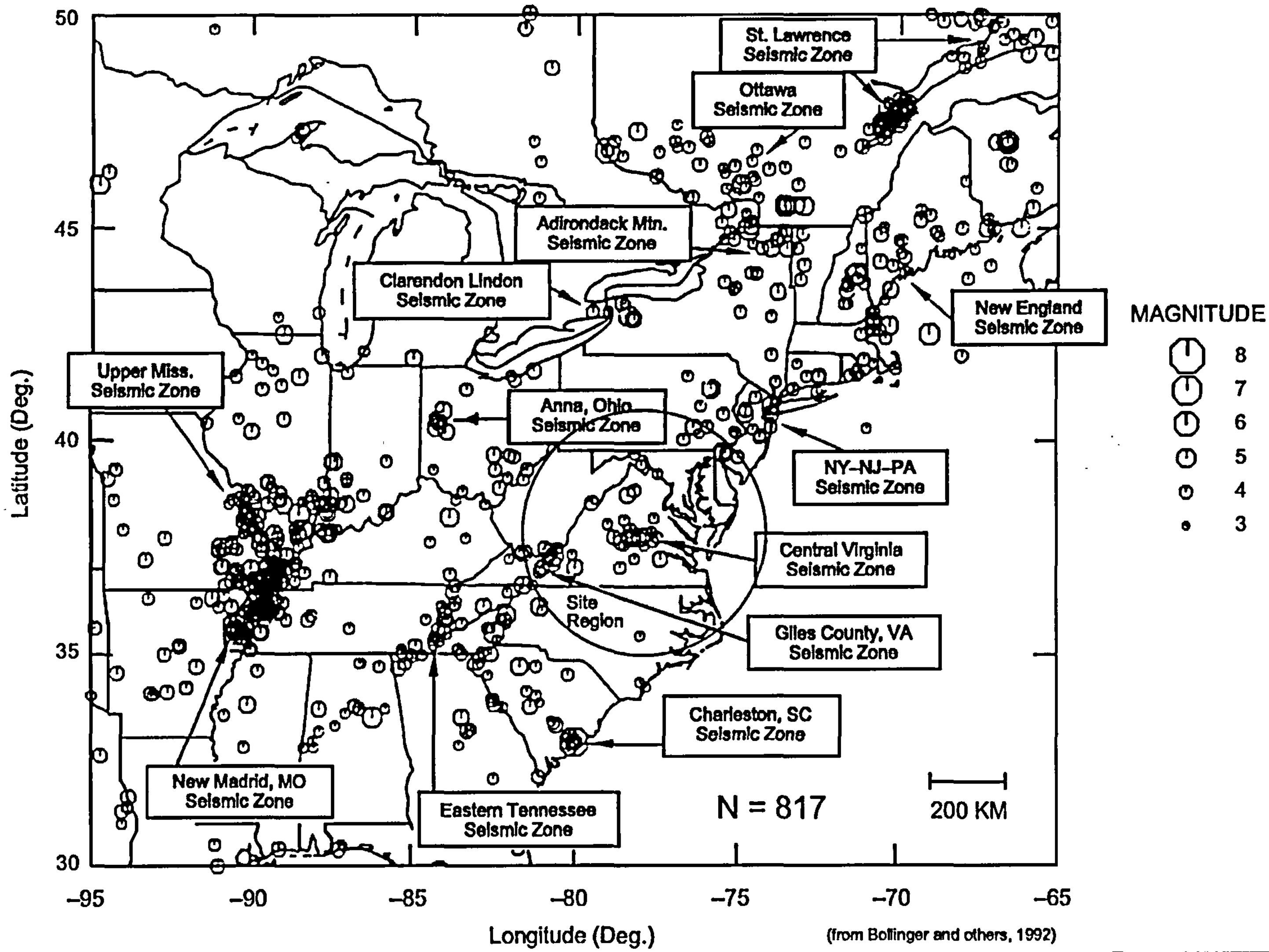
- Resolution for remaining ESP issues appears achievable
  - Seismic
- ESP application relies on existing North Anna emergency plan
- Lake Anna water usage

Dominion

Dominion®


Mountain Run fault zone
Lake of the Woods thrust faults
Long Branch thrust fault
Chopawamsic thrust fault
Spotsylvania thrust fault
Hazel Run fault
Brooke fault
Arcadia fault
Fall Hill fault
Dumfries Fault Zone
North Anna Site
SPOTSYLVANIA
KING GEORGE
CAROLINE
FLUVANNA
Seismic Epicenters
(by magnitude)
Eastern US seismicity
(1985 - 2001)
3.00 - 3.99
4.00 - 4.99
5.00 - 5.99
6.00 - 6.99
7.00 - 7.99
EPRI catalog seismic epicenters
(1627-1984)
3.00 - 3.99
4.00 - 4.99
5.00 - 5.99
6.00 - 6.99
7.00 - 7.99
Site Vicinity Geologic Map
25-Mile Radius
North Anna Site
25-Mile Radius
N
0 2.5 5 7.5 10 Miles
Virginia Division of Mines and Geology, Geologic Map of Virginia, 1993, Scale 1:500,000
Virginia State Plane (North) coordinate system, Fipszone 4501, NAD 1927 Horizontal datum.

Central and Eastern North American Seismicity
1568 – 1987



(from Bollinger and others, 1992)

Changes to Proposed Rule Risk-Informing 10 CFR 50.46

Briefing for ACRS
Richard Dudley, NRR Rulemaking Section
March 3, 2005

ACRS Comments

- Maintain mitigation of accidents up to largest DEGB
- For TBS consider single-ended vs. double ended break
- Additional work needed to quantify risk benefits of a smaller TBS

3/2/2005 2:59 PM 2

Changes to Proposed Rule

- TBS break now single-ended
  - Gary Hammer
- Studies initiated on risk benefits of smaller break size
  - Ralph Landry
- Changes in risk assessment requirements
  - Mike Tschlitz

3/2/2005 2:59 PM 3

Proposed Rule Schedule

- Mar. 1, 2005 - Office concurrence/comments
- Mar. 3, 2003 - ACRS full committee meeting
- Mar. 10, 2005 - Resolve any open issues
- Mar. 11, 2005 - ACRS letter (tentative)
- Mar. 23, 2005 - Proposed rule pkg. to EDO
- Mar. 31, 2005 - Proposed rule to Commission

3/2/2005 2:59 PM 4

Planning Schedule*

- May 31, 2005 - SRM from Commission (estimate)
- June 2005 - Publish proposed rule in FR
- June 30, 2005 - Complete first draft of Reg guide
- Summer 2005 - Initiate discussion on Reg guide with ACRS subcommittee
- Late summer/early fall 2005 - Publish Reg guide for comment (75 days)

*** Dates for planning purposes only based on typical rulemaking schedules**

3/2/2005 2:59 PM 5

Planning Schedule* (cont.)

- Sept. 2005 - Proposed rule comment period ends
- Fall 2005 - RG public comments due
- Winter 2005/2006 - Complete final rule package and Reg guide
- Winter/2006 - Meet with ACRS on RG & final rule
- Spring 2006 - Final rule to Commission

*** Dates for planning purposes only based on typical rulemaking schedules**

3/2/2005 2:59 PM 6

Selection of Transition Break Size for Risk-Informing 50.46 ECCS

Briefing for ACRS

Gary Hammer, NRR/DE

March 3, 2005

Background

- Staff met with ACRS subcommittee on October 28, 2004 and with full committee on December 2, 2004
- Staff outlined the basis for the TBS selection
  - Used expert elicitation LOCA frequencies.
  - Uncertainties and sensitivities included.
  - Adjustments considered to account for other LOCA frequency contributions.

Background (cont)

- Selected TBS ultimately based on the sizes of the pipes attached to the RCS main loop.
  - Attached piping has 95th percentile break frequency of about 1E-5/RY.
  - Piping larger than this is the main loop piping which has a much smaller frequency of a double-ended guillotine break.
- TBS was postulated as a double-ended break at the limiting location.

3/2/2005 2:59 PM 3

Recent Evaluation of TBS

- Recently the staff studied two issues regarding the TBS selection:

- Can the size of the TBS vary with respect to location?
  - Staff could not quantify differences in frequencies between complete breaks in attached piping and same size partial breaks in main loop piping.
  - Staff concluded that same TBS based on size of largest attached pipe should be applied to all locations.

3/2/2005 2:59 PM 4

Recent Evaluation of TBS (cont)

- Does the TBS need to be modeled as a double-ended break?
  - Current regulations require that the design-basis LOCA be based on a double-ended rupture of the largest pipe in the RCS.
  - Rupture of some pipes ≤ TBS in size (i.e. PWR pressurizer surge line and BWR reactor recirculation lines) result in a double-ended discharge.
  - However, the effects of TBS size breaks are essentially bounded by modeling the breaks as single-ended.
  - Also, expert elicitation estimates were based on single-ended breaks.

3/2/2005 2:59 PM 5

TBS for the Proposed Rule

- TBS to be based on the size of the largest pipe attached to the main loop RCS piping.
- TBS to be applied to the limiting location in the RCS.
- TBS to be modeled as a single-ended break.

3/2/2005 2:59 PM 6

Risk-Informing 50.46 Safety Benefits Calculations

Briefing for ACRS

Ralph Landry, NRR/DSSA

March 3, 2005

Overview

- Reactor Coolant System Calculations
  - Performed by industry and NRC
- Containment Calculations
  - Performed by industry and NRC
- PRA Estimates
  - Performed by industry

Reactor Coolant System

- Breaks to be Analyzed – 5 break cases
  - Worst case SBLOCA
  - Hot Leg – Pressurizer surge line area
  - Cold Leg – Accumulator/SI line area with break placed at bottom of CL pipe
  - Cold Leg – Accumulator/SI line area ±20%
- The above breaks with normal EDG delay (10 sec) and additional delay (60 sec)

3/2/2005 2:59 PM 3

Containment

- Generic GOTHIC model
- CONTAIN model derived from GOTHIC model
- Look at effect of varying spray actuation – extension of RWST to Sump switchover

3/2/2005 2:59 PM 4

PRA

- Adjust containment spray timing and flow
  - Conserve RWST inventory
  - Reduce debris wash down and improve pump NPSH from sump
  - Extend time for operator action for switchover to recirculation
- Improve EDG reliability
  - Longer start times
  - Less demanding load sequencing

3/2/2005 2:59 PM 5

Schedule

- Calculations to be completed in May 2005 to support the draft regulatory guide

- Results and insights will be discussed with the appropriate ACRS subcommittee as they are available

3/2/2005 2:59 PM 6

Changes to Risk-Informing 50.46 Draft Proposed Rule Language Risk Assessment

Briefing for ACRS

Michael Tschiltz, SPSB-NRR

March 3, 2005

Changes to Rule Related to Risk Assessment

- Late Release Frequency (LRF) no longer included as risk metric with a specific acceptance criteria
- Cumulative tracking of risk associated with inconsequential changes no longer required
- Reduced level of detail in RG 1.174 related requirements in 10 CFR 50.46a
- Acceptance of Bundling Related / Unrelated Changes

Late Release Frequency

- LRF acceptance criteria removed from proposed rule
- Proposed rule was revised to clarify that for changes that impact containment performance the assessment of the increase in the probability late containment failure will be required
- LRF will be evaluated when considering defense-in-depth.

3/2/2005 3:00 PM 3

Inconsequential Changes

- Inconsequential changes will be individually evaluated (cumulative tracking of inconsequential risk increases will not be required)
- Where feasible, quantitative methods should be used.
- Proposed guideline for quantifiable changes is that each change should result in a CDF increase less than 1E-7/year and a LERF increase less than 1E-8/year.
- Qualitative methods may be sufficient to demonstrate that some changes lead to an inconsequential risk increase.
- Each 24 months, the licensee must submit a short description of all inconsequential changes since the last report

3/2/2005 3:00 PM 4

Reduced Level of Detail in RG 1.174 Related Requirements

- Guidance in RG 1.174 is not legally enforceable
- Proposed §50.46a rule should include a minimum level of legal requirements
- The draft proposed rule includes only high level criteria that deal with
  - PRA scope and quality
  - Risk acceptance criteria
  - Reporting requirements

3/2/2005 3:00 PM 5

Bundling will be Permitted

- "Bundling" refers to combining changes un-related to 50.46a together with changes enabled by 50.46a
- Allowing Bundling
  - Results in either
    - net decrease in risk
    - smaller overall increase in risk
  - Encourages licensees to take advantage of opportunities to reduce risk

3/2/2005 3:00 PM 6

Limitations on Bundling

- If a change were necessary to bring a facility into compliance with NRC regulations, it could not be bundled
- Changes that are Bundled together must not
  - Increase the risk from significant accident sequences
  - Cause lower ranked accident sequences to become significant
  - Create new significant accident sequences

3/2/2005 3:00 PM 7

Revised Draft NUREG on Estimating LOCA Frequencies through the Elicitation Process

Robert L. Tregoning
Lee Abramson
Carolyn Fairbanks
RES

Paul Scott
Battelle

520th ACRS Meeting
March 3, 2005

Presentation Objectives

- Identify major changes to Draft NUREG Report, "Estimating Loss-of-Coolant Accident Frequencies through the Elicitation Process."

- Discuss ACRS comments (ML04350369) and staff response (ML050240436) with respect to letter from M.V. Bonaca to N.J. Diaz, "Estimating Loss-of-Coolant Accident Frequencies through the Elicitation Process," dated December 10, 2004.

- Request ACRS letter for proceeding with public comment for draft NUREG report.

Previous ACRS Briefings and Recent Program Milestones

- Previous ACRS briefings.
  - December 2004: Main Committee on draft NUREG.
  - November 2004: RPP Subcommittee on draft NUREG.
  - July 2004: Main Committee on results, sensitivity analyses and use of results for transition break size selection.
  - March/April, 2004: RPP Subcommittee and Main Committee on expert elicitation results.
  - November, 2003: RPP Subcommittee on expert elicitation approach and base case development.
  - July, 2003: Main Committee on the status and approach of expert elicitation.
  - May, 2002: Combined M&M, THP, R&PRA subcommittee briefing on interim LOCA frequency elicitation and LOCA break size redefinition plans.
  - June, July, November, 2001: Overviews of LOCA frequency and break size redefinition effort provided to outline its importance within 10 CFR 50.46 revision framework.
  - March, 2001: Technical issues necessitating LOCA reevaluation.

- Program milestones since December 2004.
  - Completed draft NUREG including responses addressing ACRS comments.
  - Submitted draft NUREG for NRR and ACRS review.

ACRS Comments from November 2004

1. The report should include a better explanation of what a generic frequency value for the fleet of plants means and to what extent plant-to-plant variability affected the results.
2. The report should state clearly what the understanding of the experts was when they answered questions about LOCA size categories.
3. This practice (geometric averaging) is at variance with the methods employed in References 5-7, in which the arithmetic method is applied to the probability distributions of the experts.
4. The final distribution reported in the Executive Summary should be the composite distribution that the analysts, based on the sensitivity analyses, believe represents the expert community's current state of knowledge regarding LOCA frequencies.

Changes to (11/04) Draft NUREG

- Sections were re-lettered.
- Sections with no changes or minor changes:
  - Section A – Background
  - Section B – Objective and Scope
  - Section D – Base Case Results
  - Section F – Qualitative Results and Discussion
  - Section H – Ongoing Work
- Section C – Elicitation Approach
  - Added discussion to clarify definition of LOCA categories in Section C.7. (ACRS Comment #2)

Changes to (11/04) Draft NUREG: Section E

Analysis of Elicitation Responses

- Analysis sections completed to reflect prior quantitative results (Section G).
  - Section E.3.4 (Sum of Distributions)
  - Section E.3.4.1 (Calculation of the Mean)
  - Section E.3.4.2 (Calculation of the Variance and Percentiles)
- New sections describing additional or modified sensitivity analyses.
  - Section E.6.1 (Mean Determination)
  - Section E.6.3 (Correlation Structure)
  - Section E.6.4.3 (Aggregation Parameters)
  - Section E.6.4.4 (Mixture Distribution Aggregation)

Changes to (11/04) Draft NUREG: Section G

Quantitative Results

- Section G previously reflected the current analysis methodology.
- Sections added to reflect additional/modified sensitivity analyses:
  - Section G.6.1 (Mean Determination)
  - Section G.6.3 (Correlation Structure)
  - Section G.6.4.4 (Mixture Distribution Aggregation)
  - Section G.8 (Summary Results)
- Revised summary results based on overconfidence adjustment using the error factor scheme.
  - Improved group LOCA frequency estimates
  - Summary results utilized in Executive Summary.
  - Comparisons with historical results with respect to revised summary estimates.

Changes to (11/04) Draft NUREG: Abstract, Conclusions and Exec. Summary

- Executive Summary
  - The table and figure results now reflect the revised summary results. (ACRS Comment #4)
  - Clarifies what is meant by generic frequencies. (ACRS Comment #1)
  - Summarizes the rationale for using the geometric mean and why mixture distribution aggregation is not appropriate for the actual elicitation results. (ACRS Comment #3)
  - Clarifies that the study results are designed to best represent the expert panel's current state of knowledge regarding LOCA frequencies.

- Abstract and Conclusions
  - Modified to reflect current executive summary.

ACRS Comments

- ACRS Comment #1
  - The report should include a better explanation of what a generic frequency value for the fleet of plants means and to what extent plant-to-plant variability affected the results.
- Staff response
  - Expert panel instructed to develop generic/average values.
  - Panel considered the service history for the entire population of plants.
  - Only factors that impact a large number of plants can significantly affect the average.
  - Therefore, the panel was instructed to account only for broad plant-specific factors and not plant-to-plant variability.
  - Executive Summary clarified to reflect this comment.

ACRS Comments

- ACRS Comment #2
  - The report should state clearly what the understanding of the experts was when they answered questions about LOCA size categories.
- Staff response
  - Key technical terms, including LOCA size categories, were defined during the elicitation process.
  - LOCA size categories defined as cumulative frequencies at a given flow rate; flow rates then converted to flow areas using simple correlations. Flow areas converted to an equivalent break diameter.
  - Each LOCA size category represents the cumulative frequency of a single-ended break of the cited size, and all larger breaks (including DEGB) of that size and larger pipe.
  - Section D clarified to reflect this comment.

ACRS Comments

- ACRS Comment #3
  - This practice (geometric averaging) is at variance with the methods employed in References 5-7 (NUREG-1150, EPRI Report NP-4726, NUREG/CR-6372) in which the arithmetic averaging method is applied to the probability distributions of the experts.
- Staff response
  - Fundamental consideration in this elicitation was to aggregate such that the final results represent the opinions of the panel as a whole.
    - Outlined this philosophy to the experts.
    - Consensus-type estimate (near center of individual opinions).
    - Geometric mean aggregation satisfies consideration.
    - This philosophy was endorsed by the decision analyst on the external peer review panel.

ACRS Comments

- Staff response to ACRS comment #3, continued.
  - Alternative aggregation methods investigated are consistent with Ref. 5-7 approaches.
    - Mixture distribution and arithmetic mean techniques.
    - Neither technique provides a consensus-type estimate.
    - Outlier opinions significantly affect estimates.
  - Large differences in results due to choice in aggregation methods.
    - Frequency estimates utilized in any application should reflect risk implications.
    - User has best understanding of risk implications.
    - TBS selection in 50.46 was appropriately cognizant of frequency differences resulting from aggregation methods.
    - Geometric mean (GM) aggregation may be more appropriate for applications which require "best estimate" results.

ACRS Comments

- ACRS Comment #4
  - The final distribution reported in the Executive Summary should be the composite distribution that the analysts, based on the sensitivity analyses, believe represents the expert community's current state of knowledge regarding LOCA frequencies.
- Staff response
  - Elicitation did not attempt to determine the state of knowledge of the expert community.
    - The study represents the expert panel's current state of knowledge regarding LOCA frequencies for the stated study objectives. (Executive Summary revised).
    - Cannot claim that the study represents the state of knowledge of the expert community.
    - Personal opinions were sought, not their assessment or perception of the expert community's opinion.

ACRS Comments

- Staff response to ACRS Comment #4, continued.
  - However, panel selection was designed to represent broad organizational, experiential, and international differences within the community.
    - Panel carefully chosen to obtain relevant diversity.
    - The diversity of the experts was intended to encompass the full breadth of views in the expert community.

Summary

- Draft NUREG on expert elicitation has been extensively reviewed.
  - Expert panelists.
  - External peer review.
  - ACRS review.
  - Internal staff review.

- Important to ensure that NUREG is available concurrently with proposed 10 CFR 50.46 rule and statement of considerations.

- Request ACRS letter for proceeding with public comment for draft NUREG report.